12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 316

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CHAPTER 13

Natural classifications, realism, and psychiatric science

Chapter contents

Session 1 **Hempel and two New Agendas for psychiatric classifications** *320*

Session 2 **Values, natural classifications, and the Absolute Conception** *341*

Session 3 **Scientific realism in physics** *352*

Session 4 **The third New Agenda – an Agenda modelled on the philosophy of physics** *364*

Conclusions: being about what science is about *376*

Reading guide *378*

References *379*

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 317

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A classification is a way of seeing the world at a point in time.
Norman Sartorius (1992, in the Preface to ICD-10, p. vii)[1]

This chapter is about what science is about. It is about the real world 'out there' as distinct from our ideas about what is there. It is about what there *is* as distinct from our *beliefs* about what there is. It is about the objects—the entities and events—of which the real world is made up as distinct from the shapes that from this or that perspective we impose on the world. It is about what in philosophy has come to be debated by way of such contrasts as *objectivity* versus *subjectivity, realism* versus *idealism, essentialism* versus *nominalism*. (See Box 13.1 for brief initial definitions of realism and related terms.)

Natural classifications and what science is about

Classification, although of course not unique to science, is fundamental to the processes by which science seeks to find out what the world is really like. Classification lies at the heart of prediction and explanation. New phenomena are judged to be of the same kind as previously encountered phenomena and can thus be expected to behave in relevantly similar respects reflecting similar underlying causal mechanisms.

Scientific classifications come in many different forms. There can be groupings into rigid and mutually exclusive kinds, groupings on the basis of a number of possibly overlapping factors, groupings on the basis of mathematical quantities. But of any of these varieties of scientific classification we can ask: is it indeed a *natural* classification, a classification that, reflecting real, objective features of nature, provides insights into genuine causal mechanisms as a basis for explanation and prediction? Or is it, despite scientific appearances, an artificial classification that may be useful for us but that expresses *our* perspectives on the world as distinct from the nature of the world as it really is?

Natural classifications and what psychiatry is about

A natural classification, then, to adapt Sartorius' (1992) helpful aphorism (above), is the way in which a given science sees the world at a point in time. Small wonder, therefore, given the challenges of psychiatric science outlined in Chapter 11, that psychiatric classifications have been and remain deeply problematic. The two most widely used international psychiatric classifications of mental disorders are chapter V of the World Health Organization's (WHO) *International Classification of Disease* (current edition, ICD-10, WHO, 1992) and the American Psychiatric Association's *Diagnostic and Statistical Manual* (current edition, DSM-IVTR, American Psychiatric Association, 2000–TR stands for Text Revision; so this edition is DSM-IV, published in 1994, with mostly minor revisions). Corresponding,

[1] Chapter V, Mental and behavioural disorders, of the 10th Edition of the World Health Organization's *International Classification of Diseases* (ICD-10) is available in several different versions for different purposes. The version from which this quote is taken, *Clinical descriptions and diagnostic guidelines*, is the version used for general clinical and educational purposes. It is also the basis of all the other versions (WHO, 1992).

Box 13.1 Philosophical terminology: realism and related terms

The debate about the status of psychiatric classification is related to long-standing debates about realism throughout the history of philosophy. If you are new to these philosophical debates, the variety of terms and distinctions in this area can be somewhat bewildering. This is not helped by the fact that the terms and distinctions in question are sometimes used inconsistently. We give here a few illustrative examples (see also Reading Guide).

One early philosophical debate was concerned with properties:

- *Realists* argued that there really were universals—in the form of general abstract properties—out there in nature which were merely instantiated or realized in the properties of particular things (or 'particulars'). Thus the colour of a British post-box might be an instance of the universal (the general abstract property) redness. Plato was a realist about universals believing them to inhabit a realm separate from the natural world.
- *Nominalists* (such as William of Occam) argued that there were only particulars. The appearance of universals was a reflection of our linguistic practice of grouping together particular red things (British post-boxes, traffic lights at 'stop', etc.) but really there was nothing further—no general abstract property of redness—out there in nature.

Another early debate concerned the opposition to realism of *idealism*. Here,

- *Realists* (such as Locke, Hume, and other seventeenth century philosophers) denied idealism.
- *Idealists* argued that the world, or at least some central features of it, depended on our ideas of it rather than the other way round. Idealism is thus connected to the central role of ideas in seventeenth century British Empiricist theories of mind (see Part V). Thus a quick way of generating a form of idealism is to take the underlying realism implicit in Empiricist talk of people having ideas and impressions *of* the world and simply cancel the world out. Very roughly this was the eighteenth century philosopher, Bishop George Berkeley's strategy. It raises questions, among others, about who the subject of the ideas is. Kant (see Chapters 12 and 22) argued that how the world appeared to us, including its being spatially and temporally ordered, were products of the observing mind; however, he distinguished between the *empirical* self with which we normally identify ourselves and a *transcendental* self whose unknowable workings were responsible for this conceptual structuring (again, see Chapter 12).

Realism has more recently been opposed to other forms of antirealism such as antirealism about the past and the future,

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 318

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about mathematics and about scientific theories. As we will see in Session 3, there are really a number of distinct debates here. One important distinction is between an *ontological* debate about realism and antirealism, and an *epistemological* debate. Ontology is about *what* is there. Epistemology is about what in principle we can *know* about what is there (i.e. in principle as distinct from any contingent limitations arising from, e.g. the limits of current instrumentation).

however, with the variety of alternatives to the psychiatric way of seeing the world of mental distress and disorder that we explored in Part I, there are competing classifications—psychological, psychoanalytic, and so forth. There are also those who, consistently with the view that mental disorders are really problems of living (also examined in Part I), reject outright psychiatry's medical-scientific way of seeing the world (Kutchins and Kirk, 1997).

Natural classifications and the validity of what psychiatry is about

Even, though, among those most committed to psychiatry's medical-scientific way of seeing the world, there are continuing debates about both the form and content of psychiatric classification. Many of these debates are authoritatively drawn together in *A Research Agenda for DSM-V* edited by three key figures in the DSM, the North American psychiatrists, David Kupfer, Michael First, and Darryl Regier (2002). We will be looking at the *Research Agenda for DSM-V* in detail in Session 1 of this chapter. But as Kupfer *et al*. point out in their Introduction, while psychiatry has seen dramatic improvements over the last three decades in the reliability of its classifications, and hence in their utility for purposes of communication, there has been little in the way of corresponding improvements in validity. Again, we return to these three key issues for classification—reliability, utility, and validity—in Session 1 (they are also defined briefly in Box 13.2). But the starting point of this chapter is that within psychiatry itself, as a medical-scientific discipline, there is deep concern about whether current psychiatric classifications are scientifically valid, and, hence, whether they are, in the sense of the term outlined above, *natural* classifications. Kupfer *et al*. put current concerns about validity at the very heart of their *Research Agenda for DSM-V*:

> 'Those of us' they say at the end of the first paragraph of their Introduction, 'who have worked for several decades to improve the reliability of our diagnostic criteria are now searching for new approaches to an understanding of etiological and pathophysiological mechanisms—an understanding that can improve the validity of our diagnoses and the consequent power of our preventive and treatment interventions'
>
> Kupfer *et al.* (2002, p. xv)

Strategy of the chapter

In this chapter we will be tackling the problems of validity presented by psychiatric classification, not directly, as they are set out in

Box 13.2 Psychiatric terminology: utility, reliability, and validity

Three terms are widely used in debates about the merits or otherwise of medical and psychiatric classifications. In descending order of difficulty of definition, they are utility, reliability, and validity:

- *Utility* is the extent to which a classification is 'good for purpose'. Easy to define, this concept is behind much of the debate about psychiatric classification, in part because of the diversity of purposes to which it is put, in part because of the diversity of values by which, as outlined in Part I, psychiatry itself is characterized.
- *Reliability* is the degree of agreement between users of a classification. Two main kinds of reliability are usually recognized. *Interobserver* or *inter-rater* reliability is the extent to which two or more observers agree about the classification of a given case on a given occasion. *Test–retest* reliability is the degree of agreement by a given observer in his or her classification of a given case over a period of time.
- *Validity* is a more complex notion. As noted in the text, broadly speaking the validity of a scientific classification is the extent to which it reflects the real world (or that aspect of the real world with which the science in question is concerned). Thus an alchemist's classification of chemical elements might have been very reliable, but it would have been a less valid classification in this sense than the chemical periodic table.

However, this 'absolute validity', as it might be called, is beyond the reach of many sciences, including psychiatry. Hence, 'lesser validities' are adopted, one or more of which might reasonably be taken to be a sign, albeit second hand, of absolute validity. In the psychiatric literature at least four kinds of lesser validity are distinguished (there are many others, e.g. concurrent validity, discriminantability, and, in the psychological literature a whole different approach through statistical tests.

- *Face validity*: the extent to which a classification appears to be of relevant features (which has consequences for the acceptability of a classification to its users; see e.g. Rust and Golombok, 1989, p. 78).
- *Construct validity*: roughly, the extent to which a classification relates to underlying theory. Thus, one commentator defines this form of validity as 'the extent to which the test may be said to measure a theoretical construct or trait' (Anastasi, 1968, p. 114). In medicine, reflecting the 'theory' that a patient's symptoms reflect an underlying pathophysiological causal mechanism, specific kinds of construct validity are often proposed. Thus in a classic paper, the American psychiatrists, Robins and Guze, emphasize the

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importance of family aggregation, reflecting presumed genetic factors (Robbins and Guze, 1970).

- *Predictive validity*: the extent to which the classification allows us to predict future properties: in medicine, these include prognosis (course and outcome) and response to treatment.
- *Content validity*: in respect of psychiatric diagnosis, Kendell (see Chapter 1) defines this as 'the demonstration that the defining characteristics of a given disorder are indeed enquired into and elicited before that diagnosis is made' (Kendell, 1975, p. 40).

The chemical periodic table, to return to our example above, has a high degree of validity in all these respects, it is prima facie relevant (face validity), it is strongly related to underlying theory (it has good construct validity, in the explanation of chemical properties based on electron 'shells'), it has high predictive validity (even to the extent of anticipating the existence of new elements), and it has good content validity (chemists really do enquire into the defining characteristics of an element before assigning it to a given class).

the *Research Agenda for DSM-V*, but indirectly, i.e. by exploring the potential relevance to these problems of a (far from exhaustive) selection of strands of work from the philosophy of science.

Brief plan of the chapter

The chapter is divided into four sessions dealing, respectively, with: (1) the influence of Logical Empiricism on current psychiatric classifications; (2) Bernard Williams' concept of an Absolute Conception and the significance of the traditional division between primary and secondary qualities for the place of values in a scientific classification; (3) recent philosophical work on realism in the philosophy of physics; and, finally, (4) the relevance of all these topics for the future development of scientific classifications of mental distress and disorder.

More detailed plan of the chapter

This is a relatively long chapter covering a good deal of new ground, both philosophical and psychiatric, so we will set out the overall line of argument briefly before starting on Session 1. Thus:

- *Session 1: Hempel and two New Agendas for Psychiatric classification*. We show, first, that the APA's current *Research Agenda for DSM-V* includes an embedded, albeit largely implict, series of conceptual difficulties, that, together, make up the first of our new agendas, a ***Conceptual*** *Research Agenda for DSM-V*. The session then picks up on the themes of Chapter 12 by showing how developments in psychiatric classification over the last 50 years, culminating in and including the APA's *Research Agenda for DSM-V,* have been influenced by a core feature of Logical Empiricism, namely the separation of observational (or descriptive) statements from statements of theory. The standard story has been that Logical Empiricism influenced psychiatric classification directly through the work of one of its chief exponents, the American philosopher, Carl Hempel (we met Hempel in Chapter 12, of course). However, as we will see, there is a surprising twist to this story, a twist that will lead us to the second of our new Agendas for psychiatric classification, an agenda that we will call the *Conceptual Research Agenda for ICD-9*.
- *Session 2: Values, natural classifications, and the Absolute Conception*. Logical Empiricism, however, as we saw in Chapter 12, ultimately failed as a rigorous characterization of the traditional model of science. Hence in Session 2 we consider a more recent attempt to characterize the traditional model, Bernard Williams' 'Absolute Conception'. In the event, although superficially promising, this and other similar strands of work in the philosophy of science (for example on 'natural kinds', see Reading Guide), turn out not to be directly helpful in relation to the difficulties of psychiatric classification. These strands of work are indirectly helpful, on the other hand, in that they suggest the need for psychiatry to go beyond the traditional model, whether represented by Logical Empiricism or its more recent successors. In particular, arguments against the Absolute Conception, notably by the Pittsburgh-based philosopher John McDowell, suggest that, contrary to the traditional model, there is no obvious 'knock-down' argument why values may not have an entirely proper place in a natural classification.
- *Session 3: Scientific realism in physics*. We do not, however, pursue further the role of values in classification in this part of the book (we return to this topic in Part IV). Rather, in this part we move beyond the traditional model by exploring, in Session 3, a sample of ideas on the nature of science from the philosophy of physics—Van Fraassen's 'Constructive Empiricism', Boyd and McMullen's appeal to the 'inference to the best explanation', Cartwright's 'inference to the most probable cause', Hacking's work on realism and our ability to manipulate things, and Fine's 'Natural Ontological Attitude' (NOA). Despite their perhaps unlikely origins in the philosophy of physics, these strands of philosophical work turn out, in contrast to those considered in Session 2, to be directly illuminating in different ways for psychiatry.
- *Session 4: The third New Agenda – an agenda for an Extended Family of Classifictions*. In our concluding Session 4, therefore, we return to the current problems of validity facing psychiatric classification, and to the implications for these problems particularly of work in the philosophy of science modelled on the example of physics. By setting these implications from the philosophy of physics in context with our two new agendas developed in Session 1, we propose a third new Agenda, an *Agenda for an Extended Family of Psychiatric Classifications*, that, we argue, by analogy with the corresponding family of classifications in physics, will take us a step closer to satisfying current demands for improvements in the validity of psychiatric classifications by more closely reflecting what psychiatry, the *science* of psychiatry, is about.

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Session 1 Hempel and two New Agendas for psychiatric classifications

In this session we will explore a number of key concepts in psychiatric classification by way of a mini 'history of ideas' of the ICD and DSM. But we will be looking forward not back. Our aim will be to identify the lessons from the history of ideas behind the ICD and DSM that will support future developments in scientific classifications of mental disorder.

Plan of the session

As this is a chapter-length session we will give a brief overview of the line of argument as it develops in each section.

With the first section, we start in the here and now with a reading (linked with Exercise 1) from the American Psychiatric Association's *A Research Agenda for DSM-V* (Kupfer *et al*., 2002). The *Research Agenda for DSM-V*, as we noted in the introduction to this chapter, gives an authoritative picture of how, in the terms of Sartorius' (1992) aphorism at the start of this chapter, psychiatry sees the world of mental disorders at this point in time. Focusing in particular on Kupfer *et al*.'s Introduction, we draw out the philosophically-relevant aspects of their Agenda, thus developing the first of our new agendas, a *Conceptual Research Agenda for DSM-V*.

The second section, on *Hempel and the origins of ICD and DSM*, explores the history of ideas behind both DSM and ICD. In this section we jump back over 40 years, to 1959. This was the year of a joint WHO and American Psychopathological Association conference on classification to which the North American philosopher of science, Carl Hempel, made a crucial contribution, and from which, with the publication of ICD-8 in 1967 (WHO, 1967), a decisive shift in psychiatric classification was to be made from a theoretical (aetiology based) to a descriptive (symptom based) structure. A close reading of two passages from Hempel's paper (1961; see readings linked with Exercises 3 and 4) allows us to take a first look at a number of key concepts in scientific (including psychiatric) classifications: descriptive adequacy; Empirical Import; Systematic Import; objectivity; operationalism; and reliability.

These concepts, as set out by Hempel in his paper, have all turned out to be important in the subsequent development of ICD and DSM. But, and this is a big 'but', the direction in which ICD and DSM actually developed post-1959, proved, as we will see, to be very different from the direction anticipated by Hempel. There is a missing link in this story, therefore, which we identify in the next section (on *Hempel and the missing link to ICD and DSM*) with a reading from the transcript of the discussion that followed Hempel's paper.

Having identified the missing link, we are ready to derive a second new Agenda, which we develop in a section on a *Conceptual Research Agenda for ICD-9*. This Agenda is a counterpart of the agenda we developed in the first section for DSM-V. It is the philosophically-relevant Agenda for psychiatric classification as it stood at the time of the publication of the Glossary to ICD-8 in 1974 (WHO, 1974).

With our derived *Conceptual Research Agenda for ICD-9* in place, we then return to Hempel's paper, with a section on *Operationalism, reliability, validity, and a first look at what lies ahead*. As this rather long-winded title suggests, we look in detail, and at times critically, at Hempel's Logical Empiricist account of some of the concepts that have been particularly influential in the development of ICD and DSM through successive editions from ICD-8 to Kupfer *et al*.'s *Research Agenda for DSM-V* (2002). This suggests a number of preliminary ideas on the future development of psychiatric classification to which we will be returning in Session 4 of this chapter, after, as we indicate in a brief concluding section to this session, exploring in Sessions 2 and 3 of the chapter, various aspects of post-Logical Empiricist philosophy of science relevant to psychiatry.

The first new agenda – a conceptual research agenda for DSM-V

In this first section, then, we focus on the American Psychiatric Association's *A Research Agenda for DSM-V*. Published in 2002, it was edited as we noted in the introduction to this chapter, by three key players in the DSM, David Kupfer, Professor and Chair of Psychiatry at the University of Pittsburgh, Michael First, Associate Professor of Clinical Psychiatry at the Columbia University College of Physicians and Surgeons in New York, and Darryl Regier, Director of Research in the American Psychiatric Association. Our aim will not be to review the Agenda as it stands: as we will see, it is mainly an (authoritative) agenda for *empirical* research. Our aim, rather, will be to draw out, largely from Kupfer *et al*.'s editorial Introduction, the items on their agenda as they are relevant particularly to the possible contributions of *conceptual* research, alongside the contributions of empirical research, to the future development of psychiatric classifications. It is by drawing out these conceptually-relevant agenda items that we will develop the first our two new agendas for psychiatric classification, a *Conceptual Research Agenda for DSM-V*. This new agenda, although superficially very different from Kupfer *et al*.'s original *Research Agenda for DSM-V*, is, we will argue, already fully implicit within it.

We summarize our derived new agenda, *A Conceptual Research Agenda for DSM-V,* in Table 13.1. We will be referring to the agenda items in Table 13.1 *seriatim* as they emerge from our discussion of Kupfer *et al*.'s original. These agenda items (there are 12 in all) will help to guide you through the discussion. We suggest, however, that, consistently with the 'skills-development' approach of this book, you defer looking at the details of Table 13.1 until you have worked through the two exercises in this section and come to your own views on the conceptually relevant agenda items implicit in Kupfer *et al.'s* original agenda. Remember that, coming to these materials with your own particular background resources of knowledge, skills, and experience, you may well come up with different, and better, ideas than ours.

Kupfer *et al*.'s *Research Agenda for DSM-V,* then, has six main chapters based, in part, on a series of workshops convened jointly

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Table 13.1 The First New Agenda – a Conceptual Research Agenda for DSM-V

		DSM-V (conceptual agenda)
(1)	Time-line	Back to DSM-III Forword to DSM-VI or even DSM-VII
(2)	Uses of psychiatric classification	Diagnosis by clinicians
(3)	Misuses of psychiatric classification	Reification of categories of mental disorder as diseases
(4)	Status of psychiatric science	Deficit model: current classification paradigm is 'limited' because it has proved to be 'almost wholly lacking in validity'
(5)	Concepts	Important: but not named for what they are. Conceptual issues are writ large but called, e.g. 'nomenclature' issues; and conceptual research methods are not mentioned.
(6)	Reliability	Run out of steam: only useful for improving agreement, hence communication between clinicians; as such, may even have worked against validity
(7)	Validity	THE BIG ISSUE: we need scientifically sound systems of classification; this means that they must become 'aetiologically based'
(8)	Structural solution	Aetiology-based: we need a 'fundamentally' new classification paradigm drawing on biological and clinical sciences
(9)	Process solutions	Empirical research: the route to vaildity is by way of empirical research to, (a) enrich the 'empirical database', and (b) facilitate the 'integration of findings' from different areas of biological and clinical research
(10)	Utility (of new classifications)	Better health-care services: a more valid (i.e. aetiology-based) classification will provide more powerful 'preventive and treatment interventions'
(11)	Overall tenor	Downbeat: as above, current paradigm is 'limited' and needs to be 'fundamentally' altered
(12)	Any other business	none

by the American Psychiatric Association and the National Institutes of Mental Health (NIMH—the principal source of Federal research funds for psychiatry in the USA). There were in all 40 North American authors of the six chapters of the *Research Agenda for DSM-V*, who, together with the three editors and two representatives of the NIMH, represented a wide range of expertise in North American psychiatry, psychology, and related clinical and research health-care disciplines. There were also four distinguished non-North American contributors, Bedirhan Üstün, the Coordinator of the ICD Revision Programme at the WHO, Robert Kendell from the UK (this is the Kendell of Part I), Margarita Alegria, Professor of Health Services Administration at the University of Puerto Rico, and Gavin Andrews, a Professor of Psychiatry at the University of New South Wales in Australia. Our first reading is from Kupfer *et al.*'s Introduction.

EXERCISE 1 (20 minutes)

Read the short passage from:

Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. xv–xvii

Link with Reading 13.1

As you read this passage, which comes on the first page of the Introduction, write down a checklist of as many agenda items for the DSM-V as you can identify from what the authors say. The passage includes the short section quoted, in the introduction to this chapter, in which the authors indicate the need for improved validity. So this is one item on their *Research Agenda for DSM-V*. But what do they mean by validity? And what other items on their agenda can you detect from this passage?

As noted above, it is important to write down your own ideas on this before reading on. This is important generally because of the skills-development aims of this textbook, as we have emphasized on many occasions. It is especially important here, in relation to psychiatric science, where, as we will see, we are very much at the cutting edge with no 'established corpus' of received ideas, and where, as Kupfer *et al.* themselves emphasize (see below), we are overdue for a fundamental shift of paradigms. So, your ideas are as likely to contribute to the required shift of paradigms as anyone else's!

In this passage, as noted in the introduction to this chapter, the big issue for the *Research Agenda for DSM-V* is identified by Kupfer *et al.* as the need for improved *validity*. We have included validity as Agenda Item 7 in Table 13.1. In this passage, a valid

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classification is spelled out by the authors to mean one that is an 'etiologically based, scientifically sound classificatory system.'

New agenda items

However, there are a number of further agenda items that we can glean from this passage. Thus,

1. *Reliability* (Agenda Item 6 in Table 13.1). Reliability, although something on which the authors emphasize they have worked 'for several decades', now seems to them to have run out of steam. To achieve validity, they say, we need 'new approaches'.

   So, what are the required approaches, according to Kupfer *et al.*,? They can be summarized under two further agenda items that we will call respectively, 'structural solutions' and 'process solutions'.

2. *Structural solutions* (Agenda Item 8 in Table 13.1). In seeking new approaches to psychiatric classification, Kupfer *et al.* suggest, we need to make *fundamental* changes in the basic structure of our classifications: we need, they say, to 'fundamentally alter the limited classification paradigm now in use'.

   Later on, Kupfer *et al.* add further details of the structural changes they envisage: their fundamental changes will involve basing the DSM-V (or later classifications) on categories that are defined by the findings from 'animal studies, genetics, neuroscience, epidemiology, clinical research, and cross-cultural services.' Correspondingly, therefore, among the six main chapters of the DSM-V Agenda, we find chapters on a 'Neuroscience research agenda to guide development of a pathophysiologically based' system of classification (Charney *et al.*, 2002, chapter 2), on 'Advances in developmental science' (Pine *et al.*, 2002, chapter 3); and, representing clinical as distinct from biological sciences, chapters on 'Personality disorders and relational disorders' (First *et al.*, 2002, chapter 4), 'Mental disorders and disability' (Lehman *et al.*, 2002, chapter 5) and 'Culture and psychiatric disorder' (Alarcón *et al.*, 2002, chapter 6).

3. *Process solutions* (Agenda Item 9 in Table 13.1). The processes by which these fundamental changes in the structure of our classifications will be brought about are, according to Kupfer *et al.*, those of empirical research designed, (a) to enrich the 'empirical database', and (b) to facilitate effective 'integration of findings' from the biological and clinical fields listed under 'structural solutions' above.

There is much work to be done here. But if successful, it will have, to come to a further Agenda Item for DSM-V, considerable *Utility* (Agenda Item 10 in Table 13.1). For the outcomes of empirical research of the kind proposed in the DSM-V Agenda will go to the heart of what health care is all about, improving, as Kupfer *et al.* put it, the 'power of our preventive and treatment interventions'.

Of course, none of this will happen overnight. Hence we have yet another Agenda Item for DSM-V, of *Time Line* (Agenda Item 1 in Table 13.1). Kupfer *et al.* are clear that we may not see improved scientific classifications, based on aetiological theories derived from empirical research in the biological and clinical sciences, until, perhaps, 'DSM-VI or even DSM-VII'.

The need for fundamental change?

The next reading (linked with Exercise 2) fills out Kupfer *et al.*'s Agenda for fundamental changes with two further passages from their Introduction. These passages come in a section that is titled the 'Need to explore the possibility of fundamental changes in the neo-Kraepelinian diagnostic paradigm'. This is a key section of Kupfer et al's *Agenda*. It is the second of two subsections of the section, titled 'Background', which immediately follows Kupfer *et al.*'s opening paragraphs (we included the first paragraph of the Background as the last paragraph of the reading in Exercise 1).

The first subsection of the Background explores the relationship between the DSM process and the 'research database'. Filling out our agenda items, respectively for Process Solutions and for Reliability above. This first subsection, (1) describes the field trials and other evidence-based processes that supported the development of DSM-III and DSM-IV, (2) observes that the focus in these classifications was on improved reliability, and (3) spells out what Kupfer *et al.* mean by improved reliability, namely agreement between clinicians: improved reliability, they say, ensures that '. . . multiple clinicians could come to the same diagnostic conclusions . . .'; and this in turn allows '. . . reproducible diagnosis in multiple clinical and cultural settings '(both p. xvi).

The second subsection of the Background, the subsection from which the next readings are taken, then develops Kupfer *et al.* 's theme of the need for fundamental changes to allow us to move on from reliability to validity. As we will see, this subsection fills out the *Research Agenda for DSM-V* still further by giving a particularly clear picture of how the authors perceive the current status of psychiatry's international classifications, their strengths and their limitations, at this point in time.

EXERCISE 2 (20 minutes)

Read the two passages from the Introduction to the *DSM-V Agenda* on why, in the authors' view, we need to explore the possibility of fundamental changes in the DSM and, by implication, the ICD:

Kupfer, D.J., First, M.B., and Regier, D.E. (2002). Introduction. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. xviii–xix

Link with Reading 13.2

As you read these passages, go back to your checklist of items from the *DSM-V Agenda* in Exercise 1. Make notes on:

1. The way in which this passage expands on any particular items already on that Agenda. We expanded on the agenda

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item for reliability in the lead up to this exercise (in Kupfer et al's *Agenda*, we noted, reliability is defined as meaning agreement between clinicians). Similarly, then, in this exercise, see what further expansions or clarifications you can find of other items in the *DSM-V Agenda*. Then,

2. Look for any new items that are added to the Agenda (if any). Finally,
3. Think about the general tenor of this passage: is it overall upbeat or downbeat?

A deficit model of psychiatric science

Taking our third question in Exercise 2 first, our question about the general tenor of this passage of Kupfer et al's *Agenda*, it is a remarkable passage in the extent of its self-criticism. The 'limited classification paradigm', as it was described in the first reading, now reappears in this reading as a classification that is more or less wholly lacking in any of the requirements of scientific validity ('not one laboratory marker has been found…', etc.). So dismissive is much of the passage in tone and temper that it would not be out of place among the writings of those who, in the antipsychiatry movement, have attacked the very status of the DSM as a medical classification. The downbeat tenor of the Introduction to Kupfer et al's *Agenda* is an important point of contact between philosophy and psychiatry to which we will return at the end of this chapter.

As to specific items on Kupfer et al's *Agenda*, to return to question 1 of Exercise 2, much of this passage is taken up with expanding on what we called above (under Exercise 1, Agenda Item 7), the 'big issue' of validity. The thrust of this passage, however, as just noted, is remarkably negative. The passage, then, to come to question 2 in the exercise, gives us a further agenda item, Agenda Item 4 in Table 13.1, namely how the status of psychiatric science is perceived. Kupfer *et al*., in their introduction to their *Agenda*, present what might be called a 'deficit model' of psychiatric science: the problems of psychiatric classification, that is to say, are portrayed as a sign not of the *difficulty* of psychiatric science but of its *deficits*. The hope (of earlier classifiers in both ICD and DSM) was that reliability would lead on to validity. But this has not happened. The specific need, then, the particular deficit that needs, according to Kupfer *et al*., to be remedied, is, as we saw earlier, for biological (e.g. genetic) categories of mental disorder comparable with, it is implied, the more valid (= scientifically sound, aetiology based) categories to be found in the (by implication non-defective) classifications of bodily medicine.

Kupfer *et al*. further imply that correcting the deficit in validity may actually be hindered by concerns about reliability. Thus in the first paragraph of this reading, they argue that the major advantage of a 'descriptive approach' is 'improved reliability', which in turn gives it its 'primary strength', which is its 'ability to improve communication among clinicians and researchers, *not* its validity' (emphasis added). In the second passage, similarly, while Kupfer *et al*. acknowledge that there is value in improved 'diagnostic reliability', none the less, they point out, 'reification of DSM-IV entities… is more likely to obscure than to elucidate research findings.'

We will return later in this session to the relationship between reliability (of diagnostic criteria) and (scientific) validity. But it is important to note, in these and similar passages, how close Kupfer et al's *Agenda* comes to suggesting that improved reliability, far from being a step towards a more scientific classification of mental disorders, may have been achieved actually at the *expense* of progress towards (scientific) validity, thus contributing to the (perceived) deficits of psychiatric science.

Conceptual issues…

There is, however, an even more important agenda item in this passage, namely, *conceptual issues* (Agenda Item 5 in Table 13.1). The actual term 'conceptual' is not used by Kupfer *et al*. Indeed, although conceptual issues figure prominently in the *Research Agenda for DSM-V*, there is something of a reluctance throughout the *Research Agenda for DSM-V* to name them for what they are. None the less, as an agenda item, 'conceptual issues' are writ large in the two passages in this exercise. The very origins of our current problems (and hence the need for 'fundamental changes') is identified by Kupfer *et al*. at the start of the first passage in the reading, with what we might call the conceptual sins of the fathers: the 'neo-Kraepelinian approach' of focusing on syndromes rather than on aetiology. In the second passage, similarly, we find the concern that 'researchers' slavish adoption of DSM-IV definitions', and their 'reification of DSM-IV entities, to the point that they are considered to be equivalent to diseases', may have hindered research into aetiology. And in the final paragraph of the second passage, the proposed solution, directly echoing one of the roles of philosophy in healthcare outlined in our preface, is said to involve new 'ways of thinking' to achieve 'an as yet unknown paradigm shift' if we are to be more successful in uncovering the 'underlying aetiologies' of mental disorder.

…a plenty…

Given the importance of conceptual issues in driving Kupfer *et al*.'s call for fundamental changes in DSM, it is no surprise to find a substantive discussion of these issues later in the *Research Agenda for DSM-V*. The first chapter, by Bruce Rounsaville and colleagues (2002), although titled (consistently with the reluctance in the *Research Agenda for DSM-V* to name conceptual problems for what they are) issues of 'Basic nomenclature', actually includes discussions of such explicitly conceptual issues as the definitions of key concepts (including the concepts of mental disorder and illness) and the nature of validity. The discussion of validity in the chapter by Rounsaville *et al*., furthermore, building on their discussion of definitions of key concepts, extends to such essentially conceptual topics as, (1) the need to develop a 'System for rating (the validity of) diagnostic categories', and (2) the 'Rationale for changing criteria'. There is also discussion in this

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 324

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chapter of: (1) conceptual aspects of the tension between dimensional and categorical approaches; (2) methods for reducing the discrepancies in categories and criteria between DSM and ICD; (3) cross-cultural aspects of DSM-V; and (4) the use of DSM-V in non-psychiatric settings. The latter is particularly interesting conceptually in that it signals the need to develop versions of the DSM that rely less heavily on 'clinical judgement' (p. 21).

. . . in all but name . . .

Conceptual issues are evident, too, in other more overtly empirical chapters of Kupfer et al's *Agenda*. Thus chapter 5, on 'Mental disorders and disabilities: time to re-evaluate the relationship?', starts with a discussion of what the authors themselves call the 'conceptual' separation of 'descriptive information regarding individuals' psychiatric presentation' into 'psychiatric symptoms' and 'functional impairment' (Lehman *et al.*, 2002, p. 201). Similarly, the final chapter of the *Research Agenda for DSM-V*, on cultural psychiatry, starts from 'the fundamental value of social context and meaning in the human experience', thus directly paralleling Jaspers' concern (explored fully in Part II) that psychopathology should encompass meanings as well as causal explanations (Alarcón *et al.*, 2002, p. 219); and this chapter concludes with the explicitly philosophical claim that the systemic approach of cultural psychiatry '. . . surpasses, despite some limitations, the mind-body dichotomy' (Alarcón *et al.*, 2002, p. 265).

The *de facto* importance of conceptual issues in the *Research Agenda for DSM-V*, makes it somewhat puzzling that the *term* 'conceptual' is so rarely used. There is no category for conceptual issues in the index (though the term does appear in some subcategories); the chapter on conceptual issues, as we noted above, is called 'Basic *nomenclature* issues for DSM-V' (Rounsaville *et al.*, 2002, p. 1, emphasis added); and the thrust of the editors' Introduction is very much to the effect that the *Research Agenda for DSM-V* is an agenda for *empirical* research: as you will see if you refer back to the first reading (linked with Exercise 1), even their one reference to an 'analytic' agenda is a reference, not to analytic research on concepts, but to a 'research and analytic agenda' for integrating the findings from basic science and clinical research into an enriched 'empirical database' (Kupfer *et al.*, 2002, p. xv).

. . . and in all but methodology

The thrust of Kupfer *et al.*'s Introduction, that this is not just a 'Research Agenda for DSM-V' but, specifically, an '*Empirical* Research Agenda for DSM-V', is carried through in the proposals for research outlined in their Chapter 1. This chapter, although covering, as noted above, a whole range of conceptual issues, makes proposals for research, that, even on the overtly conceptual problems raised by concepts of mental disorder, are limited to empirical field studies. Again, we return to the relevance of such studies at the end of this chapter. Combined with conceptual research methods, empirical field studies are a key part of what in Chapter 4, employing J.L. Austin's term, we called 'philosophical fieldwork'. But if the additional methodological and interpretive resources of conceptual research are absent, such studies, however descriptively informative, are likely to compound rather than to illuminate the difficulties (and, as formulated in Part I, remember, they are very much *difficulties* and not deficiencies) of psychiatric science.

A new conceptual research agenda for DSM-V

In this first section of this session we have now built up an agenda for DSM-V, which although derived directly from materials in the original (Kupfer *et al.*'s Introduction together with supporting chapters), paints a very different overall picture. The original *Research Agenda for DSM-V*, although including conceptual issues, is largely taken up with empirical research. Even where, *de facto*, the *Research Agenda for DSM-V* is concerned with conceptual issues, it calls them, as we have seen, something else (issues of basic nomenclature) and it makes no mention whatsoever of conceptual research methods. Our new *Conceptual Research Agenda for DSM-V*, by contrast, emphasizes and draws out the significance of the conceptual issues identified in the original.

We summarize our new *Conceptual Research Agenda for DSM-V* in Table 13.1 (above). Later in this chapter we will be comparing this with a second new agenda, a *Conceptual Research Agenda for ICD-9*, using the same agenda items. We will come to our *Conceptual Research Agenda for ICD-9*, however, by way of an extended discussion of the history of ideas behind current classifications. This in turn will provide a foundation from which to assess the theoretical significance and practical consequences of the conceptual issues identified in this section as being implicit in Kupfer *et al.*'s *Research Agenda for DSM-V*.

Hempel and the origins of ICD and DSM

How, then, did we get to our current classifications of mental disease? What are their origins in the modern history of psychiatry? Kupfer *et al.*, in the Introduction to the *Research Agenda for DSM-V*, provide a clear account of the origins of their *Agenda* in successive revisions of DSM back to DSM-III (Kupfer *et al.*, 2002, pp. xvi–xviii). But of course DSM-III did not arise *sui generis*. In this section, therefore, we are going to push the story back a little further than DSM-III, specifically to the international conference on psychiatric classification in 1959 noted in the introduction to this session.

Back from DSM-III to 1959

Our purpose in going back to the 1959 conference is to look in some detail at the influence on the ICD/DSM story of the North American philosopher of science, Carl Hempel. Hempel, you will recall from Chapter 12, was one of the founders of the model of science developed by the Vienna Circle in the 1930s and 1940s, called Logical Empiricism. The central aim of Logical Empiricism, to establish a sharp separation between observational (or descriptive) statements and statements of theory, is directly reflected in the critical move in post-Second World War psychiatry, noted in the introduction to this session, from the theoretical (aetiology based) ICD-6 and ICD-7 to the descriptive (symptom based)

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ICD-8. The link between Hempel and ICD-8 is generally taken to be the British psychiatrist, Erwin Stengel (see, e.g. Kendell, 1975, p. 93). As we will see, however, important as Stengel's influence on ICD-8 certainly was, there is a crucial missing link in this story. We will be uncovering this missing link in this section, and, in the process, taking an important step towards the derivation of our second new agenda, our *Conceptual Research Agenda for ICD-9*.

The background to Hempel's 1959 paper

First, then, Carl Hempel's role in post-Second World War psychiatric classification. As an internationally respected philosopher of science, Hempel was invited to present a paper at the conference on psychiatric classification organised by Joe Zubin (see Zubin, 1961) in 1959 in New York under the auspices of the American Psychopathological Association.

The aim of the 1959 conference was to contribute to a review process that would lead to the establishment of a classification of mental disorders, that, being acceptable on an international basis, would meet the WHO's requirements for comparative epidemiological studies and statistical reporting. Immediately on its foundation in 1945, the WHO had published a sixth revision of the *International List of Causes of Death* (which had previously been published by the French Government) renaming it as the *Manual of the International Statistical Classification of Diseases, Injuries and Causes of Death*, and including for the first time a classification of mental disorders (WHO, 1948). Most of the chapters in the new classification, those dealing with bodily disorders, were well received and readily adopted around the world. The mental disorders section, however, was one of the very few that proved to be problematic. Eleven years later it was found to have been adopted only in Finland, Peru, Thailand, and the United Kingdom. (We owe these historical details to chapter 7 of R.E. Kendell's authoritative book, *The Role of Diagnosis in Psychiatry*, 1975.)

The 1959 conference

Faced with these difficulties, the WHO commissioned the British psychiatrist, Erwin Stengel, to investigate and to make recommendations for a classification that would be more widely acceptable. It was in this connection that, at the 1959 conference, Hempel, as a philosopher of science, was invited to speak. Stengel was the first respondent to Hempel's paper and Hempel's insights into scientific classification, notably those dealing with the Logical Empiricist separation of observational (or descriptive) statements and statements of theory, appear, with full acknowledgements, in key sections of Stengel's subsequent report to the WHO (Stengel, 1959).

From 1959 to ICD-10 and DSM-IV

ICD-8 consequently appeared in 1967 (WHO, 1967), and, with the subsequent publication of a glossary (WHO, 1974), became the first predominantly symptom-based modern classification of mental disorders (the mental disorders section of ICD-7 was identical with ICD-6). The American Psychiatric Association adopted the ICD-8 nomenclature in its DSM-II (American Psychiatric Association, 1968), abandoning, against much domestic opposition (Kendell, 1975), the predominantly psychodynamic-theoretical basis of DSM-I (American Psychiatric Association, 1952). ICD-9, which largely incorporated the ICD-8 glossary, followed in 1978 (WHO, 1978). The DSM-III (American Psychiatric Association, 1980) introduced explicit inclusion and exclusion criteria (the DSM-III Taskforce was led by Robert Spitzer). Finally, DSM-IV explicitly (American Psychiatric Association, 1994, p. xv), and ICD-10 implicitly (WHO, 1992), established evidence-based approaches to refining and modifying the categories in their respective classifications. (The DSM-IV Taskforce was led by Allen Frances, and the ICD-10 team at WHO was led by Norman Sartorius (who had previously worked on ICDs 7, 8 and 9) as Head of the Mental Health Section at the WHO.)

Hempel's 1959 paper

Hempel's paper, then, by so clearly flagging the Logical Empiricist separation of observation and theory at that crucial conference in 1959, had, on the face of it, a decisive influence on the subsequent development of both ICD and DSM. But what exactly did he say? In this section we will be looking at several extracts from Hempel's paper. It was published originally as part of an edited collection (see Hempel, 1961) but it has been reprinted a number of times, most recently in the edited collection by the American psychiatrist and philosopher, John Sadler and colleagues, in their *Philosophical Perspectives on Psychiatric Diagnostic Classification* (Sadler *et al*., 1994). Sadler *et al*.'s book was published, with a foreword by Allen Frances, to coincide with the launch of DSM-IV and the collection includes a number of important contributions to the field. Our readings in this section are taken from Sadler *et al*.'s reprint of Hempel's paper (which is more or less identical with the original but with a number of helpful endnotes added by Hempel himself).

EXERCISE 3 (30 minutes)

Read the short extract from:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press. (Extract pps 317–318.)

Link with Reading 13.3

This first extract comes early in Hempel's paper and gives an initial summary statement of two key requirements that he argues should be satisfied if a classification is to be genuinely scientific. The passage follows an opening section in which Hempel gives a brief but very clear review of the logic of classes relevant to classification and it is followed by two long sections in which Hempel discusses his two requirements in more detail.

The extract begins with the final paragraph of Hempel's introductory section. Here he distinguishes, very much along

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Logical Empiricist lines, scientific *concepts* from the *language* in which those concepts are expressed (his 'vocabulary of science'). He then goes on to set out his two requirements.

As you read the extract, think about the following five questions:

1. What are the two requirements that Hempel places on scientific classifications?
2. How are these two requirements related to the Logical Empiricist model of science outlined in Chapter 12?
3. How are Hempel's two requirements related to the overall development of post-Second World War international psychiatric classification outlined above?
4. Is there anything that strikes you as odd about what Hempel has to say about psychiatric classification, given the storyline of this session, namely that we owe the essentially descriptive (symptom-based) form of current psychiatric classifications in part to his paper?
5. Similarly, given that the topic of this chapter is about what science is about, is there anything that strikes you as odd about the way Hempel describes the second of his two requirements?

Many of the key messages for psychiatric classification from Hempel's paper are captured by this brief extract. We will take our five questions in turn.

Five questions from Hempel's paper

1. *Hempel's two requirements*. The two requirements that Hempel places on a classification if it is to be scientific are that the terms (or vocabulary) in which it is expressed, and hence the concepts it reflects,
   (a) 'permit an adequate *description*' of the objects of the science in question, and
   (b) 'permit the establishment of general laws or theories by means of which particular events may be *explained* and *predicted* and thus *scientifically understood*; . . .' (Hempel, 1994, p. 317, emphasis in original).

   In his subsequent development of these two requirements, Hempel calls them respectively the 'Empirical Import' (or sometimes 'descriptive adequacy') and the 'Systematic Import' of a scientific classification. We will be exploring particularly the first of these two requirements, Empirical Import, in more detail later in this session.
2. *Logical Empiricism and Hempel's two requirements*. The origins of Hempel's two requirements in Logical Empiricism are clear: note, first, the emphasis on language—Hempel casts his two requirements in terms of the 'vocabulary' of science; and, second, the implied primary division into a language of observation (here, 'description') and a language of underlying theory (here, 'general laws and theories').
3. *Hempel's two requirements and post-Second World War psychiatric classification*. Again, the link between Hempel's two requirements and developments in post-Second World War psychiatric classifications is, at least at first glance, clear. As described above, the key move in these developments was from, in Hempel's terms, the language of theory (aetiology) to a language of description (symptoms). In the second paragraph of the extract, furthermore, Hempel observes how (with 'some oversimplification') the development of a science can be understood as being marked by a progressive shift from descriptive to theoretical terms. It is a small further step, therefore, from this observation to a recognition of the need for a science to base its classifications on the stage that it is at in its development. Psychiatry, then, if lacking an agreed theoretical basis at this stage in its development, should base its classifications on symptoms (as in ICD-8 and subsequent classifications) rather than on theory (as in classifications prior to ICD-8).
4. *But what is odd about this?* What is odd about Hempel's paper, however, given Hempel's influence on developments in psychiatric classification, is that Hempel himself does not take this small further step. To the contrary, in this extract (para 3), he argues that with the development of psychodynamic theory, notably as the basis of the 'DSM' of the time (now called DSM-I), psychiatry is already following the route of general medicine from a descriptive to a theoretical stage in its development. Thus, citing an example (of conversion reaction) from the DSM of the day, Hempel notes that the diagnostic terms used are not observational but 'theoretically assumed psychodynamic factors'. And he goes on to compare the DSM in this respect favourably with physical theory (such terms as 'gravitational field', etc.).

   Later in the article, it is true, Hempel speaks less favourably both of psychodynamic theory and of the DSM of the day. He says that the central concepts of contemporary psychodynamic theory 'lack clear and uniform criteria of application' (p. 318). But this criticism is merely by way of a preliminary to offering a remedy ('operationalism', as we will see in the next reading) rather than by way of rejecting psychodynamic theory in principle as a basis for psychiatric classification. Similarly, as to the contemporary DSM, the essence of Hempel's critique is not *that* it is a theoretical rather than descriptive classification, but rather that many of the terms it employs are value-laden, they have what Hempel calls 'valuational overtones' (p. 322). And he goes on to suggest that this, too, can be remedied.

   We return to what Hempel has to say on values in psychiatric diagnosis in Session 2. For the moment, though, the point is that, far from suggesting that psychiatry should move to a more descriptive basis for its classification, the implication

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is that it is on the right track in following the rest of medicine from a descriptive to a more theoretical set of diagnostic concepts. Furthermore, Hempel sees nothing in principle against psychodynamic theory being the basis of psychiatric classification. Indeed he describes the gradual replacement of observational with explanatory theoretical terms in a genuinely scientific classification as being 'nicely illustrated' by the psychodynamically based DSM of the day (p. 326). And while he leaves it open as to whether the major theories used in the future in psychiatry will be 'biophysiological' or 'psychodynamic' (p. 327), he suggests that 'theories of either kind can satisfy the basic requirements for scientific theories' (p. 327), and he concludes with an express prediction that, whichever kind of theory is adopted, psychiatric classification will continue to shift from systems based on 'observable characteristics' to 'systems based on theoretical concepts' (p. 330).

All this is odd, then, given that, in the event, psychiatric classification moved sharply against Hempel's prediction, becoming more descriptive rather than more theoretical in orientation, and that, in the case specifically of the DSM, the move to a descriptive basis was achieved in the teeth of a domestic opposition intent on retaining psychodynamic theory.

5. *What is odd about Hempel's first summary statement of his two requirements?* What is odd about Hempel's first summary statement of his two requirements for a classification to be scientific, given that this chapter is about what science is about, is, in a word, that he makes no reference to realism: there is no requirement, explicitly at any rate, for a scientific classification to be about what is 'really there'. There is no explicit requirement even for a scientific classification to be 'objective'.

   Interestingly, by contrast with his first summary statement, explicit references to the 'real' appear right at the start of Hempel's more detailed account of both requirements. The first sentence of his section on 'Empirical Import', as we will see in the next reading (linked with Exercise 4), is 'Science aims at knowledge that is *objective*...' (p. 318, emphasis in original). Similarly, at the start of his section on 'Systematic Import', he repeats the brief definition given in this first extract but with the crucial additional element that the 'general laws and theoretical principles' towards the generation of which a scientific classification should incline, should be such as 'reflect uniformities in the subject matter under study and which *thus* provide a basis for explanations, prediction, and generally scientific understanding' (p. 323, emphasis added).

   We should not make too much of this, of course. Hempel's failure, in his first summary statement, to place any explicit requirement on a scientific classification to be about what science is about, is a natural enough slip even for perhaps the last great twentieth century philosophical champion of the traditional model of science. But Hempel's slip reminds us just how easily what science is about slips out of the picture when we are struggling with problems of classification. Again, we will return to this point later. But if even Hempel, one of the founders of Logical Empiricism, can let what science is about slip below the threshold of visibility, how much more readily will this happen in a practical discipline, like psychiatry, that, whatever its scientific basis, is subject to a wide range of conflicting social and political pressures.

Hempel's first requirement for a scientific classification: Empirical Import

All the same, as the opening passages in Hempel's fuller treatment of his two requirements, cited above, make clear, Hempel *is* concerned with what science is about. So, how do his two requirements on a scientific classification, the requirements of Empirical Import and Systematic Import, help to ensure that a classification is indeed about what science is about, that it reflects, or aims to reflect, objective information about what is really there?

We will look first, and in most detail, at Hempel's first requirement, Empirical Import. As we will see, it is Hempel's discussion of Empirical Import that has most directly influenced the course of subsequent developments in psychiatric classification. Hempel's discussion of Systematic Import raises a whole series of key topics for our understanding of what science is about. However, we will look only briefly at these topics in this session, and mainly as they relate to validity, by way of preparation for more detailed discussions both in later sessions in this chapter (values, Session 2; and realism in physics, Session 3) and in later chapters in this part (explanation and scientific laws, Chapter 14; and causation, Chapter 15).

Descriptive adequacy and empirical import

First, then, Empirical Import. The next reading, which follows immediately on from the last reading (linked with Exercise 3), includes the concluding paragraph of Hempel's summary of his two requirements and the opening paragraphs of his longer treatment of his first requirement.

EXERCISE 4 (20 minutes)

Read the short passage from:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press. (Extract, p 318).

Link with Reading 13.4

This is a very short passage from Hempel's paper but it has a great deal packed into it! As you read it think about:

1. The significance of the difference in the terminology Hempel uses of his first requirement in the paragraph before the subheading and in the subheading itself.
2. The sense that Hempel gives to that key word 'objective' as an aim of the kind of knowledge to which science aspires.

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3. Hempel's criticisms of 'contemporary psychodynamic theories', in particular:
   (a) Are these criticisms justified in his own terms?
   (b) Why do you think Hempel introduces psychodynamic theory as an example at the start of a section on the Empirical rather than Systematic Import of a classification?
   (c) How is what Hempel says here about psychodynamic theory related to his comments in the last paragraph of the previous reading, the reading linked with Exercise 3. (Remember that these two passages about psychodynamic theory are separated in his paper by only a few lines.)
   (d) What are the implications of all this for the subsequent rejection of psychodynamic theory in the move from DSM-I to DSM-II.

Write down your own ideas on these points before going on.

Again, we will take the above three questions in turn.

1. *Hempel's terminology*. In the final paragraph of Hempel's summary introduction to his two requirements, he refers to the first as 'descriptive'; and 'descriptive adequacy', with appropriate variations, is the term he uses throughout his summary, including in the subheading to the summary (as in the reading above linked with Exercise 3). In the subheading to the next section, though, the term 'descriptive adequacy' has been replaced with 'Empirical Import'. There is a slide here, then, from the demand that a classification should be descriptively adequate to the idea that, in consequence, it has Empirical Import.

   The link between descriptive adequacy and Empirical Import in Logical Empiricism is, as we saw in Chapter 12, observation: it is observation statements that in Logical Empiricism are the basis for distinguishing between theories that are true as to 'what is there' and theories that are not; it is thus observation statements that have *empirical* content; and observation statements *describe* what is observed. This logical 'audit trail' will be important to us later when we came to the link between reliability (agreement in observational statements) and validity (being true to what is there). For the moment, though, it is important to note that the terms 'descriptive' and 'empirical' are so closely melded together in Hempel's thinking that they are used almost as synonyms.

2. *What Hempel means by 'objective'*. Hempel defines 'objective' in the first few lines of the section on Empirical Import consistently with Logical Empiricism. Thus 'objective' knowledge is the kind of knowledge that science aims to deliver, i.e. knowledge that is 'inter-subjectively certifiable, independently of individual opinion or preference, on the basis of data obtainable by suitable experiments or observations'.

   There are many qualifications and caveats between the lines of this definition that reflect refinements in the theory of Logical Empiricism. Thus objective knowledge is not equivalent merely to intersubjective agreement: it is a matter, rather, of intersubjective *certification*; such certification, moreover, should not be on the basis of 'individual opinion', still less, anticipating Hempel's later concerns about values, 'preferences'; rather, certification should be on the basis of data that, if not actually to hand is 'obtainable' in principle (this is, roughly, the requirement of 'testability' in the traditional model of science, and, broadly, of 'verification' as discussed in Chapter 12); and, finally, though crucially, the data should be obtainable in principle on the basis of 'suitable experiments or observations', i.e. experiments or observations that are 'suitable' in the sense of being appropriate to the knowledge being 'certified' in the case in question (Hempel's word 'suitable' of course suggests one place at which, contrary to the tenets of Logical Empiricism, theory, in the form of conceptual prestructuring (see Chapter 12), 'infects' data). Hempel's next sentence then completes the connection between observation and description. For, he says, 'this' (i.e. intersubjective certification on the basis of observation) 'requires that the terms used in formulating scientific statements' (note the Logical Empiricist return to the importance of language) 'have clearly specified meanings and be understood in the same sense by all those who use them.'

   At one level, all this makes perfect sense. If those involved in a process of intersubjective certification are using the same *terms* to describe their observations but with different *meanings*, whether through lack of clarity of meaning or for any other reason, then there will be no determinate way of knowing whether they agree or not. There are difficulties with this idea to which, with Hempel himself, we return later (see reading linked with Exercise 5 below). But the point for now is that Hempel does not specify that clear and agreed meanings are to be attached only to observational terms. He uses the generic 'scientific statements'. There is another slide here, then, for 'statement' could mean 'statement of observation' (i.e. *descriptive* statement) or 'statement of theory'. This slide is important to an understanding of the (apparent) criticism Hempel makes of 'contemporary' psychodynamics, by way of illustration, in the second half of the paragraph, and it is to this that we turn in the third part of this exercise.

3. *Hempel's example of psychodynamic theory*. Having defined what he means by 'objective' in the first half of the opening paragraph of this section, Hempel goes on to give an example. What is wrong with 'contemporary psychodynamic theories', he suggests, presumably with the example of conversion reaction (given a few lines above) in mind, is that their 'central concepts lack clear and uniform criteria of application'. As a consequence of this, therefore, 'there are no definite and unequivocal ways of putting the theories to a test by applying them to concrete cases'.

   What is odd, then, and surely unjustified, about this example is not that it is critical of psychodynamically theory. It is rather that Hempel's criticism is directed, not against the

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 329

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observational basis of psychodynamically based diagnosis, but against its *theory*. Remember that this whole section is about the Empirical Import of a classification, i.e. its observational (or descriptive) basis, rather than its Systematic Import (Hempel's second requirement). Further, in the preceding section (in the reading linked to Exercise 3), Hempel appears to comment favourably on psychodynamic theory, by loose analogy with gravitational theory, as helping to move psychiatry along the course of true science from a descriptive to a systematic (or theoretical) basis for its classifications.

The appropriate response to Hempel's critique of contemporary psychodynamic *theories* in this section, therefore, is not, with DSM-II, to reject them; the appropriate response is rather to clean up their observational basis, in other words to provide the clear and uniform criteria of application that Hempel suggests they lack. This may be difficult to do: Hempel suggests later on that one attempt to provide such criteria (by Ellis, p. 321) fails. But, as we noted in the last exercise, Hempel expressly states at the end of his article that there are, in his view, no barriers of principle to adequate formulations of psychodynamic theory. And subsequent empirical studies, for example by the British psychiatrist and philosopher, Peter Hobson, have indeed shown that psychodynamic concepts are perfectly capable of being formulated in reliable terms (Hobson *et al.*, 1998).

Operationalism

In the next reading Hempel makes a key suggestion for how the Empirical Import of the terms in a scientific classification might be improved.

EXERCISE 5 (30 minutes)

Read the two passages from:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press

Link with Reading 13.5

This reading from Hempel's paper introduces a term—operationalism—from the philosophy of physics that has subsequently passed into the language of psychiatry. As you read these passages, think about precisely what operationalism means in physics, in particular its relationship to:

1. the Empirical Import of a classification understood, in Hempel's term in the first reading from his article, as descriptive adequacy; and
2. the relationship between the Empirical (descriptive) Import and Systematic (theoretical) Import of a classification and the relevance of this relationship to current concerns about the validity of psychiatric classifications as expressed in Kupfer *et al.*'s *Research Agenda* for *DSM-V*.

Operationalism, then, as Hempel describes in the first passage in this reading, was first fully set out by the physicist P.W. Bridgman in his book on *The Logic of Modern Physics* (1927). Essentially, operationalism seeks to make descriptive terms capable of unambiguous application by defining them by reference to the results of an operation: thus, in Hempel's example, the descriptive term 'harder than' means 'one material is harder than another if a sharply pointed sample of the former is capable of making a scratch on a sample of the latter'.

Operationalism and descriptive adequacy

In physics, then, this seems a sensible approach to providing clear criteria of the meaning of key terms, essentially because physics deals with things that, directly or indirectly, we can get hold of, and on which we can thus carry out operations. This indeed, as Hempel suggests in the second passage, is what makes the assertions of physics 'capable of objective test'. For, as he goes on to say, the ability to carry out operations is what gives the assertions of physics 'definite public criteria of application'. As a 'test' of the scientific, he further suggests, that is 'closely akin to the empiricist' (hence Logical Empiricist) 'insistence that meaningful scientific hypotheses and theories should be capable, in principle, of intersubjective test by observational data'.

This passage is important, not least, because it makes clear, again, the very tight link in Hempel's thinking between Empirical Import, or 'descriptive adequacy' as he also calls it, and Systematic (or theoretical) Import. Psychiatry has derived from Hempel's paper the need to establish a descriptively adequate set of criteria for its classifications. This, as Hempel suggests, is a pre-condition for objectivity, where objectivity means intersubjective agreement on whether a descriptive term applies in a given case. But the *point* of this is always integrally linked to providing a test of *theory*. It is this point, perhaps, that psychiatry, in focusing on descriptive adequacy has tended to lose sight of. It is this point, indeed, the point of advancing theory, that is behind current concerns, as in Kupfer *et al.*'s *Research Agenda for DSM-V*, about the *validity* of psychiatric classifications. Again, we return to validity and to its relationship with reliability below in the final part of this session.

Reliability

But before we get to linking up Empirical and Systematic Import, description and theory, we need to fill in a key step. For, as we saw earlier in this session, current concerns about the validity of psychiatric classification stand in contrast to claims, not, directly, of descriptive adequacy (Hempel's first term), still less of Empirical Import (his second term), but rather of reliability. In the next reading we look at the link in Hempel's paper between operational criteria, as one possible way of achieving Empirical Import in medical diagnosis, to a particular kind of difficulty that may arise in psychiatric diagnosis relevant to reliability.

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 330

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EXERCISE 6 (20 minutes)

Read the two short passages from:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press

Link with Reading 13.6

Here we want you to consider just one question.

- How strong or otherwise is the link Hempel makes between operationalism and reliability (agreement in the use of diagnostic terms—see Box 13.2).

This reading starts with a bald assertion that most medical diagnostic procedures are based on operational criteria. In the passage of his paper immediately preceding this reading, Hempel has given an example of a medical diagnosis based on carrying out a chemical test on a sample of a patient's urine. Fair enough, then. Such an 'operation' is capable (technical competence permitting) of unambiguous application (in most cases). Hempel then contrasts this example of a medical diagnosis with an example of a psychiatric diagnosis based on a characteristic subjective response, the 'praecox feeling', in the examiner. The 'praecox feeling', he says, does not satisfy the requirements of operationalism because its application 'is *not* independent of the examiner' (p. 312, emphasis in original; we return to Hempel on the praecox feeling later in this session).

Hempel on reliability

The link between operationalism of this kind and reliability in Hempel's paper is, however, surprisingly weak. 'Surprisingly' because, as we have seen, reliability has figured so prominently, and so prominently alongside claims to operationalism, in post-Second World War developments in psychiatric classification. Thus in the first passage in this reading, Hempel starts, as we have seen, by using the example of the 'praecox feeling' to mark the distinction between satisfactory operational diagnostic criteria in bodily medicine and unsatisfactory criteria in psychiatry. In the gap between the two passages in the reading, Hempel defines the link, that we looked at in the second paragraph of the reading linked with Exercise 5, between Bridgman's operationalism and empiricism. Then, in the second passage in this reading, Hempel starts by noting the interest in operationalism in psychology and sociology, reflected in the use of structured psychometric instruments; he follows this with a simple assertion, no more, at the start of the next paragraph that the 'concern' among 'psychologists and social scientists with the *reliability* of their terms reflects the importance attributed to objectivity of use' (p. 320, emphasis in original); and he then moves swiftly on in the rest of the paragraph to elaborate on the two main kinds of reliability (see Box 13.2) and how they are assessed.

This passage thus leaves entirely open precisely *why* or *how* reliability—agreement in the use of a term—is linked with either descriptive adequacy or Empirical Import as the focus of this section of Hempel's paper. We can only speculate on how Hempel might have unpacked this link. But it is important to be aware just how little Hempel himself makes of reliability, given the importance of reliability in current psychiatric classifications and its (not wholly satisfactory) relationship with validity—recall that Kupfer *et al.* (2002) imply, contra Hempel, that reliability has been achieved perhaps even at the expense of validity. This would be contrary to Hempel's expectations if Empirical Import (via descriptive adequacy) were indeed a step towards Systematic (or theoretical) Import and if reliability were sufficient for descriptive adequacy.

We return to the links between operationalism, reliability, and validity at the end of this session after we have explored how Hempel's ideas found their way (much modified) through into ICD-8 and, hence, set the agenda for psychiatric classification running towards its current status as summarized in Kupfer *et al.*'s *Research Agenda for DSM-V*.

Hempel and the missing link to ICD and DSM

We have now looked at a number of key points made by Hempel relevant to the subsequent development of psychiatric classification: (1) Hempel's two requirements for a scientific classification, Empirical Import (or descriptive adequacy) and Systematic (or theoretical) Import; (2) the connections Hempel makes between these and the objectivity of a science; (3) his comments on operationalism as a way of improving objectivity; and (4) the links he draws, such as they are, between objectivity, operationalism, and reliability.

EXERCISE 7 (20 minutes)

At this point, reflect on what you have learnt about Hempel's paper thus far and the way in which, with ICD-8, psychiatric classification subsequently developed. In what respects has psychiatry adopted Hempel's message? In what respects has it not done so?

A rough 'audit' of Hempel's influence, on the basis of what we have read thus far, suggests that there is a broad consonance between his two requirements for a classification to be a scientific classification and how psychiatry's classifications actually developed with ICD-8. Hempel distinguished descriptive (or empirical) adequacy from theoretical (or systematic) import; and he noted that sciences tend to evolve from descriptive to theoretical stages. On the other hand, however, he also suggested that psychiatry, as a medical science was already showing positive signs of development towards a theoretical stage; and that, while the dominant theory at the time (at least in DSM), psychodynamic theory, needed to be operationalized, and while there were competing theories (notably biological theory), he none the less expected psychiatry to continue its established trend towards ever greater systematization. This of course was directly contrary to the direction psychiatry actually took. Similarly, although Hempel emphasized the potential of operationalism as a method for improving objectivity, and although this term did come into general use in psychiatric classification,

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 331

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Hempel mentions reliability, which as Kupfer *et al.* (2002) note (above), and as we illustrate further in the Reading Guide, has occupied far more of the research agenda, only in passing.

A missing link

So there is a missing link. Hempel, and the influences generally of Logical Empiricism, are evident enough in ICD-8 and its successor classifications. But the particular effects of these influences, how they actually played out, suggest that some other factor must also have been involved. As already noted, Erwin Stengel is often credited with being the link between Hempel and ICD-8 (see for example Kendell, 1975, p. 93). Stengel was certainly influential on ICD-8. As noted earlier, he was the first respondent to Hempel's paper in the 1959 conference; and his remarks in that context were reproduced, in closely similar form, in his report to the WHO (Stengel, 1959). A closer reading of Stengel, though, in both places, suggests that he largely follows Hempel's line, i.e. suggesting that at the very least, psychiatric classifications will continue to be based on *both* symptoms (descriptive criteria) *and* aetiology (theoretical criteria). Again, then, there must be a missing link, some additional factor standing between what both Hempel and Stengel had to say about the future direction of development of psychiatric classification, and the direction that, in the event, it actually took.

The discussion following Hempel's paper

The next reading (linked with Exercise 8) is from a transcript of the discussion that was reproduced alongside the first publication of Hempel's paper in Zubin, 1961. We reference it here as Discussion, Various Contributors, 1961.

Stengel opens the discussion, covering essentially the material he was later to include in his report to the WHO, and a number of other contributors raise various points. About halfway through the discussion, in response to one of the points raised, Hempel picks up on what he had to say in his paper about the difference between a natural and an artificial classification, noting that the difference is not 'sharp and definitive, but a matter of degree; and secondly, that it is relative to the particular purpose which a classification is to serve' (Discussion, Various Contributors, 1961, p. 33). Hempel then goes on to develop his point, explaining that it is the *scientific* purposes of 'finding a system of laws or correlations by which explanations or predictions may be achieved', that was behind his requirement that if a classification is to aspire to be a natural classification, it must have, in addition to Empirical Import (or 'descriptive adequacy'), Systematic Import.

A decisive intervention

Hempel's comments in this passage of the discussion thus emphasize the extent to which, contrary to the direction that subsequent psychiatric classifications were to take, he focused on the importance of Systematic Import (rather than Empirical Import or descriptive adequacy) in a scientific classification. But it is at precisely this point that someone who has not yet spoken comes into the discussion. In a brief but decisive intervention, this person, (1) suggests a different way in which classifications may be divided up; (2) connects this different way of dividing up classifications with the particular purposes served by an international psychiatric classification, and then, (3) delivers a punchline in the form of a specific proposal for developing ICD-8. In the next reading, we look at this decisive intervention in the discussion following Hempel's paper in full.

EXERCISE 8 (15 minutes)

Read the short extract from:

Discussion, Various Contributors (1961). In *Field Studies in the Mental Disorders* (ed. J. Zubin). New York, Grune and Stratton. This extract comes on page 34.

Link with Reading 13.7

This reading is the whole of the decisive intervention by a person (at this stage unnamed) in the discussion following Hempel's paper. It is also the only intervention by this person. It occupies just half a page in a total of 28 pages, i.e. less than 3% of the 'column inches'. Yet it was indeed to prove decisive. As you read the passage, think about:

1. What the speaker takes from Hempel and what he does not
2. Precisely what the speaker's proposal for ICD-8 amounts to

Finally, who do you think the speaker is?

The speaker in this passage starts by connecting up what he (he is a 'he') has to say with Hempel's remarks by picking up on Hempel's point about the form of a classification, whether natural or artificial, being driven by the purposes it is intended to serve. Hempel, as we noted above, had talked about the purposes of scientific (or natural) classifications in general. The speaker in this passage, however, focuses rather on the particular purposes of particular sciences. Viewed in this way, he says, among scientific classifications 'we might properly distinguish between public classifications and private classifications'. It is the former, public classifications, that 'are most valuable for epidemiological work, since we need to make comparisons of findings in different countries, and unless there is uniformity of usage, that is impractical'. Private classifications, by contrast, although perfectly capable of uniformity of usage within a given group with a uniform background, are, none the less, only suitable for the usage of the group in question.

The speaker, therefore, in this passage, with his talk of 'epidemiological work' and of 'comparisons of findings in different countries', clearly has WHO's need for an international psychiatric classification in mind. But what does this mean for the basis of such a classification?

A decisive proposal for ICD-8

Here, again, the speaker draws on Hempel's insights into the nature of a scientific classification, but adapting them in a decisive way to meet the requirements of an international psychiatric classification. A scientific classification, he agrees with Hempel,

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should ideally be based on theory. For some private classifications, used by a group for whom a given theory seems 'workable', a theoretical basis may be appropriate. But no current psychiatric theory can meet the requirements Hempel has set out for adequate Systematic Import. Hence, the speaker suggests, coming to the punchline of his proposal for ICD-8, 'for the purpose of public classification we should eschew categories based on theoretical concepts and restrict ourselves to the operationalized, descriptive type of classification . . .'.

The missing link

Here, then, is an intervention capturing precisely the direction that psychiatric classification, from ICD-8 onwards, subsequently took . . .

. . . and the speaker turns out to be, as described in the transcript, one 'Dr Lewis'. This is none other than *the* Aubrey Lewis, then, Professor Sir Aubrey Lewis, the architect in the 1940s and 1950s of the Maudsley 'school' of rigorous scientific psychiatry, that we met in Part I: you will recall his analysis of concepts of disorder as 'part functions' (Part I, Chapter 2 Reading Guide) and his definitional attack on the concept of psychosis (Part I, Chapter 3, Philosophical Annotation on Insight).

Lewis adapts Hempel

Important, then, as Hempel's and in turn Stengel's influence was on ICD-8, it was from Lewis that the decisive move from theory-based to a descriptive- or symptom-based classification in ICD-8 came. Lewis, moreover, in suggesting this move, although drawing on Hempel's general insights into the nature and purposes of scientific classification, does so in an active rather than passive way. Lewis adapts Hempel's general insights into scientific classifications to the particular needs of international psychiatry at the time for statistical reporting and for research based on the methods of epidemiological science.

Fig. 13.1 Professor Sir Aubrey Lewis

Lewis later went on to make what the WHO, in the Preface to the ICD-9 (WHO, 1978, p. 5) described as a 'fundamental' contribution to the Glossary to ICD-8. We look at Lewis' own Preface to ICD-8 in detail below. Before coming to ICD-8, though, we need to fill in briefly how the story developed following Lewis' decisive intervention at the 1959 conference.

Not (yet) a 'done deal'

Lewis' proposed move to a predominantly descriptive basis for ICD appears to have gone down, with many of those at the 1959 conference, like a lead balloon. Lewis' intervention is picked up enthusiastically by some of the other delegates; but it is ignored or objected to by others (Hempel appears to ignore it); and although the Chair, Dr Reid, refers favourably to it in his summing up, as being 'in the interest of unambiguous communication and uniformity of use', he does so with qualifications; and he had rather more to say about the evolution of operational terms and about future scientific progress being based on the development of new instrumentation and corresponding advances in theory. It is an evolution of this (theoretical) kind, Dr Reid concludes, that is 'to be expected also in the domain of psychiatric research' (Discussion, Various Contributors, 1961, p. 49).

Similarly, in a separate summary of the discussion written by John Clausen (presumably at a later date), although there is reference to a possible move to a descriptive basis for psychiatric classification, Lewis is not credited with the suggestion, the objections to it are emphasized, and Clausen's summary, like the Chair's (Dr Reid's) summing up, ends with Hempel's comments on the need for progressive refinement of psychiatric classification in response to future developments in theory.

Dr Reid and John Clausen, it is important to emphasize, in making so little of Lewis' intervention, were both faithfully reflecting the discussion as a whole. Later in this part, in Chapter 16, we consider the factors that make or mar the chances of a scientific idea, however well founded, seeing the light of day. That Lewis was a tough and determined character who was in a position to see his suggestion through, was likely to have been a factor in this case. A second important factor was Lewis' ability to turn his general theoretical suggestion for a more exclusively symptom-based classification to good practical effect in the detailed development of the specific categories in the Glossary to ICD-8.

From Lewis with Sartorius through to ICD-10 and DSM-IV

This was indeed what Lewis did. What happened was that Lewis was invited to Geneva by the WHO and spent 4 days working intensively with Norman Sartorius to complete the Glossary to ICD-8 (Sartorius, 2004), building on an existing UK glossary (General Register Office, 1968) that had been produced by a small group (including Stengel) chaired by Lewis. DSM-II, as we noted earlier, subsequently adopted the ICD-8 nomenclature; ICD-9

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 333

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incorporated the ICD-8 Glossary with a few minor changes; DSM-III took the idea of 'operational' criteria further with explicit inclusion and exclusion criteria for class membership; and ICD-10 and DSM-IV added an evidence-based approach to the process of developing and changing criteria and classes.

Hand in hand with these developments in psychiatric classification, as we noted in Chapter 12, have gone corresponding developments in structured and semi-structured instruments for assessing psychopathology. These instruments, to the extent that they seek to operationalize the observations on which clinical diagnosis depends in terms of carefully defined activities (or operations) for eliciting and recording symptoms, are, perhaps more so even than the criteria in modern psychiatric classifications, faithful to the principles of operationalism developed by Bridgman, referred to by Hempel, and embraced by Stengel and Lewis. Sartorius, as we saw in Chapter 12, was one of the three authors of the first semi-structured, internationally used and adapted mental state interview with known reliability, the PSE (Wing *et al.*, 1974). Sartorius was also to continue at WHO, eventually as the Director of the Mental Health Section, in which capacity he co-ordinated the production of both ICD-9 and ICD-10. But the decisive move, the (tactical) retreat from premature theory to a descriptive symptom-based structure for psychiatric classification, we owe, for good or ill, to Lewis.

For good or ill?

We say 'for good or ill', for of course, as we saw from Kupfer *et al.*'s *Research Agenda for DSM-V* at the start of this session, although psychiatry has indeed enjoyed the expected gains in reliability, and corresponding improvements in communication, at least among psychiatrists, and although there have been advances in epidemiological understanding, psychiatric classifications remain much embattled from without and contested from within. We will return to the current difficulties of psychiatric classification in a preliminary way in the final part of this session and again and in more detail in Session 4 of this chapter. We will now pause, though, for a moment to take stock of the agenda for psychiatric classification as it stood at the time of the publication of ICD-8. As we will see, the agenda at this time, the agenda that we will call our *Conceptual Research Agenda for ICD-9*, was considerably more upbeat than Kupfer *et al.*'s current *Research Agenda for DSM-V*.

The second new agenda – a conceptual research agenda for ICD-9

The next reading (linked with Exercise 9) is the Foreword to the Glossary to ICD-8 written by Sir Aubrey Lewis (as he had by then become). Published by the WHO in 1974, Lewis' Preface to the Glossary to ICD-8 was also included as a Foreword to ICD-9 '… in recognition of [the late] Sir Aubrey's fundamental contribution to that glossary [ie the ICD-8 Glossary], which constitutes the core of the glossary that is now an official part of the mental disorders chapter of the Ninth Revision [of ICD-9]' (WHO, 1978, p. 5). It is the latter (the Foreword to ICD-9) that is reproduced in the reading below. As just noted, you will see that Lewis is considerably more upbeat about the research agenda for psychiatric classification at the time of the publication of the Glossary to ICD-8, than his successors, Kupfer *et al.*, some 30 years later, in their Introduction to the *Research Agenda for DSM-V*.

EXERCISE 9 (30 minutes)

Read Lewis' Foreword to the ICD-8 Glossary:

WHO (1974). *Glossary of Mental Disorders and Guide to their Classification, for use in Conjunction with the International Classification of Diseases*, 8th revision. Geneva: World Health Organization

Link with Reading 13.8

Lewis' Preface to ICD-8 is short but it packs, as you would expect from someone with Lewis' depth of knowledge and powerful discursive skills, a whole series of punches. As you read what Lewis says, think about the Agenda for psychiatric classification implicit in Lewis' Preface. Lewis of course does not use the term 'agenda'. None the less, his Preface amounts to a *Conceptual Research Agenda for ICD-9.*

In this part of the exercise you may want to use the agenda items listed in Table 13.1 (our Conceptual Research Agenda for DSM-V).

There are three stages to this exercise:

1. draw up your *own list of agenda items* as they are identifiable in Lewis' Preface. As before, it is important to complete your own list before referring to our suggestions below. Then,
2. *compare and contrast* Lewis' Agenda with that of Kupfer *et al.* (as summarized in Table 13.1). The main difference between them, as we have already indicated, is in their respective 'big issues', reliability (for Lewis at the time of ICD-8) and validity (for Kupfer *et al.* 30 years later in 'their' Research *Agenda for DSM-V*). But there are many other differences and also some important similarities between them. Finally,
3. consider the *wider influences* of the main difference between Lewis and Kupfer *et al.* in their 'big issues' (respectively of reliability and validity), i.e. how does the emphasis on reliability in Lewis' Agenda and the emphasis on validity in Kupfer *et al.*'s Agenda influence the ways they think about other items on their respective Agendas for psychiatric classification.

Lewis' Preface is a masterpiece of compression of thought. It is difficult to summarize what he says without writing a much longer piece! There are a whole series of points relevant to the research agenda for psychiatric classification at the time. We draw these points out here in the order in which they are given above in our *Conceptual Research Agenda for DSM-V*.

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 334

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Thus, among the points Lewis manages to compress into this short piece are:

- *Agenda Item 1: Timeline*. Lewis, like Kupfer *et al*., takes a long view: he notes that psychiatric classification stands in a long tradition, back to the second century BC.
- *Agenda Item 2: Uses of Psychiatric Classifications*. Lewis observes that classifications serve many purposes—note his observations on the 'multifarious needs' served by glossaries and classifications. But he focuses, like Kupfer *et al*., on the uses of psychiatric classifications for diagnoses made by clinicians, in this case as the basis of international epidemiological research and statistical reporting.
- *Agenda Item 3: Misuses of Psychiatric Classifications*. Although a 'respectable' tradition, Lewis says, compiling glossaries, and by implication compiling classifications, also has a shadow side. The term 'glossary', Lewis continues, comes from the same root as to 'gloss over' or 'to gloze'. Hence we find pejorative associations with psychiatric classification, many of which, as listed by Lewis, remain all too familiar today: 'mere labelling', 'the neat complacency of classification', 'nosological stamp collecting', and a 'medical hortus siccus' (a dry garden).

  Such name-calling is generally unwarranted, no doubt, but one particular reason for classifications falling into disrepute is, Lewis says, 'the excesses to which classification was pushed in the late 18th and early 19th centuries'. Beware, then, is Lewis' implication, similar excesses today as successive editions of both ICD and DSM trump each other in length and complexity. Kupfer *et al*. are concerned similarly with overextension, albeit a different kind of overextension, namely reification of the concepts in the DSM.
- *Agenda Item 4: Status of Psychiatric Science*. Lewis is clear that psychiatric classification is a more difficult case of medical classification. The 'modern psychiatric glossarist', he says, faces similar difficulties as the 'compiler of other medical classifications', only more so. Lewis' 'more so', however, is expressed, much as Kupfer *et al*.'s corresponding agenda item is expressed, in deficit terms: the difficulties of psychiatric classification are the same as for medical classifications, Lewis says, but 'aggravated by hazards arising from *paucity* of the objective data on which definition and diagnosis must depend': again, he [the modern psychiatric glossarist] has to *contrive* appropriate criteria . . .' (both second paragraph, emphases added).
- *Agenda Item 5: Concepts*. Lewis pinpoints one particular difficulty in producing a classification of mental disorders, namely in the concept of disorder itself. In this respect, then, in naming the conceptual difficulties for what they are, Lewis differs from Kupfer *et al*. There is no surprise in the fact that Lewis is alert to conceptual difficulties: you will recall his papers on diseases as 'part functions' and on the difficulties associated with the core concept of traditional psychopathology, psychosis, in Part I. What does come as a surprise, though, is that Lewis pinpoints the difficulty, not in any particular property of the concept of mental disorder (or of a mental disease, as he would have said), but rather in the abstract status of concepts themselves. This is a surprise because, as Lewis himself makes clear, diseases in *general* (not just in psychiatry) are 'abstract concepts'.

  This is how Lewis puts it: 'since diseases are in any case abstract concepts, it is no wonder that the disease constructs which psychiatrists work with have shimmering outline boundaries and overlap.' But put like this, it *is* a wonder, a *big* wonder, why the disease constructs with which *psychiatrists* work should have 'shimmering outline boundaries and overlap', while those of *bodily* medicine, which equally according to Lewis are 'abstract concepts', do not. (Remember that in this context Lewis is concerned with the *added* difficulty of psychiatric classification.) This brings us, therefore, in our Agenda, firmly to issues of reliability and of validity.
- *Agenda Item 6: Reliability*. As we have several times noted, where validity is the big issue for Kupfer *et al*., in their *Research Agenda for DSM-V*, the big issue for Lewis is reliability. Interestingly, given the volume of research on reliability, Lewis does not confine himself to the need merely for reliability. Instead, he couches his remarks in terms of the wider need for psychiatry to improve the observational basis of its science. Thus, 'Observer variation', Lewis says in the second paragraph in the reading, 'is disconcertingly (note the deficit word) in evidence; [and] reliability is too low for scientific comfort' (yet another deficit phrase).

  But if the observational difficulties that beset psychiatric science are defects (as just noted, Lewis, like Kupfer *et al*., uses deficit words of psychiatric science), such defects, Lewis continues, are to an extent remediable, depending on whether they arise from '. . . inexact perception, personal bias, or divergency of the nosological systems or terms used'. So, Lewis says, in the next paragraph, 'The glossary put forward here, when faithfully applied, reduces the scope for error.' Why? Well, in providing for *con*vergence in the nosological terms used. But this is not a stand alone solution. It needs to be combined with constant attention to the need for 'accurate observation' rather than falling back on, in Lewis' words from his intervention in the 1959 conference (see reading linked with Exercise 9), '. . . the many hypothetical and unconfirmed schemas of 'psychodynamic mechanisms', and the concern with etiological inference . . .'. Thus it is, Lewis says, that 'since the disorders listed in this glossary are identified by criteria that are predominantly *descriptive*, its use should encourage an emphasis on careful *observation*' (emphases added).
- *Agenda Item 7: Validity*. Validity, although the big issue for Kupfer *et al*., was important for Lewis, too. However, Lewis (although not using the word 'validity') takes it for granted that as a medical classification, a valid psychiatric classification will be concerned with diseases. The aim is to 'contrive appropriate criteria for differentiating one disease from another . . . a consistent scheme with which they [psychiatric diseases] will all fit'. Thus far, then, if we translate Lewis' 'diseases' into Kupfer *et al*.'s 'pathophysiological mechanisms', the two agendas are in agreement. A valid psychiatric classification, however, Lewis continues, reflecting his comments in the discussion following Hempel's paper, may take a wide variety of different forms. It may be descriptive or theoret-

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 335

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ical in structure: it 'may be based on clinical patterns (syndromes) or on clinical course; it may be psychodynamic, etiological (genetic), or pathological'. Lewis, then, is considerably more open in what he will allow as a valid scientific theory than Kupfer *et al.* (who focused on only the last two of Lewis' list).

- *Agenda Item 8: Structural Solutions*. As noted above (under Agenda Item 6, Reliability) Lewis, building directly on what he said in his intervention in the discussion following Hempel's paper during the 1959 conference, makes fully explicit that in the ICD-8 classification and its supporting Glossary, to repeat the key phrase quoted above, the 'disorders listed . . . are identified by criteria that are predominantly *descriptive* . . .' (again, emphasis added). Lewis, in contrast to Kupfer *et al.*, is thus confident of a 'reliability first, validity second' approach to building more scientific classifications of mental disorder.

- *Agenda Item 9: Process Solutions*. Lewis emphasizes the importance of collegiality (Lewis' 'spirit of cooperation'), of international collaboration (Lewis' 'international group of collaborators and advisors'), and of contributions from different perspectives (Lewis' 'advisors of such diversity of background and outlook'), all in his final paragraph.

  Also important was the fact that all those involved had a shared aim. What drew everyone together, what secured collegiality, international collaboration, and diverse disciplinary perspectives, what allowed 'the emergence of an agreed version', albeit with some residual 'compromises and anomalies', was, in Lewis' closing phrase, the 'common recognition of an urgent need for better means of communication'.

- *Agenda Item 10: Utility (of new Classifications)*. Lewis retains here his focus on the role of the new descriptively based classification as a tool primarily for improved international epidemiological research and statistical reporting by way of improved communication. As noted above, it is implicit that this will lead on as in other areas of medicine, to a variety of aetiological theories; but this is left unsaid; and of course, in his intervention in the 1959 conference, Lewis left open the possibility, with his distinction between public and private classifications, that other quite different classifications may serve the purposes of development of theory within different research programmes.

- *Agenda Item 11: Overall Tenor*. In his Preface, as we noted earlier, Lewis is a good deal more positive and optimistic than Kupfer *et al.* in their *Research Agenda for DSM-V*. Taking Agenda Items 9, 10, and 11 together, then, we can see that Kupfer *et al.* share with Lewis the goal of improved communication achieved through inter-disciplinary and international collaboration. The main difference between them, however, is that Kupfer *et al.*, while acknowledging (and indeed themselves contributing to) the improvements in the reliability of our classifications that have flowed from improvements in communication between clinicians, are, with the benefit of 30 years hindsight, considerably more downbeat than Lewis about the prospects for improvements in validity.

We summarize the above points in Table 13.2. This table also compares our *Conceptual Research Agenda for ICD-9*, as the second of our two new agendas, with the main points from our first new agenda, set out fully in Table 13.1, our *Conceptual Research Agenda for DSM-V* (as desired from Kupfer *et al.*'s *Research Agenda for DSM-V*).

What Table 13.2 brings out, to come to the third part of Exercise 9, is the importance of their respective 'big issues', reliability for Lewis in *ICD-9* and validity for Kupfer *et al.*, in *DSM-V*, in driving the similarities and differences between them on other agenda items. Thus, for Items 1–5 (the items coming before 'reliability') the two agendas, although separated by some 30 years, are in many respects remarkably similar: both (Item 1) adopt long timelines; both (Item 2) recognize the importance of communication between clinicians; both (Item 3) warn of misuses, respectively through overextension and by reification of categories; both (Item 4) assume a deficit model of psychiatric science; and both (Item 5) recognize the importance of conceptual issues (although Kupfer *et al.* do not name them as such).

From this point, however, their agendas diverge. For Lewis, improved reliability (Item 6) is a key step towards improved validity (Item 6), which, he assumes, will follow switching from a theoretical (aetiology-based) to a descriptive (symptom-based) classificatory structure (Item 8), settled by way of a collegial international process (Item 9), aimed at producing, in the first instance, 'public' classifications suitable for international epidemiological and statistical purposes (Item 10), but with a good prospect of ultimately leading to wider gains in validity (Item 11). Kupfer *et al.*, by contrast, starting from their perception that improved reliability (Item 6) has failed to deliver improved validity (Item 7), hope to cut directly to aetiology-based classifications (Item 8) by way of empirical research (Item 9) as the foundation for improved treatments (Item 10), all of which, in their estimation, the last 30 years of developments in psychiatric classification, and by implication psychiatric science, have singularly failed to deliver (Item 11).

Reliability and validity

Reliability and validity, then, are the fulcrum around which everything else on both agendas turns. This is why, in developing Table 13.1 as a summary of our *Conceptual Research Agenda for DSM-V*, we put these two items in the middle of the agenda, as a fulcrum or pivot, even though we dealt with them first. But how exactly are reliability and validity pivotal? As we saw earlier, translated and redirected by Lewis and Sartorius into the symptom-based classification of ICD-8, Hempel's ideas have been crucial to setting the course along which psychiatric classification has subsequently developed. But this course, in focusing on improved reliability (as an aspect of Hempel's Empirical Import) has been in the opposite direction to that anticipated by Hempel (towards ever-greater Systematic Import). In assessing, therefore, the relationship between the emphasis on reliability in Lewis' *Agenda for ICD-9* and the emphasis on validity in Kupfer *et al.*'s *Agenda for DSM-V*, it will be important to look carefully at what Hempel actually said about reliability and validity. We began to unpack Hempel's ideas on these two topics earlier in this session when (in

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 336

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Table 13.2 The Second New Agenda – a Conceptual Research Agenda for ICD-9 – compared with the first new agenda

		ICD-9 (second new agenda)	DSM-V (first new agenda–table 13.1)
(1)	Time-line	Back to 2nd century BC	Back: to DSM-III Forward: to DSM-VI/VII
(2)	Uses of psychiatric classification	ICD primarily for diagnoses made by clincians as the basis of epidemiological research and statistical reporting	Diagnosis by clinicians
(3)	Misuses of psychiatric classification	Overextension: i.e. where classifications are driven to excessive lengths	Reification of categories
(4)	Status of psychiatric science	Deficit model: implicit in terms used to describe current diagnostic classification, e.g. 'paucity', 'contrive'	Deficit model
(5)	Concepts	Concepts explicitly identified as problematic—but medical concepts in general, not concepts specifically of mental disorder (i.e. not differentiated from concepts of bodily disorder)	Important but not named as such
(6)	Reliability	THE BIG ISSUE: poor, though remediable, reliability reflecting unsound observational base at the root of difficulties of psychiatric classification	Run out of steam
(7)	Validity	Assumed to follow: wide variety of possible scientific theories assumed to follow improved reliability (provided observational basis remains secure) much as in bodily medicine	THE BIG ISSUE
(8)	Structural solution	Improved reliability: achieved by switching from premature aetiology-based to descriptive symptom-based criteria	Aetiology-based
(9)	Process solutions	Importance of collegiality, international collaboration, and incorporation of different perspectives, all within a shared aim	Empirical research
(10)	Utility (of new calssifications)	'Public' (like ICD): for (international) epidemiological and statistical purposes. 'Private': for 'local' use in connection with development of theory.	Better health-care services
(11)	Overall tenor	Up-beat: positive about the opportunities for improved reliability as the basis for international epidemiological studies, (and improved communication generally between researchers) which could contribute to improved validity.	Downbeat
(12)	Any other business	none	none

Exercise 6) we looked at a key passage in his paper in which he introduced the idea of operationalism. In the next and final section of this session we look at a further passage from Hempel's paper, with a view to unpacking further, and to an extent critiquing, what Hempel has to say in the light of subsequent developments in psychiatric classification.

Operationalism, reliability, validity, and a first look at what lies ahead

First, then, we return to the key passage from Hempel's paper on operationalism.

EXERCISE 10 (20 minutes)

This reading follows on immediately from the passage we looked at in Exercise 6:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press

Link with Reading 13.9

In the earlier passage, Hempel, you will recall, introduced the idea of operationalism, which, although derived from physics, offered one way of responding to the difficulties of psychiatric classification. Here, by contrast, Hempel focuses on some of the limitations of operationalism.

1. What are these limitations?
2. Do you agree with the consequences that Hempel draws from these limitations for the relationship between psychiatric classification and diagnosis?
3. What conclusions can we draw from Hempel's account of the limitations of operationalism for reliability and validity and, hence, the possible future direction of the development of psychiatric classifications?

Hempel's three limitations of operationalism

In this important passage Hempel notes three (although he says 'two,' see below) important limitations of the application of the principles of operationalism, as formulated by Bridgman, to psychiatry. The second of these limitations has been widely recognized in psychiatry (it is noted by Stengel, 1959, p. 612, for example), but, we will argue, also widely misunderstood. We will start with this

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second limitation, therefore, before turning to Hempel's other two limitations, which, we will argue, despite being less widely recognized in psychiatry, turn out to be even more important.

Limitation 1: mere observation

First, then, Hempel's second (and our first) limitation on operationalism: this is the limitation that, for psychiatry, the insistence on an operational specification of meaning '. . . has to be taken in a very liberal sense which does not require manipulation of the objects under consideration: the mere observation of an object, for example, must be allowed to count as an operation . . .'. Hempel then goes on a little later to spell out that this liberal sense of operationalism does not let us off the hook of defining criteria with '. . . reasonably precise meaning and (that) are used, by all investigators concerned, with high intersubjective uniformity . . .', that is, and here Hempel comes back in effect to reliability, '. . . a high degree of agreement among different observers . . .' (Hempel, 1961, pp. 320–321).

It is Hempel's use of the phrase 'mere observation' in this passage that has descended, via Stengel (1959, p. 612) and others, into psychiatry as the justification for allowing the history taking and other clinical observations on which psychiatric assessments are based, to count as operations. Hempel, indeed, seems to warrant this directly, to the extent that, in the text cited in the paragraph immediately above, he appears to endorse such an interpretation of a 'liberal operationalism', provided that the criteria adopted in translating the 'direct observations' derived clinically into diagnostic categories, are reliable (provided, that is, they show '. . . a high degree of agreement between different observers').

Mere 'behaviourist' observations

However, if we look more carefully at the passage, it appears there is rather more in Hempel's mind than, 'merely', the interview processes on which psychiatric diagnostic assessments depend. Thus, in developing his point Hempel starts by asking us to consider the example of endomorphy. Endomorphy, he says, by way of illustrating what he means by a 'liberalized' operationalism, is defined by 'directly observable features'. Well, fair enough. But endomorphy is defined by certain *bodily* features. Hence, Hempel's example of what the 'mere observation' of a liberalized operationalism would amount to, is already a perhaps somewhat surprising step away from the application of operationalism to the features of *mental* states with which psychiatry is characteristically concerned. And two paragraphs later, Hempel's 'directly observable' becomes, for purposes of operational formulations of psychological terms '. . . *publicly* observable aspects of behaviour . . .' (emphasis added), which in turn is unpacked as '. . . publicly observable aspects of *behaviour* a subject shows in response to a specified publicly observable stimulus situation . . .' (emphasis added). Hempel's 'liberal' operationalism, then, is very far from being 'mere observation' if 'mere' means interviews of the kind on which psychiatry characteristically relies for its diagnostic formulations. By a 'liberal' operationalism, what Hempel appears to have in mind is, rather, an essentially *behaviourist* interpretation of observation in the psychological (including by implication psychiatric) sciences.

Operationalism gets the credit but reliability does the work

To what extent, therefore, even the semi-structured interviews discussed in Chapter 12 (such as the PSE), let alone general clinical diagnostic assessment in psychiatry, can be considered to satisfy behaviourist principles, and hence Hempel's notion of a 'liberal' operationalism, is at the very least a matter for debate. 'Mere' observation may, indeed, be capable of satisfying operationalist principles in the psychological sciences. But strip away the language of operationalism, and it seems that much of the work of operationalism in improving the Empirical Import of psychiatric classificatory terms, as set out there by Hempel in his paper, falls back on the shoulders of reliability. The Empirical Import of psychiatric classificatory terms, that is to say, if we follow Hempel's unpacking of his point (as set out above), comes down, to repeat Hempel's concluding phrase cited above, '. . . a high degree of agreement between observers'.

Physics and psychiatry

Psychiatric classifiers, therefore, from Stengel onwards, may have been a little hasty in assuming that Hempel's phrase 'mere observation' warranted their oft repeated claim that modern psychiatric classifications reflect the principles of operationalism as developed by Bridgman for the case of physics. We will see later in this chapter, particularly in Sessions 3 and 4, that physics, and the philosophy of physics, have much to teach us in psychiatry, and for the best of reasons, namely that the two disciplines, physics and psychiatry, are alike in being peculiarly difficult sciences. But as we will also see particularly in Session 4, the differences between physics and psychiatry, notably that where physics is concerned with particles, psychiatry is concerned with people, mean that we have to exercise extreme care in applying any lessons learned from the one to the other.

In this instance, with Hempel's suggestion that operationalism may be helpful in psychiatry, we have seen that a careful reading of Hempel's own account of his suggestion, shows that it is, really, reliability that is doing the work. There is nothing extra that operationalism adds beyond the requirement (which of course is generic rather than a requirement specifically of operationalism) for the use of terms with '. . . reasonably precise meaning(s) . . .'. Recognizing that the work of Empirical Import is being done by reliability rather than by operationalism is important in principle for psychiatry today because of what we will argue below is the tendency, as in Kupfer *et al.*'s *Research Agenda for DSM-V*, to over-react to the (perceived) failure of reliability to deliver validity. First, though, we take a step back, to Hempel's first limitation on operationalism, as set out in this reading, what he calls (and what we renumber here as Limitation 2), the limitation of 'partial criteria of application'.

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Limitation 2: partial criteria of application

Hempel's first limitation on operationalism, as set out in this reading, is that 'the operational criteria of application available for a term often amount to less than a full definition' (p. 320). This limitation on operationalism, as we noted above, is less widely recognized in psychiatry than Hempel's 'mere observation' limitation. There are two reasons why this might be so. First, it is a 'limitation', not of operationalism as it has to be 'liberalized' (Hempel's term) for purposes of application to the mental sciences, but of operationalism in *general*, including as it applies to the physical sciences. Hempel indeed gives the example of different thermometers being used across different ranges of temperature and being designed for different purposes. A thermometer, Hempel points out, provides an operational criterion of temperature. But no one thermometer can provide a complete operational definition of temperature. Indeed, while this or that thermometer may make an operational test possible in a given situation, 'there are reasons to doubt the possibility of providing *full* operational definitions for all theoretical terms in science…' (p. 320, emphasis in original).

Psychiatry, then, might reasonably have neglected this limitation on operationalism, it being a limitation that is inherent in operationalism itself rather than as applied specifically to psychiatric science. Moreover, the specific sense that Hempel gives to this limitation when he first introduces it, that the limitation arises from the 'partial criteria of application' of a concept, is, it would seem, readily and unproblematically transferable to psychiatry. Hempel's example of different thermometers, used across different ranges of temperature and designed for different purposes, is transferable more or less directly to psychiatry's wide variety of different semi-structured interview schedules (noted in Chapter 12) that, for a given area of psychopathology (such as depression), are used across different ranges of severity and are designed for different purposes (clinical, research, etc.).

As noted above, not much is lost to psychiatry through it being relatively unaware of the Limitation of Partial Criteria of Application. It might sometimes be helpful for those working with psychiatric diagnostic concepts to be more aware of this limitation of operationalism. This could help avoid, for example, the all-too-frequent assumption that a particular score on, say, the Beck Depression Inventory (which is a highly reliable instrument, Beck *et al.*, 1961), is sufficient to give an operationalized answer, across all kinds of depression and for all purposes, to the diagnostic question whether a given person is depressed.

Limitation 3: antecedently understood terms

Hempel, however, in the last paragraph of the reading, in developing his liberalized version of operationalism, comes to a further and quite different limitation of the operational definitions of terms, a limitation that, in contrast with the Limitation of Partial Criteria of Application, and more so even than the Limitation of Mere Observation, really is important for psychiatry. It is because this further limitation is so important for psychiatry, that we have characterized it here as a distinct third limitation of operationalism, rather than, with Hempel, subsuming it to his Limitation of Partial Criteria of Application. The third limitation of operationalism, then, as set out by Hempel in this passage, is that 'It would be unreasonable to demand that *all* of the terms used in a given scientific discipline [can] be given an operational specification of meaning…' Why? Because '… then the process of specifying the meaning of the defining terms, and so forth, would lead to an infinite regress' (p. 321, emphasis in original). Why? Because, Hempel continues, 'In any definitional context (quite independently of the issue of operationalism), some terms must be antecedently understood…'.

Two key points for psychiatry

There are two key points for psychiatric classification to take from this important paragraph of Hempel's paper. The first key point is the point made above, that although merged by Hempel, the Limitation of Partial Criterion of Application, as illustrated by thermometers and semi-structured interviews, is very different in kind from the Limitation of Antecedently Understood Terms. The Limitation of Partial Criteria of Application is about the need for more than one equally well operationalized instrument (across different domains of a given variable and for different purposes). The Limitation of Antecedently Understood Terms, by contrast, is about a given definition—appropriate in and of itself for a given domain of application and for a given purpose—being, in and of itself, not fully operationalizable.

It is Hempel's *explanation* of this limitation that gives us the second key point for psychiatric classification that we should take from this paragraph. Recall how Hempel puts this: 'In *any* definitional context', he says, 'some terms must be antecedently understood' (p. 321, emphasis added this time). Put like this, then, Hempel's point in this passage on the limitations of operational definitions is thus nothing more nor less than precisely the point about, and hence carries all the importance for psychiatry of, the limitations of definitions in general that we explored in detail in Chapter 4 of Part I. Hempel indeed, in the parenthetical addition to his spelling out of his point, in the passage as quoted above, makes clear that this limitation is one that applies generally to all definitions and 'quite independently of the issue of operationalism' (p. 321).

Consequences for psychiatry

With Hempel's third (as we have numbered it) limitation on operationalism, then, the Limitation of Antecedently Understood Terms, go all the consequences for psychiatric science of the limitations of definition that we explored in Part I. It will be worth reminding ourselves of two of these consequences briefly as they apply to the definitions in scientific, including psychiatric scientific, classifications:

1. '*First define your terms*'. Once the limitations of definition are recognized, the 'first define your terms' assumption of traditional medical science becomes, with Austin and the 'philosophical fieldwork' of linguistic analysis, 'first look to the *uses*

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of your terms'. Recall that in Austin's formulation, it is our ability to *use* high-level concepts (Hempel's *antecedently understood* terms) that allows the definitions of lower-level terms to be meaningful—in one of our examples from Part I, we can define the lower-level 'watch' only because we already understand, in the sense that we can already use, the higher-level 'time', even though the concept of time is deeply resistant to explicit definition. Correspondingly, therefore, in Hempel's terms, we can say that operational definitions of time in physics, in terms of accurate 'watches' such as atomic clocks, rely directly on an antecedent understanding (ability to use) the concept of time—for how, otherwise, with an extension of this point relevant to psychiatry's concerns with validity, would we know whether an operational definition of time was indeed of time or of something else; how, otherwise, to put the point in more familiar terms, would we know that, say, a watch (being a small instrument normally worn on the body and used for purposes of counting intervals of time) is indeed a kind of clock and not, say, a pedometer (being a small instrument normally worn on the body and used for purposes of counting intervals of space)! It is (generally) obvious in practice which is which. But it is only obvious because we understand the meanings of the relevant higher-level concepts, time and space, 'antecedently' as Hempel says here in Part III, and as evidenced by our ability to 'use' them as Austin said in Part I.

2. *Use and definition of terms in psychiatry*. In bodily medicine, the 'first define your terms' assumption of the traditional model works, by and large, fine, because, by and large, the relevant diagnostic terms for practical purposes are, in the formulation suggested by linguistic analytic philosophy, relatively low-level terms. In other words, the relevant diagnostic concepts in bodily medicine are, for example, the lower-level *heart* disease' not the higher-level '*disease*'. But of course in psychiatry, corresponding with its conceptually more difficult science, the higher-level concepts—illness, disease, dysfunction, disorder, etc.—are often precisely those that are relevant diagnostically. This is why there has been such vigorous debate about these concepts among those concerned practically, as users or as providers of services, within psychiatry, while in bodily medicine, by contrast, although there has been some debate at a theoretical level about the higher-level concepts, at a practical level debate has been considerably more limited. This is also why, to come to classification, the definitions in psychiatry's classifications so often appear muddled and incomplete compared with the definitions in the classifications of bodily medicine. The conceptual difficulties of psychiatric classification, indeed, as we saw in Part I, arise not only with concepts such as illness and disease, at the top of the medical hierarchy as it were, but with the concepts a level or two further down: thus 'delusion', as we saw, is more difficult to define than the lower-level 'delusion of guilt'; and, conversely, 'delusion', as a particular kind of psychotic symptom, is easier to define than 'psychosis'.

Conceptual difficulties: explicit and implicit

With these two considerations from Part I, therefore, taken together with Hempel's third limitation of operationalism (the Limitation of Antecendently Understood Terms), we are brought back squarely to the conceptual difficulties of psychiatric classification as recognized by Lewis (and summarized in Table 13.2, our *Conceptual Research Agenda for ICD-9*) and as they are implied by Kupfer *et al.* (and summarized in Table 13.1, our *Conceptual Research Agenda for DSM-V*). We are brought back squarely to these difficulties as *explicitly* conceptual difficulties about concepts of disorder (under Agenda Item 5). We are also brought back squarely to them as *implicitly* conceptual difficulties about reliability (under Agenda Item 6) and about validity (under Agenda Item 7). We will not, here, attempt a full explication of the conceptual difficulties raised under the latter two agenda items, either separately or taken together (the latter being the conceptual difficulties raised by the links between reliability and validity). Exploring these difficulties, even using the resources only of Logical Empiricism, would take us into all the deep difficulties about realism, essentialism, and so forth, with which this chapter as a whole, and the background philosophical traditions on which it draws, are concerned. We will return to some of these later. For the moment, though, we will note just two implications, the first for reliability (Agenda Item 6) and the second for validity (Agenda Item 7), by way of a promissory note for a further exploration of the potential relevance of the philosophy of science for psychiatric classification in Session 4.

Reliability

Reliability, as we have several times noted, although the big issue for psychiatric classification with Lewis in 1974, becomes, by the time of Kupfer *et al.*'s *Research Agenda for DSM-V* in 2002, at best a side-show, at worst a possible barrier to the big issue for DSM-V, validity. The implication, then, is that with future psychiatric classifications, reliability might have to be sacrificed for purposes of improving validity. A careful reading of Hempel's paper, and of the way in which his ideas were translated by Lewis and Sartorius into psychiatric classification in the ICD-8 and ICD-9, suggests that any inclination to trade reliability for validity, risks throwing out the baby with the bath water.

We return to the risk of throwing out the baby of reliability in Session 4, when we develop our third new agenda for psychiatric classification, drawing, particularly, on work in the philosophy of physics. For now, though, the key point about reliability to take from a more careful reading of Hempel's paper is that *reliability is a small but still necessary step to validity*. Thus, neither Hempel nor Lewis and Sartorius intended that reliability should become the focus of research on psychiatric classification at the expense of validity. On the contrary, Hempel explicitly, and Lewis and Sartorius implicitly, turned to reliability only as a *step*, and a *small* step at that, towards improved validity. Hempel, in particular, as we have seen, sketches, no more, only a tangential link between reliability and even Empirical Import, let alone Systematic (theoretical)

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Import. Reliability, however, *is* none the less important. It may be only a *step*, it may be only a *small* step, but it is a *necessary* step. We will indicate below, in Session 2, some of the many ways in which reliability and validity (at least as reflected in objectivity) are linked. But the importance of reliability as a small but necessary step to validity is signalled here by the fact that notwithstanding Hempel's limited references to reliability, in his own presentation of operationalism, it is reliability (and not operationalism) that, as we saw earlier, ends up doing the (empirical) work.

Hempel's example of the 'praecox feeling' makes the point. And it is a point, as it turns out, against himself! Earlier in his paper, as we saw in the reading linked with Exercise 6, he had criticized the praecox feeling as the basis for a satisfactory diagnostic criterion, because, he said, it is a matter of *feeling* not of observation. And of course compared with the criteria for endomorphy (Hempel's example), the praecox feeling is, to any obvious reading, more subjective. But if the real work of operationalism is being done by reliability, what matters is not whether a criterion is based on observations that are more or less subjective, but on whether the criterion in question is based on observations that are reliable. The praecox feeling may or may not be reliable (i.e. capable of being identified with a high degree of agreement between observers). But it is reliability that is the key issue in Hempel's own terms.

Validity

Recognizing that reliability is what is doing the work of 'operationalism' in psychiatry is important for psychiatric classification because, if we get too hung up on 'operational' criteria, rather than relying on reliability, we risk cutting ourselves off from whole rafts of observational data that may have crucial significance for the development of theory. This is why, as we said, talk of trading reliability for validity risks throwing out the baby with the bath water.

There is indeed more than one baby here. First, there is the 'baby' of phenomenology and related disciplines. As just noted, the subjective praecox feeling may or may not be reliably identifiable. But all of phenomenology, on which as we saw in Part II descriptive psychopathology is based, is about the structure of subjectivity. Phenomenology, furthermore, as we noted in Part I, has recently been making a strong comeback into mainstream psychiatry in part as a direct response to the needs of the new neurosciences; and the new 'mental sciences', as we might call them, all the many new methods now available for the rigorous study of subjectivity, are already producing results, which, combined with the results of the new neurosciences, have considerable potential significance for the advancement of psychiatric scientific theory (Fulford *et al*., 2003).

Second, and closer perhaps to the immediate concerns of current psychiatric classifications, there is the 'baby' of the concepts of traditional psychopathology, some of which, without regard to their reliability, and hence potential for the development of theory, have already been expelled from our classifications for want of clear explicit definition. The concept of psychosis, as we saw in Part I, is the prime case in point. Psychosis is indeed difficult to define. This is why, as Lewis himself originally advocated (Lewis, 1934), the concept was (ostensibly) dropped between ICD-9 and ICD-10. Yet, at least as expressed in such paradigm psychotic symptoms as delusion, psychotic loss of insight is identifiable with a high degree of reliability (Wing et al., 1974) (see Chapter 3). The concept of psychosis, then, has, as we could now say in Hempel's terms, considerable Empirical Import and hence may also have considerable (albeit still only potential) Systematic Import.

Third, and finally, there is the 'baby' of psychoanalysis. As we noted above, Hempel, although critiquing the (as he saw them) subjective terms in which the psychodynamic categories of the DSM of his day were defined, implied that psychiatry's response to this should be to 'operationalize' the terms in question, not to reject psychodynamic theory as such. So far as Hempel's influence on psychiatric classification is concerned, therefore, it seems that psychodynamic theory has been eliminated from ICD and DSM on the basis of a misunderstanding. There may be other reasons for not basing an international psychiatric classification on psychodynamic concepts. But Hempel's (supposed) critique of their Empirical Import is not one of them. Hempel's criticisms of psychodynamic theory, as we saw earlier in this session, were criticisms, not of its status as a theory, but of what he perceived to be the poorly defined terms in which, at the time, it was formulated. Yet these terms, to cite again the work of Peter Hobson (1998) and others, whatever their Systematic Import, have no less Empirical Import (to the extent that they are no less capable of reliable use) than many of the symptom categories in the PSE.

Again, our point here is not to endorse or otherwise this or that theoretical approach, this or that theoretical concept, but rather to be clear that we increase, rather than decrease, our chances of achieving validity by taking, with Hempel, Lewis, Sartorius, Spitzer, Frances, and others concerned with the development of our current classifications, the first step of reliability.

Conclusions: from logical empiricist to post-logical empiricist philosophy of science

In this session we have explored (in reverse order) the agenda for psychiatric classification as it stood with Lewis at the launch of the Glossary to ICD-8 (our *Conceptual Research Agenda for ICD-9*, see Table 13.2) and the agenda for psychiatric classification as it stands today, as represented by Kupfer *et al*.'s Research Agenda for DSM-V (our *Conceptual Research Agenda for DSM-V*, see Table 13.1). We have also examined the origins of both agendas in Logical Empiricism as represented by Hempel's key paper to the 1959 conference on psychiatric classification, as translated and developed through the work of Lewis, Sartorius, Spitzer, Frances, and others.

In the final session of the chapter, we will bring together our two new agendas, our conceptual agendas respectively for ICD-9 and

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 341

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for DSM-V, with a third agenda, an agenda for what we will call an Extended Family of Psychiatric Classifications. This third agenda seeks to build on the strengths of both earlier agendas (for ICD-9 and DSM-V) by drawing on the resources not just of Logical Empiricism, but also of post-Logical Empiricist philosophy of science. For Logical Empiricism, as we saw in Chapter 12, although a determined and rigorous attempt to identify the essential features of scientific method, ultimately failed to provide a full and final characterization of what science is about. Recent decades have thus witnessed a vigorous development of post-Logical Empiricist philosophy of science, on which, in addition to the (still pertinent) resources of Logical Empiricism, we can draw in developing our understanding of what psychiatric science, and hence psychiatric classification, is about.

It is with post-Logical Empiricist philosophy of science, therefore, that we will be occupied in the next two sessions. Post-Logical Empiricist Philosophy of Science covers a wide range of different approaches. Some approaches are realist in tone, others antirealist. Some seek to secure a broadly traditional model of what science is about, others attack one or other aspects of this model. As we should expect from the considerations of Part I, what will become clear is that there is no simple equation or code that can fully capture what science is about. But as we should also expect from Part I, taken together, these different approaches will add up to a rich potential resource for investigating the complex 'concept + data' driven problems that are at the heart of what psychiatric science is about.

Reflection on the session and self-test questions

Write down your own reflections on the materials in this session drawing out any points that are particularly significant for you. Then write brief notes about the following:

1. What is the 'big issue' on the agenda for those developing the next edition of the DSM (the DSM-V)?
2. What broad kind of issue is evident in the DSM-V Agenda but not named as such?
3. What feature of Logical Empiricism has been carried over into modern international psychiatric classifications?
4. Which American philosopher was particularly influential in this respect? Which British psychiatrist? And which official of the WHO?
5. Which classification was the first to show a direct influence of Logical Empiricism and in what way?
6. What have subsequent classifications added from the account of what it is to be scientific outlined by the philosopher referred to in question 4?
7. What was the 'big issue' on the agenda of psychiatric classification in the run up to ICD-9?
8. What are the three limitations of operationalism identified by the philosopher referred to question 4? Which of these are generic and which are limitations specifically of operationalism in psychiatric science?
9. Name at least two ways in which the development of modern international psychiatric classifications has proceeded differently from expectations since ICD-9? (We noted four.)

Session 2 Values, natural classifications, and the Absolute Conception

In this session we return to one of the recurring themes of this book, namely the role of values in psychiatry. We explored the role of values in relation to concepts of disorder in detail in Part I, and, though less extensively, in relation to Jaspers' psychopathology in Part II. Here the presence of values goes to the heart of the focus of this chapter on what science is about. The familiar intuition, which as we noted in Chapter 11 is a core though usually tacit feature of the traditional model of science, is that, like oil and water, the objectivity of science is immiscible with the subjectivity of values. It follows, therefore, according to this traditional model, that if psychiatry is value-laden it cannot be objective; and that, correspondingly, if its classifications are value-laden they cannot be natural classifications.

Are values and science like oil and water?

It is the oil-and-water intuition about values and science that, as we will see, is behind the line that Thomas Szasz (1972) takes in one of his arguments against the reality of mental illness. It is the oil-and-water intuition, too, that motivates the arguments of those, like R.E. Kendell and Christopher Boorse, who, as we saw in Part I, have sought to exclude values from at least the scientific 'core' of psychiatric theory. But is the familiar oil-and-water intuition about science and values well founded philosophically? Is it really true that the presence of values in a discipline puts it beyond the remit of science?

In this session we will be looking in detail at an attempt by the British philosopher, Bernard Williams, to show that values and science are indeed immiscible. At the heart of the objectivity of science, Williams argued, is an 'Absolute Conception' of the world. Scientific progress consists in approximating ever more closely to the Absolute Conception by progressively shedding the conceptions of the world that we form from (i.e. relative to) this or that particular point of view. Williams based his argument on the progressive replacement in science of secondary qualities (like

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 342

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colours) with primary qualities (like the wave length of light). Secondary qualities, he argued, being perspectival, have no place in the Absolute Conception, and neither therefore (others have argued) have values.

Williams, you will recall from Part I, was one of those 'depressive' philosophers who have been unduly pessimistic about the practical effectiveness of philosophy. His argument for the Absolute Conception can thus be considered to be part of his overall programme of marking the contrast between what he took to be the effectiveness of science compared with the impotence of philosophy. As we will see later in this session, however, there are philosophical counter-arguments to William's Absolute Conception, and, indeed, free-standing arguments to the effect that values and science are not immiscible at all.

Plan of the session

The plan of the session is this. We look first at what Hempel says about values and science in his 1959 conference paper (from Session 1): he is surprisingly mild on values, given his background in Logical Empiricism. We pair Hempel's arguments with some interesting observations on the objectivity or otherwise of humour by the British philosopher, Crispin Wright. We then contrast Hempel's argument with the familiar intuition (the oil-and-water intuition) as deployed by Szasz. This leads into a detailed examination of Williams' Absolute Conception and of some of the arguments, particularly by John McDowell, against it. The bottom line of the session is that there is no 'knock down' argument to support the intuition that values and science are immiscible. In the conclusions to this session, we look briefly at the implications of this bottom line for the role of values in psychiatric classification. We consider the role of values in psychiatric classification in detail in Part IV.

Hempel, Crispin Wright, and Szasz on values and science

Given the extent of debate about the role of values in concepts of disorder, and the centrality of this issue particularly in the psychiatry/antipsychiatry aspects of the debate, Hempel has surprisingly little to say about values in his paper to the 1959 conference on psychiatric classification. True, much of the debate about concepts took off in the 1960s. However, the issues were there, and not unimportant philosophically, all along: in the long-running is-ought debate in ethics, for example (see Part I); in the *Methodenstreit* and, consequently, in Jaspers' work (see Part II); and as a particular (critical) focus of the positivist movement in the philosophy of science in the 1920s and 1930s, of which Logical Empiricism was a development.

Hempel, then, writing as a philosopher of science, and for a conference on the difficulties of psychiatric classification, might have been expected to have had a good deal to say about the role of values. In fact, what he says comes largely in just one short paragraph fairly late in the paper.

EXERCISE 11 (20 minutes)

Read the paragraph from Hempel's paper in which he discusses the role of values in psychiatric classifications:

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press

Link with Reading 13.10

1. How are Hempel's comments here connected with his general arguments about the objectivity of a classification and the ways in which this may be undermined (think about the two requirements he placed on a scientific classification, as outlined in Session 1).
2. What is the significance of the relatively low priority Hempel gives to the role of values?

In Session 1 we noted that Hempel placed two requirements on a scientific classification. The terms employed in a scientific classification, Hempel said, (1) should have Empirical Import, that is they should have clear, public criteria of application, and (2) they should have Systematic Import, they should lend themselves to the formulation of general laws that reflect underlying uniformities in nature. Both of these requirements in Hempel's account can be understood as reflecting different aspects of the more general requirement that scientific classifications should be *objective*.

Hempel's two requirements for objectivity

Hempel's account of scientific classifications thus suggests two routes by which classifications may fail to be objective. The first route is that they may fail to provide clear-cut standards for classificatory judgements: lack of reliability, as we have seen, is connected with this route in Hempel's account. The second route is that they may fail to track underlying natural uniformities. This is a failure of validity, then, a failure to reflect valid insights into the way the world really is.

Two threats to objectivity

You will recall that in his paper Hempel draws only a weak link between his first requirement for a classification to be scientific (hence objective) and reliability. There *is* a link, as we saw in the last part of Session 1; and the link, as we will see in this session, is at least in part by way of *values*. But to see this, and hence to understand what Hempel is saying in this passage about the 'valuational' aspects of psychiatric classification, we need to fill out a little Hempel's own account of the link between objectivity and the reliability of classifications.

Values and loss of objectivity: route 1 (via reliability)

As we saw in Session 1, Hempel touches on the issue of reliability in his section on Empirical Import, called earlier in his paper

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'descriptive adequacy'. Unless, he says in one of the passages examined in Session 1 (reading linked with Exercise 6), there are clear rules for applying descriptions, for fitting the phenomena under investigation into descriptive categories, then the classification in question will not be *reliable*. Hempel discusses the two main meanings of reliability—test–retest and inter-rater reliability—noted in Box 13.2 above. If a classification is *not* reliable, then a single investigator might make different judgements about the same case on different occasions given the same information (test–retest *un*reliability); or two investigators may disagree in the application of a classificatory term in the face of the same, and indeed all relevant, information (inter-rater *un*reliability).

Disagreements of these kinds will have practical consequences for psychiatric treatment and research. They will undermine any possibility of consistency in treatment both over time and between clinicians, and, thus, across different geographical areas. As such, though, all this says nothing directly about objectivity.

A link between lack of reliability and lack of validity

But there is another feature of this kind of disagreement, which Hempel himself does not stress but which *does* have important consequences for the objectivity of a classification. The key point, to put it first in the negative, is that, *absent* objectivity, *none of the divergent judgements in the use of (an unreliable) classification need be mistaken*. This is because, if the standards that govern the correct use of a classification are themselves flexible or open-ended, then the fact that judgements differ with respect to classifying a given phenomenon by reference to those standards need not imply that the standards have been broken. Flexible standards can bend without being broken. But of course, to come now to the positive side of the point, it is precisely such flexibility in the use of a classification that undermines any claim it might have to objectivity. For without the idea that when two judgements differ as to how a given phenomenon should be classified then at least one of the judgements must be mistaken, the difference between a true (valid) and false (invalid) claim about what is 'really there' in the world is fatally undermined.

In other words, a breakdown of reliability not only causes difficulties because of the practical consequences—on the surface, as it were—of that breakdown of agreement, it also suggests a deeper underlying malaise. Flexible standards for judgements imply that those judgements do not track underlying objective features of the world. This is not to say that in *every* case where there can be disagreement without mistake then nothing objective is being judged. Disagreements about *how much* evidence is needed to support a diagnosis might be subject to vagueness at the boundary. Judgements about colours are vague across boundaries in an analogous way without undermining their objectivity (although there may be other reasons to doubt their objectivity, as we will see below). But if disagreement does *not* imply error, and not just because of vagueness of this boundary sort, then this undermines the claims to objectivity (and hence validity) of the judgements in question.

Is humour objective?

The link between reliability and objectivity can be illustrated by an example. It is at least arguable that the classification of things (events, stories, jokes) into those that are funny and those that are not, is not reliable and thus not objective in the sense just outlined. People's senses of humour vary widely. Thus, if two people who share all the relevant facts about how a joke is supposed to work, and who grasp the allusions being made, still disagree about whether it is funny, *neither need have made a mistake*. They may, and indeed ordinarily would be said to, simply differ in their sense of humour. Unlike the items marshalled into scientific classifications, few think that there are objective comic facts. So there seems to be a connection between, on the one hand, the idea that in some cases disagreement (unreliability) in judgements need not imply that a mistake has been made, and, on the other hand, the thought that the subject matter of such judgements is not objective.

Cognitive command and objectivity

The example of comedy comes from the British philosopher and logician, Crispin Wright's, *Truth and Objectivity* (1992). Wright suggests that one mark of the objectivity of at least certain kinds of judgement is that they possess 'cognitive command'. An interesting feature of Wright's book is that he suggests several different tests for different sorts of objectivity and provides no reason to think that these different tests will apply to the same sorts of judgements. In other words, he thinks that there might be kinds of judgement that possess one sort of objectivity (provided by cognitive command, for example) while other kinds of judgement possess different forms of objectivity altogether. In the following passage, he is concerned only with the 'test' of 'cognitive command'. He sets up the link between the possibility of genuine agreement in judgement and 'cognitive command' initially by way of the reciprocal link between mere differences of opinion and 'cognitive shortcomings'. This is how Wright puts it:

> It is a priori [i.e. roughly, true by definition—see Chapter 5] that differences of opinion formulated within the discourse [in question], unless excusable as a result of vagueness in a disputed statement, or in the standards of acceptability, or variation in personal evidence thresholds, so to speak, will involve something which may properly be regarded as a cognitive shortcoming. (p. 144)

Values and reliability

If this principle is a priori then the type of judgement in question possesses one kind of objectivity. To recapitulate, this principle is designed as a test for the objectivity of the subject matter of a class of judgements. Think of the reciprocal connection first. If agreement between judgements of a certain class is *commanded* by their subject matter, i.e. the relevant objective facts, if they are in this sense genuinely matters of *cognising*, then disagreements will have to imply that someone has made a *mistake*. Wright suggests that the converse also holds. If there is a genuine notion

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of mistake at play, then that notion implies a kind of objectivity. Conversely, if there is not (and not just because of boundary-vagueness), then this kind of objectivity (hence validity) is lacking.

'Cognitive' in psychiatry and in philosophy

To understand this passage we need to pause for a moment on that key word 'cognitive'. In psychiatry, as we saw in Chapter 3, 'cognitive' is used, as in the Mental State Examination, of the 'cognitive functions' of orientation, attention, memory, and general IQ, as distinct from such functions as affect, volition, perception, and belief. Wright, by contrast, uses 'cognitive' in this passage, as it is generally used in philosophy, to mean, roughly, that which is known, where knowledge in turn means 'justified true belief', of which the paradigm cases are always beliefs about the world. This is what lies behind his initial apparently rather question-begging conditional, 'If it is a priori . . .'. Most philosophers, Wright is able to assume, will take it for granted that if a judgement is a matter of genuine 'cognising', i.e. of *knowing* something about (= having justified true beliefs about) the world, then it is indeed true by definition that the truth or falsehood of that judgement is, in Wright's sense, *commanded*.

With this point of clarification in place, the force of Wright's example of humour is now clear. The judgement that a joke is or is not funny is not 'commanded' by any objective feature of its subject matter, i.e. of the joke in question. There is no 'cognitive shortcoming', then, involved in a difference of opinion; no one need have made a mistake about whether the joke in question is or is not funny. By contrast, judging the time the joke took to tell, for example, really is 'commanded' by an objective feature of the world (ordinarily understood), namely the actual passage of time that telling the joke really took. Here, correspondingly, it is possible to be mistaken about an objective feature of the world, to 'get it wrong'. And to be mistaken, or to get it wrong, is a matter, in Wright's term, of 'cognitive shortcoming'.

Granted, then, the above point of clarification about the meaning of 'cognitive', and Wright's example of humour, his claim that 'cognitive command' is an a priori test of one kind of objectivity, seems fair enough. It is a different matter just how the test could be applied in practice other than in a question begging way. Wright is not concerned with this. But in capturing clearly one important intuition about objectivity (that it is possible to be right or wrong about objective features of the world), Wright helps to unpack the idea behind Hempel's comments on values in his 1959 paper.

Back to Hempel on values

We are now ready to come back to what Hempel says in the reading linked with Exercise 11 about values and the reliability of psychiatric classification, and to fill in the link to objectivity.

Hempel, then, in the above passage, suggests that one source of unreliability in psychiatric classifications is what he calls their 'valuational aspect'. As examples he picks out such notions as: 'inadequacy of response, inadaptability, ineptness and poor judgement', which were all used to characterize the signs of Inadequate Personality in DSM-I. These seem not to be straightforwardly matters of fact but to involve value judgements by clinicians. And Crispin Wright's notion of 'cognitive command' fills in the implied link to objectivity. For if Hempel is right, the values involved in psychiatric diagnosis might lead to lack of reliability (i.e. because clinician's values may differ). By analogy with humour, then, such diagnostic judgements may lack 'cognitive command', which, if Wright on humour and Hempel on values are right, suggests that such judgements do not reflect objective features of the world, and hence that, in this sense, they lack validity.

As already noted, there is a further question about how a judgement would be 'tested' for cognitive command in practice. But leaving this aside, it is still important to ask, at least of values, whether Hempel *is* right? Is it necessarily the case that values lead to unreliability? The considerations of Part I, drawing particularly on Hare's work, suggest that while values *may* lead to discrepant diagnostic judgements, often they will not do so. Specifically, values will not lead to unreliability where they are *shared*. This of course leads back to the bottom line of the implications of Hare's work for the debate about mental illness. For it suggests, consistently with the 'full field', fact + value, model to which we came in Part I, that classifications of bodily disorders are, despite appearances, not value-free. They appear to be value-free only because and to the extent that they embody values that, in contrast to the values in classifications of mental disorders, are widely shared.

We return to the practical significance of the role that values may play in psychiatric classifications at some length in Part IV. But it is worth noting Hempel's attitude to them in this paper. He argues that the formulation of more reliable classifications that are not valuational is 'very desirable' but not an 'easy task'. It is thus not something to be insisted on from the start but rather something towards which we should aim as an eventual goal. This is a natural corollary of the traditional model. Our conclusion in Part IV, by contrast, will be that as values are there and essential, rather than seeking to eliminate them, we should make them explicit with a view to working with them in a more rigorous and effective way.

Values and loss of objectivity: route 2 (via validity)

Exactly what working with values in a more 'rigorous and effective way' means in this context will be a key topic for Part IV. But Hempel's two requirements allow a second link between values and loss of objectivity, via validity. This as we have seen is the basis of the familiar oil-and-water intuition about values and science. It is on the oil-and-water intuition that Szasz draws in the next reading in developing one of his arguments against the reality of mental illness.

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EXERCISE 12 (30 minutes)

Read the extract below from:

Szasz, T.S. (1972). The myth of mental illness. *The Myth of Mental Illness*. Paladin (Extract pp. 14–17.)

Link with Reading 13.11

We examined Szasz' arguments in detail in Part I. In this reading, think particularly about three questions:

1. What sort of argument does Szasz advance against the reality of mental illness in this passage as against or in addition to his arguments in Part I?
2. How, in particular, does Szasz employ the idea that values undermine objectivity to advance his argument?
3. What role does causality play in his argument?
4. Are his arguments sound?

Szasz's core argument

At the heart of this passage is Szasz' core argument against the reality of mental illness that we explored in detail in Part I, namely his claim that mental illnesses are defined by reference to value norms ('psycho-social, ethical and legal'), whereas (real) bodily diseases are defined by factual norms ('anatomical and physiological').

This core argument, as we saw in Part I, is highly effective in that it relies precisely on the 'medical' model that is common ground between Szasz and his opponents, such as R.E. Kendell. Both sides in the debate about mental illness, as we saw in Part I, have generally taken it as a given that bodily diseases (whether thought of as entities or processes) are, like atoms, weather systems, and stars, objective elements in the real world 'out there' and independently of anything we might think of them. Given, then, the overtly value-laden nature of mental illness compared with bodily illness, the burden of proof falls very squarely on those who, in Szasz' phrase in this piece, believe that 'mental illness is just as "real" and "objective" as bodily illness'.

Realism and Szasz's core argument

Here, though, Szasz tops and tails his core argument from Part I using the language of scientific realism. Thus, the passage opens by pointing out that in the modern world people struggle not so much with *biological* (implies objective) as with social (implies, at least as in Szasz' core argument, evaluative) challenges; he continues with a critique of the idea that mental illness could be a *cause* of a deformity in the personality (we look at the substance of Szasz' argument from causation in a moment); similarly, it is, he urges, absurd to use *medical* (again implies objective) means for a problem that is 'psycho-social and ethical'; this, he says, reflects the 'currently prevalent claim that 'mental illness is just as '*real*' and '*objective*' as bodily illness'; which claim, if true, would, Szasz argues, mean that we could 'catch', 'have or harbor it' [a mental illness], that one could 'transmit it to others' and 'get rid of it'.

Now, there is much that, as in Part I, could be said both for and against Szasz' position in this passage. He overstates his case, no doubt, in places: an attitude, for example, such as racism, although defined in part by values, can be, as Szasz says a disease can be, 'caught', 'had or harbored', 'transmitted to others' and 'got rid of'. So these are certainly not attributes that are confined to and hence provide a marker of 'real and objective' entities. In support of Szasz' position, on the other hand, writing as he is in the late 1960s and early 1970s, it is worth pointing out that he is truly prescient in his remarks about the overuse of medical treatments such as medications. Szasz' final comment, furthermore, about mental illnesses being 'communications' has a powerful resonance with the claims of both Lewis in ICD-8 and of Kupfer *et al*. (2002) in *A Research Agenda for DSM-V*, that the great success of modern more reliable psychiatric classifications is to improve communication (see Session 1 above)! But the key point to take from this passage is that, just as Szasz relies in his core argument against the reality of mental illness on the distinction between evaluative and descriptive (or scientific) norms, so, here, in reinforcing his core argument, he relies on a largely direct appeal to the widespread traditional intuition that values are immiscible with what science is about, i.e. to repeat Szasz' phrase, ' "real" and "objective" ' features of the world.

The force of Szasz' direct appeal to realism

Szasz' appeal to the traditional oil-and-water intuition about science and values is thus, with one partial exception (see below), direct: he does not (here) attempt actually to argue from one or other component of philosophical realism (objectivity, etc.) to the non-reality of mental illness. He just relies directly on the oil-and-water intuition that values (and hence the value-laden mental illness) equal non-science (and hence that the value-laden mental illness cannot be a species of the (as he takes it to be) scientific concept of disease.

Szasz' direct appeal to the oil-and-water intuition, moreover, is effective in much the way that his core argument is effective, i.e. by relying on the assumptions of his opponents. His core argument, as just noted, takes off from the traditional medical model. The burden of proof thus falls on those who (taking the traditional medical-scientific model for granted) believe that mental illness, despite being value-laden, is, really, as value-free as bodily illness (in the traditional model) is taken to be. Similarly here, then, the appeal to the traditional oil-and-water intuition puts the burden of proof on those who (taking the traditional oil-and-water intuition for granted) believe that mental illness, despite being value-laden, is, really, as much a matter for medical-scientific intervention as bodily illness. The message, in both cases, is put up or shut up!

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Szasz' argument from causality

There is of course a third way, here as in Part I, between Szasz and his opponents, which is to resist tradition, the traditional medical-scientific model in Part I, the traditional oil-and-water intuition here. Before turning to this third way, however, it will be worth looking at the one actual argument from scientific realism that Szasz advances in this passage. Although presented only briefly by Szasz, it is an important argument because it is concerned with an element of realism, causality, that figures large in medical science. We will be examining causality in detail in Chapter 15. In this passage, Szasz deploys the argument that causality often appears in the views of those who defend the reality of mental illness both as cause and as effect. Mental illnesses are defined by reference to disturbances of behaviour (they are used to identify features of, or more specifically deformities of, personality). But mental illnesses are also, Szasz points out, invoked *causally* to explain those same disturbances of behaviour. Since, then, Szasz concludes, nothing can cause itself, the concept of a mental illness is incoherent.

Again, this is a powerful argument. In this case, however, the strength of the argument depends on the premiss that mental illnesses really are merely short hands for behaviour. An alternative position would be to claim instead that, although mental illnesses are identified via behaviours, they are not identical *with* those behaviours but are instead their underlying causes. As we will see in Part V, the contemporary American philosopher Donald Davidson defends a conception of mental states of just this sort. As he points out, we often describe events by their effects. The act of firing a gun may also be described as an act of murdering a president. The same can apply to describing mental states. However, this does not require a behaviourist identification of mental states and behaviour.

Szasz' argument in this passage, whether or not he needs to place any weight on it, does provide one clear way of understanding, a *causal* way of understanding, what the denial of the reality of mental illness amounts to. Realism about mental illness of this causal kind involves the claim that mental states play an underlying causal role in the production of behaviour. This conception of what the reality of mental illness would amount to is thus related via causality to Hempel's discussion of the Systematic Import of a natural classification. Szasz in this passage can be interpreted as adding to his core argument that mental illness is (because value-laden) a myth, the claim that a classification of mental disorders cannot be a natural classification because it does not describe any kind of underlying mental causal mechanisms, and hence that, in Hempel's terms, it lacks (at least this causal kind of) Systematic Import.

A third way?

Returning, then, to Szasz' core argument, and to the reinforcement of his core argument with the appeal to realism, what is our third way here? In Part I, our third way was to resist the assumptions of the traditional medical-scientific model by drawing on the work of Hare and others in the Oxford 'school' of linguistic analytic philosophy. Szasz (1972) took the more value-laden connotations of mental illness to show that mental illness is a myth; his opponents, such as Kendell, took these same value-connotations to be merely provisional on the development of a more advanced psychiatric science. Our third way was to show that if the medical concepts—illness, disease, dysfunction, etc.—are indeed value terms, then, simply in sharing the logical properties of value terms in general (as described by Hare, Warnock, Urmson, Foot, and others), the uses of these concepts in psychiatry would necessarily be more value-laden than in bodily medicine, not because psychiatry is less scientific than bodily medicine, nor because it is concerned merely with 'problems of living', but because human values are more diverse in the areas with which psychiatry is concerned (emotion, belief, sexuality, etc.) than in the corresponding areas with which, in acute hospital-based medicine at least, bodily medicine is concerned (severe bodily pain, imminent threat of death, etc.).

Opening up a space

Here, in responding to Szasz' reinforcement of his core argument with the language of realism, our third way is to resist the assumptions of the traditional oil-and-water immiscibility of science and values by drawing on more recent work in the philosophy of science and epistemology. We will not be able to do more than take a few preliminary steps along this particular third way. The topic, after all, realism, is considerably wider and more multifacetted than the relatively well-defined topic, the is-ought debate, on which we drew in Part I. Even more so than in Part I, therefore, our objectives in opening up a third way are modest. Our objectives here as in Part I, are not to 'prove' that the third way is right. The is-ought debate, after all, continues; and debates about realism, although now often badged as epistemology rather than philosophy of science, having continued for over 2000 years, show no sign of running out of steam.

Our objective, therefore, here as in Part I, is the more modest objective of opening up a space between traditional polarities, a space for new intellectual enquiries, a space for new practical initiatives. The space opened up in Part I, by drawing on the resources of Oxford linguistic-analytic philosophy in the middle decades of the twentieth century, is already becoming occupied by new models of service delivery and of clinical skills training, models to which we will come in Part IV. The space to be opened up here, although considerably more complex and less well explored, has a corresponding potential for occupation by new (or at any rate newly expanded) ways of understanding psychiatric science, new ways of understanding psychiatric science that, we will suggest in Session 4 of this chapter, are overdue for at least partial incorporation into psychiatry's classifications of mental disorder.

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From Szasz to Williams and McDowell

So, what are the arguments from the philosophy of science (and epistemology) that will open up a third way between Szaszian antirealism and psychiatry's realism about mental disorder?

In the next part of this session we will examine one particularly carefully framed philosophical argument that some have taken to provide grounds for believing that science and values are like oil and water, Bernard Williams' argument for an 'Absolute Conception' of the world. We say 'some have taken' because, as we will see, Williams' argument is aimed primarily at excluding from science the secondary as distinct from primary qualities (a primary quality is, e.g. the wavelength of light, a secondary quality is, e.g. the colour we see). However, the Australian philosopher (also at Oxford) J.L. Mackie (1977, p. 15), for example, is among recent philosophers who have taken Williams' argument about secondary qualities to provide an argument for the exclusion of values from science.

By way of context, it is important to be aware that other philosophers have taken very different lines on the relationship between values and science: the English philosopher, Jonathan Dancy, for example, has argued that there are grounds, independent of and in some respects in opposition to any arguments by extension from secondary qualities, for believing that there are 'moral facts' (Dancy, 1993). Here, though, we will confine ourselves to, (1) Williams' argument for an Absolute Conception as a potential 'knock down' philosophical argument to show that values and science are immiscible, and (2) a counter-argument from the Pittsburgh-based philosopher, John McDowell, a counter-argument that, in being applicable equally to secondary qualities and to values, will serve our modest objective of opening up a third way in the form of a space between Szasz and psychiatry, a space for potential occupation by an enriched model of psychiatric science.

Bernard Williams and the Absolute Conception versus John McDowell

Williams' argument for an Absolute Conception comes in his book on Descartes. He outlines what he means by the 'Absolute Conception' in the next reading (linked with Exercise 13). Williams, as we will see, presupposes a knowledge of the long-standing philosophical distinction between primary and secondary qualities. Familiar in classical philosophy, this distinction was reintroduced into modern philosophy in the seventeenth century by the British Empiricist philosopher, John Locke (1632–1704), as the distinction between things such as shape, weight, mass, or velocity, which seem to belong to things independently of observers, and other things such as taste, smell and colour, which depend on our particular psychological features and other properties. The distinction was made more popular in the seventeenth century by the rise of an atomistic natural philosophy promoted, among others, by the chemist Robert Boyle (1627–91).

Fig. 13.2 Bernard Williams

EXERCISE 13 (30 minutes)

Read the two extracts from:

Williams, B. (1978). *Descartes: the project of pure inquiry*. London: Penguin (Extracts pp. 64–65 and 244–246.)

Link with reading 13.12

Think carefully about Williams' argument for an Absolute Conception and what that conception is of.

- What are the implications of Williams' account of secondary qualities for the role of values in psychiatry?

Again, it is important to work through the reading critically before going on to what *we* have to say in answer to this question.

The objectivity of knowledge

Williams develops his account of the Absolute Conception in this reading in order to explain more fully what he takes to be an everyday understanding of what knowledge (as justified true belief) consist in. This involves the idea that justified (as district from accidentally) true beliefs answer to a world which is independent of our beliefs. 'Knowledge is of what is there *anyway*'. It is this basic assumption about the independence of the subject matter of justified true beliefs or knowledge that seems to be threatened by the thought that mental illnesses are partially constituted by societal and other values. In such a case it seems that

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 348

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the object of one's knowledge is precisely *not* objective or independent, is not there 'anyway'. The Absolute Conception is supposed by Williams to constitute a filling out of what it is that our beliefs answer to when we say that they answer to the world. It is part of an explanation of what the objectivity of justified true beliefs, or knowledge, amounts to.

The argument for the Absolute Conception

Williams' way of substantiating the Absolute Conception develops from the following thought experiment. Two people, A and B as Williams calls them, both have different but equally true beliefs about the world. Perhaps their different beliefs stem from the fact that they are looking at the same thing but from different perspectives. If so, then it must be possible to give a larger account of the set up, an account that includes how A and B's beliefs, as different representations of the same thing, are related both to each other and to the thing of which their respective beliefs are representations. But this larger account is itself a *representation* of the world that includes A and B and explains how their beliefs are related to each other and to the world. Hence, in giving the larger account, we form a conception of a *representation* of the part of the world in question (construed broadly to include both A and B and A and B's representations), which is distinct from our conception of that *part of the world* itself. Thus the larger story maintains the contrast between, on the one hand, a representation *as* a representation of the world, and, on the other, the world of which the representation is (merely) a representation.

But now it can be seen that postulating two people, A and B, with different beliefs is unnecessary for the thought experiment. The new representation can itself be the subject of a broader account of how it itself relates to, but is distinct from, the world it represents and to other true representations of the world that could be given. Clearly this process can be iterated until, finally, it includes the relation between one representation (the Absolute Conception) and all other possible representations. The conclusion that Williams draws, is that only if one can form this *Absolute Conception* of the world, which includes how all possible true representations are related to it, can one have a proper conception of any individual item of knowledge since that must include the contrast between representation and the world. Reflection on the latter escalates into the former.

The dilemma facing the Absolute Conception

Williams' conclusion can be clarified by examining a dilemma that the Absolute Conception raises. This concerns how the content of the 'Absolute Conception' can itself be filled out. Without some additional substantiation, there is a risk that the phrase remains empty, meaning vacuously merely 'what true representations represent'. But if so, then according to Williams, it will be impossible to understand the contrast between a representation and the independent reality from which it stands distinct. On the other hand, any specific account of reality that we may be tempted to offer runs the risk of being merely another of our *representations* reflecting our local perspective and thus not a characterization of an independent reality after all.

Williams' own suggestion for evading both horns of this dilemma is to invoke the progress of science. This allows the non-local characterization of the Absolute Conception as that which is progressively revealed by science. This will include the findings of physics, chemistry, and biology—or whatever sciences succeed them—concerning the fabric of the world in a narrow sense, but we can also add in neurology, psychology, and psychiatry (cf. p. 245). These latter sciences will play a role in explaining both how it is possible to acquire the knowledge of the world we have obtained and also how it was possible to form the preceding *false* beliefs which have littered the history of science, as these also, in a broader sense, are a part of the world or world history. We will return to an assessment of this suggestion shortly. What is important for now are the consequences it suggests both for secondary qualities and for values.

Primary and secondary qualities

One way of interpreting the history of science is to conclude that some of the features that humans have previously taken to form part of the world—in the narrower sense—turn out to have been instead local features of our perspective on the world. Thus while it was once assumed that the world really was coloured, we now 'realize' that colour is an artefact of the way in which humans and some other animals perceive the world. We can thus now entertain the thought that a combination of, perhaps, physics, neurophysiology, and psychology will, in the future, describe not just how light and bodies interact but also how we see in colour, how we once projected this experience on to the world, and how we later realized that this was wrong and came to develop the account of the world and ourselves that we are now contemplating.

On Williams' picture, the difference between primary and secondary qualities is this. Primary qualities are those that will be required by a future physics to describe the behaviour of the fundamental fabric of the world. Secondary qualities are those that are features merely of our local perspective on the world, of how the world appears to us with our particular sense organs. The progress of science is in part characterized by an increasing ability to describe the world in a way that prescinds away from our specific sensory experiences and describes it instead as though from no particular perspective. (Secondary qualities will still feature in scientific accounts but only as items to be explained in the history of science. They will be part of the broader world history that includes our history but not part of a narrower natural history of the world as it really is.) Thus any account couched instead in secondary quality terms is not fully objective because secondary qualities are not proper parts of the world.

Values and the objectivity of diagnosis

The philosophical model presented here of what is involved in the very idea of knowledge provides one connection between the presence of values in psychiatric classification and its supposed failures of objectivity and realism. The idea, for example as

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developed by Mackie (1977) is that, by analogy with secondary qualities, values reflect features of our perception of the world and thus cannot properly be regarded as part of the world (in the narrow sense) that is independent of us. As science aspires towards an account of the world that prescinds away from local features of our perspective and uncovers what would also be discovered by *any* scientific inquiry, whatever the details of the individuals or species carrying it out, any classification that relies on mere values, like any classification that relies on mere secondary qualities, cannot be properly objective.

EXERCISE 14 (15 minutes)

Before going on, think about the account that Williams (1978) provides. Is the Absolute Conception itself a coherent idea? Does it really provide a principled reason for denying the objectivity of secondary qualities?

Critically reflect on how Williams suggests that we can form a conception of the world in this way. Think in particular about the way he deploys the distinction between primary and secondary qualities. What is the significance of the fact that, as we flagged in the run up to Exercise 13 above, Williams' takes knowledge of this distinction for granted.

One criticism of Williams' account of the Absolute Conception is, precisely, that he merely presupposes his key distinction between primary and secondary qualities rather than providing an independent argument for it. What the Absolute Conception does is rather to give a further *interpretation* of that distinction on the assumption that it exists. Thus one way of attacking the conclusion that secondary qualities cannot form part of an objective account of the world would be simply to deny that there is any such distinction as that which is expressed by the traditional distinction between primary and secondary qualities. For our purposes, of understanding the role of values in psychiatric classifications, corresponding analogical moves include:

1. to deny that psychiatric classifications are (at least) essentially value-laden: this as we have seen is the nub of what those defending an essentially value-free model, like Kendell and Boorse, seek to do; or
2. to claim that despite being value-laden, those values were somehow independent of us and objective features of the world: although not deployed directly in the psychiatry/antipsychiatry debate, there is a large literature in ethical theory claiming, in one way or another, that there can be what amount to moral 'facts' (see, e.g. Dancy, 1993; McDowell, 1998).

But there is a different approach altogether to denying the Williams-inspired conclusion, an approach that we owe to John McDowell. It is to this that we turn next.

John McDowell's account

John McDowell is the contemporary South African born, Oxford educated, and now Pittsburgh-based philosopher of mind, language, and metaphysics, that we introduced in Chapter 12. McDowell, as we noted, has written mainly short papers on a number of different but inter-related themes. What might be called his metaphysical world view is set out in a book of lectures, *Mind and World* (1994), the first chapter of which we examined in Chapter 12.

McDowell's arguments against Williams' Absolute Conception come in his paper 'Aesthetic value, objectivity, and the fabric of the world'. This paper, from which the next reading is taken (linked with Exercise 15 below), was written in part as a response to J.L. Mackie's book *Ethics: inventing right and wrong* (1977), and comes also in a different collection of his essays, called *Mind, Value, and Reality* (1998). The general thread of McDowell's arguments in this book concern whether values can be found in the world or have, as Mackie supposes, to be projected on to it. In this context, McDowell gives, as we will see, two arguments against Williams' Absolute Conception. Before coming to the specifics of these arguments, we will briefly fill out the background to them as set out in McDowell's paper.

Secondary qualities as *sensory* qualities

McDowell's starting assumption is that there is indeed a fundamental distinction between primary and secondary qualities. To this extent he agrees with Williams. McDowell argues that secondary qualities, unlike primary qualities, have to be characterized by appeal to our experiences.

Note that a different approach might have been to deny that supposed secondary qualities such as colour really do differ in kind from primary qualities. P.M.S. Hacker, an Oxford philosopher who has written extensively on Wittgenstein, argues in his book *Appearance and Reality* (1987) that there is no such distinction. Colours such as red are defined by appeal to red *samples* and not by appeal to our inner *experiences* on looking at red objects. They are thus similar to supposed primary qualities such as length, which are also defined by appeal to public samples.

But McDowell agrees with Williams that there is an important difference. To be red, for example, is to be experienced as red. He defines secondary qualities in general in this way:

> A secondary quality is a property the ascription of which to an object is not adequately understood except as true, if it is true, in virtue of the object's disposition to present a certain sort of perceptual appearance: specifically, an appearance characterisable by using a word for the property itself to say how the object perceptually appears McDowell (1998, p. 133)

McDowell, following the Oxford philosopher of thought Gareth Evans, calls secondary qualities so understood '*sensory*' qualities (Evans, 1980). It is this connection to a subject's experience that will play an important part in his criticism of the Absolute Conception (see below).

One further terminological point is important, however. McDowell also calls secondary or sensory qualities 'subjective'. He defines a subjective property thus: 'A subjective property, in the relevant sense, is one such that no adequate conception of what it is for a thing to possess it is available except in terms of how the

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thing would, in suitable circumstances, affect a subject—a sentient being'. (McDowell, 1998, p. 113).

So secondary qualities are sensory and subjective. But while this may be an intuitive assimilation, McDowell's use of the term 'subjective' does not mean that a quality so described cannot be 'out there' as part of the fabric of the world. McDowell's diagnostic distinction between objective and subjective does *not* simply amount to what is found in the world and what is projected onto it. His idea is that a quality may be both subjective in the sense described above—i.e. conceptually connected to our experiences—but also the sort of thing that makes up reality.

In part his challenge is simply to point out this conceptual possibility. In other words, in a classic philosophical move, he points out that while we may assume that if a quality depends conceptually on our experiences it cannot really be out there, be a valid quality, actually this assumption may not be well founded and needs an argument. Secondarily he undermines one argument for this, which depends on the Absolute Conception and to which we will now turn.

Recall the dilemma that the Absolute Conception faces. On the one hand, to serve as a genuine contrast to representation in characterizing the objectivity of representation, the Absolute Conception must mean more than merely: 'whatever our representations represent'. But on the other hand the Absolute Conception cannot be substantiated by merely invoking one of our local representations. That will collapse the necessary contrast between our local representations and what our local representations are of.

Williams invokes science to escape this dilemma. We have a conception of the conception science will eventually give us in terms of non-local absolute concepts. (It is a kind of meta-conception.) Part of the moral of this is that secondary qualities drop out from the account of features of the world (in the narrow sense) and appear merely as aspects of our local perspective on the world to be explained (as they are part of the world history in the broader sense). Secondary qualities cannot form part of the fabric of the world and thus are not properly objective or real. Our primary interest here is that the same argument could be applied to psychiatric classifications if these depend on human evaluations.

We will now turn to McDowell's main argument against this. McDowell advances two lines of criticism against Williams's Absolute Conception. We will look at his account of the first (in the next reading) and summarize the second below.

EXERCISE 15 (20 minutes)

Read the extract from:

McDowell, J. (1998). Aesthetic value, objectivity, and the fabric of the world pp. 112–130, in *Mind, Value, and Reality*. Cambridge, MA: Harvard University Press (Extract: pp. 122–123.)

Link with Reading 13.13

- What is McDowell's argument against the possibility of fitting secondary or subjective qualities into the Absolute Conception?
- Think how this argument might be extended to cover values as well as secondary qualities.

McDowell's first argument against Williams

The reading covers McDowell's first main argument against Williams' Absolute Conception. On Williams' picture we aspire towards moving from our local perspective to what the American philosopher, Thomas Nagel, has called, in a book of the same name, the 'view from nowhere' (Nagel, 1986). But while the Absolute Conception is supposed to *contrast* with our local perspective, it also has to *include* it as even our local representations need explanation. But how can secondary, or as in the Evans/McDowell reformulation *sensory*, qualities be explained except by *adopting* the relevant perspective? Knowing what it is that is to be explained, requires adopting the perspective from which ex hypothesis they make sense. Thus there is a tension in the very idea of the 'view from nowhere' implicit in the Absolute Conception.

McDowell's argument turns on the assumption that secondary qualities are properly understood as sensory qualities and are thus subjective in the sense defined above. If so, then it seems that the only way of characterizing secondary qualities will be by adopting a subjective, i.e. *local*, perspective. It would be a separate matter to describe, e.g. the physics of light that might underpin colour. But to understand the secondary quality of colour itself, to understand the subjective sensory quality of colour, McDowell suggests, requires that we understand what it is like to *have* colour experiences. And that is part of a local perspective, which, in direct contrast with the central requirement of Williams' Absolute Conception, is precisely *not* available to all possible subjects.

In the case of values, there is nothing akin to the physics of light. And thus it appears that there will be even less that an Absolute Conception can describe given that it is not allowed to describe our responses to the world. Surely, then, runs the implication of McDowell's argument against Williams' Absolute Conception (as developed in respect of values by Mackie), there is no prospect of understanding values without understanding how things matter to us as subjects.

McDowell's second argument against Williams

McDowell's second argument against Williams' Absolute Conception, a little later in his paper, disputes the idea that invoking the progress of science can be of any help with the dilemma of which Williams is aware in developing his Absolute Conception. The dilemma, you will recall, ran thus: on the one hand, to serve as a genuine contrast to representation in characterizing the objectivity of representation, the Absolute

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Conception must mean more than merely 'whatever our representations represent'; however, on the other hand, the Absolute Conception cannot be substantiated by merely invoking one of our *local* representations. Williams invokes the progress of science to escape from the horns of this dilemma: the progress of science, Williams points out, consists of a progressive substitution of local perspectives (secondary qualities) in favour of evermore non-local perspectives (primary qualities); however, our ability to have a conception of progress of this kind, Williams argues, depends on our already having a conception of an absolutely non-local perspective (Nagel's view from nowhere, Williams' Absolute Conception) with which all local perspectives are contrasted.

The problem that McDowell points out with using the progress of science in this way, is that it depends on giving an account of science—so as to form a conception of the Absolute Conception—which falls prey to essentially the same sort of dilemma that arises in trying to characterize the world (of which our representations are representations) directly. In other words, invoking the progress of science, while it may get us off the horns of Williams' original dilemma, leaves us just as firmly stuck on the horns of a new dilemma that is no less fatal to the claims of the Absolute Conception to preclude secondary qualities from science. The two dilemmas indeed, Williams' original dilemma and the new dilemma pointed out by McDowell, have essentially the same form. The Williams' original dilemma, the dilemma that Williams invoked the progress of science to resolve, was the dilemma that, on the one hand, if we describe the world in a concrete way, if we simply say that the world which our representations are of is thus and so, then we merely use other of our *local* representations and thus fail to describe the *Absolute* Conception; however, if, on the other hand, we say that the world is just whatever our representations are of, we fall into vacuity. Agreed, says McDowell. But invoking the progress of science, far from getting us off the horns of this dilemma, impales us even more firmly on the horns of a new dilemma, a dilemma in essentially the same form, which arises when we attempt a characterization of science itself that is sufficient for Williams' purposes. McDowell's new dilemma is that, on the one hand, if we describe science in a concrete way, spelling out specific methods for example, we are simply using features of our *local* perspective on science and thus fail to describe anything that could be part of Williams' Absolute Conception; but on the other hand, if we say 'science is whatever gives us (non-local) access to the world', we again fall into vacuity.

Conclusions to the session: no knock-down argument

The line of argument of this session has had a restricted purpose: to show that, to date, no one has come up with a knock-down argument to support the assumption of the traditional model that values and science are, like oil and water, immiscible.

Williams' and McDowell's arguments

Williams' Absolute Conception is a sophisticated attempt to provide such an argument. Williams, as we have seen, focuses on the traditional distinction between primary and secondary qualities; and his conclusion, that secondary qualities have no place in science, has been taken by others (such as Mackie) to show, equally, that values have no place in science. McDowell's counter-arguments to Williams, building on Evans' interpretation of secondary qualities as sensory (hence essentially subjective) qualities, suggest that the key step in Williams' argument, of invoking the progress of science to get us off the horns of a dilemma on which we are impaled by the Absolute Conception, leaves us impaled on a new dilemma with essentially the same pair of horns. Williams' Absolute Conception, then, sophisticated as it is, fails to provide a knock-down argument against subjectivity, as a hallmark of secondary qualities, and hence values, having a place in science.

Other arguments

There are other lines of argument that are relevant here: as noted above, we have said nothing about 'natural kinds', for example, as a possible route to *excluding* values from what science is about; nor, conversely, have we considered 'moral facts', as explored by Dancy and others, as a possible route to *including* values in what science is about. Again, we have said nothing, here, about values and science (evaluation and description) having, perhaps, complementary roles. This is the line of argument that we will be developing in Part IV. The claim, made particularly in Chapters 20 and 21 of Part IV, that values have a role in diagnosis, although consistent with the bottom line of this chapter, that no one has yet come up with a knock-down argument against values having a proper place is science, is not dependent on that bottom line. The arguments of Part IV depend, rather, on the Hare–Putnam-based account of the medical concepts developed in Part I, as concepts that combine distinguishable (though not necessarily separable) elements of evaluative and of descriptive meaning, with the evaluative elements of meaning becoming practically (as well as theoretically) important where, as in psychiatric diagnosis, the operative values are diverse rather than shared.

Fact and value in Part IV

That model, then, the fact + value, the full field model of Part I, being the model that will be applied to psychiatric diagnosis in Part IV, leaves it entirely open whether the classifications underpinning psychiatric diagnosis should fully include, fully exclude, or partially include and partially exclude, values. Current practice includes examples of both versions of the latter: the ICD, for example, partially excludes values by separating off issues of good and bad functioning in a classification, the ICF (or International Classification of Functioning, WHO, 2001), which, although described as being part of the same family of classifications as the ICD, is published separately; the DSM, by contrast, more fully includes values by including criteria of good and bad functioning within the body of the classification itself (e.g. its Criterion B for schizophrenia, see Part IV).

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Facts in physics

Again, we return to the significance of such overtly evaluative criteria appearing at the heart of psychiatry's most self-consciously scientific classification (the significance is entirely positive) in Part IV. For now, though, for purposes of focusing in this part on what science is about, we will turn from post-Logical Empiricist attempts to exclude subjectivity and values from science, as a way of trying to hang on to a traditional model, to post-Logical Empiricist philosophy that has been engaged with taking a fresh look at science itself, in the form of the paradigm natural science, physics. It is with post-Logical Empiricist philosophy of physics, then, that we will be concerned in the next session, before returning, in Session 4, to developing our third new agenda, our agenda for an *Extended Family of Psychiatric Classifications*.

Reflection on the session and self-test questions

Write down your own reflections on the materials in this session drawing out any points that are particularly significant for you. Then write brief notes about the following:

1. What did Hempel say about values in science?
2. How does this compare with what Szasz says about psychiatry?
3. Why did Crispin Wright argue that humour is not objective?
4. What is Bernard Williams 'Absolute Conception'?
5. How did Williams' employ the distinction between primary and secondary qualities?
6. What implications did J.L. Mackie (and others working in ethics) draw from Williams' account of the Absolute Conception for the place of values in science?
7. Give one of John McDowell's arguments against Bernard William's Absolute Conception (we noted a preliminary and two main arguments).
8. What is the implication of John McDowell's work for the place of values in science?

Session 3 Scientific realism in physics

This session will examine the debate about realism in the philosophy of science that has largely focussed on physics. It will consider the arguments for and against taking a realist attitude (of one kind or another) to the entities—such as atoms and electromagnetic fields—that are the basis of theory in the physical sciences.

Why physics?

The session will function at one level as 'time out' from the problems of classification in psychiatric science. It will put much needed air and space around those problems by exploring the extent of the comparable problems in physics. One consequence of this will be to provide a basis for shifting attitudes from the deficit model of psychiatric science, evident as we saw in the agendas for both Lewis for ICD-9 and Kupfer *et al.*, for DSM-V, to a *strengths* model. A further consequence will be to derive a number of specific lessons for the future development of classifications in psychiatry and the wider mental health movement. We will not be drawing these consequences in this session, however (we return to them in Session 4). In this session, we will be exploring the problems of realism in physics in their own right as a way of deepening our understanding of what science itself, in general and in physics as the hardest of hard sciences, is about.

Plan of the session

The session starts with the background to current debates in Logical Empiricism and a philosophical successor to Logical Empiricism, Constructive Empiricism. Something of the complexity of modern debates is reflected in the fact that Constructive Empiricism, in the work particularly of the Dutch-born philosopher (who works at Princeton University) Bas (B.C.) Van Fraassen, combines, as we will see, semantic and metaphysical forms of realism with a version of epistemological antirealism. Constructive Empiricism then leads into a discussion of three different realist responses to the epistemological antirealism of Constructive Empiricism: realism about explanations (Boyd and McMullen); realism about causal explanation (Cartwright); and realism about what can be manipulated (Hacking).

The session concludes with the work of the American philosopher of physics, Arthur Fine, on what he calls the Natural Ontological Attitude or NOA. Fine, as we will see, rejects both realist and antirealist positions in the traditional debate about what science is about. He argues that the traditional debate is premissed on an impossible-to-achieve perspective from *outside science*, that philosophers should instead confine their attention to what *particular* scientific disciplines are about, and that they should work by engaging directly in the *practices* of particular scientific disciplines. (Fine himself has contributed to the detailed interpretation of modern physical theory.)

Logical Empiricism and Constructive Empiricism

Logical Empiricism, positivism, and instrumentalism

First then, Logical Empiricism and one of its philosophical successors, Van Fraassen's Constructive Empiricism.

In the early part of this century, as we saw in Chapter 12, many philosophers of science advocated an instrumentalist and positivistic attitude towards science. They argued that theoretical claims about the world did not function as literally descriptive claims. In part, this emphasis was the result of the precise way in which observation was credited with a central role in science as

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described in Chapter 12. As we saw, the Logical Empiricists attempted to defend the central role of impartial observation by distinguishing between an independent language of observational concepts and a dependent language of theoretical concepts. Given this distinction, it was plausible (although by no means mandatory) to construe statements framed with observational concepts as making genuine empirical claims while construing theoretical statements as merely playing a heuristic role in organizing the former. When so construed, theoretical statements are not themselves fact stating or capable of truth and falsity. They play a part that is not one of assertion but rather as an instrument for organizing genuinely fact stating claims.

Such a view reflects a positivistic attitude to the connection between meaningfulness and verification. As theoretical statements cannot themselves be directly verified and receive whatever confirmation or refutation indirectly via observational statements, they were regarded by many as possessing a different kind of meaning from verifiable observational statements.

While positivist and instrumentalist attitudes are still found among philosophically influenced practising scientists, they are nowadays rare among philosophers of science. One reason for this is the attack on the distinction between theory and observation, explained in chapter 12, that undermines the possibility of marking a firm semantic distinction between the meaning of observation statements and that of theoretical statements. If theoretical and observational statements are made in the same language—if complete separation is not possible between theoretical and observational concepts—then theoretical statements cannot be sharply marked out as playing a different kind of role from observation statements. The difference between theory and observation is a matter of degree.

Scientific realism—the current orthodoxy

In place of a positivistic and instrumentalist view of scientific theory, the current orthodoxy is a form of realism about science called scientific realism. As we will see, 'realism' describes more than one philosophical position. The varieties of realism and antirealism, and their different combinations in the views of different authors can be somewhat bewildering—hence we have summarized the key positions that we will discuss in this session in Table 13.3. We will approach the issues via one particular attack on realism that shares some of the features of a positivistic view but which also shares some of the underlying metaphysical assumptions of scientific realism.

Table 13.3 Realism and antirealism in the philosophy of physics

Philosopher	Position	Strapline
Van Fraassen	(1) Ontological realist (2) Semantic realist (3) Epistemological antirealist	There really are things (like electrons) we can't see and in the same sense that there really are things (like chains) we can see
Boyd/McMullen	Ontological, semantic and epistemological realists	inference to the best explanation
Cartwright	Realist about causes (hence also causally effective 'unobservable' entities)	Inference to most probable cause
Hacking	Realist about what can be manipulated	What can be manipulated is real
Fine	Realism and antirealism equally flawed because they attempt to stand outside particular sciences—hence what is valid is a matter for local determination within the theory and practices of a particular science	Natural Ontological Attitude (NOA)

Constructive Empiricism—a current form of scientific antirealism

The following short extract is taken from Bas Van Fraassen's influential book published in 1980, *The Scientific Image*. Van Fraassen is a contemporary philosopher of science who has also written on the nature of probability and on the philosophy of quantum physics. In this reading, however, he focuses more generally on what the aim of science should be.

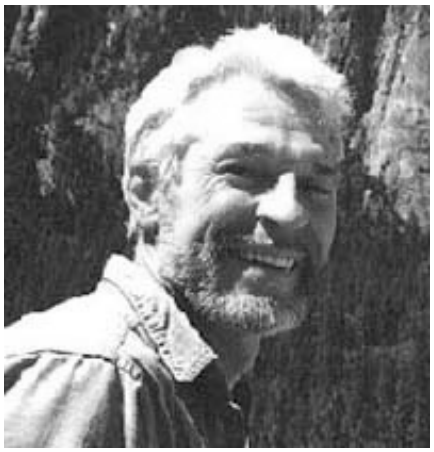

Fig. 13.3 Van Fraassen

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EXERCISE 16 (20 minutes)

Read the short extract from:

Van Fraassen, B.C. (1999). To save the phenomena. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 187–194. (Extract, p 380).

Link with Reading 13.14

In this brief passage we only get a hint of Van Fraassen's argument. But from what is described, how does Van Fraassen's position differ from a positivistic or instrumentalist view of science? Think also how this relates to Szasz's attack on the reality of mental illness.

Van Fraassen's relation to realism

Van Fraassen's position is a form of antirealism. But it is important to be clear just what it is that he denies. As he says on p. 187, one element of a *realist* attitude to science is that science aims to give us a 'literally true story of what the world is like'. But this claim itself involves two aspects according to whether one focuses on the claim to (scientific) truth about the world or the language in which that claim is expressed:

> This [ie the two aspects of the realist claim] divides the antirealists into two sorts. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. The antirealism I shall advocate belongs to the second sort. (p. 187)

Semantic and metaphysical forms of realism

Van Fraassen's position is thus *not* the same as a positivistic or instrumentalist account. Those accounts deny that theoretical statements are capable of truth or falsity but serve a different, purely organizational, role. In other words they can be 'true' in so far as they organize successfully, but this is not the same as literal truth (i.e. giving us a literally true picture of what the world is really like). By contrast, Van Fraassen accepts the realist view that theoretical scientific statements are either true or false when construed literally. Just as they appear to do, theoretical statements really *do* make claims about the world. This marks out a *semantic* realist position. Furthermore, Van Fraassen combines this with a further metaphysical view about the nature of truth. Theoretical statements are true (or false) in virtue of how the world *is* or what the facts *are*. Truth is a matter of correspondence with a mind-independent objective world. This is a form of '*ontological*' realism, therefore, realism about what there is 'out there'.

Constructive Empiricism is *epistemological* antirealism

But Van Fraassen combines his semantic and ontological realist views with a form of *epistemological* antirealism. Although theoretical statements are construed as (really) either true or false in virtue of the world, we can never arrive at sufficient evidence to discover or to know which statements are true and which are false. Thus, we should refrain from committing ourselves to the truth of theoretical statements. To do so is to take an unnecessary risk. Instead science should aim merely to arrive at theories that are *empirically adequate*. Acceptance of a theory should similarly involve merely belief that it is empirically adequate. A theory is empirically adequate if and only if 'what it says about the observable things and events in this world, is true—exactly if it *saves the phenomena* (emphasis added). And 'saving the phenomena' means accounting comprehensively for our observations. We should aim at nothing beyond this.

In his writings, Van Fraassen gives considerable theoretical underpinning to the two key terms in this account of what science is about, 'empirical adequacy' and 'saving the phenomena'. The latter phrase, by the way, is not original to Van Fraassen (although Van Fraassen gives it a particular meaning within his theory): it appears in the title of a book published originally in 1908 by the great French philosopher, physicist and mathematician, Pierre Duhem (whom we met in Chapter 12, see Duhem, 1985), and dates back via Ptolemy to Plato. At all events, you can get something of the force of his position from the traditional contrast between *phenomena*, as that which appears to us (through sight, hearing, etc.), and *numena*, as that which is really there and the source or origin of phenomena. In terms of this contrast, Van Fraassen's Constructive Empiricism says, in effect, that science is about the phenomena not the numena. You will see the parallels here with positivism and instrumentalism. But what Van Fraassen adds to these positions is an original and particularly clear analysis of how one can be at the same time an *ontological realist* (about what is there) and *semantic realist* (using 'real' with the same meaning of observable objects like chairs as of unobservable objects like electrons) and yet an *epistemological antirealist* (denying that there is anything one can know about what is really there beyond what appears to be there, i.e. the phenomena).

Empirical adequacy is enough?

So characterized, then, Constructive Empiricism is prescriptive. It says that one should not accept the truth of theoretical statements in science to the extent that this involves accepting anything over and above the empirical adequacy of the theory within which the statements in question are made. But this prescription is not arbitrary and results from epistemological arguments about what can and cannot be known. In *The Scientific Image* chapter 1, Van Fraassen's argument for Constructive Empiricism takes the form of a series of attacks on epistemological realism. In effect he argues that as believing in the (ontological) truth of theories involves more than believing merely in their empirical adequacy, there should be an argument for this extra step and that no such argument is successful.

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An argument for Constructive Empiricism

On a first encounter, however, this may seem an unusual way of apportioning the burden of proof. As Constructive Empiricism is counter-intuitive, a positive argument, rather than or in addition to Van Fraassen's negative arguments against realism, might be needed to motivate it. One such argument might run as follows:

1. At any given time, the only evidence for a theory is the set of relevant observations already made.
2. For any finite body of observations, there is always more than one theory that is consistent with it. These different theories differ in what they say about unobservables. (They might also differ in what they say about future observables, but we will ignore this point for the moment.)
3. As the only evidence for theories concerns the observable realm, there is no way of deciding which theory correctly describes the unobservable realm.
4. Thus we should not take the risk of choosing between these different theories and accept instead what they all have in common: their descriptions of the observable realm. We should, in other words, adopt Constructive Empiricism.
5. However, as all the consequences that can matter for us are observable we also loose nothing by this.

The underdetermination of theory by data

As we will see, this argument is not watertight because it rests on premisses that can reasonably be doubted. However, the basic assumption that drives it—and which will not be questioned in what follows—is the thesis of what has become widely known from the work of the twentieth century American Pragmatist W.V.O. Quine (1975) as the Underdetermination of Theory by Data.

There are stronger and weaker versions of this thesis. At its strongest it claims that even given all the *possible* evidence that we could amass, there would remain more than one theory consistent with it. Some care has to be taken in how one construes 'all possible evidence'. If it means simply 'all the facts' then it is hard to see how this would not determine which theory were true. Instead it has to be construed—even in this strong version of the thesis—as meaning 'all the evidence that it is possible for we *humans*, having extended our senses through instruments, to amass'. But there is a weaker thesis that will serve our present purposes. It is the claim that for any *finite* body of evidence, there is always more than one theory consistent with it.

Even this weak reading of the Underdetermination Thesis appears to justify Van Fraassen's claim, then. Thus, theories that are consistent with the data will have to differ one from another in what they say either about future observable matters or about unobservable matters. A version of Constructive Empiricism that restricted our beliefs to past observable states of affairs would be one that followed Hume's sceptical attack on induction. This would be a radical position that denied that we could know anything about the future. Van Fraassen's position is more reasonable. Granting that induction is sometimes possible, he concentrates instead on those aspects of theories that concern unobservable matters and says that we should withhold judgement on these. The reason for this restriction is the claim that the only rational method of judging between theories is evidence, and that, by definition, there cannot be evidence that concerns the unobservable realm. That would require that the realm were not unobservable.

Coupled with the claim that we cannot have knowledge of the unobservable realm, Van Fraassen also suggests that we lose nothing by abstaining from judgement about it. All that can interest us are the observable consequences of theories, because these are the only aspects that have practical consequences. Thus we should not take the risk of commitment to particular accounts of unobservables but neither is this of any practical loss.

We will shortly he going on to outline three critical responses to Van Fraassen below. But it is important—as ever in philosophy—to think through in advance and for yourself whether Van Fraassen's is a good argument, and if not, why not.

EXERCISE 17 (15 minutes)

How convincing are the arguments for Constructive Empiricism? Is it a satisfactory account of science? What realist counter-arguments to Constructive Empiricism are possible? Make a note of any such counter-arguments, and think what further arguments Van Fraassen might offer in reply, before reading on.

Here are two common counter-arguments to Van Fraassen's Constructive Empiricism. The first depends on the discussion in Chapter 12 on the Theory Dependence of Observation. The second concerns the way in which theories can be combined. Constructive Empiricism has answers to both these criticisms but they are worth looking at briefly before coming directly to modern accounts of scientific realism. As elsewhere in philosophy, the criticisms help to clarify the account (here, a form of antirealism) to which other accounts are a response—in this case the three forms of realism to be considered below.

Constructive Empiricism and the theory dependence of observation

The brief presentation above of Constructive Empiricism presupposed a sharp distinction between the observable and the unobservable. But, as the arguments in Chapter 12 show, no such distinction can be drawn. In so far as there is a distinction it is both one of degree, not of kind, and one which itself presupposes a background of theory. Changing the theoretical background changes what is deemed directly observable and what is a matter of inference. So, can an argument for Constructive Empiricism be advanced even without such a sharp distinction?

Van Fraassen's approach is to distinguish two senses of observable. In some cases we cannot observe an entity with the naked eye because it is too far away. While it is not directly observed, it is observ-*able* in that, if one travelled closer to it, one would be able

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to observe it. In other cases, however, there is no possibility of direct observation. Thus sub-atomic particles, for example, are not observable. Van Fraassen then suggests that one should advocate Constructive Empiricism about the latter only.

As we will see later, this response is vulnerable to the objection that we can literally see through microscopes. (This is the point made by Ian Hacking below.) But another response is available. Rather than maintaining a distinction between theory and observation, one could, instead, accept that there is a scale ranging from the purely observable (relative to some background theory) to the purely theoretical. A Constructive Empiricist prescription can now be reformulated as follows. Adopt a degree of belief in a theoretical statement in proportion to its degree of observationality.

Degrees of belief

Talk of 'degrees of belief' may seem obscure and artificial. But it is simply a codification of an everyday phenomenon. If I take an umbrella out on a Spring day it does not mean that I am certain that it will rain. I may not even think that it is as likely as a fifty per cent certainty. Providing the 'cost' of taking the umbrella—its inconvenience and such like—is sufficiently low, then it will be worth taking even if I think that the chances are very remote. But if I am certain that it will not rain, and there is even the smallest inconvenience attached to taking the umbrella, then it would be irrational to take it.

Another way of making my degree of belief explicit is by examining the bets I would be prepared to make. If I think that the chances of it raining are fifty per cent then I should be indifferent between a gamble in which I am rewarded if it rains and one in which I am rewarded if it does not. Degrees of belief can be formally codified using preparedness to accept bets. But for now all that matters is an informal understanding of the idea. This can then be pressed into the service of Constructive Empiricism.

Constructive Empiricism about novel predictions from combined theories

The second preliminary argument against Constructive Empiricism turns on the effect of combining theories. Put simply the challenge is this. If one takes a Constructive Empiricist attitude to a theory then one should believe only its observable consequences. Thus if one adopts this attitude to two theories one should believe the combination of their observable consequences. But the observable consequences of the combination of two theories is not necessarily the same as the combination of the observable consequences of the two theories considered in isolation. The reason is this. The unobservable (or theoretical) component of one theory may 'interact' with either the theoretical or observable component of the other to generate fresh observable consequences, i.e. consequences that flow from neither theory working in isolation from the other. Given this fact, it seems that Constructive Empiricism is an inadequate philosophy of science because it cannot account for the fresh predictions that result from combining theories.

This issue will receive further discussion later. But for now the following response is sufficient. The combination of theories does nothing to change the basic position. The observational consequences of their combination still underdetermines the microscopic (unobservable) realm. Thus there is still no reason to believe in anything more than the empirical adequacy of the theories in question whether we consider them working together in combination or separately in isolation.

Three current scientific realist defences against the antirealism of Constructive Empiricism

The next three short extracts provide arguments against Van Fraassen's Constructive Empiricism. The first by the American philosopher of science, Richard Boyd, represents the orthodoxy of scientific realism. The following two are both qualifications to such straightforward realism. This first paper, however, attempts to defend head on a form of inference that Van Fraassen rejects.

EXERCISE 18 (20 minutes)

Read the extract from:

Boyd, R. (1999). On the current status of scientific realism. pages 195–222. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press (Extract pp. 207, paras 3–5)

Link with Reading 13.15

- How does Boyd's account of scientific realism counter the arguments for Constructive Empiricism?
- What extra ingredient does he introduce?

Boyd introduces an extra ingredient to considerations of the relation between theory and observational data. The Underdetermination Thesis played a key role in the brief sketch of an argument for Constructive Empiricism above. The Underdetermination Thesis is based on the claim that there is more than one theory that is *consistent* with any finite body of evidence. But that claim is remarkably weak. It simply means that there is more than one theory that is not incompatible with the data, that there is more than one theory whose falsity is not implied by the data. Thus the claim that there is more than one such theory may come as no surprise.

The relation of theory and evidence: consistency versus inductive support

Normally in science, in contrast with the Underdetermination Thesis, we think that there is a stronger relation between data and theory than mere consistency. We think of data as *supporting* theory. Data provide positive evidence or warrant for our beliefs. That is what is involved in taking events in the past as a *reason* for

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holding beliefs about what will happen in the future. Inductive inference involves not just eliminating theories that are inconsistent with past events but also using past events as a guide to choosing between the consistent theories that remain. The fact that Van Fraassen accepts that this is possible is shown by his acceptance of theories that make predictions about future observable matters. But this leaves the problem, according to Van Fraassen, that in the case of unobservables, so long as they remain genuinely *un*observable, we cannot begin to establish inductive generalizations linking them with observables because we are never in a position to check them.

Inference to the best explanation

But as Boyd points out, there is another kind of relation between theory and data, a relation provided by *explanation*. If a theory enjoys explanatory success, then, according to Boyd, it is likely to be true, or likely to be largely or approximately true. This suggests that there is a way of supporting detailed claims about unobservable realms. One looks for the best explanation of observable phenomena whether or not it employs unobservable or microscopic features. This is the nub or strap-line of modern Scientific Realism, that we should be realist about whatever it is (observable or unobservable) that supports an inference to the best explanation.

McMullin on lunar mountains

Another realist philosopher, Ernan McMullin (1987, pp. 51–73), gives the example of Galileo's explanation of the changing shadows on the moon that invokes lunar mountains. Because lunar mountains are the best explanation of the shadows, there is good reason, given the existence of the shadows, to believe in the existence of the mountains. This example would be classed by Van Fraassen as concerning observables because lunar mountains could eventually be observed with the naked eye. But the same kind of explanation-based strategy could also be applied in microscopic cases (such as subatomic particles) that are not directly observable even in principle.

In the case of Galileo and lunar mountains, the qualification 'best' in inference to the best explanation, may seem superfluous. Knowing what we know now, what other explanation of their shadows could there be? But given the status of the astronomical telescope as a scientific instrument at the time, even in this case an alternative might have been to explain the observed phenomenon away as a mere artefact of the instrument. Mere 'noise' is often an appropriate rival explanation in the case of putative observations made with complex modern instruments (whether space telescopes, vats of cleaning fluid for the detection of neutrinos or electron microscopes). Furthermore, while the shadows of mountains was a perfectly reasonable explanation, many others were available: the greening (or silvering) by algal growth (rather as Earth's appearance changes with the seasons); a change in chemical and reflective properties of the lunar surface in response to heat and light; etc. Again, knowing what we know now, these alternative theories would have dropped away. But it has always been a difficulty for Scientific Realism, that it relies on past performance and is able to say little or nothing about current difficulties or the direction that future developments of theory should take. Past performance, as the financial adverts remind us, is no guarantee of future performance!

Summary so far

Scientific Realism is typically regarded as the combination of semantic, ontological, and epistemological views. We have already seen that Van Fraassen holds realist views about both the semantics and the ontology of science. Scientific statements, literally construed, are either true or false. And they are true or false in virtue of the state of a mind-independent world. What we can *know* about the world, however, is always limited: one reason for this limitation being that for any given set of observations, more than one theory is possible. Hence Van Fraassen is an antirealist about what we can know about the world, he is an epistemological antirealist. Realists, such as Boyd and McMullin, seek to counter Van Fraassen's epistemological antirealism by arguing that we can indeed claim to know the truth, or approximate truth, of the full range of scientific statements. As we have seen, the key tool for this claim is explanatory success or more specifically inference to the best explanation.

The issue of whether explanatory success can ground claims about the truth of scientific claims across the board is also contested—in a different way from Van Fraassen—by the contemporary American philosopher of science and of physics, Nancy Cartwright.

Cartwright's attack on the validity of scientific laws

EXERCISE 19 (30 minutes)

Read the extract from:

Cartwright, N. (1999). The reality of causes in a world of instrumental laws. pp. 379–386. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press (Extract pp. 379–380)

Link with Reading 13.16

- How does Cartwright's position differ on the one hand from Scientific Realism, on the other from the epistemological antirealism of Van Fraassen?

Two arguments against inference to the best explanation

Cartwright suggests a middle path between full Scientific Realism as advocated by Boyd, McMullin, and others and the epistemological antirealism of Van Fraassen. Roughly speaking, she is a realist about those unobservable entities that are invoked to give the best causal explanation of observable effects. She rejects

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Fig. 13.4 Nancy Cartwright

inference to the best (theoretical) explanation but accepts inference to the most probable cause. There are two general arguments that underpin this distinction.

1. *Cartwright's general claim about theoretical explanation*. Theoretical explanation is, she suggests following Van Fraassen, a matter of providing a unifying account of diverse phenomena. In the physical sciences this is provided by mathematical theories from which statements about observable phenomena can be derived. But such theoretical explanations do not presuppose the truth of the explanatory theory. That a description of an observable phenomenon can be derived from a theory coupled with the fact that the description is true, does not imply that the theory is also true. The truth of a description of a phenomenon derived from a given theory, is no more a guarantee of the truth of that theory, than is the fact that one philosophical paper precedes another true philosophical paper a guarantee that the former is also true. Truth is an optional extra. (This is a contentious claim about explanation as we will see in the Chapter 14. Others argue that for an explanation to count as an explanation it has to be true.)
2. *Cartwright argues that there are, in theoretical physics at least, very often more than one, strictly incompatible, theoretical accounts that could, in principle, be applied to the same phenomena*. Scientists take a pragmatic attitude to choosing which theoretical treatment to use depending on which is most appropriate to any particular case. Appropriateness in this case depends on ease of mathematical application as well as accuracy. But if the theories used in explanation had to be true, then no such pragmatic attitude would be consistent. Scientists would have to determine which was the *right* theory or withhold explanation.

Theoretical versus causal explanation

Cartwright differs from Van Fraassen, however, in distinguishing theoretical explanations from causal explanations. Causal explanation, she argues, does provide reason for believing in the existence of the entities that are invoked in the explanation (hence this is sometimes called entity realism). The key difference between the two kinds of explanation is that (higher-level) explanatory theories do not bring about the (lower-level) descriptions that can be derived from them, but causes do bring about their effects. Thus if a causal explanation were to fail to pick out real entities, it would fail as an explanation. By contrast, if a high-level theoretical explanation turns out to be false it might none the less organize the lower-level descriptions of the world in an efficient and clear manner.

Cartwright's favoured examples concern the 'gap' between the Schrödinger equation in quantum physics and its application in particular problems. She suggests that, as a matter of fact, there are a number of different and conflicting assumptions and approximations that allow physicists to connect the high-level equations of theoretical physics with the practical applications of the theory in, for example, work on electronics. But they cannot all be true, because they conflict. Cartwright suggests that high-level theories or laws do not make lower-level theories true in the way that claims about causal entities can make true claims about observable phenomena.

Of course from some perspectives, belief in neither may seem very attractive. But this helps reiterate an implicit point that all these argument forms are, from a logical point of view, invalid: i.e. true premises can lead to false conclusions. As we saw in Chapter 5, the conclusions of a valid argument in logic are true because those conclusions are derived by nothing more than 'unpacking' the premises of the argument. In the case of a scientific argument, by contrast, the conclusions (about this or that underlying cause) are derived by inference (substantive but therefore fallible inference) from descriptions of an observed effect (serving as premises).

An objection to the distinction

We will return to the analysis of scientific explanation in Chapter 14. But it is worth noting here a natural general objection to Cartwright's claim that theoretical explanation does not require the truth of the explanatory theory. Surely if a theory is false, it cannot provide an explanation of anything. Here is an analogy. A false belief cannot form the basis of knowledge. There is no such thing as false knowledge, just apparent knowledge which, in fact, is not knowledge at all. Likewise, an 'explanation' based on a false theory is not an

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explanation at all but rather merely an *apparent* explanation that is not one at all.

Aside from simply rejecting the claim that truth is an essential part of any explanation, Cartwright has two responses to this objection:

1. One has already been mentioned. It is to note that in fact this is not how high-level theoretical explanations are regarded by scientists. They take a pragmatic attitude to the multiple and mutually inconsistent theoretical treatments that are available.

Now one might respond to this by saying that while this may be what happens *in fact*, it *ought not* to. Perhaps it reflects the present incomplete stage of scientific knowledge. Cartwright's second response provides a clue to why she thinks that this is not the reason for such a pragmatic approach to high-level theories.

2. Cartwright claims that it is *no coincidence that high-level laws are all false*.

Explanation versus truth as the goal of science

The argument for this startling claim (put forward especially in chapters 2 and 3 of Cartwright's *How the Laws of Physics Lie*, 1983) is that explanation and truth are incompatible goals. To be explanatory a theory must abstract from the descriptions of individual cases, an underlying pattern. An example is Newton's law of gravitational attraction that states that the force exerted on any particle in the universe by any other is proportional to the product of their masses divided by the square of their distance apart. But such a theory neglects other forces that also act on particles, such as electromagnetic forces. To be accurate one would have to say 'all other things being equal', or *ceteris paribus*, the force is proportional to.... But as other things are rarely equal, such a law, although true, would not be explanatory. In general the higher the level the theory or law, the more explanatory but the less true will it be.

Vector addition as a counter-argument?

Again there is a natural counter-argument. One might say that the law of gravitational attraction gives the force due to gravity and that the resultant force acting on a particle depends on the vector sum of this force and all other forces. Aside from some specific arguments against the reality of the component forces involved in that calculation Cartwright's general argument is that there is no general analogue of vector addition for laws with *ceteris paribus* clauses. Snell's law, which governs refraction, applies only to isotropic media and so should be read as relying on a *ceteris paribus* clause. But if other things are not equal, if the media are not isotropic there is no mechanical way of producing an appropriate refinement. In general there is no neutral way of combining the effects of different *ceteris paribus* laws, which all apply to some specific circumstance.

Cartwright's view of nature

This view is a reflection of a more fundamental view of nature which is best expressed in Cartwright's (1983) own words:

> I imagine that natural objects are much like people in societies. Their behaviour is constrained by some specific laws and by a handful of general principles, but it is not determined in detail even statistically. What happens on most occasions is dictated by no law at all. This is not a metaphysical picture that I urge. My claim is that this picture is as plausible as the alternative. God may have written just a few laws and grown tired.
>
> Cartwright (1983, p. 49)

Or again:

> The laws that describe this world are a patchwork, not a pyramid. They do not take after the simple, elegant and abstract structure of a system of axioms and theorems. Rather they look like—and steadfastly stick to looking like—science as we know it: apportioned into disciplines, apparently arbitrarily grown up; governing different sets of properties at different levels of abstraction; pockets of great precision; large parcels of qualitative maxims resisting precise formulation; erratic overlaps; here and there, once in a while, corners that line up, but mostly ragged edges; and always the cover of law just loosely attached to the jumbled world of material things. For all we know, most of what happens in nature occurs by hap (sic), subject to no law at all. What happens is more like an outcome of negotiation between domains than the logical consequence of a system of order.
>
> Cartwright (2000, p. 1)

Is realism about entities really distinct from realism about theories?

It is worth noting that this view is an expression of a form of antirealism that is not merely epistemological but also (despite Cartwright's protestation and her commitment to realism specifically about entities) ontological. It is not merely a prescription about which elements of science one should believe but also a claim about how the world is. Although she is a realist about entities, she believes that they really exist and that we can know about them (and is thus both an ontological and epistemological realist about entities), yet she also thinks that the world really might lack a kind of underlying order or structure. We will return to ontological antirealism shortly. First, however, it is worth considering a further objection that might be raised against Cartwright's position, namely that the distinction presupposed by a combination of entity realism and theory antirealism is unstable. Surely, runs the objection, to be a realist about entities requires believing in the theories that are used to identify the entities. Electrons, for example, are just those things of which our relevant atomic theories are true.

There is some truth in this objection. The distinction between theory and entity realism cannot be as sharp as Cartwright suggests. On the other hand, our theories about electrons have changed almost beyond recognition during the course of the twentieth century but we still take them to have been concerned

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throughout with the very same things. The original theories are now understood not to have been true, but not to have been so completely false that they were not describing anything real. It is just that we believe we now have a better idea what elections are 'really' like. The case of electrons thus differs from that of theories about phlogiston, for example, which are now taken to have been so false that they did not even refer to anything. (Although Putnam reports the suggestion that even those theories can be saved from complete vacuity providing they are construed as describing valence electrons, see Putnam, 1996, p. 15).

Hacking's view of nature

There is, however, another motive for distinguishing between explanatory theories and causal-explanatory theoretical entities, and that concerns our practical engagement with the world. This is the view expressed by the Vancouver born, Toronto-based philosopher of science, Ian Hacking.

The following passage complements the previous one in that both Hacking and Cartwright share a suspicion of theory and a contrasting commitment to (causally efficacious) entities. But Hacking develops further the role of experimental manipulation of entities. (This is based on Hacking's original book, *Representing and Intervening*, 1983.)

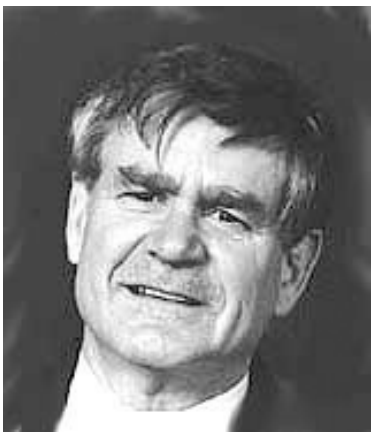

Fig. 13.5 Ian Hacking

EXERCISE 20 (30 minutes)

Read the extract from:

Hacking, I. (1999). Experimentation and scientific realism. pp. 247–260. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press (Extract p. 248).

Link with Reading 13.17

- To what extent is the kind of practical manipulation Hacking describes applicable to psychiatry?

Realism and practical manipulation

Hacking agrees with Cartwright that successful explanation does not provide much justification or warrant for the truth of a scientific theory. But he argues that successful theoretical explanation is not the only way to confirm the existence of specific unobservable particles. One can also develop techniques to measure and manipulate them. It is this practical hands-on component that helps support a form of realism about entities very like that of Cartwright. These practical techniques are based on a general understanding of the causal properties of unobservables. Thus 'the "direct" proof of electrons and the like is our ability to manipulate them using well-understood low-level causal properties.'

Hacking's example in the paper from which this extract is taken is the development of the PEGGY II electron gun. It is because this successful machine relies on spraying electrons, that no one could plausibly deny the existence of electrons. This is true despite the fact that the theories used to describe the machine may well not be correct. The ability to manipulate and the ability successfully and correctly to explain do not go hand in hand.

Links to later chapters

Hacking's claims about entity realism are connected to two subjects which will be discussed later but which are worth flagging now.

1. One is the degree of tacit knowledge in scientific work that often goes unnoticed. This is particularly apparent in the case of debugging PEGGY II where there is no checklist to determine in advance what factors are relevant—and what are not—to proper working. This will be discussed in Chapter 14.
2. The other is the way machines can serve as black-boxed embodiments of scientific claims about the world. Once one has built a machine that either measures some 'unobservable' or manipulates it to measure something else, then doubts about its status seem flimsy. Bruno Latour, the French sociologist, describes this rhetorical use of machines in *Science in Action: how to follow scientists and engineers through society* (1987). We will describe briefly other aspects of this sociological approach to science in Chapter 16.

But while entity-realism seems plausible in the context of physical and engineering science it is less directly applicable in psychiatry. This is an issue that has been hanging over us since considering Cartwright: to what extent can a distinction between realism about theories and realism about entities be drawn for a science of the mind? Hacking adds to that question the further

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dimension that, in some sciences, the entities postulated in theories can eventually be the subject of practical manipulation. But is there any analogue of this in psychiatry that could be used to address the issue of the validity of psychiatric classification?

The natural ontological attitude: assessing scientific validity from the inside?

The discussion of scientific realism so far has focused on the epistemological claim that we can come to know about hidden or unobservable aspects of the world via science. But it is worth remembering that this claim is made against a background of ontological realism. It is to ontological realism and antirealism that we will now turn by considering the American philosopher, Arthur Fine's, Natural Ontological Attitude or NOA.

Fig. 13.6 Arthur Fine

EXERCISE 21 (30 minutes)

Read the extract from:

Fine, A. (1999). The natural ontological attitude pp 261–277. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press (Extract pp. 271–272.)

Link with Reading 13.18

- What is the Natural Ontological Attitude?

The circularity of inference to the best explanation

Fine begins by arguing against the effectiveness of realism as an account of the success of science. The epistemological antirealist is characterized by Fine as resisting the claim that a successful explanation is likely to be true. This fits Van Fraassen. Realists claim, by contrast, that realism is the only account of science that does not make its success a miracle. But the argument used to support epistemological realism is thus of the following form: *scientific realism explains the success of science and thus it is likely to be true.* In other words it trades on just the inference from explanatory success to truth that epistemological antirealists deny. Fine's key claim is thus that deploying realism to explain the success of science simply reiterates the form of reasoning that is in question.

Notice that the issue about realism is precisely the issue as to whether we should believe in the reality of those individuals, properties, relations, processes, and so forth, used in well-supported explanatory hypotheses. In the case at hand—psychiatric classification—it is whether we should take the categories set out to answer to genuine structural features of our mental lives and mental health and ill-health.

Now what is the hypothesis of realism, as it arises as an explanation of scientific practice? It is just the hypothesis that our accepted scientific theories are approximately true, where 'being approximately true' is taken to denote an extra-theoretical relation between theories and the world. Thus, to address doubts over the reality of relations posited by explanatory hypotheses, the realist proceeds to introduce a further explanatory hypothesis (realism), itself positing such a relation (approximate truth). Surely, Fine argues, anyone serious about the issue of realism, and with an open mind about it, would be behaving inconsistently if he were to accept the realist move as satisfactory.

The natural ontological attitude

But, although Fine argues against traditional epistemological realism, he does not support a form of epistemological antirealism either. Instead he argues that the debate between realism and antirealism is itself the result of a mistaken metaphysical urge. What both realists and antirealists should—and he claims typically do—accept is a 'homely line'. This 'homely line' Fine calls the Natural Ontological Attitude (or NOA). NOA is what scientists working in a particular science find it natural to take for granted about what is real. This core position comprises an acceptance of well-grounded scientific claims as having the same status as everyday knowledge claims. Philosophers, Fine argues, of whatever realist or antirealist persuasion, take over and assume this NOA. But realists and antirealists go on to qualify this common core with further and different metaphysical claims. The difference between realists and antirealists, then, Fine argues, is that they go on to add to the common core further and unsupportable ontological additions.

Does Fine's argument apply to Van Fraassen and Cartwright?

Before going on to summarize those ontological additions, it is worth emphasizing that Fine's homely line would not be acceptable to Van Fraassen. He, by contrast, distinguishes between claims that would be regarded as of equal status by Fine because his distinction between claims made about observables and unobservables is revisionary of ordinary scientific practice. Things are less clear with regard to Cartwright and Hacking. There is some reason to believe that Cartwright's distrust of high-level

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explanatory scientific theories is itself an accurate reflection of scientific practice. This is certainly one of the arguments she presents for it. Scientists are, she suggests, pragmatically open to the use of any of a range of different and incompatible theories, selecting between them on the basis of usefulness. On the other hand, a sharp distinction between realism about theories and entities would be revisionary in that a well-established theory might well be accepted as having the same status as the claim that electrons exist.

Philosophical glosses

Fine then suggests that realists and antirealists add to the basic core position a further philosophical gloss. Antirealists might add an idealist theory of existence that is tied to human experience. Or they may construe the truth of a belief as coherence with our other beliefs or as being a useful belief, as some Pragmatists claim. Each of these modifications reinterprets the common core. Recall, however, that Van Fraassen suggested that there were two forms of antirealism. The first sort holds that science is or aims to be true, properly (but not literally) construed. The second holds that the language of science should be literally construed, but its theories need not be true to be good. On Fine's account, antirealism is of the former kind. Van Fraassen claimed that his Constructive Empiricism was of the latter. We will return to this distinction shortly.

What realism adds to the Natural Ontological Attitude

Fine's characterization of what a realist adds to the common core is simpler: 'what the realist adds on is a desk-thumping, foot-stamping shout of "Really!" '. The reason for this is that:

> The realist, as it were, tries to stand outside the arena watching the ongoing game [of science] and then tries to judge (from this external point of view) what the point is. It is, he says, about some area external to the game. The realist, I think, is fooling himself. For he cannot (really!) stand outside the arena, nor can he survey some area off the playing field and mark it out as what the game is about. (p. 131)

And antirealism?

In another paper, Fine (1984) offers a parallel criticism of anti-realists in the philosophy of science. Again he assumes that antirealists do not enter the nitty gritty of scientific disputes. They assume the same core beliefs about scientific entities as realists—the Natural Ontological Attitude again—but, like realists, offer a particular interpretation of their status. Where the realist adds a 'table thumping "Really" ', the antirealist offers a particular account of what the truth of claims about entities or theories consists in. Rather than taking it to be a matter of correspondence with the facts—as realists typically do—they define it merely as an acceptable belief.

> [S]cientific anti-realists . . ., recently, have tried to promote some kind of consensus-cum-pragmatic picture. I will try to give this picture a canonical representation so that we can identify the features that these particular anti-realisms have in common. So represented, it portrays the truth of a statement P as amounting to the fact that a certain class of subjects would accept P under a certain set of circumstances. If we let the subjects be 'perfectly rational' agents and the circumstances be 'ideal' ones for the purposes of the knowledge trade . . ., then we get the picture of truth as ideal rational acceptance . . .
>
> For the antirealism expressed in the idea of truth-as-acceptance is just as metaphysical and idle as the realism expressed by a correspondence theory. (pp. 360–361)

Natural Ontological Attitude without Philosophical additions

Realism and antirealism, then, attempt to add something to a common homely line, their shared NOA. Ergo, Fine suggests, the best philosophical response is to adopt the homely line, the NOA, as such, without adding anything further to it. One should adopt merely a NOA that is faithful to the beliefs and commitments of practising science. Thus the only guide to what is real within a given science is what those working within that science come to believe is real. What is real is defined solely by the praxis and related unfolding view of what is real which is internal to scientific work in a given area.

Philosophers, then, this line of argument implies, instead of working from an assumed perspective outside of science, should get stuck in. They should work in close partnership with scientists in particular disciplines—and this as we noted above, is what Fine has done. Philosophy cannot bring a fruitful external perspective to this issue. This is not to deny that philosophical reflection may have some bearing on what is real. But such reflection should be a proper part of scientific method and not an attempt at an Archimedean perspective from outside science.

But what is Fine's Natural Ontological Attitude really directed against?

Fine's account is attractive because of its simultaneous distancing from both realism and antirealism and because of its emphasis on a naturalistic solution. The degree of realism one should adopt to a subject matter should depend on the local scientific case for it. But it is also clear that Fine is characterizing a different kind of debate from the one with which we have been most concerned earlier in this session. The NOA is distinguished from realism and the latter is characterized as trying to say something further, something illicit. But in the discussion of Constructive Empiricism above, belief in specific claims about unobservables—which comprises a rejection of that form of ontological antirealism and which would often be part of a NOA—was called realism. Fine's description of realism appears to be not merely an opposition to a specific epistemological antirealist attack on some class of beliefs—unobservables, high-level theories—but a further claim in its own right. It is an attempt to step outside our practices and provide them with further philosophical foundations. While Fine is successful in casting doubt on the coherence of any such move, it is not clear that anyone should want to make it.

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This point can be spelt out like this. Boyd and McMullin argued (as described above) that the best explanation of the success of science was that its theories (about unobservables) were approximately true and that this suggested that those theories were indeed approximately true. If this argument is construed as a piece of scientific reasoning—perhaps a standard piece of arguing from effects to inferred causes to block Van Fraassen's worry—it surely lies within the NOA and does not amount to a further tendentious form of realism.

If, on the other hand, it is adopted as an attempt to provide additional reassurance that the claims that science makes as true really are true, then it does amount to a further gratuitous form of realism and Fine's argument against it is well directed. What would be the point of this further claim? Against what position could it be directed? The most obvious answer is that it could be used against an ontological antirealist: someone who denied that we could begin to describe a world independent of us, that apparently world-involving claims are really claims about sense-data or ideas.

Thus Fine might be seen as running together issues that are best characterized as part of an epistemological debate and issues from an ontological debate. But his solution to both is a form of philosophical quietism. To determine those elements of a science in which one should believe, one should turn to the science itself. One should reject philosophical prescriptions about which beliefs are sound and which are not. A dispute about which scientific beliefs are sound is a matter for local resolution within the science concerned. One should refrain from attempts to provide extra-scientific foundations for resolving such disputes by saying, for example, that science as a whole must be right because it is successful. That amounts merely to a table-thumping cry of 'really!' and adds nothing to the debate.

Conclusions of the session

What conclusions can we now reach at the end of this session? We have seen that there has been considerable debate about the kind of inference that is justified within science in general, or at any rate particularly within physics, broadly speaking from observable to unobservable matters. Van Fraassen argues that such inference is never justified. McMullin argues that it is justified whenever it is grounded in an explanation of observable phenomena. Cartwright argues that it is justified but only if the explanations in question cite the causes of observable phenomena and not just higher-level theories from which the explanandum can be derived. Hacking adds to Cartwright's causal explanation, the importance of our ability directly to manipulate the (unobservable) causes in question.

Fine takes a different line which starts by diagnosing what he takes to be the problem with this kind of reflection in general. Fine suggests that if arguments about realism and antirealism are construed as attempts to validate—or undermine—scientific reasoning from a perspective outside science, then, although they highlight some of the difficulties of scientific reasoning, they cannot succeed in legislating for good scientific practice. This is because it is impossible to take the sort of external perspective on science that these arguments seem to require. We should thus be content with, Fine concludes, and work within, the NOA (Natural Ontological Attitude) found in and proper to a given science.

If on the other hand, reflections of the kind outlined in this session are seen as continuous with and part of a scientific perspective, then they are useful in a modest way: as reminders of the sorts of consideration that can be brought to bear in the sort of informal reasoning that even the hard sciences such as physics have to employ. This is broadly how, in the next and final session of this chapter, we will be using the ideas outlined in this session in relation to psychiatric classification. It is worth pointing out, however, that the specific suggestions of both Van Fraassen and Cartwright about what inferences one should draw, seem too simplistic. Sometimes belief in unobservables is justified in the circumstances and sometimes not. Sometimes there is good evidence for high-level theories and sometimes not. In fact, as we will see in Chapter 16, high-level theories often play a role as methodological guides and are held immune from refutation by the evidence. They serve as guides to the development over time of research programmes rather than as simple descriptions of the world.

Reflection on the session and self-test questions

Write down your own reflections on the materials in this session drawing out any points that are particularly significant for you. Then write brief notes about the following:

1. What are the three key elements of Van Fraassen's 'constructive empiricism'?
2. What is 'empirical adequacy' as defined by Van Fraassen?
3. Among other 'post-Logical Empiricist' philosophers of science, what key ideas about what science reveals are associated with each of the following:
   (a) Boyd and McMullen
   (b) Cartwright
   (c) Hacking.
4. Is Arthur Fine a realist or antirealist about science? What is the Natural Ontological Attitude?
5. On which science has recent philosophical work on validity (realism) been particularly focused? How are the implications of this work relevant to issues of validity in psychiatric classification?
6. What key idea relevant to issues of validity in psychiatric classifications do we owe to Fine?

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Session 4 The third New Agenda – an Agenda modelled on the Philosophy of Physics

As psychiatry entered its post-Second World War phase in the development of its classifications, with the publication of ICD-6 from the WHO, it shared with the rest of medicine, and indeed with most other scientific disciplines, the traditional model of science outlined in Chapter 11. Theoretical physics aside, it was at this period still common ground among scientific disciplines, that what we get from empirical research is, if not *the* right way of seeing what the world is really like, then at any rate an ever closer *approximation* to the right way of seeing what the world is really like.

From one to many pictures

The philosophical project of Logical Empiricism, in the middle decades of the twentieth century, as we described in Chapter 12, aimed to put the traditional model of science on a rigorous basis by way of an in principle separation of the language of observation from the language of theory. The failure of Logical Empiricism, and subsequent work over the last 50 years in the philosophy of science, a sample of which we have examined in the last two sessions of this chapter, has shown that what we get from science, and hence what is captured in a scientific classification, is, in the terms of Sartorius' aphorism at the start of this chapter, not one but many ways of seeing the world. Somewhat like a photograph, then, the ways of seeing the world that we get from a science depend, among other things, on what kind of camera we use and on where we choose to point the camera. The picture also depends, as we will see in Chapters 14 and 15, on the judgements we make, implicit and explicit, when it comes to interpreting the resulting image.

From one to a family of classifications

One conclusion, then, from post-Logical Empiricist philosophy of science is that, in psychiatry as in physics, we need, at least in the present state of the development of the field, more than one picture, more than one classification. This conclusion is indeed anticipated to some extent by the WHO in its growing Family of International Classifications (the FIC, see http://www.who.int/classifications/en). In addition to the original International Classification of Diseases (the ICD, of which the Mental Disorders section is one chapter), the WHO's 'extended family' of classifications now includes an International Classification of Functioning (ICF) and a Classification of Psychosocial Stressors (CPS, at http://www3.who.int/icf/icftemplate.cfm). The American Psychiatric Association, similarly, includes within the one DSM family 'album', additional diagnostic criteria and nosological 'axes' covering much the same extended family members as the WHO's FIC (DSM includes criteria for social and occupational functioning, for example, to which we return in Part IV; and a separate axis for psychosocial stressors). Established clinical practice in psychiatry, too, reflects the need for more than one way of seeing the world in its diagnostic formulations. As we saw in Chapters 2 and 3, a diagnostic assessment in psychiatry is often summarized as a 'formulation' in which descriptively defined disorders (phobia, obsessional disorder, etc.) are separated out from possible aetiological factors (psychological and social as well as biological).

Is it rocket science?

So, someone may say, the bottom line is that we need more than one way of seeing the world—that's hardly rocket science! But a second conclusion from post-Logical Empiricist philosophy of science is that, well, actually, in psychiatry as in physics, classification, to the extent that it reflects the ways that we see the world at a point in time, really *is* rocket science.

Post-Logical Empiricist philosophy of science has replaced the relatively simple picture of the traditional model with something more like a kaleidoscope. We have touched on some of the complexities involved in understanding what science (notably physical science) is about, as revealed by post-Logical Empiricist philosophy of science, in Session 3. We will be looking at these and at other aspects of this complexity in more detail in later chapters in this Part. But this second conclusion—that psychiatry, like physics, is a very *difficult* science—is fundamental in its own right. Run the story of psychiatric classification only from the publication of DSM-III, and the disaggregation of psychiatry's classifications into a family of different ways of seeing the world has all the implications of failure carried by Kupfer *et al.*'s (2002) *Research Agenda for DSM-V*. Take the story back a few years further to the critical move, with the Lewis/Sartorius ICD-8, from an aetiological to a descriptive basis for a classification designed primarily for the purposes of international epidemiological and statistical studies, and the subsequent evolution of psychiatric science towards a family of classifications reflecting different ways of seeing the world appropriate for other and different purposes, becomes a success. Take the story back further still, with Lewis to the second century BC, and the emergence for the first time of a genuine *family* of classifications, in place of the two millennia of disparate and often competing ways of understanding mental distress and disorder that we reviewed in Chapter 7, becomes a truly *remarkable* success.

Risks and challenges

There are risks and challenges here, of course. Families, especially extended families, all too readily become dysfunctional. We return below to a number of pointers from the philosophy of science to the requirements for developing and sustaining a fully functional family of ways of understanding mental distress and disorder. One particular challenge, furthermore, to which as we saw Kupfer *et al.*'s (2002) *Research Agenda for DSM-V* clearly points, is that measured against the standards of the 'nuclear families' of bodily medicine, the well-defined unitary classifications of cardiology, gastroenterology, and so forth, measured against these traditional nuclear families of bodily medicine, the extended family of psychiatric classifications may appear dangerously 'free thinking', a nosological equivalent of the threat to traditional family life posed by the 'post-modern' anything goes.

This is a serious threat to which, again, we return below (under Agenda Item 9, on 'process issues') and at the end of the chapter.

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How we respond to this challenge, we will argue, how we respond to the challenge of living within a diverse family of classifications, will critically determine whether psychiatry's extended family of ways of understanding mental distress and disorder is a functional or dysfunctional *scientific* family. But as to the *need* for an extended rather than traditional nuclear family in psychiatry, this is a reflection, no more and no less, of the fact that the psychiatric sciences, like the physical sciences, are, inherently and irreducibly, difficult sciences, delivering, at least at this stage in their development, different and not always fully commensurable ways of seeing the world.

Physical science and psychiatric science

We should be careful not to push the parallels between physics and psychiatry too far. The parallels are there, and, we will suggest below, important. But there are also antiparallels: in physics rigorous conceptual thinking is readily formalized in available mathematics; in physics the domains of application of different pictures of the world are in general easily distinguished (relativity for the large scale, quantum mechanics for the small scale—although the interface between these pictures remains highly problematic); and, crucially, in physics the 'objects of study' are particles, whereas, in psychiatry, the 'objects of study' are people—not, as the use of the term 'objects of study' might imply, theoretical entities or abstract scientific concepts, but *real* people. This last difference between psychiatry and physics is one that we have already seen, in Part II, adds a whole extra dimension of difficulty to psychiatric science, the dimension of personal meanings, of significance, of empathy and of understanding. We pick up on this extra dimension of difficulty later in this part, in Chapter 15 on Reasons and Causes, and again in Part V.

The third agenda – an agenda for an extended family of classifications

In the remainder of this fourth session we will be drawing together some of the indications particularly from post-Logical Empiricist philosophy of science for the further development of an extended family of ways of understanding mental distress and disorder, an extended family that seeks to build on the families already emerging from the ICD and DSM processes.

The indications from the philosophy of science for the future evolution of psychiatric classifications are conjectural, of course, and wide open to refutation in the event. Physics, the paradigm science from which these conjectural indications are largely taken, is, as we noted immediately above, in key respects different from psychiatry. One of the lessons, furthermore, from the 1959 conference on classification described in Session 1, is that the best informed conjectures from the philosophy of science are open to refutation in the event. Hempel's confident conclusion that psychiatry would proceed along its established course towards evermore theoretical classifications, was refuted, in the event, by psychiatry going in precisely the opposite direction. The purpose of conjecture, though, as the British philosopher of science, Sir Karl Popper, pointed out (Popper, 1963—we met Popper in Chapter 11 and return to him in Chapter 16), is not to be 'proved right'. It is rather to establish hypotheses that can then be 'refuted' (or otherwise) in the event. The purpose of conjecture, which Popper argued in science could and should often be *wild* conjecture, is precisely to lay oneself open to refutation in the event. As we will see in Chapter 16, the Hungarian philosopher of science, Imre Lakatosh, took this idea further, suggesting that it was at the heart of the distinctive way in which progress is made in science.

And it may be your ideas (your wild conjectures, even) rather than ours, that, as we have repeatedly emphasized in this book, turn out to be the ones that emerge and flourish! Before, therefore, pursuing further our ideas for an Extended Family of Psychiatric Classifications, the next exercise asks you to take some time going back carefully over the materials in this chapter by way of developing your own ideas on how psychiatric classifications should develop from here.

EXERCISE 22 (30 minutes)

In this exercise we want you to take a broad overview of the materials covered in this chapter (including, where relevant, their underpinnings from other sections of the book) and to consider their implications for the future of psychiatric diagnostic classification.

Consider in particular two questions:

1. what contributions might the philosophy of science have to make to future developments in psychiatric classification, and
2. how best might these contributions be made?

In answering these two questions, use the agenda items adopted above for our Conceptual Agendas respectively for ICD-9 (Figure 13.2) and DSM-V (Figure 13.1). What are the similarities, and what are the differences, between these two Agendas and a new philosophy-of-science driven *Agenda for an Extended Family of Classifications*?

By way of example, we have already covered ideas relevant to three of the first four items on the Agenda in the above run-up to this exercise. The *Agenda for an Extended Family of Classifications* will take, under Agenda Item 1, its time line, with Aubrey Lewis, from at least the classical period, albeit concentrating, with DSM-V, on the modern history of ideas. Under Agenda Item 2, the Uses of Classification, the *Agenda for an Extended Family of Classifications* will adopt, again with Aubrey Lewis, a 'horses for courses' approach, recognizing the need for different classifications for different purposes. Unlike, however, both the ICD and DSM Agendas, the *Agenda for an Extended Family of Classifications*, under Agenda Item 4 (we return to 3 below) will admit any charge of deficit scientific status only to the extent that theoretical physics, too, in adopting a family of ways of seeing the world at this point in time, is vulnerable to a charge of deficit scientific status. (We return to this key issue in more detail below.)

So, at this point on the Agenda, it is over to you.

As before, write down our own ideas using the agenda items adopted above for both ICD-9 and DSM-V, before reading on. This is a 'Star Trek' exercise in which we 'boldly go', as the famous split infinitive has it, where no one has been before. So, be bold; you may well have better ideas than us!

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Here are our ideas for a future Extended Family of Classifications of Mental Disorder, as guided by ideas from the philosophy of science. As in the exercise, we have ordered our ideas broadly according to the agenda items developed for the corresponding Conceptual Research Agendas for ICD-9 and DSM-V. Table 13.4, below, gives a comparative table of the three agendas summarizing the main continuities and differences between them. As you read through the following materials critical review our suggestions under each agenda item and compare them with your own.

Agenda Item 3. The Misuses of Classification

We start, then, picking up from the agenda items covered in the run up to Exercise 22, with Agenda Item 3, the Misuses of Classification. Here, post-Logical Empiricist philosophy of science puts a rather different interpretation on the concerns of both the ICD-9 and DSM-V Agendas. In the ICD-9 Agenda, Lewis was concerned with classifications becoming overextended. In the *DSM-V* Agenda, Kupfer *et al.*, as Editors (in their Introduction), and indeed Rounsaville *et al.* in chapter 1, were concerned with inappropriate reification of psychiatry's diagnostic concepts. Both sets of

Table 13.4 The Third New Agenda – an Agenda for an Extended Family of Classifications – compared with first and second new agendas

Agenda Items	ICD-9 (agenda 2 above)	DSM-V (agenda 1 above)	Extended Family of Classifications (agenda 3)
(1) Time-line	Back: to 2nd century BC	Back: to DSM-III Forward: to DSM-VI/VII	Back: at least to ICD-8 Forward: to ICD-12/DSM-VI
(2) Uses of psychiatric classification	Epidemiological and statistical	Diagnosis by clinicians	Multiple: different uses of different (coexisting) classifications in different contexts and for different purposes
(3) Misuses of psychiatric classification	Overextension	Reification of categories	Multiple: What counts as a 'misuse' of a scientific classification is context and purpose specific
(4) Status of psychiatric science	Deficit model	Deficit model	Strengths model: Deficit model is *rejected outright* in favour of a strengths model as the basis of progress
(5) Concepts	Concepts recognised as problematic	Important; but not recognised	Strengths model opens up resources for conceptual research hand in hand with empirical research
(6) Reliability	THE BIG ISSUE	Run out of steam	Reliability a small, but still *necessary*, first step to validity
(7) Validity	Assumed to follow reliability	THE BIG ISSUE	Multiple validities appropriate for an *Extended* Family of Classifications but all based on scientific criteria
(8) Structural solution	Descriptive symptom-based criteria	Aetiology based	Different classificatory structures needed for different purposes and ranges of application
(9) Process solutions	Collegiality, international collaboration, different perspectives and a shared aim	Empirical research	THE BIG ISSUE: *two* rules of (observational) science: (1) challenge received authority, and (2) listen instead to what the world (whether of 'particles' as in physics or of 'people' as in psychiatry) has to say.
(10) Utility (of new classifications)	'Public' classifications (like ICD): for (international) epidemiological and statistical purposes. 'Private' classifications: for 'local' use for particular research programmes and other specific purposes.	Better health-care services	Improved face validity: 1) results in improved communication between professionals and patients and between different professional groups, and 2) a step to improved construct validity
(11) Overall tenor	Upbeat	Downbeat	Upbeat, based on: (1) observational science, (2) challenge to received authority, (3) enough time
(12) Any other business	none	none	*New resources*: from the neurosciences, the mental sciences, national policy initiatives, and international networks *Next steps*: depend on (1) continued research initiatives, and (2) a successful 'Renaissance science' challenge to received authority

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concerns, in the context of the ICD-9 and DSM-V Agendas, represent conflicts with the traditional model: Lewis' implied contrast is between the overextended classifications of medieval taxonomy and the neat compression of information in such classifications as the chemical periodic table; the DSM-V's explicit contrast is between psychiatry's descriptive categories and the aetiological information packed into the disease concepts of bodily medicine in disciplines such as cardiology.

Post-Logical Empiricist philosophy of science, while endorsing these concerns as potential misuses of classification, suggests that the difference between use and *mis*use, between appropriate and *in*appropriate uses of classification, is context and purpose dependent. We return to this idea below, under issues of validity. It arises partly from the difficulty of 'fixing' the sense in which a scientific classification is properly about what science is about (i.e. the 'real' world); partly from the work of Arthur Fine and others suggesting that what is 'real', and hence which classifications are valid, is a matter for local determination within particular scientific disciplines; and partly from issues of utility, i.e. the many different ways in which values comes into science, including, the 'context of interest' of explanations (see Chapter 14), the epistemic values that Quine and others have argued are necessary (though of course not sufficient) determinants of theory choice, and the pragmatic considerations that arise in applying the results of scientific research in this or that particular practical context.

All of which, to repeat an oft-made but oft-needed point, is not to say that 'anything goes'. Post-Logical Empiricist philosophy of science gives no warrant to that version of postmodernism that would have us believe that any classification, any way of seeing the world, is as 'good' as any other. Certainly, a classification can become overextended: some have argued that with their increasing size and complexity, both ICD and DSM are perilously close to becoming overextended, at least for clinical purposes. Certainly, too, classificatory concepts can be inappropriately reified: the considerations of Part I of this book amount to a critique of the inappropriate reification of concepts of disorder, equally by opponents and supporters of the concept of mental illness, where 'inappropriate' reification means taking disorders to be real in the way that stars and stones are real independently of human values. But the key point here is that what counts as a *mis*use of a classification, what makes a classification *over*extended, what is *inappropriate* reification of classificatory concepts, is not something to be judged against a generic standard of what a scientific classification should be like. What counts as a misuse of classificatory concepts has to be judged rather on the particulars of a given science according to the state of development of that science and the purposes for which the classification in question is to be used.

Agenda Item 4. Status of Psychiatric Science

An easy one, this, to a first approximation at least—as noted in Exercise 22 (above), what is good enough for the complex science of theoretical physics (i.e. a family of ways of seeing the world), is good enough for the complex science of psychiatry.

Given, however, the importance of correcting the deficit model of psychiatric science, as a key aspect of the twentieth century stigmatization of psychiatry as an 'also ran' to bodily medicine (see Part I), it will be worth spelling out just how radically an *Agenda for an Extended Family of Classifications of Mental Disorder*, drawing particularly on the philosophy of physics, differs from both Lewis' *Agenda for ICD-9* and Kupfer *et al.*'s *Agenda for DSM-V*, in its view of how the status of psychiatric science should be understood. The ICD-9 and the DSM-V Agendas, as we have seen, are alike in embodying deficit models of psychiatric science, and hence of psychiatric classifications. The *Agenda for an Extended Family of Classifications*, by contrast, drawing on the resources of a philosophy of science focusing on physics, not only rejects any form of deficit model but also actively embraces a *strengths* model.

The move from deficit to strengths model in the *Extended Family of Classifications of Mental Disorder* is anticipated by earlier discussions in this book: the 'full-field' picture of the conceptual structure of psychiatry to which we came in Part I, offers, we argued by extension from Austin's observations on the heuristic significance of conceptual difficulties in general, a window on conceptual elements of medicine (patients' experiences, agency, and values) that, although important in all areas of health care, remain partially hidden and hence (relatively) neglected in areas of bodily medicine where these elements are (relatively) unproblematic. Similarly, then, here, the added difficulties of psychiatric classification may indeed turn out to provide a window on aspects of medical classification, medical ways of seeing the world, that, although important, have tended to remain relatively hidden because they are relatively unproblematic and hence have been relatively neglected (to date) in other (less conceptually difficult) areas of medicine.

Three steps to a strengths model

So how, exactly, do we get from deficits to strengths? The move from a deficit to a strengths model of psychiatric classification, as developed here particularly by way of the resources of the philosophy of physics, involves three steps. The *first step* is the recognition of the structural similarities between psychiatry and physics: (1) their shared conceptual as well as empirical difficulties (in neither discipline are the meanings of their key terms of art—time, space, particle, mental, illness, etc.—capable of full explication beyond operational definitions for particular purposes); (2) their shared difficulties about the separation between observer and observed (remember the EPR (Einstein–Podolsky–Rosen) paradox in Chapter 12); and (3) their shared need for more than one classification, more than one way of seeing the world.

That psychiatry shares with theoretical physics these structural similarities, however, is capable of different interpretations. This brings us to the *second step* in the move from deficit to strengths model, a step that depends crucially on post-Logical Empiricist philosophy of science. In a traditional understanding of science, the structural similarities between psychiatry and theoretical

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physics point, perhaps surprisingly, to a deficit model of the physical sciences rather than to a strengths model of psychiatric science. This is surprising because the physical sciences are the traditional paradigm of hard sciences and theoretical physics is at the cutting edge of the physical sciences. However, the difficulties of definition of the key terms of art in theoretical physics, its problems of separating observer from observed, and its need for more than one (mutually incommensurable) way of seeing the world, are all, by the standards of the traditional model of science, signs of deficit as much for theoretical physics as for psychiatry. (Recall in this respect, from Chapter 12, that Einstein conceived the EPR paradox precisely to show that quantum mechanics is, literally, absurd by the tenets of traditional scientific realism. See also d'Espagnat, 1976.)

Post-Logical Empiricist philosophy of science, by contrast, stands this story on its head. Focusing on what, in particular, theoretical physics is *really* like, as distinct from what it *should* look like according to the traditional model, post-Logical Empiricist philosophy of science shows that these structural features, although particularly transparent in psychiatry and physics, are indeed features of *all* sciences. As Austin would have anticipated, we notice these structural features of all sciences where they cause difficulty. But the fact that they cause difficulty in, of all sciences, theoretical physics, suggests that difficulties with these structural features are not a feature of a deficient science but, to the contrary, of a science at the cutting edge. By extension, therefore, the appearance of the same structural difficulties in psychiatry suggests that psychiatric science, too, is a science at the cutting edge.

There are, as we noted above in the run-up to Exercise 22 at the start of this session, important differences as well as similarities between theoretical physics and psychiatry. These differences bring us to the *third step* in the move from a deficit to a strengths model of psychiatric science. For the third step is to see that, by the lights of post-Logical Empiricist philosophy of science, the differences between theoretical physics and psychiatry, as much as the similarities, are a sign of the strengths of psychiatric science not of deficit.

The strengths—rather than deficit—implications of the differences between theoretical physics and psychiatry are well illustrated by the relatively value-laden nature of psychiatric science. Thus, as we saw in Session 2 of this chapter, post-Logical Empiricist philosophy of science has shown that the traditional opposition between science and values (their supposed immiscibility as we called it), is not sustainable: attempts to hang on to a traditional value-free model of science have (so far) failed; and, conversely, the importance of values has been directly demonstrated in a number of key stages of the scientific process (including observation, theory choice, and applications of the outputs from science to practice).

A key difference, however, among the differences that we noted above between physics and psychiatry, is that psychiatry is concerned not with particles but with *people*. This, indeed, we said, is a *key* difference between what psychiatry is about compared with what physics is about. Combine, therefore, the post-Logical Empiricist demonstration of the presence of values in all areas of science, with the demonstration in Part I of this book that psychiatry is value-laden because (following Hare) it is concerned with aspects of human experience and behaviour (emotion, sexuality, belief, etc.), in which human values are characteristically diverse, and the value-laden nature of psychiatric classifications emerges, not as a sign of a deficient science, but as a sign of a science that is properly reflective of a key aspect of the nature of what, as a science, psychiatry, in contrast with physics, is really about, i.e. people.

From deficit to strengths to progress

We have spent some time on this particular Item on the Agenda for psychiatric classification because the deficit model of psychiatric science is so deeply embedded in the lay as well as scientific (including psychiatric scientific) mind. The move from deficit to strengths model, correspondingly, is liable to be dismissed as wishful thinking equally by psychiatry's opponents and by its supporters. Merely adopting a strengths model is no guarantee of progress, of course. But the move from deficit to strengths model is the basis of a series of further moves, summarized in the rest of our Agenda for an *Extended Family of Classifications*, that together open up the space within which, we argued at the start of this session, progress will become possible.

Agenda Item 5. Concepts

The importance of moving from a deficit to a strengths model of psychiatric science in opening up the space needed to make progress possible is well illustrated by the different ways in which concepts of disorder are treated, on the one hand in the ICD-9 and DSM-V Agendas, and, on the other, in the Agenda for an *Extended Family of Classifications*.

The ICD-9 and DSM-V Agendas both recognize the importance of conceptual difficulties in relation to psychiatric classification. For both, though, working within a deficit model, these difficulties are inevitably taken to reflect deficiencies in psychiatric science. To this point, then, the deficit model does no more than take us back to the considerations of Part I. However, the importance of moving to a strengths model becomes clear when we come, in chapter 1 of Kupfer *et al.*'s *Research Agenda for DSM-V*, to the proposals advanced there for tackling these difficulties. For despite implicitly acknowledging the conceptual nature of these difficulties, the proposals in the *DSM-V Agenda* for tackling them are limited, as within a traditional model of science they are necessarily limited, to proposals for empirical (rather than conceptual) research.

Conceptual alongside empirical methodologies

We return to the specifics of the DSM-V proposals for empirical research on concepts below (under Agenda Item 9, Process). However, the point for now is that in limiting itself to the deficit model of psychiatry embedded in the traditional model of science, Kupfer *et al.*'s *Research Agenda for DSM-V* thereby cuts itself off from the now considerable resources available for conceptual research in general and research on values in particular. Just how productive working with these additional resources will prove to

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 369

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be, only time will tell. However, as we saw in Part I, Austin's concept of philosophical fieldwork, which is entirely consistent with *starting* with empirical studies of broadly the kind proposed by Rounsaville *et al.*, in Chapter 1 of the *Research Agenda for DSM-V*, has already yielded tangible results (Colombo *et al.*'s, 2003, work on models of disorder, and other examples in the Reading Guide to Chapter 4); and, as we will see in Part IV, similar work on values is already finding its way into policy and practice in mental health in a number of countries around the world.

Agenda Item 6. Reliability

Reliability, as we saw in Session 1, was the big issue for the ICD-9 Agenda. Correspondingly, much of psychiatric science in the last three decades of the twentieth century (covering the periods of both ICD-9 and -10, and DSM-III and -IV) has been concerned with improving the reliability of our diagnostic methods in relation both to assessments of psychopathology, as we saw in Chapter 12, and, as we noted in Session 1 of this chapter, in relation to the categories in our diagnostic classifications (references to a sample of classic reliability studies are given in the Reading Guide).

The danger now, though, so long as psychiatry continues to operate within the limited space provided by a deficit model of its science, is, as we noted in the final part of Session 1 of this chapter, the danger of throwing out the baby with the bathwater, the danger of losing what we had gained. The danger is that our hard won improvements in reliability will be thrown out with what is perceived within a deficit model of psychiatric science as the failure of validity. This 'failure' has been the basis of growing criticisms from, among others, but importantly, some of those most directly concerned in the new neurosciences (e.g. Hyman and Fenton, 2003). But the danger of losing what we had gained is also signalled in Kupfer *et al.*'s *Research Agenda for DSM-V* itself, by the frustration that shows through in the language used to describe this supposed failure—you will recall the shift in Kupfer *et al.*'s terminology between the two readings early in Session 1, from a 'limited' classification paradigm (linked with Exercise 1) to a long catalogue of total 'failures' of validity (linked with Exercise 2).

Reliability is only a step . . .

The danger here, of losing the gains that with so much hard labour we have made, is mitigated by going back to the very modest claims for reliability made by Hempel and Lewis at the time of the 1959 conference. Hempel, you will recall, made only a more or less passing reference to reliability as something that, as a concern particularly of the social and psychological sciences, coincided with the importance of the concepts on which scientific classifications are based having, as one of his two key requirements, Empirical Import. Lewis, similarly, when he introduced into the discussion following Hempel's paper, his distinction between public and private classifications, emphasized that international agreement (i.e. reliability) in the use of a classification was important only for the limited purposes (which of course were the WHO's purposes in developing ICD) of international epidemiological work and statistical reporting.

. . . but a necessary step

Reliability, then, as an aspect of the observational basis of a science, and even within Logical Empiricism as a version of the traditional model that placed so much emphasis on the observational basis of science, is only a step towards what science is really about, the development of theory. It is important in this respect not to lose sight of the excitement and sense of promise that went with the appearance of new and more reliable classifications, outside as well as within psychiatry: the *New York Post* (28 July 1963) heralded Robert Spitzer's work as Chair of the DSM-III Taskforce, under a headline 'Approach Holds Promise for Standardized Diagnostic Aid', describing DSM-III as 'a tool which may become the psychiatrist's thermometer and microscope and x-ray machine rolled into one'.[2]

The disappointment that things have not moved so fast and so far as anticipated, is, as we have seen, at the heart of current disenchantment, at least as expressed in Kupfer *et al.*'s *Research Agenda for DSM-V*, with our present day classifications. Hempel, no doubt, would have shared the frustration of today's psychiatric scientists, among others, both those without and those within the DSM process, that the anticipated progress from reliability to validity, has been, at best, faltering. But, and this is the big 'but' that we explored in the last part of Session 1, reliability, even as measured by a more careful reading of Hempel, *is* a step. Just exactly what that step consists in and how it may lead to validity is a matter for further debate. It may be that reliability, as Kendell argued in his 1975 book on classification, is a limiting step, i.e. that validity can never be greater than reliability (Kendell, 1975). It may be that reliability, as we concluded in the last part of Session 1 of this chapter, is a necessary step to the ultimate shared goal of validity. Either way, though, miss the first step of reliability, and we risk finding ourselves back where we started, in a nosological Tower of Babel.

Agenda Item 7. Validity

If the big issue for Lewis' *Agenda for ICD-9* was reliability, the big issue for Kupfer *et al.*'s *Research Agenda for DSM-V*, as we saw in Session 1, is validity. Given, as noted under Agenda Item 6 (Reliability), the disappointing (from the traditional point of view) rate of conversion of reliability into validity, the emphasis on issues of validity in the *Research Agenda for DSM-V* is no surprise. What is a surprise, though, at first sight, is the line on validity taken in chapter 1 of the *Research Agenda for DSM-V*, by Rounsaville and colleagues (2002) on conceptual issues (the chapter, you will recall, titled 'Basic nomenclature issues for DSM-V'). The surprise is that so much of the discussion of validity, although explicitly focused on the 'uses of validity in psychiatric nosology' (p. 7), is taken up, not, as one might expect from the traditional model, with explanatory theories, with prediction, with underlying regularities, and with hidden entities and causal laws. The discussion, when it comes to the crunch, the crunch of the 'Rationale for Changing Criteria' in the move from DSM-IV

[2] With thanks to Giovanni Stanghellini for spotting this.

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 370

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to DSM-V (pp. 10–11), is largely taken up with social and political issues.

Social and political validators?

'Traditionally', this section starts, 'when changes in criteria in a diagnostic system are contemplated, the positive features of such changes (e.g. improvements in reliability or validity, greater ease of use, or superior discriminatory ability) are emphasized'. Well, if that is 'traditional', surely, with suitable extensions of the 'e.g.' (expanding, e.g., validity to face, predictive and construct validity), these are not merely 'positive' features but features of what science, and hence the categories in a scientific classification, are all about. Yet Rounsaville *et al.*, instead of enlarging on the requirements for scientific validity, list some the 'disadvantages' of changing categories, disadvantages that, at first glance, appear to have little or nothing to do with what science is about: (1) confusing busy clinicians; (2) having to change report forms; (3) prejudicing the cumulative capacity of research (because meta-analyses require stable categories over time); (4) having to trade being thought 'old fashioned' against the need for longitudinal studies; (5) the need to develop new structured interviews; (6) the spawning of 'cottage industries' to assess the validity of new categories when research budgets are so limited; and (no surprise this one given the deficit model) (7) 'the possibility that frequent changes of diagnostic category can potentially discredit the revision process and increase the chances of the DSM becoming a subject of ridicule' (all from pp. 10–11). Small wonder, then, that Rounsaville *et al.* conclude, in the final paragraph of this section, that no 'compelling guidelines' for changing criteria can be given. They note, instead, a number of points to be taken into consideration, points that are important but again more of a political and social than (traditional) scientific nature (e.g. bias among committee members and the pressures of career advancement).

There are warning signs here, then, under this agenda item for validity as under the last for reliability, of the scientific baby being thrown out with the bath water. Faced with the failure (as the deficit model of psychiatric science sees it) of the ICD and DSM processes to date, Rounsaville *et al.* in this chapter of the *Research Agenda for DSM-V*, appear to be at risk of giving up on what science is about (what the world is really like) and of retreating to what science is decidedly not about (what we, individually and collectively, would like the world to be really like). Rounsaville *et al.*, it is important to emphasize, in their opening reference to the 'traditional validators' noted above, call these 'positive features'; and the points they make in the concluding paragraph of this section of their chapter, are all points of warning against prejudicing good scientific process by social and political factors. The baby is to this extent still in the bath. However, their discussion of the traditional validators occupies considerably fewer column inches than those devoted to the social and political factors guiding (in their view) category change. Moreover, their bottom line observation on the traditional validators, in an earlier section of their chapter, was that, since the traditional validators often point in different directions (predictive validity, for example, pointing in a different direction from family aggregation), assessing the validity of this or that category necessarily depends on a prior choice between validators, a choice which, they say, 'Unfortunately . . . is fundamentally a value judgement' (Rounsaville *et al.*, 2002, p. 8).

Validity is of course a large topic that has generated a considerable and often ingenious literature within psychiatry as well as in related disciplines such as psychology and sociology (see, for example Robbins and Guze, 1970; and Schaffner 1986 and 1993 in the Reading Guide). Our aim in this chapter, however, has not been to review this rich literature, but rather to indicate the potential resources from the philosophy of science for further strengthening the psychiatric literature on validity. Hempel, for example, drawing on the resources of Logical Empiricism and related positivist literatures in the philosophy of science, has much to offer that remains highly relevant: his check-list of key criteria for Systematic Import, for example, his Systematicity Check List as it might now be called, still bears detailed study (Hempel, 1961, pp. 327–328). And the further resources of more recent post-Logical Empiricist philosophy of science open up yet further interpretive resources for current debates.

Differentiating claims

Post-Logical Empiricist philosophy of science, for example, suggests a number of interpretations of the recent claim by Kendell and Jablensky (2003), reflecting similar claims in both the ICD-9 and DSM-V Agendas, that clinicians should think of diagnostic categories as 'simply concepts, justified only by whether they provide a useful framework for organizing and explaining the complexity of clinical experience in order to derive inferences about outcome and to guide decisions about treatment' (Kendell and Jablensky, 2003, p. 5). Kendell and Jablensky may, for example, be advocating something like Van Fraassen's Constructive Empiricism: a form of agnosticism about whether psychiatric classification is really valid. On the other hand, the success of a classification (if it is successful) in guiding treatment, in successfully intervening in subjects' health, might provide arguments for supposing the classification to be valid, arguments that would follow the work of Cartwright and Hacking in emphasizing, respectively, causal-predictive power and practical manipulation of unobservable entities (in this case disease entities). Or, again, Kendell and Jablensky may be claiming something stronger: that classificatory categories could not possibly be anything more than 'mere concepts', constructs that are neither true nor false to the 'real' world but merely help to organize data. That would be a form of instrumentalism. If so, then Fine's work suggests that the claim risks foundering on an attempt to step outside the NOA (Natural Ontological Attitude) proper to psychiatric science.

To those responsible for developing the next editions of psychiatry's classifications, rich as the additional interpretive resources of post-Logical Empiricist philosophy of science may be in principle, in practice these resources may seem merely to add further

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and unwarranted complications to an already daunting task. Certainly the issues *are* complicated. The burden of this chapter is after all that just below the surface, as it were, of the problems of validity in psychiatric classifications, lie the deep philosophical issues of realism, idealism, essentialism, and so forth, issues that in two and half millennia the brightest and best have been able to explore only in small part, and issues that in the hardest of hard sciences, theoretical physics, are today very much where the scientific action is. We pick up on further closely related and equally problematic issues, particularly as they relate to scientific theory (Hempel's Systematic Import), in later chapters of this part: the nature of explanation in Chapter 14, of causal explanation in Chapter 15, and of progress in science in Chapter 16.

No compromises

Still, if the issues *are* complicated, it would surely not be consistent with the demand for rigour in science that, for 'practical' purposes, the issues should be artificially simplified. We would not consider compromising the evidence-base of our science. Neither should we compromise its conceptual base. All of which is not to say that everything should somehow go on hold while a two-and-a-half-millennium debate about realism is suddenly and conveniently resolved! To the contrary, as in all areas of science, we should get on with doing what is do-able. And there are indeed 'quick wins' to be had in the move from a deficit to a strengths model of psychiatric science drawing on the resources of post-Logical Empiricist philosophy of science. Some of these quick wins, it is true, will perhaps not be so very quick in that they will require a shift of paradigm in the processes by which psychiatric science, and its corresponding classifications, are developed. We return to 'process' issues below, under our Agenda Item 9 for the *Extended Family of Classifications*, towards which, we have argued, developments in both ICD and DSM are already pointing.

But some quick wins to be had

Other quick wins, however, in moving from a deficit to a strengths model of psychiatric science, could be more or less immediate. Break, for example, the Procrustean bed of a single way of seeing the world assumed by the traditional model, and Rounsaville *et al.*'s observation that different validators may point in different directions ceases to be, as in a deficit model of psychiatric science, a source of confusion and conflict, and becomes, in a strengths model, a resource (i.e. of different ways of seeing the world across different domains of application and for different purposes). Break, similarly, the assumption of the traditional model that values and science are immiscibility, and the fact that, as Rounsaville *et al.* point out, the choice of validators involves value judgements, ceases to be, as in a deficit model of psychiatric science, a matter for regret (recall Rounsaville *et al.*'s word, 'unfortunately'), and becomes in a strengths model an insightful observation of the extent to which, in psychiatry as in physics, we may need, at least in the present state of development of our respective sciences, different ways of seeing the world for different purposes. Break, finally, the assumption of the traditional medical-scientific model that the (rightly celebrated) successes of the explanatory 'disease entities' (categorical and continuous) of the sciences underpinning bodily medicine warrants the adoption of essentially similar explanatory entities in the very different (and very much more difficult) sciences underpinning psychological medicine, break this myth and Rounsaville *et al.*'s conclusion noted above that 'no compelling guidelines' can be given for changing the categories in a psychiatric classification, ceases to be, as in a deficit model it inevitably is, an acknowledgement of (at least temporary) failure, and becomes in a strengths model a direct parallel of Arthur Fine's conclusion from symmetric issues in the physical sciences, that validity is irreducibly an issue for local debate and local assessment and balancing of the evidence.

Agenda Item 8. Structural Solutions

With Lewis, in our *Conceptual Research Agenda for ICD-9*, the structural 'solution', as we have seen, was to move from a theoretical (aetiology-based) to a predominantly descriptive (symptom-based) classification. In Kupfer *et al.*'s *Research Agenda for DSM-V*, there are suggestions that it may be time to move back the other way, to reintroduce theory: e.g. Kupfer *et al.*'s talk of the need for 'fundamental changes' (2002, p. xviii), for 'an as yet unknown paradigm shift' (2002, p. xix). There is of course, as Hempel emphasized in his 1959 paper (published in 1961), everything to gain in moving on when the time is right from a descriptive to a more theoretical stage in the development of a science. The concern, though, with this move as it is discussed in Kupfer *et al.*'s *Research Agenda for DSM-V*, is that it is motivated, not, as Hempel would have required, by explanatory success, but, consistently with the deficit model of psychiatric science, by (perceived) explanatory failure. We have noted a wide variety of the deficit terms used in Kupfer *et al.*'s *Research Agenda for DSM-V*. The danger, then, in making a move back to theory, if the move is motivated by a deficit model of psychiatric science, is that we will end up back where we started in the 1950s, in, as we put it above, a nosological Tower of Babel.

When the time is right . . .

It is no part of the role of philosophy to determine when the time is right for a structural change in a science. Fine's work, as we have seen, on the NOA and realism in physics, makes this point particularly clearly. Hempel, too, in his 1959 paper, held back from providing other than generic criteria for judging when the time is right for moving from Empirical Import to Systematic Import. As to specific theories, furthermore, as to whether psychiatry should develop on the basis of biological, psychodynamic or some other theory or combination of theories altogether, Hempel, as we noted above, made a point of staying strictly neutral.

There is, though, without anticipating future substantive developments in psychiatric theory, at least one clear structural implication of post-Logical Empiricist philosophy of science for psychiatric classification, a structural implication that as we will see, underpins the crucial 'process' issues to which we will turn in

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a moment. The structural implication in question has to do with the nature of the Extended Family of Classifications to which, we suggested above, developments in both ICD and DSM are already pointing.

. . . as judged by those directly concerned

Thus, in the traditional model, with its core assumption of one (true) way of seeing what the world is really like, any family of classifications must have a 'head of family'. There must be, as it were, a top classification, closer to what the world is really like than other classifications, and a classification to which, therefore, other classifications are subordinate. We need to be clear here: it is essential to the scientific process (as we will see in Chapter 16) that there should be lively competition between different ways of seeing the world. But the competition has to be run by the rules of science. And what we learn from post-Logical Empiricist philosophy of science, guided remember by the example of physics, is: (1) that the rules of science allow for heterogeneous classificatory structures, different ways of seeing the world, to coexist (across different domains of application and for different purposes); (2) that where competition does arise, there is no preassigned vantage point from outside the science in question by which the 'winner' at a given point in time is to be judged; and (3) that 'winning' is a matter for local judgement between—and this takes us directly to the key point for process—the stakeholders directly concerned.

Agenda Item 9. Process Solutions

With the structural issues in place, there is a simple, though fundamental, and to some of those concerned fundamentally ill-conceived, point to make about process. The fundamental point about process is this: if the development of an Extended Family of Psychiatric Classifications, guided by post-Logical Empiricist philosophy of science focusing on the model of physics, must involve, as we put it immediately above, 'the stakeholders directly concerned', then the stakeholders directly concerned includes those who are *most* directly concerned, namely those who *use* services as well as those who (in whatever role) provide them. Patients, therefore, their families and communities, as well as researchers, practitioners, managers, policy makers, and the executive, should be involved in the development of an *Extended Family of Classifications* if this is to be firmly based on rigorous scientific principles.

Subject to the rules of science

If there is one 'big issue' on the Agenda for an *Extended Family of Classifications*, this is it. As just noted, however, the point about process, fundamental as it is, will be considered by many, including many of those most deeply involved in bringing psychiatric classification to its present stage of development, to be ill-conceived: Robert Spitzer, for example, who, as Chair of the DSM-III Task Force, introduced clear inclusion and exclusion criteria for the first time, considered the involvement of patients and family members in the DSM-V process 'political correctness gone mad' (Spitzer, 2005, in reply to Fulford *et al.*, 2005). Perhaps Spitzer is right. But it is important to be clear that as a point about process, this is no 'post-modern' appeal to a policy of anything and everything goes, but an argument in *scientific validity*. This is because any claim to membership of the *Extended Family of Classifications*, to the extent that it lays claim to being a *scientific* classification, must conform to the rules of science. Post-Logical Empiricist philosophy of science, guided by the model of physics, has shown the rules of science to be more complicated and less capable of formal codification than had previously been realized. Observation, in particular, as we saw in Chapter 12, has turned out to be conceptually structured; the extent, similarly, to which key terms both of observation and theory can be 'operationalized', as Hempel, no less, reminded us in his 1959 lecture, has turned out to be subject to in principle limits; and, as we will consider further in Chapter 16, there is a good deal more to progress in science than observational tests of theory alone. But there *are* rules, none the less. And none of the complications to these rules identified in post-Logical Empiricist philosophy of science, justifies a return to the obscurantism of what the Italian philosopher and historian of science, Paolo Rossi, has called the 'magical thinking' from which Renaissance physics had to work so hard to escape (Rossi, 2003). Post-Logical Empiricist philosophy of science has indeed shown that there is more to the difference between science and magic than had previously been appreciated. But that there is more to science than had previously been appreciated is, as Rossi argued, a reason for building on, not for reversing, the achievements of Renaissance physics.

It is from the achievements of Renaissance physics, then, that the argument for involving all stakeholders on an equal basis in the future development of psychiatric science, at least to the extent that this is an argument in scientific validity, derives. It will be worth setting out the argument in a little more detail as it is from this shift in process, a shift from the authority of this or that group of experts to the shared authority of *all* stakeholders, that the added value of an *Extended Family of Classifications*, guided by the insights of post-Logical Empiricist philosophy of science, is most directly derived. The argument, as we will see, can be thought of as embodying two key rules of scientific process as exemplified by the achievements of Renaissance physics.

Rule 1: challenging received authority

The argument, then, starts from the achievements of Renaissance physics and the fact that these were two-sided, or bivalent, achievements. One side of the achievements of Renaissance physics, the side that is best remembered today, was to establish the precedence of observation in the development of theories about what the world is really like. It is to this side of the achievements of Renaissance science that we remain true when we hang on firmly, despite all the difficulties, to the principles of observational science (including the need for reliable observations). But the other side to the achievements of Renaissance science, the side

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 373

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that is less well remembered today, was the overthrow of received authority. It is this second side of the achievements of Renaissance science that, transposed from the *particles* with which Renaissance physics was concerned to the *real people* with whom present day psychiatric science is concerned, generates the argument in validity for the involvement of all stakeholders on an equal basis in psychiatric science. The 'authorities' in the two cases are different, of course. For Renaissance science, the received authority was the authority of the established religion. In present-day psychiatry, the received authority is the authority of the established scientific disciplines, biological, psychological, and social. But there is a received authority in both cases. In both cases, similarly, received authority is justified, not by scientific validity, but by the need for 'good order': the good moral ordering of society by established religion in the Renaissance; the good medical ordering of policy and clinical decision making in current health-care practice. And in both cases the implications for *observational* science are the same, namely that science, observational science, succeeds in giving us a valid picture of the world by setting aside received authority and attending directly to what the world—whether a world made up of particles or of people—has to tell us.

Rule 2: listening to what the world (of particles or of people) has to say

Again, it is important to be clear here—this argument, to the extent that it is an argument in scientific validity, is no post-modern *laizzez-faire* appeal to a policy of anything and everything goes. There is no warrant here for everyone doing their own brain surgery or their own brain imaging! Expertise in the natural sciences, expertise on what the world—of particles, or livers and lungs, or indeed brains—has to tell us, expertise gained by listening carefully to what the world has to say, and expertise that includes the skilled use of instruments and the skilled interpretation of what those instruments reveal, expertise in all these areas is indeed genuine expertise (though as we will see in Chapter 16, there is rather more influence of received authority even in the sciences of particles than is generally acknowledged!). In these sciences, then, the natural sciences, a valid picture of the world is one that, in Crispin Wright's (1992) evocative phrase (from Session 2), is 'commanded by' the way the world really is. But transpose Crispin Wright's concept of cognitive command from particles, as the proper concern of physics, to people, as the proper concern of psychiatric science, and the corresponding scientifically valid way of seeing the world is one that is 'commanded' by the way *people* really are; and the expertise required to see the way people really are, is, or at the very least must centrally include, people themselves, *real* people, as patients, families, and communities, with direct experience of mental distress and disorder; people, then, as *experts*, not by professional or scientific training, indispensable as such experts are, but, to borrow a phrase from one of the UK government's current policy initiatives outlined later, 'experts by experience' (see Resources, below).

Agenda Item 10. Utility (of New Classification)

There are of course many other arguments, besides the above argument in scientific validity, for involving all stakeholders on an equal basis in the development of psychiatric science. There are arguments in equity, for example (Sadler and Fulford, 2004). Perhaps the most direct argument, though, is the observation that, to date, in failing to involve all stakeholders, in failing to listen carefully enough to the people with whom psychiatric science is most directly concerned, psychiatry has so regularly got things, from the perspective of its patients, so disastrously wrong.

We discussed the gap between patient and professional in psychiatry in Part I as a gap of models: the radical antipsychiatry movement, of which there is no corresponding counterpart in, say, cardiology, was motivated by a particularly radical gap in models. We consider the gap between patients and professionals in psychiatry again in Part IV in terms of a gap of values. The counterpart of these gaps here, in psychiatric science, is a communication gap arising from a gap in face validity—a gap between (many) patients and professionals in, to return to Sartorius' metaphor, their respective ways of seeing the world.

Improved face validity

It is not difficult to see how a communication gap between patients and professionals might be opened up in a psychiatric science that is guided by the 'one right way of seeing the world' assumption of the traditional model. Lewis, you will recall, in arguing at the 1959 conference for a symptom-based classification, stressed the need for improved communication for purposes of epidemiological research and statistical reporting. And it is gains in communication that, right through to the *Agenda for DSM-V*, have been perceived as the principal return from the enhanced reliability of modern psychiatric classifications. However, the communication in question has been, from Lewis right through to DSM-V, between professionals, and indeed mainly between doctors, not between doctors and other professional groups, still less between professionals and patients.

And improved construct validity

The need, then, corresponding directly with the need identified in Part I in terms of models, is not to abandon this or that way of seeing the world of mental disorder. The need, rather, is for a range of different ways of seeing the world through which different professional groups, representing expertise in different scientific ways of seeing the world of mental distress and disorder, are able to match their expertise appropriately to the equally diverse ways of seeing the world of mental distress and disorder represented by individual patients. Improved communication is of course of considerable value in its own right clinically. Scientifically though, improvements in face validity arising from improvements in communication between all stakeholders, could turn out to be a crucial further step (a crucial further step on from reliability) to the ultimate goal of improved construct (or theoretical) validity.

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 374

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Agenda Item 11. Overall Tenor

It will be clear by now that, if Lewis in the *ICD-9 Agenda* is generally upbeat, and if Kupfer *et al*. in the *DSM-V Agenda* are somewhat downbeat, the *Agenda for an Extended Family of Classifications* will be definitely upbeat, though, as we will see, it is upbeat with strings.

Four upbeat lessons

The upbeat lessons drawn above from post-Logical Empiricist philosophy of science, guided primarily by the example of the physical sciences, are that: (1) psychiatric science is a difficult not defective science; (2) as such it demands the best of conceptual as well of empirical methods; and (3) psychiatry's models of validity, its ways of seeing the world, will be enriched by the full engagement of all stakeholders in the further development of psychiatric science. These three lessons give us reason enough to be upbeat. And we can add to them the further lesson from Part I, that the difficulties faced by psychiatric science today, arising as they do from psychiatry's irreducible engagement with people (rather than with the parts of people, their hearts, lungs, brains, etc.), will be the difficulties faced by the rest of medical science tomorrow.

Based on: (1) observational methods

The upbeat tenor of the Agenda for an *Extended Family of Classifications*, however, as noted above, comes with strings. One string is the point to which we have returned several times in this session, the need to remain true to the principles of observational science, albeit that these principles stand to be developed and deepened if they are to be applied in a valid as well as reliable way in a difficult science like psychiatric science.

(2) Challenge to received authority

A second string is the counterpart of the first, namely that hand-in-hand with the principles of observational science, as established in Renaissance physics, goes the overthrow of received authority. It is fundamental to the successful development of a fully functional *Extended Family of Classifications* that there should be no dominant way of seeing the world to which all other ways of seeing the world must be subsumed. In recent history, it has been the 'medical' model, a model based on the models of cardiology and other sciences of bodily medicine, that has been dominant in psychiatry. Before that, at least in the USA, it was psychoanalysis. The modern rise in authority of a variety of other models (noted in Part I), a variety entirely to be welcomed to the extent that it provides a balanced heterogeneity of models corresponding with the diversity of models represented by individual patients and carers, carries the risk of collapse into one or another new hegemony. This risk, if history is any guide, is all too real. The antipsychiatry movement was motivated by the myth of mental illness. However, as the German historian and psychiatrist, Paul Hoff, has eloquently put it, throughout its history psychiatry has shown itself to be all too capable of replacing one myth with another (Hoff, 2005).

(3) Enough time

A third string is the need for a long view. It is natural, faced with the reality of mental distress and disorder, to look for immediate returns on research. There are, as we noted above, quick wins to be had from opening up the additional intellectual space of a strengths model of psychiatric science created for us by post-Logical Empiricist philosophy of science. But the substantive development of theory in this most difficult of sciences is bound to take time. The research leading to improved reliability took decades, not years. We should not expect improvements in validity in an any less generous time scale. The new neurosciences, certainly, just in *being* new, promise un-looked for advances. Combined with the new mental sciences outlined in Part I, and reviewed briefly below (under 'Resources'), the currently hoped-for advances in the neurosciences may well result in improvements in validity, in better ways of seeing the world of mental distress and disorder; better as judged by those most directly concerned, patients, their families, and communities. These are exciting times, then, times that indeed hold out tantalising glimpses of Kupfer *et al*.'s 'as yet unknown paradigm shift' (p. xix). It may be that the shift will happen overnight. These things after all are by definition unpredictable. But we should not be surprised, or disappointed, if the looked-for paradigm shift takes a little longer.

Agenda Item 12. Any Other Business

Under 'Any Other Business' in the Agenda for an *Extended Family of Classifications*, are 'resources' and 'next steps'.

Resources

Much of our third agenda, our Agenda for an *Extended Family of Classifications*, like its predecessors in ICD and DSM, has been taken up with the tough challenges of psychiatric classification. And the challenges are tough indeed reflecting as they do the combined empirical and conceptual challenges of psychiatric science. But the 1990s, as we indicate in Chapter 29 of this book, and as Fulford et al., 2003, describe in detail, witnessed an explosion of new resources for meeting these challenges. We summarize these resources here under four broad categories: (1) the new neurosciences; (2) the new mental sciences; (3) new national policy initiatives; and (4) new international networks.

1. *New neurosciences*. The resources of the new neurosciences—behavioural genetics, psychopharmacology, and brain imaging—are one of the keys to the development of more effective mental health services in the twenty-first century. Yet science, as we saw particularly in Part I, despite its many dramatic successes in medicine, has had an increasingly bad press recently. One clear task for the new philosophy of psychiatry, therefore, developing as it has hand-in-hand with the new neurosciences, is to help provide a framework for a synergistic rather than mutually destructive relationship between researchers, practitioners, and (as the ultimate stakeholders in mental health) patients, carers, and others who use mental health and related

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12-Fulford-Chap13.qxd 08/06/2006 10:52 AM Page 375

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social care services. Without such a synergistic relationship, the danger is that the resources of the new neurosciences are as likely to compound as to relieve the problems of mental distress and disorder.

2. *New mental sciences*. A key resource from the new philosophy of psychiatry for the development of a synergistic relationship between the neurosciences and its stakeholders, is the wide range of methods now available for the rigorous study of subjectivity, of the meanings and personal significance of individual experience. Many of these methods, as we noted in Part I, besides being potential research partners to the new neurosciences, are already paying dividends clinically: discursive, hermeneutic, and phenomenological methods are all cases in point (for details see Reading Guides, particularly to Chapters 3, 4, 9 and 10; also the work on values in Part IV). The hope must be, therefore, that the new mental sciences, as they might be called, will continue to grow alongside and in partnership with the new neurosciences, as a resource for mental health care.

   Specifically in relation to classification, the proposals by Juan Mezzich, the Peruvian Professor of Psychiatry at the Mount Sinai Hospitals, New York, for an 'idiographic classification' (Mezzich, 2002; also Mezzich *et al*., 1996, and Mezzich *et al*., 2003) complement the existing WHO Family of Classifications, establishing the basis for a more comprehensive approach to psychiatric assessment. Mezzich has wide experience of international classification, having been a member of the workgroups developing DSM-III, DSM-IV and ICD-10, chair for many years of the WPA classification section, and currently chair of the WPA-WHO workgroup on International Classification and Diagnostic systems. As WPA president from September 2005, it is therfore particularly significant that his proposals already include not only key elements both of the subjective aspects of mental disorder that are the focus of the new mental sciences, but also of the expanded policy and practice initiatives currently emerging in a number of countries around the world.

3. *New national policies*. Policy and practice initiatives, although not important in a traditional model of psychiatric science, are a crucial resource for the development of an *Extended Family of Classifications*, guided by a post-Logical Empiricist philosophy of physics, but concerned with people rather than particles.

   It is important to be aware just how fast and far things have already moved. Thus, mental health policy in a number of countries has recently been expanded to embrace new initiatives in such areas as health promotion, psychosocial interventions, and recovery practice. In the UK, national policy is explicitly user-centred and multidisciplinary (Department of Health, 1999) and the work of the UK government's mental health policy implementation body, the National Institute for Mental Health in England (NIMHE), includes: (a) work streams in such areas as recovery, social inclusion, black and minority ethnic services, spirituality, values-based practice (to which we return in detail in Part IV), and (b) crucially for classification (Agenda Item 9, above), 'experts by experience', a programme aimed at developing leading roles for patients and informal carers in the development and implementation of services and in many areas of research.

4. *New international networks*. Complementing these national developments has been the emergence and strengthening of new international networks. The importance of international collaboration in the development of psychiatric classifications has already been noted: Aubrey Lewis, you will recall from Reading 13.8, made a point of commenting on the fundamental importance of the strongly 'collegial' international effort that led to ICD-8. Improvements in international travel, and the development of the World Wide Web, provide new opportunities for building on the success of such initiatives. The philosophy of psychiatry, notably, has developed through the work of a number of national and subject-based groups around the world. The recently launched *International Network for Philosophy and Psychiatry* (the INPP) will provide crucial communication and co-ordination support for these groups.

   Among established international organizations, the World Psychiatric Association (WPA), with its energetic regional groups and subject-based sections, provides a strong international basis for the development of an *Extended Family of Classifications*, incorporating, but also adding to, the 'families', respectively, of the ICD and DSM classifications. The WPA has already established, under the chairmanship of Juan Mezzich, a Conceptual Workgroup for Classification to spearhead work on classification. Supported as the Workgroup is by the WPA's sections respectively for Classification, for Psychopathology, and for Philosophy and the Humanities, and including as it does in its membership neuroscientists, philosophers and 'experts by experience' from many countries around the world, the WPA's Conceptual Workgroup is an exemplary example of the resources already available for building on the successes of psychiatric classification in the twentieth century to produce an *Extended Family of Classifications* as the basis for a twenty-first century science-based but also user-centred psychiatry.

Next steps

As we 'go to press', a number of small but potentially significant next steps towards the development of an *Extended Family of Classifications* have already been taken. In addition to the work just noted of international organizations, such as the WPA, there have been already three conferences specifically on new approaches to psychiatric classification.

1. *The Dallas Conference*. As early as 1997, John Sadler, Professor of Psychiatry and Director of Medical Education at UT Southwestern Medical Centre, convened an international

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conference in Dallas on philosophical aspects of psychiatric diagnosis. Besides philosophers, patients, psychiatrists, and neuroscientists, the conference included key culture carriers from the successes of twentieth century psychiatric classification, notably the Chair of the DSM-III Task Force, Robert Spitzer. (The proceedings of this conference are published in Sadler, 2002).

2. *The NIMHE conferences*. In the UK, the NIMHE (see point 3, above), although a policy implementation rather than research-funding body, has sponsored two international research meetings, one in London in 2003, the other in Windsor in 2004. Although concerned primarily with the role of values in psychiatric diagnosis (to which we turn in Part IV), the rationale for NIMHE's support for these meetings was the need for an expanded basis for psychiatric classification consistent with the expanded policy objectives for mental health adopted in the UK (noted under point 3 of 'Resources' above).

   The network of researchers, policy makers, and stakeholders established through these two meetings, and with their direct links into policy and practice in the UK, will thus be well placed to contribute to the work of the Conceptual Workgroup for Classification established by Juan Mezzich in the WPA.

   As with the Dallas conference, the two NIMHE meetings included, in addition to researchers from both empirical and phenomenological traditions, representatives of all stakeholders in mental health (users of services, clinicians, and researchers) as well as leading figures in the development of twentieth century approaches to psychiatric diagnosis and classification (Norman Sartorius and Julian Leff, for example), and, in this instance, key figures from the WPA, including Juan Lopez-Ibor, Ahmed Okasha and, specifically concerned with the new classifications, Juan Mezzich and the Secretary of the WPA's Classification Section, Claudio Banzato.

3. *The WPA Conceptual Workgroup on Classification*. As noted above under 'Resources' (point 4), the establishment by the WPA of a Conceptual Workgroup for Classification, supported by the WPA's strong structure of regional and subject-based groups, could prove to be a key step towards the development and implementation of an *Extended Family of Classifications* for international use in mental health. A series of meetings of the network has already been planned in different countries around the world within a framework for collaborative action between the WPA, the WHO (on ICD) and the DSM-V Taskforce.

So, where will these 'next steps' lead? This will depend, among other key factors, on the scientific research programmes that develop within psychiatry over coming decades. A scientific classification, after all, must reflect as well as help to shape, the science to which it relates. Where these next steps will lead will also depend on the outcome of the interplay of social and political forces represented by the disparate models of mental health that we explored in Part I. The traditional model of science would have it that these two kinds of factor—the research programmes of science and social and political factors—are unconnected. As we will see in Chapter 16, however, the connections between them are, to the contrary, crucial. It is to the connections between scientific and social and political factors, therefore, in the future developments of psychiatric classification, and to these connections as identified in this chapter in a post-Logical Empiricist model of science guided by the example of physics, that we now turn in a brief concluding session.

Reflection on the session and self-test questions

In this session we developed an Agenda for an *Extended Family of Classifications* drawing on the philosophy of science as represented particularly by the history and philosophy of physics. Write down your own reflections on the materials in this session drawing out any points that are particularly significant for you. Then write brief notes about the following:

1. What 'big issue' did our Agenda for an *Extended Family of Classifications* identity as important in the agenda for ICD-9? Why was this important also for the 'big issue' on the agenda for DSM-V?
2. What class of issue did we name in our new agenda that is both explicit and *named as such* in the agenda for ICD-9, but explicit and *not* named as such in the agenda for DSM-V?
3. What is the 'big issue' on our Agenda for an *Extended Family of Classifications*?
4. In our Extended Family of Classifications: (a) What is the assumed status of psychiatric science relative to the sciences in other areas of medicine? (b) What new category of methods of investigation become available in addition to empirical methods? and (c) What is the role of patients and other users of services?
5. What role will patients and other users of services have in our new agenda as an agenda specifically for a future *scientific* classification of mental disorder?

Conclusions: being about what science is about

'A classification', as Norman Sartorius (1992) reminded us epigrammatically in the quote from the Preface to ICD-10 at the start of this chapter, is 'a way of seeing the world at a point in time'. As we come to the end of the chapter it will be clear that the operative word in Sartorius' epigram is the indefinite article 'a'—'*a* classification is *a* way of seeing the world at *a* point in time'.

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12-Fulford-Chap13.qxd 08/06/2006 10:53 AM Page 377

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The assumption of the traditional model of science, an assumption derived from such paradigms as the Chemical Periodic Table, has been that, as science is about what the world is really like, a scientific classification should be, not *a* way, but *the* way of seeing the world, our currently best shot at seeing what the world is really like at *this* point in time.

From 'the' to 'a'

The shift from 'the' to 'a', from the definite to the indefinite, from one to many scientific ways of seeing the world at a point in time, starts, so far as the philosophy of science is concerned, with the failure of Logical Empiricism, and related broadly positivist programmes, to provide a formal codification of what science is about, an abstract test, external to the practice of this or that discipline, by which a uniquely scientific way of seeing the world might be distinguished from other ways of seeing the world that, although perhaps authoritative and important in other ways, are not consistent with the uniquely scientific way of seeing the world.

Logical Empiricism, as we saw in Chapter 12, focused on the role of observation in science. Genuinely scientific ways of seeing the world, in the paradigm of Renaissance physics, start from observation, from looking and really *seeing* what the world is like, and, thereby, differentiating between true (valid) and false (invalid) theories about what the world is like. Logical Empiricism, then, failed to the extent that it failed to find an unambiguous way of distinguishing observation statements from statements of theory. But this 'failure', the storyline of Chapter 12 suggests, was, in fact, a brilliant success. For it opened the way to a deeper understanding of the relationship between observation and theory in science, a recognition that, (1) observation itself is theory laden in the specific sense of being conceptually prestructured; hence that, (2) while observation—looking and really seeing—remains, as in the paradigm of Renaissance physics, the basis of differentiating between scientifically valid and invalid ways of seeing the world, none the less, (3) as observation itself is conceptually prestructured, it follows that the relationship between observation and theory is not linear but iterative, and, in consequence, (4) that there may be at any one time, and as there is at the present time in physics, more than one scientifically valid way of seeing what the world is really like.

From Logical Empiricist to post-Logical Empiricist science

In Chapter 12 we found evidence of the brilliant success of the failure of Logical Empiricism in the development, in the second half of the twentieth century, of reliable observational methods for psychiatric science. The PSE, we noted, and other 'semi-structured' schedules for identifying and recording mental states, provide vivid illustrations of the conceptual prestructuring of observation: recall Wing *et al.*'s (1974) account of the development of the PSE as an operational formalization of good clinical practice among clinicians working in a broadly European psychiatric tradition. In this chapter we have found further evidence of the brilliant success of the failure of Logical Empiricism in the development, also over the second half of the twentieth century, of psychiatry's two major international scientific classifications of mental disorder, the ICD and DSM, successive editions of which have built on Lewis' translation, in the 1959 conference, of Hempel's Logical Empiricist account of scientific classifications into the need for what he called 'public' and 'private' classifications for different scientific purposes. Lewis' translation of Hempel, we can now see, and the subsequent evolution of both ICD and DSM towards their respective families of classifications, and the further evolution from these families to the *Agenda for a Extended Family of Classifications* outlined in Session 4 of this chapter, are all direct parallels of the corresponding translation in post-Logical Empiricist philosophy of science, guided by the paradigm specifically of physics (as in Session 3 of this chapter), from the single valid scientific way of seeing the world assumed by the traditional model of science, into many valid scientific ways of seeing the world.

Cutting against the grain

The shift from 'the' to 'a' valid scientific way of seeing the world, and hence from one to an *Extended Family of Psychiatric Classifications*, will cut against the grain with those who have other and different agendas. It will cut against the grain, for example, with those whose personal, professional, financial, or political interests lie in promoting this or that particular way of understanding mental distress and disorder. It will cut against the grain, similarly, with those whose personal, professional, financial, or political interests lie in overthrowing this or that particular way of understanding mental distress and disorder. Corresponding with the 'full field' model to which we came in Part I, an *Extended Family of Classifications* will build on, not replace, established ways (including the established medical way) of understanding the world of mental distress and disorder.

The move from 'the' to 'a', however, the move from one to an *Extended Family of Psychiatric Classifications*, will cut most deeply against the grain with those who are convinced, honestly convinced, that, in broad outline at least, they already know what is *the* right way to understand the world of mental distress and disorder. The shift from 'the' to 'a', the honestly convinced may acknowledge, has been guided by the paradigm of Renaissance science as currently embodied in theoretical physics. The shift from 'the' to 'a', the honestly convinced may further acknowledge, is but one aspect of the discovery of radical uncertainty that is perhaps the unique intellectual heritage of the twentieth century—recall the EPR paradox in physics from Chapter 12, and, from Chapter 6, the still deeper uncertainties in mathematical logic identified by the Gödel undecidability theorems. The shift from 'the' to 'a', the honestly convinced may additionally acknowledge, provides no warrant for the relativization of scientific knowledge so often (and illegitimately) promoted under a banner of 'postmodernism'. The point, after all, the honestly convinced may continue, of recognizing (with Chapter 12) the conceptual

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prestructuring of observation, is, as Hesse (1980) argued, not to abandon the basic scientific discipline of observational testing of candidate explanatory theories, but to recognize that such 'testing' is effective only to the extent that it is carried out within a given range of candidate explanatory theories, candidate ways of seeing what the world is really like. The point, similarly, the honestly convinced may finally concede, of recognizing (with this chapter) the undetermination of theory by data, is, to extend Hesse's point, not that *any* theory may be determined by the data, but that the data serve, on the one hand to exclude from among a given range of candidate explanatory theories those that are false (invalid), i.e. because they conflict with and hence are not determined by the data at all, while, on the other hand, and at the same time, leaving wide open the range of possible future explanatory theories that may be *true*.

Back to Renaissance science

All these points, then, about the shift from 'the' to 'a' way of seeing the world scientifically, the shift from one to an *Extended Family of Psychiatric Classifications*, those who are honestly convinced they already know in broad outline what the world is really like, may acknowledge. And yet, the honestly convinced may go on to say, for *practical* purpose, for the purposes of good order in a practical discipline such as psychiatry, there is a need, if not for one way of seeing the world, at least for a *dominant* way. In physics, particularly theoretical physics, those who are honestly convinced may point out, theoreticians have the luxury of being able to 'play' with different ways of seeing the world. This 'play' may, indeed, in the long run prove to be highly productive scientifically, i.e., in giving us new and powerful ways of seeing the world. But where physics is for particles, psychiatry, as this chapter after all has repeatedly emphasized, is for people. And the practical contingencies of a discipline that is concerned not with particles but with people require, the honestly convinced may suggest, not play, however productive in the long run, but a clear line of authority to ensure good order in the development and delivery of services as the basis of effective day-to-day clinical care. Granted, therefore, the honestly convinced will conclude, that we (we the honestly convinced) already have, if not *the* scientific way of seeing the world, at any rate our current best shot at *the* scientific way of seeing the world, then the requirement for good order in clinical care leads directly to the need for a clear line of authority derived from our (the honestly convinced's) way of seeing the world…

…which is precisely the point at which, as we saw in Session 4 of this chapter, with the successful *overthrow* of a received authority justified by the honestly convinced in the name of good order, modern observational science, in the form of Renaissance physics, first came in. Building, therefore, an *Extended Family of Psychiatric Classifications,* a family of equal members, and a family in which membership is secured not by way of received authority but by way of the disciplines of rigorous observation-based ways of seeing the world, will be nothing if not all about what science is about.

Reading guide

The absolute conception

- The absolute conception is introduced by Williams in both *Descartes: The Project of Pure Inquiry* (1978), and *Ethics and the Limits of Philosophy* (1985).
- It is discussed by the philosopher Hilary Putnam (1995) in *Renewing Philosophy*, and by McDowell (1998) in *Mind, Value, and Reality*.

Realism in the philosophy of science

- A good introduction is Bird (1998) *The Philosophy of Science* (chapter 4).
- The definitive collection on scientific realism is Leplin (ed.) (1984) *Scientific Realism*, but it is also discussed in Papineau (1987) *Reality and Representation* and Laudan (1977) *Progress and its Problems*.
- Realism based on the argument from explanation is the main subject of Lipton (1991) *Inference to the Best Explanation*.
- Cartwright's work is further developed in *The Dappled World* (2000).

A valuable collection of articles on causal explanations in healthcare in the Swedish philosophers, Ingmar Lindahl and Lenart Nordenfelt's (1984) edited collection in the *Philosophy and Medicine* book series.

Natural kinds

One debate that has not been touched on in this section concerns whether disease is a natural kind. Some of the concerns of that debate should now be clear. The concept of disease groups together diverse conditions, but does this reflect fundamental and underlying similarities or is it instead that these have in common something that reflects our local interests in what affects us. See for example.

- Reznek (1987) *The Nature of Disease* (chapters 1–4);
- Reznek (1995) Dis-ease about kinds: reply to D'Amico (*Journal of Medicine and Philosophy*); and
- D'Amico (1995) Is disease a natural kind (*Journal of Medicine and Philosophy*).

You should now be in a position to assess the underlying argument here.

Psychiatric classification and diagnosis

Introductions to current systems of psychiatric classification and to diagnosis are included in all the major textbooks of psychiatry (noted in the Reading Guide to Chapter 2). As detailed in the chapter, the current version of the WHO's

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classification of mental disorders, is chapter 8 of the 10th edition of the *International Classification of Disease* (WHO, 1992). The WHO website (http://www.who.int/en) gives details of its family of classifications, including the ICF, the *International Classification of Functioning, Disability and Health* (2001). The American Psychiatric Association's current classification is a revised version of its DSM, the fourth (revised, 2000) edition of the *Diagnostic and Statistical Manual* (DSM-IV-TR—'TR' for text revision). The DSM is complemented by a valuable range of additional publications to support research and teaching—see the DSM-IV-TR Library section in the American Psychiatric Association website. As noted in the chapter, both ICD and DSM classifications are currently undergoing revision with a view to publication of new editions around 2010. Cultural issues are reviewed by Kleinman (1996) and by Mezzich *et al.* (1996), and the case for personalized 'idiographic' classifications in Mezzich (2002) and Mezzich *et al.* (2003).

Psychiatric classification remains contentious and the issues discussed in this chapter have generated a huge literature. A recent authoritative analysis, including a novel approach to the nature of validity is the American philosopher and psychiatrist, Kenneth Schaffner's (1993) *Discovery and Explanation in Biology and Medicine*. See also his (1986) edited collection *Logic of Discovery and Diagnosis in Medicine*. As noted in the chapter, Kendell and Jablensky (2003) examine validity and utility from a psychiatric perspective. Cooper (2003) provides a critical review of the Research Agenda for DSM-V.

Sadler *et al.*'s (1994) *Philosophical Perspectives on Psychiatric Diagnostic Classification* is an edited collection including chapters on many of the key conceptual issues. Sadler's (2002) more recent edited collection, *Descriptions and Prescriptions*, provides a further valuable resource with a focus particularly on the role of values (see Part IV, especially Chapters 20 and 21). Regular reviews and updates are published in *Current Opinion in Psychiatry*, both in the *History and Philosophy* section, and in specific clinical topic areas. Jennifer Radden (1994) reviewed key philosophical issues in *Philosophy, Psychiatry, & Psychology*. The opening chapters of John Sadler's (2004a) *Values and Psychiatric Diagnosis*, give a comprehensive and very readable update, including a detailed discussion of the issue of validity. Kutchins and Kirk (1997) present the case against psychiatric diagnosis. Sadler's (2004b) 'Diagnosis/ antidiagnosis' provides a succinct overview of current debates.

A classic but still invaluable source for both historical and conceptual issues in classification is R.E. Kendell's (1975) *The Role of Diagnosis in Psychiatry*. This book was written at a time when the debate between psychiatry and antipsychiatry was at its most polarized (1975 was also the year of Kendell's reply to Szasz that we looked at in detail in Part I). It includes discussions of the 'pros and cons' of diagnosis (chapter 1), of the relationship between diagnosis and concepts of disease (chapters 2 and 5), of issues of reliability and validity (chapter 3) and of practical utility (chapter 4), and of quantitative methods (chapter 8), including a forward-looking discussion of the role of computers (chapter 11), of the choice between categories and dimensions (chapter 9), and of the problems generally of defining diagnostic criteria (chapter 10). Specifically on the history of current classifications, Kendell's two chapters on 'International differences in diagnostic criteria' (chapter 6) and on 'The international classification' (chapter 7), provide a wealth of detail. John Cooper (1999) describes the development of the WHO programme of classifications. For further details of the WHO programme of classification, see Sartorius, 1976, 1991 and 1995, and Kramer *et al.*, 1979.

Kendell's book (especially chapter 3) also draws together in an easily accessible form details of the many early studies of psychiatric diagnosis that have been important in shaping current classifications, including:

1. *Wake-up calls* of the need for a more rigorous approach, for example: (a) Temerlin's (1968) experiment, in which an audio-tape of an actor talking normally was consistently interpreted as 'psychotic' by an audience of psychiatrists and psychologists primed to expect pathology, and (b) Rosenhan's (1973) experiment, in which a number of social scientists, having had themselves admitted as patients to psychiatric hospitals, found that everything they did (including keeping notes of their experiences) was interpreted by staff as symptoms of illness.
2. *Studies of reliability*: for example (a) the *Philadelphia Study* (Beck *et al.*, 1962), an early American study of reliability that established a rigorous methodology for quantifying various sources of diagnostic disagreement (differences of interview technique, different weightings of responses, loose diagnostic criteria, etc.), and (b) the *Chichester Study* (Kreitman *et al.*, 1961), a similar study in the UK. Early versions of the PSE (see chapter 12) were also important in establishing the reliability of individual symptoms (e.g. Wing *et al.*, 1967).
3. *International collaborative research*, for example: (a) the *US-UK Diagnostic Project* (Cooper *et al.*, 1972), a collaborative study of differences in rates of diagnosis between Britain and America. These had been recognized previously (see Kramer, 1969; Kramer et al., 1979) but this collaborative research design allowed the differences in rates of diagnosis of psychiatric disorders to be fully mapped out for the first time using structured interview methods; (b) the *International Pilot Study of Schizophrenia* (or IPSS, WHO, 1973), which extended the US–UK project internationally (see also Part IV).

References

Alarcón, R.D., Bell, C.C., Kirmayer, L.J., Lin, K-M., Üstün, B., and Wisner, K.L. (2002). Beyond the funhouse mirrors: research

OUP Copyright

12-Fulford-Chap13.qxd 08/06/2006 10:53 AM Page 380

OUP Copyright

agenda on culture and psychiatric diagnosis. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 219–282.

American Psychiatric Association (1952). *Diagnostic and Statistical Manual of Mental Disorders* (1st edn, DSM-I). Washington, DC: American Psychiatric Association.

American Psychiatric Association (1968). *Diagnostic and Statistical Manual of Mental Disorders* (2nd edn, DSM-II). Washington, DC: American Psychiatric Association.

American Psychiatric Association (1980). *Diagnostic and Statistical Manual of Mental Disorders* (3rd edn, DSM-III). Washington, DC: American Psychiatric Association.

American Psychiatric Association (1994). *Diagnostic and Statistical Manual of Mental Disorders* (4th edn). Washington, DC: American Psychiatric Association.

American Psychiatric Association (2000). *Diagnostic and Statistical Manual of Mental Disorders*, Fourth Edition, Text Revision. Washington, DC: American Psychiatric Association.

Anastasi, A. (1968). Psychological Testing, 3rd ed. New York: The Macmillan Company.

Beck, A.T., Ward, C., Mendelson, M., Mock, J., and Erbaugh, J. (1961). An inventory for measuring depression. *Archives of General Psychiatry*, 4: 561–571.

Bird, A. (1998). *The Philosophy of Science*. London: UCL.

Boyd, R. (1999). On the current status of scientific realism. In *The Philosophy of Science* (ed. R. Boyd, P. Gaspar, and J.D. Trout). Cambridge, MA: MIT Press, pp. 195–222.

Bridgman, P.W. (1927). *The Logic of Modern Physics*. New York: Macmillan Press.

Cartwright, N. (1983). *How the Laws of Physics Lie*. Oxford: Oxford University Press.

Cartwright, N. (1999). The reality of causes in a world of instrumental laws. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 379–386.

Cartwright, N. (2000). *The Dappled World*. Cambridge: Cambridge University Press.

Charney, D.S., Barlow, D.H., Botteron, K., Cohen, J.D., Goldman, D., Gur, R.E., Lin, K-M, López, J.F., Meador-Woodruff, J.H., Moldin, S.O., Nestler, E.J., Watson, S.J., and Zalcman, S.J. (2002). Neuroscience Research Agenda to Guide Development of a Pathophysiologically Based Classification System. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 31–84.

Colombo, A., Bendelow, G., Fulford, K.W.M., and Williams, S. (2003). Evaluating the influence of implicit models of mental disorder on processes of shared decision making within community-based multi-disciplinary teams. *Social Science & Medicine*, 56: 1557–1570.

Cooper, J.E., Kendell, R.E., Gurland, B.J., Sharpe, L., Copeland, J.R.M. and Simon, R. (1972). *Psychiatric diagnosis in New York and London*. Maudsley Monograph Series No. 20. London: Oxford University Press.

Cooper, J. (1999). Chapter 2 in *Promoting Mental Health Internationally*, edited by Girolamo et al. London: The Royal College of Psychiatrists, Gaskell Press.

Cooper, J. (2003) Editorial: Prospects for Chapter V of ICD-11 and DSM-V. *British Journal of Psychiatry*, vol 183: 379–381.

D'Amico (1995). Is disease a natural kind. *Journal of Medicine and Philosophy*, 20: 551–569.

Dancy, J. (1993). *Moral Reasons*. Oxford: Blackwell Publishers.

Department of Health (1999). *National Service Framework for Mental Health—Modern Standards and Service Models*. London: Department of Health.

d'Espagnat, B. (1976). *Conceptual Foundations of Quantum Mechanics*, (2nd edn). W.A. Benjamin Inc.

Discussion, Various Contributors (1961). In *Field Studies in the Mental Disorders* (ed. J. Zubin). New York, Grune and Stratton, pp. 23–50.

Duhem, P. (1985). *To Save the Phenomena: an essay on the idea of physical theory from Plato to Galileo*. Chicago: University of Chicago Press.

Evans, G. (1980). Things without the mind. In *Philosophical subjects: essays presented to P.F. Strawson* (ed. Z. van Straaten). Oxford: Clarendon Press.

Fine, A. (1984). 'And not anti-realism either'. *Nous* 19, 51–65. (reproduced in *Scientific Knowledge: basic issues in the philosophy of science* ed J.A. Kournay (1998). Belmont, California: Wadsworth Publishing Company, pp. 359–368.)

Fine, A. (1999). The natural ontological attitude. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 261–277.

First, M.B., Bell, C.C., Cuthbert, B., Krystal, J.H., Malison, R., and Offord, D.R. (2002). Personality disorders and relational disorders: a research agenda for addressing crucial gaps in DSM. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 123–200.

Fulford, K. W. M., Morris, K. J., Sadler, J. Z., and Stanghellini, G. (2003). Past improbable, future possible: the renaissance in philosophy and psychiatry. In *Nature and Narrative: an introduction to the new philosophy of psychiatry* (ed. K.W.M. Fulford, K.J. Morris, J.Z. Sadler, and G. Stanghellini). Oxford: Oxford University Press, pp. 1–41.

OUP Copyright

12-Fulford-Chap13.qxd 08/06/2006 10:53 AM Page 381

OUP Copyright

Fulford, K.W.M., Broome, M., Stanghellini, G., and Thornton, T. (2005). Looking With Both Eyes Open: Fact *and* Value in Psychiatric Diagnosis? *World Psychiatry*, 4: 2,78–86.

General Register Office (1968). *A Glossary of Mental Disorders. Studies on Medical and Population Subjects*. No. 22, London: HMSO.

Hacker, P.M.S. (1987). *Appearance and Reality*. Oxford: Blackwell.

Hacking, I. (1983). *Representing and Intervening*. Cambridge: Cambridge University Press.

Hacking, I. (1999). Experimentation and scientific realism. In *The Philosophy of Science* (ed. R. Boyd, P. Gaspar, and J.D. Trout). Cambridge, MA: MIT Press, pp. 247–260.

Hempel, C.G. (1961). Introduction to problems of taxonomy. In *Field Studies in the Mental Disorders*. New York: Grune and Stratton, pp. 3–22. (Reproduced in Sadler, J.Z., Wiggins, O.P., and Schwartz, M.A. (1994). *Philosophical Perspectives on Psychiatric Diagnostic Classification*. Baltimore, MD: The Johns Hopkins University Press, pp. 315–331.)

Hempel, C.G. (1994). Fundamentals of taxonomy. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press.

Hesse, M. (1980). *Revolutions and Reconstructions in the Philosophy of Science*. Harvester.

Hobson, R.P., Patrick, M.P.H., and Valentine, J.D. (1998). Objectivity in psychoanalytic judgements. *British Journal of Psychiatry*, 173: 172–177.

Hoff, P. (2005). Die psychopathologische Perspektive. In: Bormuth M, Wiesing U (eds) *Ethische Aspekte der Forschung in Psychiatrie und Psychotherapie*. Deutscher Aerzte-Verlag, Cologne. Pp:71-79

Hyman, S.E. and Fenton, W.D. (2003). What are the right targets for psychopharmacology? *Science*, 299: 350–351.

Kendell, R.E. (1975). *The Role of Diagnosis in Psychiatry*. Oxford: Blackwell Scientific Publications.

Kendell, R.E. and Jablensky, A. (2003). Distinguishing between the validity and utility of psychiatric diagnoses. *American Journal of Psychiatry*, 160: 4–12.

Kleinman, A. (1996). How is culture important for DSM-IV? In *Culture and Psychiatric Diagnosis: a DSM-IV perspective* (ed. J.E. Mezzich, A. Kleinman, H. Fabrega, and D.L. Parron). Washington: American Psychiatric Press Inc., Chapter 2.

Kramer, M. (1969). Cross-National Study of Diagnosis of the Mental Disorders: origin of the problem. *American Journal of Psychiatry*, 125 (suppl.), 1–11.

Kramer, M. Sartorius, N. Jablensky, A., Gulbinat, W. (1979). The ICD-9 Classification of Mental Disorders: A Review of its Development and Contents. *Acta Psychiatrica Scandinavica,* 59: 241–262.

Kreitman, N., Sainsbury, P., Morrissey, J., Towers, J., and Scrivener, J. (1961). The reliability of psychiatric assessment: an analysis. *Journal of Mental Science*, 107: 887–908.

Kupfer, D.J., First, M.B., and Regier, D.E. (2002). *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association.

Kutchins, H. and Kirk, S.A. (1997). *Making Us Crazy: DSM—the psychiatric bible and the creation of mental disorder*. London: Constable.

Latour, B. (1987). *Science in Action: how to follow scientists and engineers through society*. Cambridge, MA: Harvard University Press.

Laudan, L. (1977). *Progress and its Problems*. Berkeley, CA: University of California.

Lehman. A.F., Alexopoulos, G.S., Goldman, H., Jeste, D., and Üstün, B. (2002). Mental disorders and disability: time to reevaluate the relationship? In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 201–218.

Leplin, J. (ed.) (1984). *Scientific Realism*. Berkeley, CA: University of California.

Lewis, A.J. (1934). On the psychopathology of insight. *British Journal of Medicine and Psychology*, 14: 332–348.

Lindahl, B.I.B. and Nordenfelt, L. (ed.) (1984). *Health, Disease, and Causal Explanations in Medicine*, Vol. 16. In *Philosophy and Medicine Book Series* (series ed. H.T. Engelhardt and S.F. Spicker). Dordrecht-Holland: D. Reidel Publishing Company.

Lipton, P. (1991). *Inference to the Best Explanation*. London: Routledge.

Mackie, J.L. (1977). *Ethics: inventing right and wrong.* New York: Viking.

McDowell, J. (1994). *Mind and World*. Cambridge, MA: Harvard.

McDowell, J. (1998). Aesthetic value, objectivity, and the fabric of the world. pp. 122–130, in. *Mind, Value, and Reality*. Cambridge, MA: Harvard University Press.

McMullin, E. (1987). Explanatory success and the truth of theory. In *Scientific Inquiry in Philosophical Perspective* (ed. N. Rescher). New York: University Press of America, pp. 51–73.

Mezzich, J.E. (2002). Comprehensive diagnosis: a conceptual basis for future diagnostic systems. *Psychopathology*, 35(2–3): 162–165.

OUP Copyright

12-Fulford-Chap13.qxd 08/06/2006 10:53 AM Page 382

OUP Copyright

Mezzich, J.E., Kleinman, A, Fabrega, H., and Parron, D.L. (ed.) (1996). *Culture and Psychiatric Diagnosis: a DSM-IV perspective*. Washington, DC: American Psychiatric Press Inc.

Mezzich, J.E., Berganza, C.E., Von Cranach, M., Jorge, M.R., Kastrup, M.C., Murthy, R.S., Okasha, A., Pull, C., Sartorius, N., Skodol, A., and Zaudig, M. (2003). IGDA. 8: Idiographic (personalised) diagnostic formulation. In *Essentials of the World Psychiatric Association's International Guidelines for Diagnostic Assessment (IGD). The British Journal of Psychiatry*, 182(Suppl. 45): 55–57.

Nagel, T. (1986). *The View from Nowhere*. Oxford: Oxford University Press.

Papineau, D. (1987). *Reality and Representation* Oxford: Basil Blackwell.

Pine, D.S., Alegria, M., Cook, E.H. Jr, Costello, E.J., Dahl, R.E., Koretz, D., Merikangas, K.R., Reiss, A.L., and Vitiello, B. (2002). Advances in developmental science and DSM-V. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 85–122.

Popper, K. (1963). *Conjectures and Refutations*. London: Routledge.

Putnam, H. (1995). *Renewing Philosophy*. Harvard, MA: Harvard University Press.

Putnam, H. (1996). In *Words and Life* (ed. J. Conant), (3rd edn). Harvard University Press.

Quine, W.V.O. (1975). "On Empirically Equivalent Systems of the World". *Erkenntnis*, 9, pp. 313–328.

Radden, J. (1994). Recent criticism of psychiatric nosology: a review. *Philosophy, Psychiatry, & Psychology*, 1(3): 193–200.

Reznek, L. (1987). *The Nature of Disease*. London: Routledge.

Reznek, L. (1995). Dis-ease about kinds: reply to D'Amico. *Journal of Medicine and Philosophy*, 20: 571–584.

Robbins, E., and Guze, S.B. (1970). Establishment of diagnostic validity in psychiatric illness: its application to schizophrenia. *American Journal of Psychiatry*, 126: 983–987.

Rosenhan, D.L. (1973). On being sane in insane places. *Science*, 179: 250–258.

Rossi, P. (2003). Magic, science, and equality of human wits. In *Nature and Narrative: an introduction to the new philosophy of psychiatry* (ed. K.W.M. Fulford, K.J. Morris, J.Z. Sadler, and G. Stanghellini). Oxford: Oxford University Press, Chapter 17.

Rounsaville, B.J., Alarcón, R.D., Andrews, G., Jackson, J.S., Kendell, R.E., and Kendler, K. (2002). Basic nomenclature issues for DSM-V. In *A Research Agenda for DSM-V* (ed. D.J. Kupfer, M.B. First, and D.E. Regier). Washington, DC: American Psychiatric Association, pp. 1–30.

Rust, J. and Golombok, S. (1989). *Modern psychometrics*. London: Routledge.

Sadler, J.Z. (ed.) (2002). *Descriptions and Prescriptions: values, mental disorders, and the DSMs*. Baltimore, MD: Johns Hopkins University Press.

Sadler, J.Z. (2004a). *Values and Psychiatric Diagnosis*. Oxford: Oxford University Press.

Sadler, J.Z. (2004b). Diagnosis/antidiagnosis. In *The Philosophy of Psychiatry: a companion* (ed. J. Radden). New York: Oxford University Press, pp. 163–179.

Sadler, J.Z. and Fulford, K.W.M. (2004). Should patients and families contribute to the DSM-V process? *Psychiatric Services*, 55(2): 133–138.

Sadler, J.Z., Wiggins, O.P., and Schwartz, M.A. (ed.) (1994). *Philosophical Perspectives on Psychiatric Diagnostic Classification*. Baltimore, MD: Johns Hopkins University Press.

Sartorius, N. (1976). Modifications and New Approaches to Taxonomy in Long-Term Care: Advantages and Limitations of the ICD. *Medical Care*, 14: 109–115, Supplement 5.

Sartorius, N. (1991). The classification of mental disorders in the Tenth Revision of the International Classification of Diseases. *European Psychiatry*, 6: 315–322.

Sartorius, N. (1992) in the Preface to World Health Organization (1992). *The ICD-10 Classification of Mental and Behavioural Disorders: Clinical Descriptions and Diagnostic Guidelines*. Geneva: World Health Organization.

Sartorius, N. (1995). *Understanding the ICD-10 Classification of Mental Disorders. A Pocket Reference*. Science Press Limited, London. (Also published in Polish, Russian and Ukrainian).

Sartorius, N. (2004). Personal Communication (at the World Psychiatric Association conference in Florence, November 10–13, on *Treatments in Psychiatry: an Update*).

Schaffner, K.F. (ed.) (1986). *Logic of Discovery and Diagnosis in Medicine*. Pittsburgh Series in Philosophy & History of Science. Berkley, CA: University of California Press.

Schaffner, K.F. (1993). *Discovery and Explanation in Biology and Medicine*. Science & Its Conceptual Foundations. Chicago: University of Chicago Press.

Spitzer, R.L. (2005). Recipe for disaster: professional and patient equally sharing responsibility for developing psychiatric diagnosis. *World Psychiatry*, 4:2, p89.

Stengel, E. (1959). Classification of mental disorders. *Bulletin of the World Health Organization*, 21: 601–663.

Szasz, T.S. (1972). The myth of mental illness. *The Myth of Mental Illness*. St Albans, England: Paladin.

OUP Copyright

12-Fulford-Chap13.qxd 08/06/2006 10:53 AM Page 383

OUP Copyright

Temerlin, M.K. (1968). Suggestion effects in psychiatric diagnosis. Journal of Nervous and Mental Disease, 147: 349–353.

Van Fraassen, B.C. (1980). *The Scientific Image*. Oxford: Oxford University Press.

Van Fraassen, B.C. (1999). To save the phenomena. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 187–194.

Williams, B. (1978). *Descartes: the project of pure inquiry*. London: Penguin.

Williams, B. (1985). *Ethics and the Limits of Philosophy*. Cambridge, MA: Harvard University Press.

Wing, J.K., Birley, J.L.T., Cooper, J.E., Graham, P., and Isaacs, A.D. (1967). Reliability of a procedure for measuring and classifying 'Present Psychiatric State'. *British Journal of Psychiatry*, 113: 499–515.

Wing, J.K., Cooper, J.E., and Sartorius, N. (1974). *Measurement and Classification of Psychiatric Symptoms*. Cambridge: Cambridge University Press.

WHO (1948). *Manual of the International Statistical Classification of Diseases, Injuries, and Causes of Death* (ICD-6). Bulletin of the World Health Organization, Suppl. 1, Geneva: World Health Organization.

WHO (1967). *Manual of the International Statistical Classification of Diseases, Injuries, and Causes of Death* (ICD-8). Geneva: World Health Organization.

WHO (1973). *International Pilot Study of Schizophrenia*. Geneva: World Health Organization.

WHO (1974). *Glossary of Mental Disorders and Guide to their Classification, for use in Conjunction with the International Classification of Diseases*, 8th revision. Geneva: World Health Organization.

WHO (1978). *Mental Disorders: glossary and guide to their classification in accordance with the ninth revision of the International classification of diseases*. Geneva: World Health Organization.

WHO (1992). *The ICD-10 Classification of Mental and Behavioural Disorders: clinical descriptions and diagnostic guidelines*. Geneva: World Health Organization.

World Health Organization (2001). *International Classification of Functioning, Disability and Health*. Geneva: World Health Organization.

Wright, C. (1992). *Truth and Objectivity*. Cambridge, MA: Harvard University Press.

Zubin, J. (ed) (1961). *Field Studies in the Mental Disorders*. New York: Grune and Stratton.

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