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CHAPTER 12

Psychopathology and the theory dependence of data

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Observation lies at the heart of psychiatry. The clinical process, as in the rest of medicine, starts with the careful articulation of symptoms of which patients complain, and signs: patients' appearance and behaviour, including speech. Theorizing about aetiology, both as part of the diagnostic and treatment/management process and as part of general clinical research, answers to data drawn from clinical observations.

Physics, philosophy, and psychopathology

This emphasis on the role of observation fits with the claim, discussed in Chapter 11, that psychiatry is scientific. A central component of the claim that a discipline is scientific is that it aspires to an objective account of the world. And, on the traditional model of science, neutral observation, at what we called Stage 1 of the scientific process, seems to be a precondition for objectivity.

Consistently, then, descriptive psychopathology, the signs and symptoms of mental disorder (outlined in Chapter 3), places particular emphasis on careful observation. Classification lies at the heart of psychiatry and psychiatric classification, coded in, for example, ICD-10 (WHO, 1992) and DSM-IV (APA, 1994), concentrates on the definition of syndromes in broadly observable terms (signs and symptoms). These classifications, furthermore, since DSM-III (APA, 1980), have directly emulated physics, the paradigm observational science, by seeking a basis in *operational* criteria.

Operational criteria were the brainchild of the American physicist and father of 'Operationalism' Percy W. Bridgman (1882–1961). Bridgman argued that there was a close connection between concepts and empirical tests for whether they applied.

To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with a corresponding set of operations.

Bridgman (1927 p5)

Bridgman's ideas were put forward by the philosopher Carl Hempel at a conference on psychiatric classification in 1959 that will be described more in Chapter 13. In the context of psychiatry, the emphasis on 'operations' was translated into observations or observable tests that, in principle, could be elicited by the 'operation' of asking certain questions in a particular way. One consequence of this has been to make clear the connections between syndromes and the observable signs and symptoms by which they are defined. So, for example, the DSM-IV (APA, 1994) criteria for schizophrenia begin:

A. Characteristic symptoms: Two (or more) of the following, each present for a significant portion of time during a 1-month period (or less if successfully treated):

- ◆ delusions
- ◆ hallucinations

- ◆ disorganized speech (e.g., frequent derailment or incoherence)
- ◆ grossly disorganized or catatonic behavior
- ◆ negative symptoms, i.e., affective flattening, alogia, or avolition

Note: Only one Criterion A symptom is required if delusions are bizarre or hallucinations consist of a voice keeping up a running commentary on the person's behavior or thoughts, or two or more voices conversing with each other.

The criteria specify the conditions that have to be satisfied for a diagnosis of schizophrenia to be made with the conditions spelled out in more basic terms, i.e. in terms of symptoms. The idea is that the symptoms can be identified prior to, and independently of, subsequent diagnosis by well-defined 'operations' of clinical interview and investigation. Rules then specify the connection between the diagnosis and symptoms and these connections (partially) characterize the nature of the concept of schizophrenia. That, broadly, is the debt to operationalism.

A key advantage of this approach to psychiatric diagnosis has been an increase in the *reliability* of psychiatric diagnosis. Writing in *A Research Agenda for DSM-V* (Kupfer *et al.*, 2002), Bruce Rounsaville *et al.* comment: 'When DSM-III was published in 1980, one of the most important advantages was a radical improvement in the reliability of psychiatric diagnosis by virtue of its provision of operational criteria for each diagnosis.' (*ibid* p. 13)

Reliability—which will be discussed more in Chapter 13—is a measure of consistency in diagnosis both between observers and over time. Concentrating on the connection between diagnoses and more basic observational symptoms should help provide a sound foundation for agreement in complex cases and thus for reliability.

There is a further feature of a broadly operationalist approach: the emphasis on atheoretical descriptions of signs and symptoms. Again since ICD-9 (WHO, 1978) and DSM-III (APA, 1980) there has been an emphasis on descriptions of symptoms that do not presuppose any particular aetiological theory. The hope has been that, consistently with the traditional model of science outlined in Chapter 11, a sound basis of descriptive psychopathology might provide a neutral framework for the subsequent development of psychiatric theory.

One of the lessons of this chapter, however, will be that notwithstanding improvements in reliability achieved through the move towards operational definitions, it is not possible completely to separate uncontentious observational elements of descriptive psychopathology from contentious theoretical models of aetiology. Diagnostic manuals such as DSM and ICD must, inevitably, include theoretical elements in their characterization of what is directly observable. That of course puts an extra burden on the authors of such classifications that the theories presupposed are *correct* but psychiatry is not alone in facing that burden.

There is another consequence of recognizing the blurring of observation and theory. It impacts on a worry about psychiatric observation that is summarized in the following passage:

It is fashionable in some circles at the moment to decry the use of diagnostic labels, and to suggest that what doctors have to try to understand and treat are not diseases but problems, multifaceted and unclassifiable. A knowledge of the biological, psychological and social processes involved in problem-formation is recognized as indispensable, but to give some thought to their taxonomy is said to lead inevitably to sterile pigeon-holing, inflexibility and inhumanity. Giving a name to a condition, according to this view, not only serves little useful purpose but in the case of mental illnesses it can be positively harmful, since the label is often also a term of opprobrium or one implying hopelessness. Wing *et al.* (1974, p. 1)

The worry can be summarized this way. If observation is connected to classification and diagnosis in the way exemplified by DSM, ICD, and the Present State Examination (PSE), then it involves a distortion because patients' experiences are forced into pre-existing categories rather than recognized for their individuality.

But while there are indeed cases where observations are reported in distorting ways, the implicit assumption that it would be possible to shed all preconceptions and simply 'drink in' a subject's experiences in all his or her individuality is impossible. As we will suggest, there are plausible arguments that all observation is always structured by prior concepts.

Plan of the chapter

- ◆ *Session 1* characterizes the theory dependence of observation through comments made about the nature of diagnosis in one of the first structured interview schedules, the PSE.
- ◆ *Session 2* charts the origins of the (supposed) separation of theory and observation in the two-language model of Logical Empiricism (this being a philosophical version of the traditional model of science—see Chapter 11).
- ◆ *Session 3* sets out arguments *against* the separation of observational statements and theoretical statements. In other words, these arguments demonstrate the theory dependence of observation by showing that the *language* of observation (the terms in which we report observations) cannot be clearly separated from the language of theory (whether psychiatric or drawn from physics).
- ◆ *Session 4* sets out arguments against the separation of theory and observation in the actual process and experience of observing. Like the arguments of Session 3, then, these are arguments supporting the theory dependence of observations, but in this case directed against the idea that observation is based on raw or brute data. Instead the session argues that observations are always conceptually structured.
- ◆ *Session 5* draws some conclusions about the theory dependence of observation in physics and psychiatry.

Session 1 The theory dependence of everyday observations and psychopathology

A practical exercise on the theory dependence of observation

The idea that impartial observation, as the basis of objectivity in science, is in important respects illusory, comes as something of a culture shock to most of us, at least in medicine, brought up as we are within a broadly traditional understanding of science. As we saw in Chapter 11, words like 'positivism' are associated with the idea, very much dominant in science as well as philosophy through much of the twentieth century, that it is of the essence of science that it should be based on 'clear' observations, on data that genuinely reflect features of the world 'out there', rather than the perspective of this or that particular observer. Hence we will begin with an exercise designed to bring out in a straightforwardly practical way some more mundane aspects of the theory dependence of observation.

EXERCISE 1

This exercise comes in two parts. It is the core of the session and thus it is particularly important to do it 'for real' before going on.

Part 1 (10 minutes)

Make comprehensive observations of a piece of furniture nearby: a chair perhaps. Make your observations as objective as possible. Write them down. Now mark those data which could be used for re-identifying this item at some time in the future.

Part 2 (30 minutes)

Think carefully about your answers to the exercises you have just completed. In what ways do they show that there are limits to how far we can be objective even about observations of everyday objects like chairs? Write down your own conclusions before going on.

In this exercise we are concerned with a relatively simple case of observation, compared with 'observing' mental states, for example. None the less, it brings out at least two ways in which observation is theory dependent, namely (1) that the data have always to be *selected*, and (2) there has always to be an assumed *level of precision*.

Two ways of observing chairs

That observations are theory laden in these two ways may seem, once one reflects for a moment, rather obvious. If we think about them in a little more detail, though, they bring out a rather less obvious and important general point about observation. Thus:

1. *Which data?* The first and most obvious sense, then, in which observation is theory dependent is that data have to be

selected. The instruction: 'make comprehensive observations' is impossible to satisfy. In this exercise, the most obvious observations include the size, shape, weight (or mass), colour, and location of the chair. But other possible observations include the orientation of the chair with respect to other pieces of furniture, nearby wildlife and distant stars. With a little ingenuity, more potential observations can always be added to any finite list.

This may seem merely an artificial and untroubling fact until one asks how, in practice, one avoids the need to try and record every possible observation. There is indeed a crucially important tension here, between, on the one hand, being overwhelmed with too many observations, and, on the other, restricting its range too narrowly. Patients with schizophrenia are often particularly good at making novel observations. They really do notice features of their environment that most of us would not. In the case of schizophrenia, this can lead to cognitive overload. But the same alertness to novel features of the environment is central to creativity, whether in science or the arts.

All in all, then, it is clear that observation, as the basis of science, in so far as it involves the correct selection of data, is not simply a matter of reflecting the world 'out there', a property (solely) of the thing observed, but is determined (in part and in some or more ways) by the particular context in which the observations are made. In certain contexts, one kind and quantity of data are appropriate, in other contexts, other kinds and quantities will be appropriate, and so on.

2. *Level of precision.* We will be considering 'context', as a feature of scientific method, at several points in this part. The second activity in this exercise makes explicit a key aspect of 'context', namely the purposes for which an observation is made. Here, we asked you to think about the data required to re-identify the chair in question. This particular purpose immediately gives, within the possible (infinite) range of observations, particular focus to which observations are appropriate. Orientation in space, for example, will not be helpful here. Whereas the kind of wood (or whatever material the chair is made) will be. This second activity, moreover, brings out second sense in which observation is theory dependent, namely that, for whatever data are selected, there must always be an assumed level of precision.

Thus, the observations that are relevant for re-identifying a chair may not include the more bizarre examples given. The first list may be adequate. But within this list, the right level of precision for your observations also depends on context. As you carried out the exercise, you might have noted, say, a particular style of chair leg as sufficient to re-identify the chair you were observing as against others in the room. But suppose you were concerned about someone swapping your chair for a similar one next door; then, perhaps, a particular scuff mark on the chair leg would become important.

Or, again, if you were an antiques expert, the precise way in which the leg was 'turned' could become crucial.

The normativity of observation

Both in the selection of data, therefore, and in an assumed level of precision, our observations, even for the relativity straightforward case of a chair, are not impartial. They are made from the point of view of a given observer in a given context and, importantly, for a given *purpose*.

This brings us to the general point illustrated by the two aspects of the theory dependence of observation, namely that observation, far from being neutral, turns out to be *normative*. To do something for a purpose is to acknowledge standards or criteria by which it can be said to be done well or badly, correctly or incorrectly, successfully or unsuccessfully. This is why, as in the above exercise, in speaking of observation, it is natural to use phrases such as the 'correct' selection of data and the 'right' level of precision. Such phrases, recalling Austin's concept of philosophical fieldwork (from Part I, Chapter 4), are linguistic-analytic signals that, notwithstanding the traditional model of science, observation, as the basis of scientific method, is theory dependent in the sense that what we observe necessarily reflects, not only the features of that which we observe, but a selection of those features that is driven by the purposes for which the observations in question are made.

The tip of the iceberg

To anticipate a little, it is important to note that there are still deeper ways in which observation is theory laden. As we will see later, philosophy of science has been concerned mainly, not with these *quantitative* aspects of the theory ladenness of observation, but with *qualitative* aspects. Both the selection of data and an assumed level of precision take for granted that there are determinate data 'out there': they assume that there are determinate objects (such as chairs and chair legs for example) out there; and that there are determinate properties (brown, square, etc.) in terms of which these objects can be described in different ways and to different levels of precision. But modern philosophy of science has shown that the very concepts used to frame observation reports (chair, leg, brown, square, etc.) themselves presuppose theory, and that the very experience of observing is determined (in part at least) by the theories one holds. We will come to the way in which concepts shape data in the next session.

The theory dependence of observation and the PSE

From chairs to descriptive psychopathology

First, though, we will compare observation of chairs with observation of psychopathology. We will be returning to psychopathology in the last session of this chapter. At this stage we just need to note that descriptive psychopathology, indeed observations

of mental states in general, present still further complications beyond those presented by things such as chairs.

EXERCISE 2

(10 minutes)

Consider a range of psychopathological symptoms. Revisit Chapter 3 if you are hazy on these. Write down the observations necessary for establishing them. Are all symptoms equally easily and directly observed?

This exercise brings out some of the difficulties, over and above those involved in observing things such as chairs, in observing symptoms.

In the first place, it is clear that the sort of observations required for identifying symptoms varies dramatically. Some symptoms such as involuntary speech or action seem to be much more directly observable than others such as the hearing of voices. Behaviour is on the 'outside'; it is 'public' and available to inspection. 'Hearing voices' is inside someone's head; it is private and not (directly) available to inspection. This contrast is indeed conventionally expressed in medicine by the distinction between symptoms (which patients experience) and signs (which clinicians observe). But in the case of psychopathology, even 'publicly available' signs are problematic in ways that things such as chairs (which do not have mental states) are not. To take an obvious example, what appears to be involuntary speech, and hence pathological, may in fact be a piece of street theatre.

A traditional model of science

Much of psychiatry, particularly with the current vogue for 'biological psychiatry' assumes that observations of psychopathology are no more and no less problematic than observations of chairs, or, in a more nuanced way, that with a little effort observations of psychopathology can be made with more difficulty, perhaps, but in essentially the same way, as observations of chairs.

We will see in this part of this book that this is insecure science reflecting insecure philosophy! But the next reading represents a particularly clear and determined attempt to square the requirements of a traditional view of science with a sophisticated understanding of the difficulties involved in developing a science of mind.

EXERCISE 3

(40 minutes)

Look at the extract from:

Wing, M.C., Cooper, J.E., and Sartorius, M. (1974). 'Preface' in *Measurement and Classification of Psychiatric Symptoms*. Cambridge: Cambridge University Press (Extract pp. vii, viii)

Link with Reading 12.1

This is a short reading but it encapsulates many of the issues about the theory dependence of observation that we will be

thinking about in this session. So, read it carefully and consider:

1. What model of science is implicit in the authors' approach?
2. What role does observation have in this model?
3. How do the authors decide whether observations of psychopathology are 'correct', i.e. playing the role required of them?

Write down brief answers to these questions *before going on*. In each case, note (1) any indications that the authors themselves are aware of difficulties in their approach and (2) any difficulties that you are aware of.

The first author of this book, John Wing, was a psychiatrist and social scientist who based his work on a sophisticated understanding of the philosophical issues. His book, *The Reality of Mental Illness* (1978), represents a robust and detailed defence of medical psychiatry as a social as well as a biological science (the title is deliberately in opposition to Thomas Szasz's *The Myth of Mental Illness*).

Wing's basic commitment, which is made explicit in *The Reality of Mental Illness*, is to a medical model of mental disease in which psychiatry will eventually develop disease theories of the same broad kind as those in other medical specialities, such as cardiology. As in the rest of medicine, then, so here, a traditional model of science lies behind Wing's work. As we saw in Part I, psychiatry's image as a credible discipline is widely perceived as depending on this.

Observations as building blocks for theories

Much of Wing's work was thus concerned with establishing a sound observational basis for the development of psychiatry as a science traditionally understood. The PSE interview schedule, as it has come to be called, described in full in the book from which this reading is taken, was designed to do just that. Together with other similar schedules, it has been highly successful. As we will see, such schedules have been the basis of many of the most important developments in psychiatry over the last 20 years. As in other sciences, observations (here of psychopathology) have been the building blocks of scientific theories.

In the preface to the PSE, then, the broad message is that psychiatry needs to be put on a sounder scientific footing so that it can take its proper place among other medical sciences, and the basis of this is better observations. The commitment to foundational observations is evident in the references, for example, (1) to the 'good clinician' making a 'systematic exploration of the subject's mental state' in order to 'discover whether any of a finite number of abnormal mental phenomena are present'; (2) to the interviewer and patient coming to an 'exact description of the symptoms'; (3) to 'data', to 'coming closer to the truth' and,

shades of Chapter 11, in the sideswipe at psychoanalysis! It is also made explicit later in the book: for example, on p. 3, using the example of Kanner's original description of autism, we find 'No disease theory can be elaborated before the clinical syndrome has been recognised and labelled.'

Some normative considerations

In most psychiatric textbooks, as we saw in Chapter 11, the traditional model is taken for granted. Here, though, a more sophisticated approach is adopted. Despite the reference to 'the truth', the aim of the PSE is more modest. It is 'consistency' between observers achieved by formalizing (writing rules to capture) best diagnostic practice. This was an important aim at the time the PSE was being developed as a series of studies had shown that psychiatric observations were highly idiosyncratic. Different observers, or the same observer on different occasions, came up with quite different observations about the same case. Whatever the 'truth', therefore, we could have no confidence in such observations as building blocks for scientific theories.

The measure of success, therefore, the standard by which the authors of the PSE decided what the observations it produced were appropriate for their role in psychiatric science, is consistency, or, as it is usually called in medicine, *reliability*.

Can we rely on reliability?

Wing *et al.* (1974), then, although guided by a traditional notion of reflecting the truth that is 'out there' aimed only for a cautiously modest contribution to the observational route to this.

We will return to the nature of reliability in Chapter 13. But it is important to note how carefully the authors of the PSE, well aware as they are of the difficulties in a science of the mind, define the modest scope of their claims for it:

1. It is 'firmly in the European clinical school of psychiatry'—i.e., although the influence of this has 'spread widely' throughout the world, it has its origins in a specific social and cultural context and in studies of best practice in this tradition.
2. Similarly, although not everyone who used the PSE fully accepts it, they are willing to do so 'for the purposes of attaining compatibility with colleagues'—i.e. the particular purpose of these observations is specified.
3. Perhaps most important of all, it is not a passive process. This is, as noted earlier, an important *prima facie* difference between psychopathology and chairs. Chairs cannot engage in the observational process but the PSE (reflecting good practice in clinical interviews generally) directly involves the patient. To the extent that its aim is determining the presence of 'specific symptoms' (those identified as important in the European tradition) it inevitably involves a degree of 'cross examination'. But the aim of this is to

engage the patient *with* the interviewer in the process of observation.

The PSE, along with other similar instruments, has sometimes been criticized as naively positivistic. Such criticisms fail to take account of the careful way in which the context and purpose of the observations the PSE generates are specified. Given, though, the traditional model of science that is evident in the Preface, and the thrust of Wing's more overtly conceptual work in *The Reality of Mental Illness*, the PSE is perhaps properly understood as sophisticated positivism: it represents a careful attempt to accommodate descriptive psychopathology to a model of medicine that, based as it is on a traditional model of science, aspires to a positivist paradigm. The middle part of this chapter (Sessions 3 and 4) will outline key arguments against positivism as a characterization of science in general. We return in Session 5 to the implications of these arguments for observation of psychopathology as the basis of a scientific approach to mental disorder and will help justify this antipositivist stance from a philosophical perspective.

Some problems deferred

Some of the underlying difficulties of determining other people's mental states on the basis of observation, including the problem of establishing whether other people have minds at all, will be investigated more thoroughly in Part V (see especially Chapter 27). We will find there that the public-private distinction, at least as conventionally drawn, is too sharp. But in the present chapter we are mainly concerned with the problems involved in all observation. For now, then, it is sufficient to note that mental health care shares with the physical sciences a central commitment to the importance of observation within a broadly positivist model of science according to which anything that threatens the impartiality of observation threatens the objectivity of its claims.

In next session, we will examine a particular version of the positivist model of science, Logical Empiricism, that dominated the first half of the twentieth century and that encapsulates an intuitive idea of how observation can be impartial. As already noted, Logical Empiricism is the most persuasive of many attempts to develop a conceptually sound version of the traditional picture of science. While there is no firm distinction 'Logical Empiricism' is the name given to a family of views derived from the Logical Positivism of the Vienna Circle but less firmly wedded to the latter's Verification Principle, which will be discussed below. Thus it can be thought of as starting with the Vienna Circle in the 1920s and continuing to be influential up to the 1950s and 60s. Its key aim was the articulation of the logic and structure of scientific theory and the separation of genuine empirical questions from others.

Having set out the elements of the model, then, in the next Session, and indicated its continuing influence in psychiatry, Logical Empiricism will be used as a stalking horse in the discussion of the theory dependence of observation that follows.

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 general issues does the exercise on observing a chair raise about the nature of observation and about the gathering of data? How does this complicate the traditional model of science?
2. What differences are raised by consideration of the PSE by comparison with the exercise on observing the chair?
3. How does the PSE fit with the traditional model of science outlined in Chapter 11?



Fig. 12.1 Moritz Schlick

Session 2 An empiricist model of scientific theory

A good way to bring out the significance of the theory dependence of observation is to examine its impact on the Logical Empiricist model of science. As noted earlier, this model was prevalent in the first 60 years of the twentieth century and it incorporates a common sense view of the role that impartial observation plays in science. In particular, the Logical Empiricist model formalizes the traditional view of the importance of impartial observation. It presupposes that theory and observation can be separated. Setting out the consequences of the theory dependence of observation for this model shows the consequences for a worked out version of the traditional picture of science. This will show that both the process of observing and the recording of observations in observation statements presuppose some theory.

There is a second reason for sketching out the Logical Empiricist picture of science. This is that the understanding of science that has developed during the second half of this century is in part a response to *criticism* of Logical Empiricism. We can get a clearer understanding of recent work in the philosophy of science—and thus a better understanding of science—by contrasting it with Logical Empiricism. First, then, Logical Empiricism.

Logical Empiricism and the Vienna Circle

Logical Empiricism was developed by members of the Vienna Circle. The Vienna Circle was a group of philosophers and scientists who worked in the areas of philosophy, logic, mathematics, the natural and social sciences, and pioneered work in the philosophy of science in the analytic style. The circle published a manifesto, *The Scientific World View: The Vienna Circle*, in 1929.

The central figure in the Vienna Circle was Moritz Schlick, a professor of philosophy at the University of Vienna (later murdered by a student) but the group also included many of the most famous names in the philosophy of the first half of the twentieth century: Rudolf Carnap, Herbert Feigl, Philipp Frank, Kurt Gödel, Hans Hahn, Viktor Kraft, Karl Menger, Otto Neurath, Friedrich Waismann, and Edgar Zilsel. There were, in addition, various foreign 'guests', such as Hans Reichenbach, Carl G. Hempel (later to have a decisive influence on psychiatric classification, see Chapter 13), A.J. Ayer, Ernest Nagel, John von Neumann, Willard Van Orman Quine, and Alfred Tarski. There was also some peripheral contact with Ludwig Wittgenstein and Karl Popper.

What is Logical Empiricism?

The Logical Empiricist picture of science emphasizes the role of observations. In common with all forms of empiricism in philosophy, the key claim is that knowledge of the world is grounded in experience rather than in reasoning alone. Empiricism holds that while reasoning may have some part to play, all significant knowledge of the world is founded on experience.

Logical Empiricism adds to that basic emphasis on the role of observation a more precise account of how theoretical science is related to observations. Crucially, it assumes that the language with which observations are recorded is independent of the language of theory. The language of theory is then based upon the language of observation.

By making observational language independent of theoretical language (although not vice versa), Logical Empiricism attempts to explain how observation statements can be theory free

and thus serve as an impartial basis for the development of scientific theory. This 'two-language model' will be discussed below.

Two different uses of observation

But before going on to the separation of observation and theory, it is worth first highlighting two different ways in which observations play a part according to two rather different models of science, both broadly within the Logical Empiricist tradition.

1. *Using observations to verify theories:* According to 'classical' Logical Empiricism, theory-neutral observation statements are invoked to provide positive support for theories. On this model, the aim of science is to provide justification for theories by marshalling positive evidence that they are true. Observations that accord with theories provide inductive support for them. We touched on induction in Chapter 5 and will return to it in detail later in this part (Chapter 16). A standard example of induction is the justification of the theory that all swans are white by citing the observation of several white swans. Given the open-ended and universal nature of theories such as this, no observation can establish once and for all their truth. But observations can be used, according to this model, to provide albeit non-conclusive inductive evidence of their truth. Given the crucial supporting role that observation plays, then, traditional Logical Empiricism emphasizes that observation itself must be impartial and theory neutral.
2. *Using observations to falsify or refute theories:* There is, however, another influential account of the role of observations in science, an account that can share with inductivism the two-language picture of classical Logical Empiricism, but that differs from it in one key respect. This alternative was developed by Karl Popper—an Austrian philosopher of science who was influenced by and influenced the Vienna Circle—in order to escape some of the philosophical problems that have plagued the idea of positive support. (These problems are generally called 'the problem of induction', although really there are several problems—see Chapter 16.) Popper's alternative is Falsificationism. According to this model, the purpose of science is not to provide positive evidence for the truth of theories by observation. Instead, observations serve to *refute* theories. A single observation cannot prove the truth of a universal claim (such as all swans are white). But it can (according to the original and most simple versions of Falsificationism) disprove a universal claim. Imagine, for example, an observation of black antipodean swans.

The virtues or otherwise of Falsificationism and its insistence that science aims to disprove rather than to prove theories is not what is at issue in this chapter. Both simpler and more sophisticated versions of Falsificationism will be discussed in the context of scientific progress and the role of evidence in

research in Chapter 16. However, the advantages of Falsificationism of all kinds are clearest if it is assumed that the observations by which theories are taken to be falsified are themselves theory free.

In summary then, what is common to classical Logical Empiricism and at least simple, and thus the clearest, versions of Falsificationism, is that whether observations play a positive or negative role as the foundation of theory, the observations themselves are theory free.

The two languages model of Logical Empiricism

The two-language model of theory and observation

The key question, then, to return to the philosophical concerns of the Vienna Circle, is exactly how observations can play an impartial role in the assessment of theory. According to Logical Empiricism, part of the answer to this question is that there is a fundamental distinction between empirical and theoretical language.

It is this distinction that is at the heart of the two-language model of Logical Empiricism. According to this model, the terms that are used to report observations are distinct from, and more basic than, those used to make theoretical claims. The reason for this assumption is this. If the very meanings of the terms used to frame observational reports presuppose a theory, then it seems that those reports cannot be impartial tests of that or any other theory. If the very framework in which observations are reported presupposes that a theory is true, then it seems that those observation statements cannot be impartial tests of theory. (Whether this is true is a question to which we will return at the end of this chapter.)

Epistemology and semantics

Logical Empiricism thus attempts to ensure that theory does not infect observation by separating observation *language* from the theoretical *language*. In other words, an *epistemological* thesis that one can know the truth of an observation statement independently of knowing the truth of any theory is underpinned by a *semantic* thesis about the meaning of observational and theoretical terms. Specifically, Logical Empiricism makes two semantic claims: (1) that observational concepts are defined in terms of experience only, and (2) that theoretical concepts are defined in terms of observational concepts.

Spelling this out a little, Logical Empiricism claims that the meanings of concepts used to report observations are thus, on this picture, given by definitional connections to experience. The language of observation reports is defined only by reference to what is directly observable and independently of theoretical explanations of what is observed. By contrast the language of theory is then defined by reference to observational concepts. Thus while theoretical language depends on observational language, observational language does not presuppose any theory.

Ideas: blurring the distinction between concept and experience

The connections between observational concepts and experience have at different times in philosophy been considered closer or more distant. In the seventeenth century it was widely assumed that the connection between concepts and experience was formed very directly at the level of individual concepts or words (i.e. rather than whole sentences, see below). The concept of redness, for example, was considered to be *extracted* from the experience of redness.

David Hume argued that our *ideas* all (or nearly all of them—for one exception see Chapter 16) derive from *impressions* that we receive in sensation or perception. But he also suggested that the only difference between impressions and ideas was one of 'vivacity' implying a very close connection between our thoughts or concepts and our experiences. Fellow British Empiricist John Locke (1632–1704) put forward a similar account. (See David Hume (1711–76) and his *Treatise of Human Nature* first published in 1739–40 (Hume, 1967) and *Enquiries Concerning Human Understanding and Concerning the Principles of Morals* of 1748 (Hume, 1975); and also see John Locke *Essay Concerning Human Understanding*, 1690 (Locke, 1989).)

In this century, such talk of 'ideas' has been abandoned and thus the direct link between individual terms or concepts and elements of experience cannot be maintained. Instead the connection between observational language and experience has been made at the level of *whole sentences*. The reason for this is simple and intuitive. The sentence is the smallest unit of language that can be 'compared with' the world because it is the smallest unit that can be used to *assert* anything. Thus the basic unit of meaning is the whole sentence whose meaning can be identified with the state of affairs that the sentence can be used to assert. Individual word meaning is really, on this account, an abstraction from sentence meaning and only in the context of a sentence does an individual word have meaning. In the philosophy of language, this thesis is generally referred to as Frege's Context Principle. We will return to some of the issues surrounding this in Part V.

The Verification Principle

In the philosophy of science, the most famous version of such 'whole-sentence' accounts is associated with Logical Empiricism and is called the Verification Principle. At its simplest the Verification Principle states that the meaning of a sentence is its method of verification.

The idea is that one can define the meaning of a sentence by using an account of how one would check that it was true. Thus, there might be a paradigmatic method of determining whether claims about, e.g., the lengths of everyday objects were true, a method of using a ruler according to a particular protocol, and thus giving the meaning of the sentence 'This chair is 100 cm tall'. This gives a very practical picture of the connection between the meaning of observational language and the process of making observations. It is clearly closely related to Bridgman's

Operationalism, which as we will see in Chapter 13 has had a profound influence on psychiatric classification.

Central to our interests in this chapter is that such an account suggests that observational language is theory free. The reason for this is that if the meaning of a sentence is given directly by a practical test, then it does not depend on theory. The meaning of the sentence is given by the test itself. This contrasts with a more recent view called the Duhem-Quine thesis (after Pierre Duhem and W.V.O. Quine), which claims instead that any such test always presupposes a body of surrounding theory. (See below.)

From observation to theory

Having defined observational language, Logical Empiricism can then define theoretical language using observational concepts. Theoretical language is thus defined through definitional connections or bridge laws 'upwards' from observational language. Theory presupposes observation but not vice versa. It is worth noting that this is an abstract idea expressed in logical terms. Few examples were ever actually set out.

It is also worth noting parenthetically two different approaches to theoretical statements both of which are consistent with this general approach to theoretical concepts. Theoretical statements may be regarded as simply convenient ways of organizing observational claims but not themselves true or false. On this account theoretical sentences do not strictly assert anything themselves but merely serve as heuristic devices for organizing and deriving observation claims. This view is a form of instrumentalism and was influential on some positivist philosophers of science. We will return to it in Chapter 13. Alternatively, theoretical statements can be regarded as capable of literal truth and falsity, although they could be analysed into a sufficiently complex list of observational claims.

Two languages and mental health

The details of the two-language model of observation are clearly very much *philosophical* details. But at the heart of the model is a codification of an *intuitive* separation of observation and theory. It is a formal model of a common-sense idea at the heart of the traditional model of science.

Correspondingly, therefore, as applied to mental health, and indeed medicine generally, the codification of the separation of observation and theory attempted by Logical Empiricism, provides what is at first glance an intuitively attractive account of the connection between symptoms and signs (descriptive psychopathology in mental health) and subsequent diagnosis. We noted the standard two-stage model of the diagnostic problem in Chapter 11. According to this model, Stage 1 is the identification of symptoms, signs (and perhaps the results of laboratory or other 'tests'), while Stage 2 is the matching of these observations to categories in our classification of disease. If Logical Empiricism is an accurate account of science, then, and if diagnostic assessment is indeed 'scientific', in the first stage of this

process symptoms are described using an observational vocabulary which is independent of Stage 2, the subsequent assignment to a diagnostic category. Stage 2, according to Logical Empiricism, comprises a second and independent stage in which a theoretical interpretation is placed upon the symptoms.

As we will see in Chapter 13, this is the approach adopted by recent psychiatric classifications (such as the ICD and DSM). Recognizing that we lack satisfactory causal theories, these classifications are based largely on symptoms. But there is the hope and expectation that the categories so defined will eventually be linked 'upwards' to causal theories just as in areas of medicine such as cardiology they already have been.

Two senses of theory

But it is also worth noting a difference of emphasis between the philosophy of science more broadly and work on the nature of psychopathological or psychiatric classification. While the explicit aim of the psychiatric emphasis on observation has been to avoid premature *causal* theory, the aim of Logical Empiricism was to separate observation from *any* or all theory. These concerns are related and overlap but they are not identical and we will return to the distinction later in the chapter.

The PSE system discussed earlier is an attempt to codify and structure the gathering of data in clinical interviews. It comprises an ordered list of symptoms that are scored for severity and then taken as the foundation for subsequent diagnosis. As the authors comment, the system is supposed to form a 'clear-cut basis' (ibid: p. 10) for teaching, clinical work and research. The authors do not themselves make explicit claims that the symptoms are directly observational and presuppose no theory, but the suggestion of a 'clear-cut' basis for subsequent largely theoretical work strongly suggests such a distinction.

But is the two-language model right?

As we saw in Session 1, this view of the independence of observation and theory, although widespread in the sciences, is called into question by recent work on the theory dependence of observation. In fact, there are a number of different arguments against the independence of theory and observation, as we will see. One argument is to the effect that there is no distinction in kind between theoretical and observational language and thus that observation statements are themselves theory involving. They therefore fail to comprise a neutral basis for theory testing and there is no reason for supporters of rival theories to accept the same observation statements. It is with arguments of this kind that we will be concerned in Session 4. Another is that observation or perception more generally has to be understood to include conceptual elements and is thus not impartial with respect to our concepts.

The rest of this chapter will focus on several of these arguments. But it is worth recalling why the issue of the connection between the meaning of observational and theoretical terms is important.

Epistemology and semantics again

The real point at issue is whether and, if so, how observation provides a neutral and impartial test of theory thus underpinning the claims of scientific objectivity. In other words, it is whether observations can be used to test theories (or diagnoses) (whether by providing confirmation or refutation) independently of those theories (or diagnoses). In practical terms, this requires that if one wishes to test a theory by a relevant observation, knowing whether the observation statement is true must not require knowing whether the theory is true. This is an *epistemological* claim. The Logical Empiricists' *semantic* thesis that observation language is independent of theoretical language as a whole provides reassurance on this score and thus, if true, would partially explain the objectivity of science. But it is undermined by recent arguments. It is to these that we turn in the next session.

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 relation between Logical Empiricism and the traditional model of science outlined in Chapter 11?
2. What role does it ascribe to observation?
3. How does it attempt to use observation to underpin scientific objectivity?
4. What is the relation between the approach to theory and observation in Logical Empiricism and psychopathology? What role does 'theory' play in both?

Session 3 Arguments for the theory dependence of observation statements

Since the publication of ICD-9 (1978) and DSM-III (1980), psychiatric classification has concentrated on symptomatically defined syndromes in order to maximize the reliability of diagnoses and to provide a neutral starting point for aetiological theorizing. These have taken the form of observable tests. But there are powerful arguments to suggest that the distinction between theory and observation is not robust. As noted earlier, the arguments can be divided between those that concentrate on observation *statements* and those that concentrate on the process or experience of observation itself. The arguments, here as in most areas of the philosophy of science, focus on the physical sciences but they also apply to the biological sciences and indeed psychiatry.

Different statements of the theory dependence of observation

To anticipate a little, the traditional independence of observation and theory has been challenged in recent decades from a wide variety of philosophical angles. Typical claims made broadly in support of the theory dependence of observation include:

- ◆ there is no distinction of kind between observational and theoretical language;
- ◆ observation is theory laden;
- ◆ perception consists in the conceptual exploitation of sensations;
- ◆ the objects of perceptions are meanings;
- ◆ to know whether an observation statement is true requires knowing the truth of some theory.

These claims have in common the fact that they threaten the neat distinction between (impartial) observation and theory (to be tested against observation), which is presupposed by the traditional model of science. But they are strictly different claims that result from different arguments.

It is again worth remembering that the *semantic* questions, or questions of meaning, that will be discussed in this session gain their purpose from an *epistemological* thesis about the independence of observation from theory. Semantic claims, based on arguments about observation statements, aim to show that observation *reports* presuppose theory. (In Session 4 we will look more closely at the nature of observation itself and assess the possibility that it comprises the harvesting of raw or brute data. Again the session will only indirectly concern the epistemological thesis.)

Most of the claims about the theory dependence of observation in the list above are semantic: they turn on the *meaning* of observation statements or the *content* of perceptual experience. But the last claim, that 'to know whether an observation statement is true requires knowing the truth of some theory', is an epistemological claim. As we will see, although separable, there are also close connections between the semantic and the epistemological.

So why start, as in this session, with semantic arguments for the theory dependence of observation, rather than, as in Session 4, with the apparently deeper epistemological arguments?

Observation statements as public items and their unexplained connection to experience

One reason for starting with arguments concerning observation statements is that Logical Empiricism stressed the importance for an objective science of statements reporting observations as public items of language, as against observations themselves as private items of experience. Aside from a distrust of the private, however, a further motivation for starting with observational statements rather than observational experiences, was that Logical Empiricism lacked any clear account of how the two might be connected. What logical, rational, or evidential

connection can there be between an experience and a statement? Statements can stand in rational or logical relations to other statements. They can be implied by other statements or contradict them. But how can they stand in any such relation to an experience or sensation?

This is an important issue to which we will return in Session 4. (Part of the answer has to do with distinguishing experiences from sensations.) However, the focus of this session is the question of whether observation language really can be distinguished from theoretical language as the naive account requires.

Duhem's argument: observations are made in theoretical terms

Physics as a paradigm case

One way to begin is to look to a paradigm of hard science: experimental physics. This is the focus of the key book by Pierre Duhem (1861–1916), a French philosopher of science and Pragmatist from the turn of the century, *The Aim and Structure of Physical Theory*.

EXERCISE 4 (5 minutes)

Look at the passage from:

Duhem, P. (1962). *The Aim and Structure of Physical Theory*. New York: Atheneum. (Extract pp. 145 and 147–8)

Link with Reading 12.2

On the assumption that it is descriptively accurate, what does the passage suggest about the relation between observation and theory? In particular,

- ◆ Does the relation depend on the facts particular to physics?
- ◆ Does the passage suggest any argument that observation statements *must* be couched in theoretical terms or just that they generally are?
- ◆ Does Duhem's description necessarily show that the Logical Empiricist semantic separation of observational and theoretical language must be false?
- ◆ What consequences does this have for theory testing and the specifically epistemological thesis that knowing the truth of an observation statement requires knowing the truth of some theory? Make some notes before going on.

Duhem powerfully suggests that observation statements are *typically* theory laden. However, he does not provide an argument that they *must* be. It is true that statements reporting what has occurred in an experiment typically or routinely employ theoretical

concepts. Such claims require theoretical understanding both in order to know whether they are true and also what they mean. Duhem points out that what one can learn from the observation of an experiment depends on one's understanding of theory as well as complex practical procedures and that once one has mastered this repertoire, one's reporting of the experiment is in theoretical terms.

But the argument does not undermine the Logical Empiricist distinction between theoretical and observational language. Duhem's description of the habits of experimental scientists could be true even if that distinction could always be made. The fact that scientists typically report experiments using a theoretical vocabulary does not show that they *could not* report them using a pure observation language. A Logical Empiricist might argue that this is typically what happens if an experimental result is disputed. Scientists can 'retreat' from the theoretical interpretation of their experiments to a purely observational account of what was actually seen. In other words, although the results of experiments are typically reported using theoretical concepts, this does not show that they have to be or that such reports cannot be given in theory-free terms.

The Duhem–Quine thesis

The same goes for the underlying epistemological issue. The retreat mentioned above might correspond to retreat to basic independent observational knowledge. In other words this might be knowledge that requires no knowledge of other theory. In fact Duhem himself would have disputed this claim. He shared



Fig. 12.2 Pierre Duhem

with the later American Pragmatist W.V.O. Quine the view that theories can never be tested in isolation. Other 'auxiliary hypotheses', as Quine called them concerning, for example, the working of measuring equipment, are always in play. Consequently if an observation appears to contradict a theory under test it is always possible to hold that theory immune and reject instead one or other auxiliary hypothesis. This pragmatist thesis is known as the *Duhem–Quine Thesis*. Quine gives a clear statement of his view in his influential paper 'Two dogmas of empiricism'. Using the word 'reductionism' to refer to a verificationist theory of meaning, which connects statements to methods of verification (see above), he says:

But the dogma of reductionism has, in a subtler and more tenuous form, continued to influence the thought of empiricists. The notion lingers that to each statement, or each synthetic statement, there is associated a unique range of possible sensory events such that the occurrence of any of them would add to the likelihood of truth of the statement, and that there is associated also another unique range of possible sensory events whose occurrence would detract from that likelihood. This notion is of course implicit in the verification theory of meaning.

The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My counter-suggestion... is that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body... The unit of empirical significance is the whole of science.

While a verificationist approach to meaning (or 'reductionism') is consistent with a separation of theory and observation, Quine thinks that it cannot be put into practice because testing individual statements always takes place in a broader theoretical background, which might include, for example, theories about the working of instruments used to make the observations.

But plausible though Quine's view is, it relies on *first* dismissing Logical Empiricist claims that observation statements comprise a privileged basis for theory testing and thus we still need a clear argument against the semantic thesis.

By contrast with the claim that observations are *typically* reported using theoretical terms, there are arguments for a stronger claim that all observation statements must be stated in theoretical language. This stronger claim certainly contradicts the Logical Empiricist model described above. It is advanced by the contemporary American philosopher of mind and science, Paul Churchland (1942–).

Churchland's argument: translation of observation statements implicates theory

Churchland's philosophy of mind and science

Together with his wife Patricia, Paul Churchland is most noted for his work on the philosophical consequences of connectionist



Fig. 12.3 W.V.O. Quine



Fig. 12.4 Paul Churchland

models of brain architecture. One element of that work is his argument that folk psychology—the ‘theory’ with which we explain and predict others’ behaviour—is a bad theory that should be replaced by a neurophysiologically informed scientific psychology. We will return to that in Part V. His argument in the following reading is related in one respect, however. It concerns the selection of theories which should inform our thinking about and observing the world.

EXERCISE 5

(30 minutes)

Read the short extract from:

Churchland, P. (1979). *Scientific Realism and the Plasticity of Mind*. Cambridge: Cambridge University Press (Extract pp. 8–10)

Link with Reading 12.3

- ◆ Summarize the arguments that Churchland offers concerning the connection between observational experiences and theory.

Paul Churchland offers two arguments to undermine the idea that the meaning of terms used to make direct observational claims depends on the observational experiences or sensations with which they are correlated.

His first argument turns on a thought experiment. Churchland claims that the sensations we have in colour experience could

be naturally produced in an alien species when its members directly experienced heat—perhaps through infra-red vision. If this were the case then the best interpretation or translation of their utterances would not be as incorrect judgements about colour—despite the similarity of their inner experiences of heat to our experiences of colour (or surface reflectance). Any such interpretation would be totally unreliable and fail to predict their utterances because there would be no systematic connection between the colours we detected and their utterances. Instead it would be correct to interpret their utterances as judgements about heat. The evidence for this would not be appeal to the alien’s inner states but the systematic connections between their utterances and what they were responding to in the world. This systematic connection between their utterances and between their utterances and the world comprises a primitive theory, in this case of heat. What drives correct interpretation is whatever primitive theory of heat their utterances presuppose. The inner experiences thus drop out as irrelevant.

Churchland’s second argument suggests that the experiences that we have and on whose basis we make direct perceptual judgements might be used to feed a different theoretical vocabulary. It could be, for example, that our ‘sensations of heat’ were used to drive judgements made using the vocabulary of caloric. The plausibility of this example is enhanced by the fact that although caloric is an outdated and false theory, the primitive theory of heat we generally use is also false in the light of physics but this does not stop us using it.

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Churchland concludes that what matters to the meaning of observational claims is the (possibly primitive) theory presupposed by the use of concepts, not the sensations driving them.

Is descriptive psychopathology based on 'inner sensations'?

Churchland's examples concern direct perception of heat and colour and he argues that the experiences that we have when perceiving these might either indicate the presence of entirely different features of the world, or might be used to drive a different theoretical vocabulary albeit concerning the same features. In these examples it is clear what the sensations are that might be so used: sensations of heat and colour.

EXERCISE 6

(10 minutes)

- ◆ Are there similar sensations in the case of descriptive pathology?
- ◆ If there are not, what consequence does this have for applying Churchland's arguments to mental health?

Think about these questions before reading on.

It is much more natural to say that one has sensations of heat or sensations of colour than sensations of a patient presenting schizophrenia. Perhaps the nearest we come to such talk, is among phenomenologists: the German psychiatrist and phenomenologist, Alfred Kraus, for example, reminds us of the importance on occasion of the 'praecox feeling' in the diagnosis of schizophrenia (Kraus, 1994, 2003). How can the phenomenological-anthropological approach contribute to diagnosis and classification in psychiatry? But even in the case of 'feelings' like these, what is involved is not inner sensations so much as unanalysable (perhaps because tacit) judgements about what is externally the case: namely here that someone has schizophrenia.

The general point, then, is that direct perceptual judgement that a particular symptom is being manifested in a patient, does not correspond to a characteristic inner phenomenology on the part of the clinician, corresponding with simple sensations such as those of colour and heat. This point is not special to psychopathology, of course. The same might also be said, reverting to our example at the start of this chapter, in the case of the 'sensation of a chair'. By picking the cases that he does, therefore, Churchland makes it easier to imagine that qualitatively the same sensations might be enjoyed by aliens under different circumstances.

Many philosophers, however, including Quine, Dennett, Rorty, and Wittgenstein, would dispute the validity of reifying the inner feeling of judgements in this way. In the philosophy of the mind this disagreement focuses on the existence or not of raw feels or qualia, as they are called. This is not to say that Churchland supports qualia. In the philosophy of mind, Churchland himself is an opponent of qualia. But the arguments

he presents here are most easily understood as initially presupposing qualia in order then to dismiss their importance for observation statements.

A behaviourist reconstruction

Churchland's arguments, then, generalized from simple sensations (such as heat and colour) to the more complex observations involved in medicine and science, seem somewhat less persuasive.

Nevertheless, arguments similar to those that Churchland gives could be advanced by a more behaviouristic philosopher—such as Dennett, or someone influenced by Wittgenstein or Ryle, who denied that judgements involve an inner phenomenology over and above sensitivity to aspects of the world. Such philosophers deny that perception of redness involves both sensitivity to an outer colour in the world and also an inner raw feel or sensation of redness. They hold instead that the misleading talk of 'sensations of redness' is a misguided way of referring to the outer sensitivity. A reconstruction of the first argument would simply say that the correct interpretation in the first, alien, case can *only* be based on outer behaviour and consequently the meaning of observational terms will depend on the systematic underlying theory they hold. Similarly, in the case of Churchland's second argument, one could simply say that even direct perceptions are couched in a systematic framework and that this system comprises a form of theory.

Reconstructing Churchland's arguments from a more behaviouristic point of view helps to clarify what they establish. Inner experiences are irrelevant for the interpretation of reports of direct perceptions. Instead interpretation relies on the systematic connections on the one hand between different utterances and on the other between utterances and the features in the world. This picture of interpretation is akin to Davidson's idea of radical interpretation, which is discussed in Part V.

This undermines a central motivation for separating languages of theory and observation. That separation seems plausible on the assumption that observation language is defined using inner sensations. Churchland provides an argument against that assumption.

Nevertheless it remains possible, if less plausibly motivated, to suggest that the basic 'theory' of the world that underpins interpretation might itself be confined to directly observational features of the world. In other words a kind of 'a-theoretical theory'. To suggest that this is not a plausible possibility we will turn to one more argument against the independence of observational and theoretical statements, to end this section.

Hesse's argument: any division of theory and observation is itself theory relative

No distinction of principle: Mary Hesse

Establishing the theory dependence of observation statements, requires a principled argument that no distinction can be drawn between theoretical and observation languages. That is what Mary Hesse, the English philosopher and historian of science,

attempts to offer in her book *Revolutions and Reconstructions in the Philosophy of Science* (1980).

Hesse presents a developmental story of how simple predicates (such as 'round' and 'green') are introduced and then become employed in a system of general laws such as 'balls are round' or 'in summer leaves are green'. As a result of the developmental process, she argues, this system of laws can introduce 'internal misfits and even contradictions', which in turn require revision in the definitions of predicates. Her claim is that no terms—'observational terms'—are immune to revision. She sums up her point thus:

To summarise, the developmental story entails that no feature of the total landscape of functioning of a descriptive predicate is exempt from modification under pressure from its surroundings. That any empirical law may be abandoned in the face of counter-examples is trite, but it becomes less trite when the functioning of every predicate is found to depend essentially on some laws or other and when it is also correct that any correct situation of application—even that in terms of which the term was originally introduced—may become incorrect in order to preserve a system of laws and other applications. It is in this sense that I shall understand the 'theory dependence' or 'theory ladenness' of all descriptive predicates'. Hesse (1980, p. 72)

In the next exercise we look at one of Hesse's examples from physics.

EXERCISE 7

(15 minutes)

Read the extract from:

Hesse, M. (1980). *Revolutions and Reconstructions in the Philosophy of Science*. Brighton: The Harvester Press (Extract pp. 77–78)

Link with Reading 12.4

- ◆ Extract from the historical detail the principled argument Hesse offers against a once and for all distinction between observational and theoretical arguments.

Hesse points out that the purpose of a distinction between theory and observation is to delimit an impartial test of theory. This is just to repeat the claim that the point of the Logical Empiricist two-language model, as a semantic model, is to provide reassurance on an epistemological issue about the impartiality of observation. But Hesse argues that what counts as an observation claim—what can be settled by observation—depends on which theory is presupposed. Her example in this reading is observations of time. For Newton, the simultaneity of two events was an objective and directly observable matter. For Einstein it was not directly observable because it also depended on considerations of relative inertial frames, the relative states of motion of observer and observed. For this reason simultaneity became perspective-relative. The concept of simultaneity was revised to become (more) theoretical.

A simpler case concerns observation of colours. Under normal circumstances judging the colours of objects does not seem to be a matter of theory. It is theory free. But it is also true that the colours of objects such as cars and buses can look different under different lights, although most of us would agree that the colours don't actually change: a red car is still red. Under abnormal conditions they can only be inferred given a suitable theory. Hence forming judgements about colours from direct perceptions requires that one knows that conditions are normal. But that conditions are normal is not itself a matter of simple observation.

Such a case exemplifies Hesse's claim. Relative to a background theory that conditions are normal we can say that colour perception is theory free. We do not need to *infer* the colour of observed objects. But this will not serve the purposes of Logical Empiricism of providing a theory-free language by which to test rival theories because the assumption that conditions are normal can be called into question and if it is that will change the status of colour judgement. If it is claimed that the atmosphere has been polluted and changed colour perception then judging the true colour of objects may become a theoretical inference.

The moral

The moral that Hesse invites us to draw is that there is no principled way of picking out an observation language because what counts as directly observable is itself a matter of theory. Any distinction between theory and observation is tentative and will not necessarily survive changes in theory. Consequently, it cannot

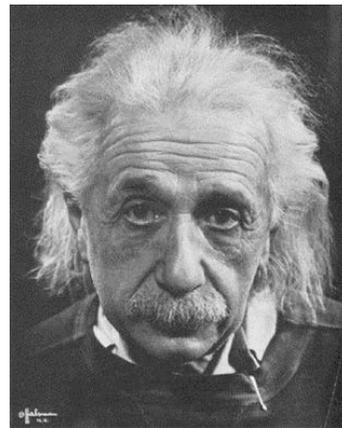


Fig. 12.5 Albert Einstein

be used to frame a theory neutral and impartial observation language. Any distinction presupposes theory and is thus not impartial.

Hesse's argument does not so much show that there cannot be a distinction between theoretical and observation languages as to present a powerful challenge to anyone attempting to define one. They must show that their observational terms are not observational merely in virtue of some assumed background theory. But it is difficult to see how this challenge could be met since the nature of the observational appears to depend on a range of theories. These include theories concerning human perceptual abilities or the working of scientific instruments when natural powers of observation are augmented by measuring instruments. But, as the example mentioned above suggests, what will count as observational will also depend on theories governing the particular subject matter of the science in question.

Hesse's argument and psychopathology

Hesse's argument applies to descriptive psychopathology in the following way. The aim of structured and semi-structured interviews such as the PSE was to establish a theory-neutral observational basis both to underpin diagnosis and to serve as a basis for research in broadly the way favoured by Logical Empiricism. But as Hesse argues, any such list of observable symptoms presupposes a background theory of what is and what is not observable. Thus it cannot act as an arbiter in cases where a difference of diagnosis or a different theoretical account of aetiology is the result of disagreement about what can and what cannot be directly observed. One physician's directly observable symptom or syndrome is another's mistaken theoretical construct.

What this suggests is that the idea of selecting directly observable psychopathological symptoms so as to provide a neutral basis for the construction and testing of theories cannot quite work. Not 'quite' because the judgement that certain phenomena are directly observable is itself a theoretical matter and thus the list arrived at need not survive a change of background theory. But that does not preclude devising, for example, classifications that are, to a first approximation, directly observable rather than a matter of inference according to best present theory. And thus the aim of devising classifications that are independent of premature causal theory is not ruled out. But even if that is successful it is not the same as suggesting that observation might be a matter of brute or raw data.

We return to the implications of the interweaving of the languages of observation and theory at the end of the chapter. First, though, we need to consider arguments showing that theory does not merely infect the sorts of observation statements codified in diagnostic manuals such as the PSE, but also the content of the experience or process of observation itself. It is to these that we turn in the next session.

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, looking back over the readings, write brief notes about the following:

1. What claim does the theory dependence of observation amount to?
2. What are the main claims advanced by each of Duhem, Churchland, and Hesse in the readings?
3. What conclusions should we draw for psychopathology?

Session 4 Arguments for the theory dependence of the content of the process or experience of observation

The previous session concerned arguments that aimed to show that the language used to frame reports of observations in science presupposes aspects of theory. To the extent that these arguments are successful they undermine the Logical Empiricist account of observation in which the languages of theory and observation are distinguished.

Logical Empiricism, as developed by the Vienna Circle, is important as a sophisticated attempt to provide a rigorous account of the first stage of the traditional model of science, as set out in Chapter 11, namely the making of observations that are themselves theory-neutral and hence can be used to test, or to differentiate between, rival theories. On the traditional model the initial description should be theory free if it is to serve as an impartial basis for theorizing. But the arguments discussed in the last session suggest that reports of observations (or observation statements), at least, cannot be, in the required sense, theory neutral.

But while, even if one accepts the force of those arguments, it is tempting still to think that there is something in the process or experience of observing that corresponds to gathering, or simply being struck by, brute data. Surely, the thought goes, when I open my eyes and take in my surroundings I am simply presented with a theory-free pattern of shades of light and colour, patterns in my two dimensional visual field. Theoretical interpretation comes later. In this session we will assess that assumption.

From selecting experience to experience itself

We have already had one indication that theory influences at least the selection of what we observe. You will recall that in the first exercise of this chapter, in which we 'observed' a chair, the particular features we picked out were determined partly by the chair

'out there', but also by our own prior 'theories', about what would be relevant for example, theories that are (pointing to one's head) 'in here'. What we decide to observe, then, depends to at least this extent on theory: relevant observations are chosen from the infinitude of possible features of our experience. We could not simply take note of everything and at every possible level of detail. This is uncontentious.

In this session, however, we consider arguments to the effect that theory also has a more intimate effect on observation: a phenomenological effect not just on what we select from our experience to attend to, but also on what we experience as such.

Empirical and conceptual sources

A final preliminary point is worth making. The arguments about the language of science described in the last session were derived from studies, historical and sociological as well as philosophical, of the 'hard' or natural sciences. The implication, therefore, was that if even in these disciplines, as paradigm sciences, no sharp distinction could be made between the language of observation and the language of theory, how much more so is this likely to be the case in the 'human' sciences.

The arguments to be described in this session, by contrast, although ostensibly concerned with the 'deeper' question of the natures of observation and theory themselves, have been influenced by the results of experiments carried out by psychologists on perception. In the previous session, then, philosophy had the role of challenging scientists to see that what they do, and hence how science works, is not quite (or at all) as they thought. In this session it is psychology, employing empirical research paradigms, which has helped to show that perceptions (the way in which we come to have experience) is not as any of us thought; and philosophy has then sought to explore why this is so. Both philosophers of mind and philosophers of science have based arguments on examples from basic psychological accounts of perception to more fundamental ideas about how experience in general has to be understood if we are to have rational contact with the world.

As between the last sessions and this session, then, there is a nice balance of empirical and conceptual, of understanding advancing through the interplay of experimental and analytic methods, of just the kind that is required, we suggested in Chapter 1, in mental health.

Card sorting: fallible but correctable perceptions

Before examining the main arguments, it will be worth getting clear what sort of connection between the perception, and prior theory is significant for the specifically philosophical concerns (albeit influenced by psychology) about the relationship between observation and theory. A passage from a book on the nature of science will help us to do this. The passage is taken from an accessible introduction to the philosophy of science written by Alan Chalmers who was based in the History and Philosophy of Science Unit of the Faculty of Science at the University of Sidney until his retirement in 1999.

EXERCISE 8

(5 minutes)

Read the passage from:

Chalmers, A.F. (1999) *What is this thing called science?*
Buckingham: Open University Press (Extract p. 25)

Link with Reading 12.5

Think whether the phenomenon it describes has any analogue in descriptive psychopathology. Does it raise a significant threat to the objectivity of the description of symptoms?

Expectations and observations

This sort of experience is probably familiar to everyone in every day life as well as clinical experience. We are influenced in our direct perceptual judgements by our expectations. Accurate expectations about what we are likely to experience makes judgement quicker and more reliable. False expectations hinder it. This is a general phenomenon and is found as much in mental health care as anywhere else. The description above adds to this familiar phenomenon the potentially more interesting claim that the subject's experience—rather than just their dispositions to make judgements—itself changed. This claim will be more dramatic to those who think of inner experience on the model of sensations. Those of a more behaviouristic cast will want to assimilate experience and dispositions to judgements and thus will not think that anything extra has been added to the everyday phenomenon about expectations and reliability.

But even with these qualifications, the card identifying experiment does not present a fundamental threat to the objectivity of our direct perceptual judgements. It simply shows something that should be no surprise: that even direct perceptual judgements are fallible. This effect can be reduced through repeated checking, double blind testing and other modifications of experimental protocol. It is no part of the traditional account of scientific method that observation is infallible, just impartial with respect to theory. In the above example, repeated checking of observations effectively removes the taint of theory. But there are arguments to say that observation is necessarily impregnated with theory. This is what is claimed as a consequence of consideration of the duck-rabbit ambiguous figure, which is the subject of the next exercise.

The duck-rabbit figure undermines an intuitive but misguided model of perception

EXERCISE 9

(15 minutes)

Look at the illustrations below. What can you see? What does this suggest about the role of concepts in experience? Can we generalize to more everyday cases of observation. Are any observations independent of concepts? Think about the general lessons that can be learnt about observation from examples such as the duck-rabbit and the Necker cube.

The duck-rabbit

Wittgenstein (1953) introduces this figure in the second part of his *Philosophical Investigations* in this way:

I shall call the following figure, derived from Jastrow, the duck-rabbit. It can be seen as a rabbit's head or as a duck's.

And I must distinguish between the 'continuous seeing' of an aspect and the 'dawning' of an aspect.

The picture might have been shewn me, and I never have seen anything but a rabbit in it. (p. 194)

Wittgenstein took the figure of the duck-rabbit from Joseph Jastrow (1863–1944) who was born in Warsaw but worked in the USA as an experimental psychologist. But figures of this sort were also a subject of 'Gestalt psychology' founded by the Czech psychologist Max Wertheimer (1880–1943).

Most people, when looking at the duck-rabbit, experience switches between perception of a duck and perception of a rabbit. The stimulus (the figure) has not changed, of course. But the very same features can be perceived either as a duck or a rabbit as though the picture itself had changed. Either aspect can dawn. If someone has not seen the ambiguity in the figure they might simply report that it is a rabbit (or perhaps a 'picture rabbit', a term Wittgenstein introduces). But if they do suddenly notice the duck aspect they might say 'Now it's a duck!'. Wittgenstein describes such an experience in a different case: 'I contemplate a face, and then suddenly notice its likeness to another. I see that it has not changed; and yet I see it differently. I call this experience 'noticing an aspect.' (ibid: p. 193)

What do cases of aspect dawning like this show?

Well, firstly, they establish the negative point that one common intuitive model of perception is false. According to this intuitive picture, to perceive something is to have an inner mental image of it. That mental image corresponds to raw brute data, impressed on a subject. But on this model, the best possible representation of seeing the duck-rabbit figure would thus simply reproduce it internally as a mental image. But that would not explain how we can perceive now the duck, now the rabbit aspect. The internal image could no more determine which aspect was perceived than the external picture can. (It is also worth thinking how reproducing the image inside the mind is supposed to 'solve' the problem of perception: how is the internal image itself perceived?)

Alongside that negative result, however, experience of the duck-rabbit figure also suggests the positive conclusion that our visual experience is, sometimes at least, affected by our conceptual resources. To see the protuberances in the figure as the rabbit's ears or as the duck's beak requires that one possesses the concepts of ears and beak. One needs to know what an ear or a beak is. This grasp of concepts, however, does not feature as a mere external feature of the experience of observing. It is not an interpretation of, or piece of ratiocination about, a neutral and prior image that is ambiguous between the duck and rabbit. Rather, the most basic experience is as of a *duck* or of a *rabbit* (or

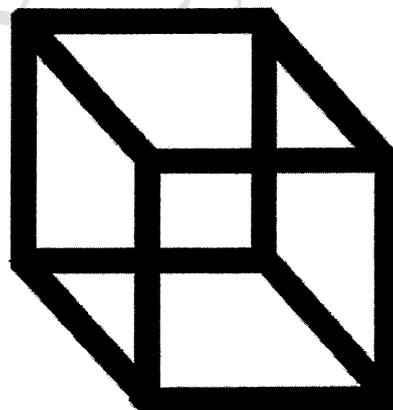


Fig. 12.6 A Duck-Rabbit and a Necker Cube

of a duck part or rabbit part if concentrating on a smaller area of the figure). That is the nature of the experience.

It may be tempting to think that there must be more basic components of perception from which our experiences are built up: typically, one might think that one perceives the lines that make up the figure and the rest is interpretation of these lines: a two component model. But in fact it takes some skill to learn to see the figure merely as lines. And our ability or lack of ability to reproduce it—to sketch it—suggests that our normal understanding of the figure is of a whole image: a duck or a rabbit from which we might then abstract the lines. (Compare copying letters in an alphabet one knows with, say, pictograms with which one is unfamiliar. This suggests that recognizing the letters as letters is more basic than recognizing them as lines.)

Furthermore, the experience of an aspect dawning, expressed in the utterance 'Now it's a rabbit!' suggests that there is no more basic way to describe that experience without referring to rabbits, their habitats, etc. The experience of seeing the rabbit aspect of



the duck–rabbit is not one of interpretation of something directly seen. Instead, the rabbit aspect is thrust upon one.

Seeing as seeing-as

In a book called *Patterns of Discovery*, the Wittgensteinian philosopher of science Norwood Hanson drew the conclusion from psychological experiments of this kind that perception is always affected by one's concepts (Hanson 1958). This might be put by saying that all seeing is 'seeing-as'. As well as sometimes seeing the duck–rabbit as a duck, we also see ducks as ducks (and correspondingly for rabbits). If so, then the duck–rabbit switching phenomenon is an unusual sign of something more general: that we always experience what we observe under some conceptual aspect. The chair aspect of an observed chair does not need to dawn on us as we cannot see it as anything else. We see, not shapes that we secondarily interpret as a chair, but, directly, 'chair'. It is only when the stimulus, as in the duck–rabbit figure, is ambiguous, that we are aware of this 'seeing as', but it is a feature of perception as a whole.

The conclusion for the philosophy of science that Hanson draws is that observation cannot be separated or insulated from one's general conceptual repertoire including one's general theoretical understanding of the world. Consequently, what we see when we observe the world depends on the theories we already have. Seeing is conceptually structured.

Phenomenology and meaning: a philosophical aside

Hanson's conclusion—that perception is never a matter of receiving brute data but is instead conceptually structured—is correct. We will look at another argument for it at the end of this session. But it ignores some subtleties in Wittgenstein's discussion, which are also worth drawing out.

After introducing the duck–rabbit figure Wittgenstein goes on to say:

Here it is useful to introduce the idea of a picture-object. For instance, [bare sketch of a face] would be a picture-face.

In some respects, I stand towards it as I stand towards a human face. I can study its expression can react to it as to the expression of the human face. A child can talk to picture-men or picture-animals, can treat them as it treats dolls.

I may, then, have seen the duck–rabbit simply as a picture-rabbit from the first. That is to say, if asked, 'what's that?' or 'what do you see here?' I should have replied: 'a picture-rabbit'. If I had further been asked what that was, I should have explained by pointing to all sorts of pictures of rabbits, should perhaps have pointed to real rabbits, talked about their habits or given an imitation of them.

I should not have answered the question 'what do you see here?' by saying: 'now I am seeing it as a picture-rabbit'. I should simply have described my perception: just as if I had said 'I see a red circle over there'. Wittgenstein (1953, pp. 194–195)

In these passages Wittgenstein hints at two important ideas that he elaborates later. One is that the relation between continuous

aspect perception and aspect dawning is complicated. On the one hand, continuous aspect perception involves the spontaneous use of a description (for example, 'a picture-rabbit') rather than an interpretation of lines and shapes and thus shares some of the spontaneity of aspect dawning. But on the other, there seems to be nothing paradoxical about continuous aspect perception and in this it contrasts with aspect dawning when a description is wrung from one e.g. 'Now it's a rabbit!', when one simultaneously knows that nothing has changed. Wittgenstein suggests that the report in a case of aspect dawning is like an expression of pain, e.g. 'It hurts', which manifests the particular experience one is having.

Secondly, while aspect dawning suggests something about the 'logic' of perception and centrally that a two component model cannot work, Wittgenstein also suggests that it is a contingent feature of human vision. The fact that we can directly experience two aspects in the duck–rabbit is merely contingently true. He suggests that there could be subjects for whom this was impossible and who were 'aspect blind'. The hint of this idea in the passages above is the reference to standing to a picture as one does to a real human face, or children playing with pictures as with dolls. That suggests an immediacy of response in our normal phenomenology that might not be true of all possible subjects. To understand this it is useful to recall where this discussion takes place.

Wittgenstein's discussion of the duck–rabbit and aspect-perception takes place in the second part of his book *Philosophical Investigations* (1953). The first part contains an extended discussion of what it is to understand the meaning of a word. At the time, understanding the meaning of a word was widely regarded as involving some process of interpretation by 'reading off' from a set of rules. Wittgenstein, in the first part of *Philosophical Investigations*, shows that understanding a word cannot be explained as the successful interpretation of a rule because this leads to an infinite regress: one needs an interpretation of the interpretation.

In the succeeding discussion of perception Wittgenstein suggests that it would be possible to be 'aspect blind'. Someone who is aspect blind cannot experience a changing of aspects looking at, for example, the duck–rabbit. However, they would still be able to understand such pictures as one does circuit diagrams. A further example he gives of the difference between those who are, and are not, aspect blind is between those who can experience meaning in a word and those who cannot. The kind of experience he means is that which can be lost by repeating the same word until it seems to lose its meaning. But even someone who can never hear the meaning directly in a word might still be able to use it perfectly correctly and thus know what it means.

Such understanding would be of an intellectual sort. It is tempting to say, in the visual case, that it would involve deriving what the picture is of from the lines by a set of rules rather than more immediately seeing the object in the picture. Someone who is aspect blind would then have to work out what the picture is

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rather as one has to work out what an information blueprint is about, i.e. by reference to a set of rules of interpretation. But we know from the earlier part of *Philosophical Investigations* that Wittgenstein denies that understanding in general can be a matter of interpretation of a set of rules. So while the aspect blind would have to have a relationship to pictures akin to our relationship to blueprints, in neither case is that explained through having to interpret what is seen.

This suggests that the discussion of aspect dawning is not as simple as Hanson's account implies. Hanson is right to stress that perception—in both continuous aspect perception and aspect dawning cases—involves concepts and that the very possibility of aspect dawning suggests that a two-component model of vision is wrong. On that model, perception involves first taking in raw data and then interpreting it. But it cannot explain aspect dawning.

On the other hand, aspect dawning is a contingent feature of human perception. It would be possible, according to Wittgenstein, to be able to perceive but without having the phenomenological immediacy that we enjoy. For humans, at least, it is possible to relate to pictures, for example, as surrogates for fellow humans, to hear meaning directly in words and so on. Wittgenstein is keen to stress the importance of this engaged perspective on the world. It will be useful now to chart a disagreement between Hanson and Churchland, both of whom agree that observation is theory laden rather than a matter of brute data.

Hanson on seeing the sunrise

As we have seen, Hanson argues that all perception depends on one's general conceptual repertoire and consequently on the theories one holds about the world. As a result, he suggests that Tycho and Kepler saw something different at 'sunrise'. He suggests that the former saw the sun rise and the latter saw the earth turn because of their different theoretical commitments.

Let us consider Johannes Kepler: imagine him on a hill watching the dawn. With him is Tycho Brahe. Kepler regarded the sun as fixed: it was the earth that moved. But Tycho followed Ptolemy and Aristotle in this much at least: the earth was fixed and all other celestial bodies moved around it. *Do Kepler and Tycho see the same thing in the east at dawn? . . .*

[S]omething about their visual experiences at dawn is the same for both: a brilliant yellow-white disc centred between green and blue colour patches. Sketches of what they both see could be identical—congruent. In this sense Tycho and Kepler see the same thing at dawn. The sun appears to them in the same way. The same view, or scene, is presented to them both . . .

[S]aying that Kepler and Tycho see the same thing at dawn just because their eyes are similarly affected is an elementary mistake. There is a difference between a physical state and a visual experience.

Kepler and Tycho agree on [much] . . . Their visual fields are organized in much the same way. Neither sees the sun about to break out in a grin, or about to crack into ice cubes. (The

baby is not 'set' even against these eventualities.) Most people today see the same thing at dawn in an even stronger sense: we share much knowledge of the sun. Hence Tycho and Kepler see different things, and yet they see the same thing. That these things can be said depends on their knowledge, experience, and theories.

Kepler and Tycho are to the sun as we are to [the duck-rabbit], when I see the [duck] and you see only the [rabbit]. The elements of their experiences are identical; but their conceptual organization is vastly different. Can their visual fields have a different organization? Then they can see different things in the east at dawn.
Hanson (1958, p. 5–18)

The only way of resisting the conclusion that they saw something different would be to invoke the notion of a common brute datum as the two-component model postulates; however, that idea is rejected because of consideration of aspect dawning.

But is the claim that Tycho and Kepler see something different plausible? Do we all now see the earth turn rather than the sun rise?

Churchland on seeing the sunrise

Churchland (1979) takes issue with Hanson on this point. He claims that, as a matter of phenomenological fact, Tycho and Kepler, like the rest of us, see the same thing. None of us can help but see the sun rise. That 'theory' is too embedded to escape from merely by learning Kepler's alternative.

Churchland claims that we report observations using a theoretical language and that theory can affect perception, but he does not think that our experience is automatically transformed by any particular theory we hold. Thus although we may all report sunrise as the earth turning when attempting to speak accurately, Copernican theory does not inform our perceptions themselves. We have to re-educate ourselves to perceive in accordance with our theories. The connection between the act of perception and the theories we hold is not, according to Churchland, as close as Hanson claims it is. They agree that our perceptions are affected by our theories. They disagree on which theories our perceptions are affected by.

Churchland claims that one can teach oneself in certain conditions to 'see the earth spin' instead. This involves certain practical steps such as finding a suitable location in which the earth's horizon is unarresting and turning one's neck so that one's head points in a direction perpendicular to the ecliptic. It helps if there are planets visible so as to make the plane of the solar system clear. When successful it seems that the earth is spinning rather than the sun is rising. (Of course, unlike a fairground round, there are no physiological sensations of movement.)

What is the disagreement between Hanson and Churchland?

Both Hanson and Churchland agree that observation is theory laden but they disagree about how to apply that claim to the



apparently simple case of the sunrise. What should we learn from this?

By looking in a little more detail at Wittgenstein's discussion of aspect perception we can adjudicate this debate. The disagreement does not threaten the basic idea that observation is not a matter of brute data but always, instead, conceptually structured. Concepts play a fundamental role in shaping of perceptual experience. But aspect perception also suggests that perception has a further (and contingent) immediacy, which is shown in the way perceptual experience of the duck-rabbit can 'flip' between the two aspects. (In addition, Wittgenstein suggests this is connected to our ability to experience meaning in the use of a word and lose it if the word is repeated.)

Churchland's account of the experiences one can have at sunrise look to a case of this latter phenomenological fact. Success in seeing the earth turn is like success in seeing the duck or rabbit aspect of the duck-rabbit. The criterion of achieving it might be saying 'Now it seems as though the earth is turning' like 'Now it is a duck'. If so, while it deepens our understanding of how perception, concepts, and experience are interconnected the disagreement between Churchland and Hanson is, strictly, orthogonal to the main issue, which is that, in Churchland's phrase, observation is a matter of the conceptual exploitation of the senses.

The coup de grace for Logical Empiricism?

This point marks a return to the Logical Empiricist emphasis on the importance of observation statements or reports. So it is worth recapping the argument of this session. Hanson argues that the experience of changes of aspect (of the duck-rabbit) shows that all seeing is seeing-as and thus directly concept-involving. That may be true—and we will look at another argument for it shortly—but it also neglects some of the subtlety of Wittgenstein's discussion. Wittgenstein's account of aspect blindness suggests that there is something immediate about human perception, which, in principle, need not be a feature of all perception. This in turn helps shed light on the disagreement between Churchland and Hanson who otherwise agree that experience involves concepts.

This leaves the following question. If conceptualized observation reports are what are relevant for theory testing and if these do not depend on the interpretation of neutral sense data (as Hanson, Wittgenstein, and Churchland all agree), how exactly are they connected to experience? This is the question to which the Logical Empiricists had no answer. And for that reason, there was something mysterious about the status of observation reports. Indeed, as we will note in Chapter 16, Imre Lakatos, drawing on this tradition, was forced to say that we adopt observation statements *by convention*. But surely, observation statements are derived, in some way, from experiences? We may choose to drive on the left or the right by convention but surely there is more to observation statements than that?

One possible answer to this question is that sensations or experiences merely *cause* trained observers to make specific sorts of observation report. Sensations or experiences, that is to say, have a role as part of the causal process that leads to an observational report. This is the kind of account offered by behaviouristically minded philosophers like W.V.O. Quine (see his *Word and Object*, 1960). But it raises the question of what makes the report the *right* sort of report to make about the world. A merely causal story here makes reports of observations no more rational responses to the world than the noise made by an apple falling from a tree on to the ground. Reports of observations, if merely caused by sensations or experiences, thus seem unlikely to be any more *about* anything than the noise of the apple falling is *about* gravity. We will return to these matters in the philosophy of thought and language in Part V. But a final short description here will serve as a tempter to these philosophically deep waters.

More general considerations of the role of experience suggest that it is conceptually structured

McDowell is a contemporary philosopher who has written mainly on the philosophy of thought and language. *Mind and World* is a series of lectures in which he attempts to present a coherent overall metaphysical account of the connection between our experience of the world and the world—of mind and world (McDowell 1994). Broadly, he attempts to unite a Kantian framework with more recent analytic and continental philosophy. The first chapter focuses on the question of how our experiences can provide a rational test for our beliefs and thus serve to connect our beliefs with how the world is. McDowell's key thought, although somewhat understated, is that only if there *is* such a rational connection, can beliefs actually have the content or meaning that they *do*. Epistemology—in this case having reasons for beliefs—and the philosophy of thought and language go hand in hand.

McDowell thus reverses the normal order of philosophical battling. Most philosophers have sought arguments from the nature of perception to show how it is that we can have rational beliefs about the world. McDowell takes the fact that we do have rational beliefs about the world to show that it is in the essential nature of perception that it is conceptually structured.

But what is the Kantian framework?

The Kantian framework

One way to think about the relation of theory and observation in experience is to start with a slogan from Kant's first Critique: the *Critique of Pure Reason*. Kant says: 'Thoughts without content are empty, intuitions without concepts are blind' (Kant, 1929, p. 93, A51, B75).



This phrase expresses the interdependence of two aspects that together make experience possible: concepts and intuitions. Concepts are the responsibility of the faculty of spontaneity that looks after active judgement. Intuitions are the responsibility of the faculty of receptivity, which takes in how the world is. The slogan suggests that in empirical judgements, experiences of how the world is, the two aspects work in harmony. Thoughts require some intuitive content, and intuitions must be structured by concepts.

Some commentators such as Sellars and McDowell take it to express two further and potentially stronger claims. First, that thought in general is only possible because of the interplay of concepts and intuitions. In other words, experience is a condition of possibility of thought in general. Thus to say 'Thoughts without content are empty' is to say not that they are a special sort of empty thought but rather no thought at all. Secondly, that there is no such thing as pure receptivity. Experience is always a synthesis of conceptual and intuitive elements, but it would not be possible for us to have intuitions that were purely receptive—conceptual content is always involved.

Whether or not these stronger claims are accurate accounts of Kant—and we will return to McDowell's account shortly—the discussion of the thesis of the theory dependence of observation can be related to the slogan in this way. No observation is pure receptivity, a matter of merely passively taking in features of the world. Empirical judgements and the experiences they depend upon are always a synthesis of a worldly input in a conceptual form.

There is a further point worth making here that is also suggested by something else Kant says in a chapter on the 'The schematism of the pure concepts of understanding'. The point can be put in the following thoroughly un-Kantian way. Imagine that experiences are put together like this: the subconscious mind takes in some purely intuitive content, then selects an appropriate concept and synthesizes a conceptually structured experience such that a subject can, e.g., see that there is cat in front of her. The conceptualized content of the experience is given by what follows the 'that': *there is cat in front of me*.

But now consider how a suitable concept—'cat'—is to be picked. Not just any concept will do. The purely intuitive content cannot be squeezed into just any conceptual clothing. A cat does not look like an elephant, for example. So it looks as though a rule is needed that relates a class of intuitions to each general concept: cat, dog, elephant, etc. If so the particular intuition can now be compared with the rules and thus a concept selected. This, however, suggests a regress. The rule that connects the concept 'cat' to a class of intuitions will have to represent that class in some general way. And that looks to be a kind of concept. If so there will have to be another 'second order' rule to select which 'first order' rule should be used to select an appropriate concept. This initiates an infinite regress. One way out of this, although not Kant's way who, arguably devoted his third Critique in part to addressing this problem, is to block the regress at the start. Once

one has been initiated into a language, one's experiences are always conceptualized. Part of what it is to grasp a concept such as 'cat' is to have mastered an ability to have cat experiences. The pure intuition described in the thought experiment above is a myth. This is of a piece with the denial that observation can involve taking in raw data.

We will now turn to a sketch of McDowell's argument.

Foundationalism and Coherentism

McDowell provides a schematic argument that experience is always conceptually structured. He argues that observation *must* be always already conceptualized for there to be a *rational* connection between our beliefs about the world and the state of the world itself. Thus, he invites us to consider the alternative. Suppose that we conceive of observation as giving us *non*-conceptualized experience, then we have two possibilities:

1. Either experience merely *causes* us to have the beliefs we have and a causal connection is not a rational connection. This would be a form of Foundationalism in which the ultimate grounds for our beliefs would be brute unconceptualized sense data. McDowell calls this the Myth of the Given, arguing that without a rational connection to states of the world, our beliefs would not be *about* anything.
2. Or our beliefs are justified only by *other beliefs* in which case we do not have a grip on the world. McDowell calls this a form of Coherentism and dismisses it with the claim that it leaves our beliefs as no more than a 'frictionless spinning in the void' and thus, again, not *about* anything.

The label 'Coherentism' picks up a distinction between two rival views of truth. Correspondence theorists suggest that truth is correspondence with a fact. A belief is true if it corresponds with the facts. But that leaves the problem of explaining what 'correspondence' and 'fact' mean in ways that do not simply presuppose, e.g. 'truth' and 'belief'. Coherentists claim that a belief is true if it coheres with other beliefs. But they then have to explain both what 'coherence' means and show that only true beliefs cohere.)

Concepts play a part in experiences as well as beliefs

Having thus rejected both Foundationalism and Coherentism, McDowell argues that the immediate output of observation must be always already conceptualized. The 'space of reasons' extends out to the most worldly part of our mind: our experience of the world. When we open our eyes, concepts that we have learnt actively to apply in judgements or beliefs are passively drawn into operation in experiences. McDowell argues that this enables us to be directly open to the world in experience. He goes on to claim that:

[T]here is no ontological gap between the sort of thing one can mean, or generally the sort of thing one can think, and the sort of thing that can be the case. When one thinks truly, what one



thinks *is* what is the case. So since the world is everything that is the case . . . there is no gap between thought, as such, and the world. Of course thought can be distanced from the world by being false, but there is no distance from the world implicit in the very idea of thought. (p. 27)

Of course, this leaves the question of how one can distinguish what is true from what is false. That question might be asked in the philosophical context of a discussion of scepticism or in a practical context of choosing between diagnoses and would receive different kinds of answer. But McDowell aims to separate that question from the more basic question of how thought can make contact with the world. His answer is that providing one is not in error, the connection is very direct.

Observation and concepts for antipsychiatry

At the start of this chapter we mentioned a concern about psychiatric observation that runs as follows. If observation is connected to classification in the way exemplified by the PSE then it involves a distortion because patients' experiences are forced into pre-existing categories rather than recognized for their individuality.

With McDowell's argument in place, however, we can see that this objection trades on the false assumption that observation could ever be concept-free. As every observation is conceptually structured there is no possibility of completely unconstrained 'taking in' of patients' experiences.

Of course this is not to say that labelling is never distorting nor that the concepts in play at any time should not be critically reflected on and revised in the light of theory. However, it does help remove the false promise that observation could be carried out without preconceptions. It is precisely preconceptions that give observation its voice.

Unanswered questions

McDowell's picture leaves several questions to be thought through. Here are two:

1. Our experience is always already conceptualized because we are language users and thus already possessors of concepts that can be actively employed in judgements. These same concepts are then drawn passively into play when we open our eyes to the world. What then of the experience of non-language users?
2. What is the connection between McDowell's philosophical claim that experience cannot be broken down into non-conceptual elements and neurophysiological work on the underlying causal processes of vision?

We will return to these issues in Part V. In the final session of this chapter we will look briefly at some of the implications of these issues by comparing the complexity of observation in psychopathology with recent work revealing the complexity of observation in the hardest of the hard sciences: physics.

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. How are concepts implicated in the experience or process of observation? What lessons can be learnt from experiments on the observation of bogus playing cards?
2. What lessons can be drawn from aspect figures such as the duck-rabbit?
3. What do they imply about the role of concepts?
4. What, if any, significance does our ability directly to experience the dawning of an aspect have for scientific observation?
5. How do general Kantian considerations count against the idea of raw data?

Session 5 The consequences for observation in psychiatry and in physics

Observation and theory, then, for any given science, are, if Hesse is right, interconnected in an essentially open-ended way: as theory develops so what counts as *dry* observation will change, in turn changing theory, and so on, without limit, or at any rate without a limit that can be either predicted or defined a priori, within the science itself.

It is important to emphasize that Hesse has in mind here, primarily, not the psychological and social sciences, but the hardest of hard sciences, physics. Her example cited above is taken from special relativity. A still deeper interweaving of observation and theory is evident in the development of quantum mechanics. The inherent limits on the observation of physical variables defined by the German physicist, Werner Heisenberg's (1901–76) complementary formalisms are well known: pairs of variables previously thought to be independent turn out to be complementary in the sense that, for a given experimental set-up, the more accurately you choose to observe one of a complementary pair of variables, the less accurately will you be able to observe the other: observe the *position* of a particle closely and its *momentum* (its state of motion) will be correspondingly uncertain.

Less well recognized than complementarity, but involving an even more radical break with a traditional understanding of observation, is what has become known as entanglement. As a feature of quantum mechanics, the puzzles generated by entanglement were first made fully explicit by a thought experiment



Fig. 12.7 Heisenberg



Fig. 12.8 Neils Bohr

called the Einstein–Podolsky–Rosen paradox, the ‘EPR’ paradox. It will be worth spending a few minutes on this. It takes us to the heart of the difficulties about observation in science, and indeed related difficulties about the nature of objectivity and the role of the subject, as these difficulties arise not speculatively in philosophy but in practice at the very cutting edge of physical theory.

As is well known, Einstein, early in his career, besides more or less single-handedly founding relativity, made crucial contributions to the development of quantum mechanics: his analysis of the spectrum of radiation given off by a hot but not glowing object, for example, the so-called ‘black body radiation’, was critical to establishing that energy is (under certain experimental set-ups, at least) ‘quantised’, i.e. broken up into discrete packets. Yet Einstein remained throughout his life deeply dissatisfied with quantum mechanics. Relativity, he believed, although novel, was consistent with a well-behaved traditional understanding of the world and of how science gives us access to it. Quantum mechanics, by contrast, Einstein insisted, is both probabilistic, and, more troubling still from a classical point of view, in some respects solipsistic—it winds the observer, and what the observer chooses to observe, in a most unclassical way into what is observed.

Einstein’s concerns about the probabilistic nature of quantum mechanics were summed up in his famous aphorism ‘God does not play dice’. Quantum mechanics works with probabilities: it gives, for example, the probability that a radioactive particle will decay over a given period giving off a particular pulse of radiation. Einstein argued that behind these probabilities there must be ‘hidden variables’, which, with future developments in

instrumentation, will translate the probabilities of quantum mechanics into the determinate observations and predictions of classical (including relativistic) physics. The situation with fundamental particles, Einstein argued, is in principle similar to, say, tossing a coin. Behind the apparently chance fall of a coin as heads or tails, lie hidden but none the less in principle observable variables—mass, energy, etc.—by which the actual path of an actual coin is rigidly determined.

Quantum mechanics denies Einstein’s (common sense) determinism (putatively) underlying its probabilities. In quantum mechanics the probabilities—the fall of God’s dice—go all the way back. There are no hidden variables. What you see (observe) is what you get.

Notwithstanding Einstein’s opposition, the ‘what you see is what you get’ interpretation of quantum mechanics rapidly became the standard or ‘Copenhagen’ interpretation in the 1920s—‘Copenhagen’ because it was argued with particular effectiveness by the Danish physicist, Niels Bohr, who was based in Copenhagen. Bohr’s arguments were based on complementarity. Tying down one of a complementary pair of variables more tightly will necessarily, according to quantum mechanics, loosen up the other. This, Bohr argued, showed that there cannot be a determinate set of discrete variables ‘lying hidden behind’ quantum mechanical observations.

Enter the EPR paradox! Einstein produced a whole series of counter-arguments to the Copenhagen interpretation, all of which Bohr was able successfully to counter, except one, the EPR paradox. The paradox arises from a ‘thought experiment’

devised by Einstein together with two graduate students, Podolsky and Rosen. We do not have space to go into the details here: published originally in *Physical Review Letters B*, an authoritative non-mathematical account has been given by the French theoretical physicist, Bernard d'Espagnat (1983). The essence of the EPR thought experiment is that it distinguishes, in principle, between, on the one hand Einstein's classical (hidden variables) interpretation of quantum mechanics, and, on the other, the Copenhagen or standard (no hidden variables) interpretation. The experimental set up in the EPR, that is to say, provides a test of the two interpretations, which does not depend on the demonstration (or failure to demonstrate) hidden variables as such. You set the experiment up: if you get one result there *are* hidden variables there waiting to be discovered; if you get a different result there are *no* hidden variables there waiting to be discovered.

The rub, though, has Einstein, Podolsky, and Rosen pointed out, is that the nul result, the standard Copenhagen no-hidden-variables result, was absurd. It required a direct and simultaneous influence of the observer (and of what the observer chooses to observe) not only in the immediate vicinity of the experimental set-up but also at (in principle) unlimited distances across the universe. Simultaneous action at a distance of this kind is bad enough from the point of view of a traditional model of science (it is literally impossible in relativity theory). That the experimental result, including such action at a distance, is a product not (just) of the physical system but of what the observer chooses to do, is tantamount to solipsism. Far from a clean separation of observer and observed, then, as in a traditional model of science, the results of the EPR thought experiment, if the experiment comes out consistently with the predictions of quantum mechanics, mean that the properties of a physical system (including parts of the system now at remote distances from the observer) are (in part) actually determined by the observations that the observer chooses to make. Absurd! Einstein concluded. Does the moon cease to exist, he said, in another famous quip, this time against the solipsism of quantum mechanics, when no one is looking at it?

Well, the absurd happened! To cut a long story short (given in full in d'Espagnat's book, above), in the 1960s a Northern Ireland physicist, John Bell, working at CERN (the Centre Européen pour la Recherche Nucléaire near Geneva), devised an ingenious mathematical treatment of the EPR paradox that made it possible in principle actually to carry out the EPR thought experiment; in the 1970s, with the development of lasers, the John Bell version of the original EPR thought experiment was carried out for real (by a number of researchers, but most decisively by Alain Aspect at the Institut d'Optique Théorique et Appliquée at Orsay in France); and the results, unequivocally, were *for* the 'absurd' (no-hidden-variables) Copenhagen interpretation of quantum mechanics and *against* Einstein's traditional and common-sense (hidden variables) interpretation.

What are the morals of this story for psychopathology as the observational basis of psychiatric science?

First, for theory. The EPR story shows the difficulties of observation and interpretation of experience in psychiatry, compared with bodily medicine, in a positive light. As we saw in Part I, these difficulties, in the twentieth century, were the basis of stigmatizing 'psychiatry second' attitudes. A traditional model of science, premised as it is on clear-cut objective observations as the basis of experimental tests of theory, inevitably led to pejorative comparisons between psychiatry and, say, cardiology as scientific disciplines. The EPR story shows, substantively, what Duhem, Quine, Churchland, Hesse, and others had suggested philosophically, that the traditional model, in this as in other respects, oversimplifies the nature of science. If, therefore, scientific progress at the cutting edge of the hardest of hard sciences, has complicated rather than resolved our understanding of the relationship between observer and observed, how much more will this be true with the added complications arising from the more explicit merging of observer and observed in the psychological sciences.

Just *how* observation and theory fit together, remains an open question. Even in quantum mechanics the Bell-Aspect results deepen rather than resolve this question. Any future physical theory must incorporate the Bell-Aspect results including their (by traditional standards) absurd implications. But just what such theories will look like remains to be seen. This indeed is an area in which the psychological sciences may, in the future, provide insights for the physical sciences rather than vice versa (Fulford, 1989, p 274). John Bell himself made the point strongly, in an interview with another theoretical physicist, Paul Davies, on the BBC's Radio 3 (Davies and Brown, 1986, p. 48):

Davies: So that these issues [about the nature of observation in physics] haven't been fully resolved, at least to your satisfaction?

Bell: Absolutely not. And the experiment of Aspect and the Einstein-Podolsky-Rosen correlations do not help to resolve this problem, but make it harder, because Einstein's view that behind the quantum world lies a familiar classical world was a possible (and now discredited) way of solving this measurement problem—a way of reducing the observer to an incidental role in the physical world.

Second, for practice. Important as the EPR/Bell/Aspect story is for showing the limitations of the traditional picture of observation in science, the story might seem to 'practical' men (recall Bertrand Russell's use of this term in Part I, p 114), too abstract and arcane to be of practical significance. The story was indeed, for a long time, neglected even by most physicists as being rather too close to mere philosophical 'playing with words'. Interesting, yes, was the standard line, even ingenious; but it could never be used for anything practical (like sending a signal). And certainly the original motivations behind the story were theoretical rather than, directly, practical. But as is the way with theoretically motivated research, there are signs, already, that the EPR/Bell/Aspect story could provide the window through which a whole new physics is opened up to us.

We return to the nature of progress in science, and to the way in which long periods of settled theory may suddenly and

unexpectedly be disrupted, in Chapter 16. Einstein was responsible for one such disruption at the start of the twentieth century, with relativity; and (in part) for a second, in quantum mechanics. It may be that with the EPR/Bell/Aspect story, Einstein will end up as being (reluctantly) responsible for a third disruption, at the start of the twenty-first century! But the moral for psychopathology is that deep theory, explorations of the concepts underpinning research and practice in mental health, is not antithetical to practical outcomes. Such explanations are not driven by practical pay-offs. It may be that work of this kind is actually inhibited if connected too directly to practical pay-offs, at least of the kind defined by current science. But the lesson of history is that science progresses in practice through a combination of theory and observation, of conceptual and empirical studies, working together in a complex, and perhaps not fully codifiable, but none the less mutually interdependent way.

Third, no easy option. A third moral of the EPR/Bell/Aspect story for psychopathology, is that the theory dependence of observation is no easy option. Philosophers and perhaps even more sociologists of science, have sometimes appeared to conclude from their several demonstrations of the theory dependence of observation, that science is, in a word, bunk. This is unwarranted. Science is more complicated than in the traditional model. But it is not 'bunk' because it works: sociologists working in the 'strong' programme, notwithstanding their hostility to scientific truth, still went to brain surgeons for brain surgery. Nor, indeed, is even the distinction between observation and theory 'bunk'. Recall Putnam's point (from Part I, p 13), that such distinctions may not be dichotomies (capable of being driven all the way back). But they may still be useful tools for conceptual analysis. Useful, and, even, necessary: for of course the very demonstration of the theory dependence of observation implies the distinction (between observation and theory) some have taken it to deny.

So, work of this kind is not a way out of difficulty, but a way in. It sharpens up and highlights difficulties, producing results, theoretical and practical, not so much by 'solving' problems as by giving us deeper insights into some small part of the whole. There is a danger of obscurantism here, of course, of a retreat to what the Italian philosopher and historian of science, Paolo Rossi (2003), identifies as 'magical' thinking, insights reserved to the elite or chosen. As Rossi has so carefully charted, the scientific Renaissance of the fourteenth and fifteenth centuries consisted in a hard won climb up out of the thralldom of magical thinking towards '... logical rigour, experimental control ... and the public character of results and methods ...' (Rossi, 2003, p263). It was towards just such objectives for descriptive psychopathology that Wing and others, in developing instruments like the PSE, rightly aimed. It is towards the same objectives that a new philosophical psychopathology, building on twentieth century advances, must aim. The specifics will be different from quantum mechanics (the subject matters are different). But the principle is the same, that achieving a mature psychopathology will require, among other things, not the rejection of observation, but a deeper

understanding of its nature and role at the heart of science and hence of psychiatric science. It is to psychiatric science as reflected in current international classifications of mental disorder that we turn in Chapter 13.

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 general lessons for observation in psychopathology are suggested by recent work in physics (as in the EPR paradox)?
2. What role does this suggest for the PSE?

Reading guide

For further discussion of structured interviews see Farmer, McGuffin, and Williams (2002) *Measuring Psychopathology*.

Logical Empiricism and the Vienna Circle

A good account of the Logical Empiricist two-language model in a secondary text is Harold I. Brown's (1977) *Perception, Theory and Commitment*, especially chapters 1 and 3.

A useful collection on aspects of Logical Empiricism is Oswald Hanfling's (ed.) (1981) *Essential Readings in Logical Positivism*.

For an account of the Verification Principle in Logical Positivism see Waismann's 'Verification and definition' and Schlick's 'Meaning and verification' in Oswald Hanfling's (1981) *Essential Readings in Logical Positivism*.

The theory dependence of observation

Other accounts of the theory dependence of observation in the philosophy of science can be found in Bird's (1998) *The Philosophy of Science*, chapter 4, Chalmers' (1999) *What is This Thing Called Science?*, chapter 3, and Ladyman's (2002) *Understanding Philosophy of Science*, chapter 4.

A cognitive psychology approach is provided by William F. Brewer and Bruce L. Lambert (2001) 'The theory-ladenness of observation and the theory-ladenness of the rest of the scientific process', which responds to an influential paper by Fodor (1984, 'Observation reconsidered') in which he criticizes Churchland.

Perception and concepts

The view that all seeing is a form of seeing-as is defended from a phenomenological perspective in Mulhall's (1990) *On Being*

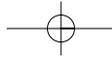
in the World, which compares Wittgenstein and Heidegger. Wittgenstein's account is discussed in McGinn's (1997) *Wittgenstein and the Philosophical Investigations*.

McDowell's difficult book *Mind and World* is summarized by McDowell himself and discussed by a number of other philosophers in the *Journal of Philosophy and Phenomenological Research* (Vol. 58, 1998). Thornton's (2004) *John McDowell* is a secondary text covering his philosophical work as a whole.

The opposing view that at least some forms of perception turn on non-conceptual content can be found in Gunther's (2003) *Essays on Nonconceptual Content*.

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