CHAPTER 14

Diagnosis, explanation, and tacit knowledge

Chapter contents

The central role of diagnosis

In this chapter we move from symptoms of mental disorder and their classification to the diagnostic process: how we come to explain or claim knowledge of someone's condition. Diagnostic explanation in medicine, as we saw in Chapter 3, is partly in terms of syndromes (identifying a patient's symptoms with a recognized pattern that carries implications for treatment and prognosis) and partly in terms of aetiology (causal information).

In general medicine, diagnostic explanations are more in terms of aetiology; in psychiatry and nosology, they are more in terms of symptoms and syndromes (they are 'descriptive', in the sense of that term used in ICD and DSM, see Chapter 13). Either way, though, diagnostic explanations are perceived as a species of scientific explanation. This prompts the question, therefore, just what a sound scientific explanation consists in. This question, as we will see, has been the subject of close scrutiny in the philosophy of science, and, although no definitive answers have been given, a number of key points have been established which are relevant to our understanding of the nature of scientific explanation in general and hence of diagnostic explanation in particular.

Diagnostic difficulties

As with many other aspects of medicine, psychiatric diagnosis has proven peculiarly problematic. One reason for this, highlighted above (Chapter 13), is that the aetiological theories, available in general medicine, have been much harder to establish for the brain. Hence psychiatry, from ICD-9 and DSM-III onwards, has focused, consistently with a traditional model of the process of science, on what can be directly observed, i.e. symptoms and signs. An important benefit of this, as we saw in Chapters 12 and 13, has been clearer specification of the rules (criteria) for establishing a given diagnosis. This in turn has led to improvements in reliability, which, it is (reasonably) assumed, reflects a closer approach to objectivity in psychiatric diagnosis, and, as a further aim, to the future establishment of causal accounts comparable with those available in other (non-neurological) medical disciplines. But this prompts the following general question that will be examined in the first half of this chapter. Can scientific explanations in general, and hence diagnostic explanations in particular, be analysed to provide an objective measure, a standard of correctness that any diagnosis has to pass? Is there a logical recipe that governs the form of every diagnosis? Can diagnosis, as a species of scientific exploration, be fully codified?

The focus of the first half of the chapter

To consider this question the chapter will begin by examining formal models of explanation from the philosophy of science. The reason for this is that both diagnosis and explanation share a common element. Both are ways of bringing bodies of general knowledge to bear on particular cases or facts. Work in the philosophy of science has produced a number of formal models of explanation that may thus shed light on diagnosis, as a species of explanation.

The main conclusion from the first half of the chapter, in Sessions 1 and 2, will in fact be a negative conclusion, namely that the project of full explicit codification of what counts as a sound scientific explanation, is not fully completable. This is an important conclusion particularly for psychiatric diagnosis, because it shows that, for all the benefits explicit codification has brought, something more, something additional to the critera spelled out in our official classification, will be needed for sound diagnostic explanation. This is not because psychiatric diagnostic explanation is unscientific but because of a feature (i.e. resistance to full explicit codification) that it shares with scientific explanation in general.

This leaves a gap, however, between the explicitly codified rules of diagnostic classification and their applications in diagnostic practice.

This suggests a second general question to be addressed in this chapter: Can diagnostic expertise in general be codified? Or does it, by contrast, rely on an element of tacit or implicit knowledge that resists such codification?

These two questions—about the possibility of a formal model of diagnosis drawing on work on explanation, and about the role of tacit knowledge—are distinct. But we will look at both in this chapter because, if developing a formal model of diagnosis is not finally successful, it suggests, at the very least, the possibility of a role for tacit knowledge that might also be independently supported by philosophical argument. That is what we will find. Philosophical models of explanation suggest a role for both laws and for causality in a proper understanding of diagnosis. But they also suggest the importance of tacit knowledge embodied in experience and practical skills.

The plan of the chapter

The plan of the chapter, then, is this:

1. The first session will examine Hempel's logical model of explanation: the Deductive-Nomological (DN) model.
2. The second session will concern two less formal models of scientific explanation that stress the role of causation and also context.
3. The third session will examine arguments for the claim that there is an element of uncodifiable tacit knowledge present in engineering and physical sciences.
4. Finally in the last session we will turn to the issue of whether a science of the mind presents particular problems for the formal regimentation and representation of knowledge and how it is applied to particular cases. This last session will provide a link between the phenomenological account of diagnosis explored in Part II and the scientific approaches discussed here. It will also lead directly into the discussion of reasons and causes in the next chapter, Chapter 15.

Session 1 The Deductive-Nomological model of explanation

Diagnostic explanation: descriptive and causal

To get started it is necessary first to have some clear idea of what is involved in diagnosis. That is the purpose of the first exercise.

EXERCISE 1 (15 minutes)

Think about the nature of medical diagnosis. What does diagnosis add to a description of signs and symptoms? What makes a diagnosis correct? What is the purpose of diagnosis? Look at the DSM-IV criteria below and think whether the general conclusions apply in this case.

DSM-IV diagnostic criteria for Attention-Deficit/ Hyperactivity Disorder

A. Either (1) or (2):

1. six (or more) of the following symptoms of **inattention** have persisted for at least 6 months to a degree that is maladaptive and inconsistent with developmental level:
   a. often fails to give close attention to details or makes careless mistakes in schoolwork, work, or other activities
   b. often has difficulty sustaining attention in tasks or play activities
   c. often does not seem to listen when spoken to directly
   d. often does not follow through on instructions and fails to finish schoolwork, chores, or duties in the workplace (not due to oppositional behavior or failure to understand instructions)
   e. often has difficulty organizing tasks and activities
   f. often avoids, dislikes, or is reluctant to engage in tasks that require sustained mental effort (such as schoolwork or homework)
   g. often loses things necessary for tasks or activities (e.g., toys, school assignments, pencils, books, or tools)
   h. is often easily distracted by extraneous stimuli
   i. is often forgetful in daily activities
2. six (or more) of the following symptoms of **hyperactivity-impulsivity** have persisted for at least 6 months to a degree that is maladaptive and inconsistent with developmental level:

   Hyperactivity
   a. often fidgets with hands or feet or squirms in seat
   b. often leaves seat in classroom or in other situations in which remaining seated is expected
   c. often runs about or climbs excessively in situations in which it is inappropriate (in adolescents or adults, may be limited to subjective feelings of restlessness)
   d. often has difficulty playing or engaging in leisure activities quietly
   e. is often 'on the go' or often acts as if 'driven by a motor'
   f. often talks excessively

   Impulsivity
   g. often blurts out answers before questions have been completed
   h. often has difficulty awaiting turn
   i. often interrupts or intrudes on others (e.g., butts into conversations or games)

B. Some hyperactive-impulsive or inattentive symptoms that caused impairment were present before age 7 years.

C. Some impairment from the symptoms is present in two or more settings (e.g., at school [or work] and at home).

D. There must be clear evidence of clinically significant impairment in social, academic, or occupational functioning.

E. The symptoms do not occur exclusively during the course of a Pervasive Developmental Disorder, Schizophrenia, or other Psychotic Disorder and are not better accounted for by another mental disorder (e.g., Mood Disorder, Anxiety Disorder, Dissociative Disorder, or a Personality Disorder).

Diagnosis, explanation, and causal information

Medical diagnosis is usually based on the signs and symptoms presented by a patient to a clinician. But the aim of diagnosis is usually more than just the articulation and description of those signs and symptoms. It usually involves a further inference. This, however, is disguised in the recent concentration in DSM and ICD on operational criteria for diagnosis.

Consider two contrasting cases. One informal indication of diabetes used especially among itinerant workers in the middle of the twentieth century was the presence of white spots on footwear.

This works as an indication because of the following connections. Diabetes raises the level of sugar dissolved in urine. Whether or not suffering from diabetes, men sometimes splash their footwear when urinating. In the case of diabetics, however, when such splashed urine dries it leaves a precipitate of sugar: a white spot on the footwear. And, because they are, at some remove, *effects* of it, these spots are an *indicator* of diabetes.

In this case there is a clear gap between the signs and what they are signs of. Diabetes does not *comprise* having white spots on the footwear. It takes an inference, a judgement, to move from them to the underlying illness condition.

Things seem less clear in the criteria for attention-deficit/ hyperactivity disorder (ADHD) set out above. In this case there is more of a temptation to say that ADHD is not so much indicated by the behavioural criteria as simply being, or being constituted

by, that pattern of behaviour. If so, it may seem that no further inference is required once the signs are articulated. But, in fact, whether or not that is a fair characterization there would still be a judgement in place even so in moving from the specific signs and symptoms of a particular patient to the general diagnosis of ADHD. The application of the general concept to the specific presentation is still a work of judgement.

Thus on the assumption that there is a further inference involved in diagnosis, what sort of inference is it? What constrains it? We will consider two suggestions. The first is that a diagnosis provides an *explanation* of symptoms. The other is that diagnosis aims to provide *causal information* about the symptoms.

By taking diagnosis to be a form of explanation we can shed light on it by looking at some of the more influential philosophical work on explanation. While both of the influential models of explanation considered below have difficulties they suggest the importance for explanation of both laws and of causality. (The problems turn on how *exactly* these play a role.) At the same time one of the morals that looking at these models will raise is that an explanation is offered in a particular context and given particular interests. This does not itself strictly imply that explanation, and thus diagnosis, necessarily contains a tacit element but it is suggestive.

Hempel's formal models of explanation

The classic model of explanation, which has dominated the philosophy of science during most of the twentieth century, is Hempel's DN model. You will recall Hempel's influence on psychiatric classification in Chapter 13. The effect of his work on scientific taxonomy by the psychiatrists working on ICD and DSM was to shift away from a premature aetiological focus to a descriptive or symptom-based approach.

Hempel's model of explanation dovetails with the Logical Empiricist model of science because it provides a *logical* recipe for satisfactory explanation. The recipe specifies the logical structure in which the sentences that comprise an explanation have to stand. By providing a required formal structure, the model aims to provide necessary and sufficient conditions for scientific explanation. It aims to establish what a satisfactory explanation is in a general and objective manner, independently of the particular subject matter being explained. Hempel described his model in many different papers, making various adjustments.

EXERCISE 2 (20 minutes)

Read the short extract from:

Hempel, C.G. (1999). Laws and their role in scientific explanation. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 299–315 (extract pp. 301–302)

Link with Reading 14.1

- What are the key conditions that, according to Hempel, any explanation has to meet.

Two sorts of explanation

Hempel characterizes two forms of explanation: DN explanation and Inductive Statistical (IS) explanation. To begin with we will focus on the former because the central ideas behind it are clearer.

Explanation as a logical argument

In very general terms, the key idea behind the DN model is that explanation is a *sound argument that establishes the truth of its conclusion*. The sentence that describes the event to be explained (the explanandum sentence) forms the conclusion of an argument whose premisses (the explanans sentences) are sentences that state general laws and initial conditions. By equating explanation and logical argument, Hempel aims to use logical machinery to stipulate the form of any satisfactory explanation. In other words, Hempel's model is an abstract or formal model that aims to account for all forms of scientific explanation independently of their subject matter. Thus, on the one hand, the model promises to deliver an objective model of adequate explanation. On the other, it presupposes that explanation can always be so codified. This general assumption needs to be substantiated.

Hempel's two conditions

In *Philosophy of Natural Science* (Hempel, 1965), Hempel puts forward two quite general conditions for satisfactory scientific explanation:

1. explanatory relevance;
2. testability.

The first is explicated through the notion of giving grounds for belief. The explanatory information provided should afford good grounds for believing that the phenomenon to be explained did, or does, occur. It is a *necessary condition* for explanation that the information that is offered is relevant to what occurred.

The second condition requires that information offered as explanation should be substantial and make a real claim about the world. Hempel exemplifies this through a contrast with the vacuous explanation of gravitational attraction as a natural tendency in the universe. Such a claim lacks content and makes no concrete claim about the world. Crucially, as far as logical empiricism is concerned, it is incapable of being tested. (This emphasis on testing reveals the verificationist aspirations of Logical Empiricism.) The statements constituting a scientific explanation must be capable of empirical test.

Hempel's four conditions

These two conditions are further explicated and augmented elsewhere in Hempel's *Aspects of Scientific Explanation* (Hempel, 1966). There he lists four conditions:

1. the explanation must be a valid deductive argument;
2. the explanans must contain essentially at least one general law;
3. the explanans must have empirical content;
4. the sentences constituting the explanans must be true.

The first three are called *logical conditions of adequacy*. The last is called the *empirical condition of adequacy*. The former concern the *structure* to which explanations must adhere. One can imagine establishing that these are met without knowing anything of the nature of the world in which we live. The latter, however, connects explanation to this world. To establish that it is met requires knowing what the world is like, which facts obtain.

Explanation is a 'success' concept . . .

This way of explaining the difference between the two sorts of conditions does, however, risk one distortion. Hempel stresses that explanations must be true as opposed to being highly confirmed by evidence. In other words explanation is a 'success concept'. This idea can be explained by another example of a success concept: knowledge.

. . . like knowledge

The traditional analysis of *knowledge* has three components:

1. justified
2. true
3. belief.

If a candidate as a piece of knowledge lacks any of these components or if any one of these conditions is not met, then it fails to count as knowledge. This means that if a belief is not true, even if it is justified by available evidence, it is not knowledge. Similarly, if is true but not justified (a lucky guess perhaps), it is not knowledge. Thus our claims to knowledge are fallible in a strong sense. If they are false they are not merely 'false-knowledge' but not knowledge at all. The same goes for explanation on Hempel's model. The requirement that the explanans is true implies that putative or supposed explanations in which this condition is not met, such as explanation of diseases by reference to humours, are not just false explanations, but not explanations at all.

An example explanation

Hempel's first two conditions on explanation stipulate the kind of logical argument that explanation is supposed to consist in. A simple example might comprise:

1. the general law that objects acted upon by a net force accelerate in the direction of that force at a rate inversely proportional to their mass;
2. the initial claim that some such object was subjected to a net force of such and such;
3. and the conclusion that it accelerated in such and such a manner.

Given the initial condition and the general law then the conclusion follows by the logical argument form called *Modus Ponens* (see Chapter 5). In this logical structure, if the premisses are true, then the conclusion *must* also be true. Thus the premisses make it rational to believe the conclusion. The argument gives grounds to believe that the conclusion is true.

These characteristics concern DN explanation proper. Such explanation requires deduction from exceptionless laws. If the DN model were supposed to serve as a model of all scientific explanation then it would be seriously flawed for the following reason. There are many fields of science that do not provide exceptionless laws and instead employ merely statistical or probabilistic laws. The laws of most if not all of the special sciences such as geology, economics, psychology are either formally statistical or couched with 'ceteris paribus' clauses. (The latter include examples such as: in general, a wandering river erodes its banks.) But the unavoidability of statistical laws is not merely a reflection of imperfect nature of these sciences. As we are all by now very familiar, the physics of the very small (quantum physics) suggests that probabilistic laws are an inevitable consequence of the nature of the world rather than merely the result of our ignorance. As a result of this, Hempel proposed his second model alongside that of the DN account: the IS model.

The Inductive Statistical model

The IS model is similar to the DN model in that it is based on the intuition that an explanation is an argument that would support the truth of the explanandum. But the argument in this case is merely a statistical or probabilistic argument for the likelihood of the conclusion. IS explanations are not strictly valid arguments because the truth of the premisses does not guarantee that the conclusion—or explanandum—must be true. While at first sight this might seem a minor difference it has at least one paradoxical consequence of which Hempel was aware. The same event can be given contradictory 'explanations' depending on how it is described.

One example runs as follows (taken from Brown, 1977, p. 58):

> Consider a case in which Jones contracts a streptococcal infection and is treated with penicillin. One explanation of what happens next can be formed in the following way. The general, statistical law is: the probability of recovering from a streptococcal infection when treated with penicillin is close to one. This can be conjoined with the particular fact that Jones has a streptococcal infection and was treated with penicillin. The result is an IS explanation of the fact that Jones recovers from the streptococcal infection.
>
> But if Jones is over 80 years old a different and contradictory explanation can be framed. In this case the general law is that the probability of a man over 80 years old not recovering from a streptococcal infection is close to one. The particular condition is that Jones is over 80 years old and has a streptococcal infection. What is explained is that Jones does not recover from streptococcal infection.

Thus whatever the outcome in this case, an appropriate IS explanation can be given. By contrast with DN arguments, two different explanations can be formed with true premisses but leading to contradictory results. Thus the apparent similarity between the two forms of argument disguise fundamental differences.

For our purposes in this chapter, however, the differences between the IS and DN models are less significant than their similarities.

EXERCISE 3 (10 minutes)

Think back to the criteria for ADHD set out at the start of this session. How would a diagnosis for ADHD be formalized using Hempel's model of explanation. What would be the explanans and explanandum? How would laws of nature figure? How might the difference between DN and IS be relevant?

Explanation and the codification of diagnosis

We are now in a position to see how the DN and IS model might shed light on diagnosis. Observed symptoms take the place of the explanandum. They are what have to be explained. General laws are provided by psychiatric theory and any particular conditions are provided, for example, by aspects of patient history. A successful diagnosis is thus one which makes clear how the observed symptoms derive from these general laws together with particular facts about the patient's life. A diagnosis sheds light on symptoms by showing how they are part of a more general picture and the way it does this can be codified or formalized by using Hempel's model.

In the case of ADHD, for example, the explanandum might take the form of signs and symptoms from a clinical encounter or a case vignette. They are what will be explained by the diagnosis. The explanans is the combination of the general laws and particular conditions. In this case, the general laws are those that describe the general behavioural features of sufferers of ADHD and the fact that the service user has ADHD. Thus the conjunction of the particular claim that a particular person has ADHD and with the general claim that anyone with ADHD will present with such and such signs and symptoms implies that the person in question will present with such and such symptoms.

With this model in mind we can see how the general laws present may be statistical rather than deductive. Looking back to the behavioural criteria we can see that they are disjunctions. Thus just because someone has ADHD does not imply that they have the sign: 'often fidgets with hands or feet or squirms in seat'. That is one of nine criteria, six of which have to be present to satisfy merely one disjunct of the overall criterion A. So at best, the diagnosis raises the probability of that sign.

Providing this analogy between explanation and diagnosis holds good—and it seems to—and providing that Hempel's formal models of explanation are sound, then it seems that a formal model can be specified for medical diagnosis. The hope, remember, is that if a formal model can be provided then diagnosis will be shown to be capable of objectivity. It will no more (and no less!) depend on subjective features of psychiatrists—on what chunks of information they choose to give and what general laws they choose to quote—than science in general depends on subjective features of scientists.

This is, of course, not to say that diagnoses might not go astray as a result of personal biases any more than that attempts at explanation might prove to be false and result from scientific error or bias. But they will fail by failing to meet up to the public and objective standards established by the formal logical model of what a diagnosis ought to be.

Objections to Hempel's model

Hempel's models have been very influential in the philosophy of science; however, they have also been subject to much criticism. Van Fraassen—a contemporary analytic philosopher of science whose work on realism was discussed in Chapter 13—summarizes a number of these, which are discussed below, in *The Scientific Image* (1980, pp. 97–112).

Van Fraassen raises a number of objections to the simple DN and IS models of explanation. Most of these objections are long standing and well known to both proponents and critics. We will focus on four examples. Their purpose here is not simply to point out that these particular models of explanation are flawed. Perhaps the models can be modified so as to escape these particular criticisms. (Hempel himself has continued to fine tune them for many years.) Rather the objections will serve to deepen our understanding of explanation in general and thus of diagnosis in mental health care.

Four counter-examples

The objections are:

1. The flagpole example: Is there a symmetric explanation for both the height of a flagpole and the length of its shadow?
2. The barometer example: Is explanation symmetric in the way that providing reasons for belief seems to be?
3. Birth control pills: Can the fact that contraceptive pills prevent pregnancy explain why a man who takes them does not himself become pregnant?
4. Syphilis and paresis. What is the explanatory relation between having untreated syphilis and developing paresis?

We will explain them in turn. In the first three examples the conditions for DN explanation can be met for cases that, pre-philosophically, we would deny were bona fide explanations. The fourth presents a slightly different case.

The flagpole

In the flagpole example, there are general laws governing trigonometry that can be applied to the straight path of light, straight flagpoles impeding it and the straight shadows that result. There are also particular facts. These comprise the angle that light from the sun makes with the ground, the height of the flagpole and the length of the shadow. Pre-philosophically there is one particular fact that can legitimately be explained among these by the other two. We can explain the length of the shadow with respect to the angle of the sun and the height of the flagpole. This explanation can be formalized in accord with the DN model. Given the general laws, and two of the particular facts (the angle of the sun and height of the flagpole) the third (the length of the shadow) can be derived.

But according to the DN model this is not the only way to assemble the laws and facts into an explanation. The model implies that either the angle of the sun, or the height of the flagpole can be explained by invoking the other fact in conjunction with the length of the shadow and the laws of trigonometry. Both of these 'explanations' accord with the DN model's prescriptions concerning the logical form of explanation. Neither, however, accord with our pre-philosophical intuitions about explanation. Intuitively there is an asymmetry about what can be explained here. The length of the shadow is explainable using the other facts and laws. But an explanation of why the flagpole is the height that it is or why the sun's light strikes the earth at its particular angle to the plain of the ground will have no use for information about the length of flagpole's shadow.

This asymmetry might also be expressed in this way. There is an asymmetry between explanation and the provision of grounds for belief. Knowledge of the length of the shadow and the height of the flagpole provides *reason to believe* that the angle of the sun is such and such and likewise for the two other particular facts. Any of the pairs of particular facts can, given the laws of trigonometry, provide reasons to believe the remaining fact. But only one fact can be *explained* by the other two. Thus, contra Hempel's original idea, explanation and having reason to believe are not symmetric.

The barometer

This last point is encapsulated in the barometer example. A barometer embodies a way of predicting the weather on the basis of current atmospheric pressure. It provides reasons to believe that the pressure is such and such and thus that the weather will be so and so. But that the barometer gives a certain reading does not explain why the pressure is such and such and why the weather will be so and so. Instead, pre-philosophically we should say that current pressure explains both the barometer's reading and the future weather, the former by simple physical laws, the latter by complex meteorological laws. But despite this, the DN model allows that any of these particular facts could be explained given the others and the laws.

Male birth control

Jones takes his wife's contraceptive pills. No one who takes contraceptives falls pregnant (let us assume). So Jones does not become pregnant.

The birth control example presents the following problem. It meets the logical conditions on explanation provided by the DN model. But, intuitively, it fails as an explanation. As men do not themselves become pregnant in any case, the fact that Jones takes his wife's birth control pills is irrelevant to his not becoming pregnant. In the earlier statement of conditions concerning explanation, Hempel attempted to preclude cases like this by stipulating that the explanans should be *relevant* to the explanandum. But the way this is 'unpacked' in the more explicit statement of four conditions is not successful. There, relevance is replaced by the prescription that the explanandum should follow logically from the explanans. That condition is met in the birth control example.

As our intuitions diverge strongly from the DN model in these three cases it is important to determine why this is so.

EXERCISE 4 (5 minutes)

Think about what principled reasons there might be for rejecting these putative explanations that the DN model allows? What conditions might be added to the DN model to rule out such cases?

Consider also the following claim: There may be *no* principled reasons for rejecting these cases. Could this claim be true?

Causation

The most pressing objection to the possibility of explaining the height of the flagpole by citing its shadow is that this gets things the wrong way round. The shadow depends on the flagpole and not vice versa. This dependence is *causal*. The height of the flagpole is part of the condition that *causes* the shadow to be the length that it is. The shadow by contrast does not causally affect the flagpole's height or the sun's angle. Similarly, the barometer reading is caused by atmospheric pressure but does *not* cause it.

The general moral that these cases suggest is that one cannot explain causes in terms of their effects, although one can explain effects by citing their causes. The asymmetry of the relationship between cause and effect explains the asymmetry highlighted in these examples between explanans and explanandum.

Reasons for belief and causation

It is worth noting that the provision of reasons to believe does not depend in this asymmetric manner on causal relations. Causes can give reasons to believe in effects *and vice versa*. Barometer readings are caused by atmospheric pressure and thus serve as instruments that respond to and detect it. In fact this is the general principle behind detecting instruments. They are designed in such a way that their readings are the causal effects of the phenomena they detect. But knowledge of causes can also provide reasons to believe that their effects will obtain. Thus knowledge of the height of the flagpole (*inter alia*) provides reason to believe that the shadow will be of a certain length. Likewise, knowledge of underlying pathologies can give reason to believe that their causal symptoms will come about but equally, knowledge of symptoms, can give reason to believe in specific underlying pathologies.

Modifying Deductive-Nomological explanation?

One response to these counter instances is to augment the basic DN model by the claim that the explanans cause the explanandum. In the next session we will examine the general plausibility of this suggestion. But note that Hempel's aim was to provide a topic-neutral account of explanation through the provision of

a purely formal and logical recipe for constructing good explanations. Adding in causation as an extra ingredient threatens that neutral account. It will impose a causal element into *all* forms of explanation. But is that a plausible claim? Think especially about those cases in psychiatric explanation that turn on the meaningfulness of certain sorts of action or reaction. There is a long-standing view in both the philosophy of science and of mind that reason explanations involve a different sort of intelligibility to subsumption under law. (We will return to this issue in Chapter 15, which will contrast subsumption of events under causal laws with rationalizing them by placing them in the 'space of reasons'.) Consider also that many diagnoses in neurology as well as psychiatry assimilate a case to a symptomatically defined category rather than specifying an underlying causally defined aetiology.

Before going on to consider the role of causation in explanation further below, remember that there is a different sort of reaction available to counter-instances to the philosophical model of explanation so far discussed. This is to give up the hope that there is a unified account of explanation that all proper explanations are instances of. It is too soon to adopt such a position yet, but it is worth keeping in mind that the assumption that every concept has an analysis is a *philosophical* assumption and rather a dubious one at that.

Syphilis and paresis

The fourth criticism listed above concerns syphilis and paresis. This example was introduced to philosophers in 1959 by the philosopher Michael Scriven. Scriven took the facts to be these: paresis or neurosyphilis occurs in patients that have progressed through the primary and secondary stages of a syphilis infection without antibiotic treatment. About one patient in four develops paresis. This is the only route to paresis. (Changes in empirical findings concerning paresis do not affect the basic point raised.)

EXERCISE 5 (15 minutes)

Consider the following questions:

- Can we explain why someone has paresis by saying they have untreated syphilis?
- Does this fit the DN or IS model from Hempel?
- Does the putative explanatory power depend on context?
- How does this case differ from the example of ADHD as regards Hempel's models?

The idea that we can explain the development of paresis by citing untreated syphilis turns on the thought that had the subject not contracted syphilis, or had it been treated, then they would not have developed paresis. Untreated syphilis is a causal step towards paresis and by preventing it, one can prevent paresis. So to that extent it looks as though it can sometimes be at least part of an explanation. Clearly this at least accords with the causal component of explanation that has been isolated above. At least part of the reason for saying that untreated syphilis explains paresis is that there is a causal connection between them.

But on the other hand, the fact that a patient suffers untreated syphilis does not provide reason to believe that he or she will develop paresis because the chances of developing it are small. Most subjects with untreated syphilis do not develop the condition. So again the example highlights a difference between explanation and have reasons for belief in the effect. In this case, it does not even seem that the explanans makes the explanandum likely.

We will return to the question of context later. But the question highlights the fact that there may be differences in the explanatory power of the explanans given the context. Think of the differences between asking the question of why Jones has paresis if Jones visits his general practitioner versus asking it if Jones is on a hospital ward for those with previously untreated syphilis.

There are a number of differences between this case and that of ADHD discussed above. In this case, the putative explanation of paresis is *causal*. It is an aetiological account albeit probabilistic (in 1959 at least). By contrast the ADHD case looks to fit the DN or IS model even though it seems to turn on *pattern recognition* at the level of syndromes rather than underlying causal mechanism.

We will develop some of these points by looking to a more explicitly causal model of explanation 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 it that diagnosis adds to a description of a patient's signs and symptoms? Why might diagnosis involve a form of inference? If so, what form of inference?
2. What is the nub of Hempel's model of explanation? What kind of thing is an explanation?
3. What role do laws play? How does explanation relate to prediction?
4. What should we learn from the counter examples to Hempel's formal models?

Session 2 A causal model of explanation

David Lewis, an influential American metaphysician, develops the causal strain in explanation that the counter examples to the DN model bring to light. Philosophical theories of the nature of causation will be the subject of Chapter 15. Here we will concentrate on the role of causation in explanation.

EXERCISE 6 (15 minutes)

Read the extract from:

Lewis, D. (1993). Causal explanation. In *Explanation* (ed. D-H Ruben). Oxford: Oxford University Press, pp. 182–206 (extract pp. 182, 185, 187) (This is a reprint of original from *Philosophical Papers*, 1986, Vol. ii. Oxford: Oxford University Press, pp. 214–240)

Link with Reading 14.2

- To what extent does Lewis's account of explanation offer guidance on what a good explanation amounts to?

The key idea on which Lewis' account is based is that events or facts are explained by providing information about their causes. This accords with the intuition above that explanation and causation are interconnected. Rather than adding a further causal condition to the DN or IS models, Lewis simply says that explanation comprises provision of causal information however that is provided. This change of approach marks a further fundamental difference.

More or less explanation

Lewis' example of crash investigation supports his claim that the notion of *complete* explanation is of no practical significance. As explanation is, according to Lewis, the provision of causal information and as there is no practical limit to the causal information that could be provided short of a history dating back to the Big Bang, explanation is always partial. All explanations provide less than the full causal story and what they do provide is selected on the basis of one's explanatory *interests*. Thus in different contexts, different explanations might be given for the same car crash.

Take the example of a car crash whose causal history includes both of the following facts (and an almost infinite number of other factors). The driver had consumed sufficient alcohol to slow his or her reactions and the car was equipped with ordinary brakes that, in the event, locked on. All other things being equal, if either the driver had not consumed alcohol or the car had possessed an antilocking braking system, the crash would not have occurred. Depending on one's interests in biology or engineering one might thus say that the explanation of the crash was alcohol consumption or having poor brakes. Providing that both these features played a causal role, neither is straight-forwardly a better candidate for *the* explanation. Both are partial explanations but citing one or the other may be more relevant given one's interests.

The importance of Lewis's 'explanatory interests' in causal explanations may seem to run counter to the 'objectivity' of science. But so far as medicine at least is concerned, the Swedish philosopher, Ingmar Lindahl, has produced direct empirical evidence for precisely this interest-led feature of causal explanations in his empirical study of attributions of causes of death in hospital records (Lindahl and Johansson, 1994). In the context at least of medicine, then, Lewis' theoretical work on scientific explanation in general, combined with Lindahl and Johansson's empirical study, provides a clear illustration of the goal-directedness of science noted in Chapter 11. This in turn connects with one of the key themes of this book, spelled out particularly in Part I (on Concepts) and Part IV (on Values), of the pervasiveness and importance of evaluation, alongside description, at least in the medical sciences.

Causal information and diagnostic validity

Thus the attraction of something like Lewis's approach can also be seen in contemporary thinking about diagnostic categories in psychiatry. Robert Kendell and Assen Jablensky put the following suggestion in 'Distinguishing between the validity and utility of psychiatric diagnosis' (2003):

> [A] few diagnostic categories in psychiatry are almost universally accepted as valid. Most of these categories designate causes of mental retardation or dementia, such as Down's syndrome, fragile X syndrome, phenylketonuria, Huntington's disease, and Jacob-Creutzfeldt disease. (p. 8)

They go on to suggest that a test for validity is:

> [i]f the category's defining characteristics are more fundamental—that is, if the category is defined by a physiological, anatomical, histological, chromosomal, or molecular abnormality—clear, qualitative differences must exist between these defining characteristics and those of other conditions with a similar syndrome. (p. 8)

The suggestion is that a diagnosis is paradigmatically valid if distinguishing causal information is available to support it.

Kendell and Jablensky (2003) do not address the context dependence of explanation present in Lewis's account. But we can begin to see how such context dependence will be less visible in cases where there is background agreement on the kinds of causal information under scrutiny in the medical profession. That might, for example, focus attention on factors that can be changed, physiological factors (rather than broader causal factors) and so on. In other words, it is possible to borrow Lewis's focus on causation and downplay his context dependence.

Some putative counter-instances

This context dependence of explanation is an important point to which we will return shortly. Whatever its prima facie attractions, however, as a model of diagnostic explanation, Lewis's general claim that explanation comprises the provision of causal information must be reconciled with some apparent counter examples. Lewis himself considers some of the following cases, which will be outlined below.

1. Teleological explanation: Is teleological explanation possible?
2. Fermat's least time principle and the gas laws: Do all explanations drawn from contemporary physics fit Lewis's account?
3. Biological teleological explanation: Is putative teleological explanation possible?

4. Intentional explanation: Does the explanation of action in terms of desires for the future fit the model?
5. Mathematical explanation: Can explanation in non-causal disciplines be accommodated?

Teleology

As the key claim is that explanation turns on causality, there can be no genuinely teleological explanation. A teleological explanation is one that explains by citing a telos or goal. But to explain an event by citing its goal is not to cite its causes as one of the marks of causality is its temporal ordering. Effects cannot precede their causes.

The Greek fourfold notion of causality

Teleological explanation dates back to the Greeks. In Greek philosophy there are four notions of causality: efficient, final, formal, and material. The concept of causation as it occurs in contemporary analytic philosophy is most closely associated with the Greek efficient cause. The other aspects have now been separated from causality. Roughly speaking, the final cause corresponds to the general telos or purpose of something. The formal cause is the form or idea lying behind it. The material cause corresponds to the underlying substance. Thus if we consider the 'causes' of a ceremonial cup then the efficient cause might be the hammering of the silversmith; the final cause might be the purpose of celebrating the gods through drink; the formal cause might be the design idea the smith had in mind; and the material cause might be the silver involved. Clearly, this rich fourfold notion of causality is only dimly related to our much slimmer notion of causation. But in so far as there is a relation it is that our idea of causation corresponds to the Greek idea of efficient cause and that we lack a genuinely teleological notion of causation.

Teleological explanation is still a live issue in psychiatry because of attempts to use it define psychiatric disorder; see, e.g. Megone, 2000.

Teleological explanation in physics?

In apparent contradiction of the claim that there can be no such thing as teleological explanation there are examples of explanation in physics, biology, and psychology, which seem to be future directed. If Lewis's account is correct then despite appearances, none of these cases can really be teleological. This is what he attempts to establish. In each case, the basic strategy is the same.

The problem with both Fermat's least time principle and the gas laws, is that events are explained (the passage of light, the behaviour of gases) by invoking future states of affairs. Light travels through those points that lie on a path that comprises the quickest way through a refracting medium. Gases tend towards the ratios of temperature, pressure and volume given by the law $PV/T = \text{constant}$. Both therefore appear to be teleological.

Teleology is only apparent in physics

Lewis's response is to deny that such explanations rely only on the laws explicitly cited. Instead, he suggests, their explanatory force stems from the causal account that, he argues, must be assumed implicitly to underlie these regularities. The laws themselves describe in a convenient form the result of purely causal processes. The explanation may *appear* teleological, therefore, but it is not.

Thus Fermat's least time principle is a heuristic for calculating the path of light that is itself made true—assuming that it really is true—by underlying non-teleological causal processes. Likewise the gas laws express relationships between volume, pressure and temperature which are themselves the macroscopic result of underlying microscopic causal interactions between gas molecules. The genuinely causal regularities that govern the behaviour of individual gas molecules give rise to, and can be used to explain, the regularities that govern gases when considered at the macroscopic level. In both these cases, the explanatory power of apparently teleological physical laws or principles derives from the genuinely causal regularities that make them true. It is because there are laws linking the causal processes with the higher level teleological principles that we can use those higher principles as explanations at all. Their teleological explanatory power is thus, according to Lewis, derivative of causal explanations.

Teleological explanation in biology?

While apparently teleological explanation is rare in the physical sciences, it is much more common in the biological sciences. Here, however, Lewis's strategy is more familiar. Perhaps the most striking feature of natural selection as an explanation of biological diversity is precisely that it attempts to show how apparently purposive adaptation is really the result of normal efficient causal processes. In the light of this theory, purposive explanations of adaptation are not completely excluded. But they receive reinterpretation. The explanation of biological features through their functions or teleology is really a shorthand for a longer and properly causal account of the selective advantages produced by random genetic variation.

The debate about the nature of teleological biological explanations has recently re-surfaced in psychiatry over the nature of dysfunctions in the characterization of mental illness. One side of the debate, represented, for example, by Wakefield (2000), claims that a proper understanding of biological function and dysfunction allows psychiatry to be purged of any evaluative notion. The other side, represented, for example, by Fulford (2000), argues that a proper understanding of biological function maintains its evaluative status.

Intentional explanation

The case is less clear-cut in the case of intentional psychological explanation: the explanation of action through the intentions of the agent involved.

The nature of this kind of explanation will be the subject of further discussion in the Part V. Here, however, the important point is that explanation of action is typically provided through

consideration of what the purpose of the action is. One might explain the adding of seasoning to a casserole by saying that the cook wanted to improve its flavour and believed that adding seasoning would bring that about. This seems to be a typical form of explanation of actions. The act of adding seasoning *during* the cooking is explained by its intended effect *later on* when the food is to be eaten. That this is a standard feature of action explanation is uncontentious. But it presents a prima facie difficulty for Lewis's model of explanation because it appears to be a case of genuinely teleological explanation.

Reasons as causes

Lewis's response is to defend his model of explanation by subscribing to a particular view of the philosophy of mind. This view is in no sense outlandish or outré. It is in fact an element of the current orthodoxy in the philosophy of mind. Nevertheless it is an important feature of Lewis's model of explanation that it does have this action at a distance in the philosophy of mind. Future directed mental states—such as the desire to improve the taste of the casserole—are really, on this view, *encoded* in present tense physical states of the body. These occurrent states cause the actions that they explain through physical and genuinely non-teleological causal connections. Thus even the intentional explanation of action turns out to be, on this philosophical interpretation, a species of causal explanations and thus to fit Lewis's model.

Rationalizing explanations

The example does, however, suggest a further limitation of Lewis' model. Intentional explanation is a combination of two elements. On the one hand it can be used to explain actions or events and the best model we have of the explanation of events is causal explanation. On the other hand it relies on the provision of *reasons* for actions. This suggests that intentional explanation is a species of rational explanation. The mental states of agents *support*, *justify*, or *undermine* one another. Mental states are in this sense normative.

Mathematical explanation

It is this normative element in intentional explanations that does not fit easily with Lewis's account. Lewis provides no insight into the *rational* power of reasons. Similarly, he provides no insight into the *normative* explanations of why such and such a conclusion follows from premisses or why a particular figure is the correct result of a mathematical calculation (e.g. the result is two, *because* one plus one equals two). Logical and mathematical explanations are not causal explanations, as required by Lewis, but they are explanations none the less.

This is a limitation in Lewis's account rather than evidence that it is wrong. While causal information may be relevant to the explanation of why events in the causal order happen, it is not relevant to normative explanations. To explain the explanatory power of normative explanations, a different account is needed. The limitation might be characterized like this. Causal explanation is needed to account for why events occur but not (except in some cases) what about them makes them the events they are. Thus they can be used to explain why a batsman in a game of cricket was dismissed Leg Before Wicket (because the bowler bowled a fast ball and the batsman did not keep his eyes on the ball) but not what about event permitted it to be classified LBW (because the ball bounced where it did before hitting the leg). Again, we return to this point later in this part, in Chapter 15 on Causes, Laws and Reasons, and in Part V.

Hempel and Lewis: a rapprochement?

But there is a more pressing problem for our purposes here. The aim of this chapter is to examine the relevance of models of explanation in the philosophy of science for the purpose of formalizing diagnosis. But Lewis's account appears to be too woolly to help in this matter. The claim that explanation comprises providing causal information does not itself suggest how this should be brought about. Perhaps it is true that diagnosis also relies on pinpointing the *causes* of the symptoms recorded by descriptive psychopathology. If so then the model provides some insight into diagnostic practice. Certainly, as Hempel (1999) pointed out, there is a sense in that the descriptive categories on which ICD and DSM are largely based, are promissory on future discoveries of underlying causes, much as the descriptive categories of Victorian medicine, such as dropsy (accumulation of fluid in the lower legs), have now been replaced by a range of different aetiological categories (heart failure, liver disease, and so forth). But Lewis' formal model of explanation does not show precisely how diagnostic explanation relies on causation (actual or promissory) and thus how diagnosis might be an objective, rule governed practice. It does not point to a substantial methodology for diagnosis.

In fact, having stressed the differences in emphasis between his account and the DN model, Lewis suggests that there could be a rapprochement between his and Hempel's accounts. There is no reason why causal information could not be provided by DN explanations. Providing that these deliver *elements* of the causal history of the explanandum and are not thought of as comprising *the* explanation, they can provide (partial) explanations.

Lewis's model and the codification of diagnosis

But, for the purpose of codifying diagnosis, there is a profound objection to Lewis's model that relates back to a feature highlighted earlier. As Lewis denies that there can be such a thing as *the* explanation of an event, what passes for 'the' explanation in the context of everyday life depends on the context of interests that guide research. A formal model of explanation, including diagnostic explanation, thus depends (*inter alia*) on a formal account of context. But as Van Fraassen (1999) argues in the reading linked with Exercise 7, there seems to be no prospect of the context of explanation itself being formalized.

Van Fraassen's emphasis on the context of explanation

EXERCISE 7 (30 minutes)

Read the extract from:

Van Fraassen, B. (1999). The pragmatics of explanation. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 317–327 (extract pp. 324–325)

Link with Reading 14.3

- Think of the consequences of this reading for the prospect of codifying explanation and thus diagnosis.

Explanation is a three-place relation

The moral that Van Fraassen (1999) draws is that explanation is not a simple relation between a fact and a body of theory and antecedent conditions as Hempel (1999) suggests. Likewise, if Lewis's claim that explanation turns on the provision of causal information, this does not imply that the relation of explanans and explanandum can be codified. Instead, Van Fraassen suggests, explanation is a three-place relation between theory, fact, and context. This is because explanation is, on this account, an answer to an implicit why question. As such, explanations need to be evaluated with respect to their implicit questions. But there is no single context for all questions of the form: why P? What exactly is being asked for is fixed by the particular context of interests.

Syphilis and paresis again

Consider again the syphilis and paresis example. Having untreated syphilis can be an explanation of developing paresis because it is necessary (although not sufficient) for developing the latter that one has the former. If a patient had not had syphilis she would not have developed paresis. But as an *explanation* this is only successful against a background assumption that it is unusual to have untreated syphilis. On a hospital ward in which everyone has syphilis, the explanation of why only Jones has developed paresis cannot simply be that she (like everyone else) has syphilis. In that context, the question involves an implicit contrast with the other patients. Why has she, in contrast to the others, contracted that disease? It may be that there simply is no explanation of that fact. Outside the context provided by the hospital ward, however, syphilis does provide an explanation because contracting it explains why Jones in contrast to her co-workers develops paresis.

Of course one issue that this example raises is an issue of principle that concerns the presence or not of other causal factors. It seems intuitive to think that if the chances of developing paresis given untreated syphilis are small, then there must be other factors in play when sufferers do develop it (factors that are missing for those who do not). And this in turn raises an interesting question of how to interpret statistical claims. If the chances of a particular group developing a particular condition are 80%, are the chances 80% for individuals within that group (i.e. there is a genuine element of indeterminacy) or are they determined as either 100% or 0% for each individual and the 80% figure overall is a measure of our ignorance of the particular details? Whatever the answer to that question of principle for each such claim, however, we do use statements of probability to explain events. But as the discussion of Hempel's IS model of explanation suggested above, such explanations suffer the complication that different probabilities apply depending on how an individual is picked out. (In the example above Jones has a nearly 100% chance of recovering from a streptococcal infection if described as a member of the general population but nearly 100% chance of not recovering if described as over 80 years old.)

Thus in the absence of completely deterministic explanations—which seem unlikely in medicine—the context will play an important part in explanation. In fact, according to models of contrastive explanation, context is important even in deterministic explanation.

Contrastive explanation

The Cambridge Professor of Philosophy of Science Peter Lipton presents a related account by focusing explicitly on contrastive explanations. In these, one aims to explain why one thing happened rather than another. Thus explanation involves both the fact that actually obtained and a foil that did not. As Lipton convincingly argues in his *Inference to the Best Explanation* (1991), contrastive explanation cannot be reduced to a more basic form. (But unlike Van Fraassen, Lipton assumes, although he does not flesh this out, that there can also be non-contrastive explanation in accord with Lewis's account. He does not counter Van Fraassen's claim that such explanation is also always implicitly contrastive.)

The strength of Lipton's account is that once a suitable foil is selected, he shows the sort of causal information that is required for a contrastive explanation. It is information about the cause of the fact and the absence of a corresponding cause in the history that leads to the foil. Such explanation works by pinpointing what it is that makes the difference between the one thing happening and the other not happening. But as Lipton admits, he provides no recipe for how to choose one contrast rather than another. He simply demonstrates that some choices will not furnish a suitable explanation because they are too different. The fact and foil must share much of their causal history because otherwise 'we do not know where to begin to answer the question'. But this will still leave an infinite number of possible foils. Lipton's point is that choosing one contrast rather than another is context dependent in a way that (thus far) resists explicit codification. So the explanation 'works' in the relevant context but between 'consenting adults', who are consenting because they share the same (largely tacit) context.

The end of diagnosis?

The suggestion that context plays an ineliminable role in the evaluation of explanation suggests that explanation cannot be formalized in a logical recipe. This suggests that diagnosis is also unformalizable. The connection runs as follows. Van Fraassen (1999) suggests that it makes no sense to talk of *the* explanation of an event because what would be a relevant explanation will depend on local matters of substance rather than general abstract features of explanation. Similarly, what makes a diagnosis the right diagnosis is not just that it explains the symptoms but that it is the right and relevant explanation in the clinical circumstances. These will include the possibility of supplying a suitable prognosis and the possibility of treatment. Van Fraassen's work suggests that while this context guides diagnosis, it cannot be codified. Does this imply that diagnosis is a tacit skill? And if so does this mark out mental health and health practice in general as less than rational or less than scientific?

Remember, however, that the arguments discussed here have all been developed with the sciences of physics and engineering in mind. To the extent, therefore, that the process of diagnosis is scientific, it shares with physics and engineering the limitations that recent philosophy of science has identified. It is a different model of science rather than non-science that we are converging on.

There is a further important theme that you may have noticed in this session. The stress on the role of context for explanation suggests that values impact on science here. But we began the part saying that we would not focus on values in science (although epistemic values were briefly mentioned in Chapter 11), but instead on the role of judgement in science even when explicit value judgements were not to the fore. Values seem, however, to return when we look at explanation. Does this imply that science contains values because it contains explanations? Van Fraassen himself does not think this is so. Because explanations rely on a particular context of interest, which is not itself a scientific matter, he says that 'science *contains no explanations*' (Van Fraassen, 1999, p. 325). In other words, one response to the question just asked attempts to keep science value free by suggesting that explanation itself is not part of science itself. The natural opposing view is to preserve the connection of science and explanation and follow where the practical context of interests guides us. It is to this we will 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 heart of Lewis's suggestion about what explanations have in common? How does his account relate to Hempel's?
2. What are the consequences of Lewis's model for a formal codification of explanation.
3. What does Van Fraassen add to this picture?

Session 3 Clinical skills and tacit knowledge

Two aspects to diagnosis

We have seen that the most influential models of explanation provided by work in the philosophy of science do not provide a regimented codification, which could apply to diagnosis. They do not provide a model of just what a complete diagnosis would consist in. That is not to say they offer no insights. Hempel's model suggests that explanation is connected to laws of nature. It is by appealing to laws that an explanation has the content it does. Lewis suggests that explanation is connected to the provision of causal information. Thus far, these insights are consistent with a traditional medical-scientific understanding of clinical diagnosis. The discussion suggests that successful diagnosis should be underpinned by laws of nature (biological, chemical, and indeed physical), and that it should explain a patient's symptoms by describing (or implying) what causes them.

Less familiar, though, in a medical context, is the idea that scientific explanations depend, according to Van Fraasen, on a context of interest. If this is right, scientific explanations in general, and hence diagnostic explanations in particular, cannot be formalized. That is to say, as there is no way of giving a formal specification of the relevant context (context being essentially particular), and as context is (if Van Fraassen is right) essential to scientific explanation, there can be no fully explicit and complete set of rules for identifying a 'good' scientific (including diagnostic) explanation. What is a good explanation in one context of interest may not be in another.

The implication for the project of codification of psychiatric diagnosis (as in ICD and DSM) is clear. Important as the project of codification has been in improving the reliability of psychiatric classification, diagnostic practice can never be fully captured by explicit 'rules', however carefully spelled out (operationalized).

Again, this is not to undermine the status of psychiatry as a science. The context dependence of scientific explanation is a feature of scientific explanation as a whole. It may be more important to recognize that explanation is context dependent in psychiatric science than in other areas (i.e. because the practical impact of context may, as a feature of the greater difficulty presented by sciences of the mind, be greater). That, as they say, is another story. But the point for now is, simply, that scientific explanation in general is not explanation of a kind that can be fully codifiable; hence we should not expect diagnostic explanation, if scientific, to be fully codifiable.

This conclusion thus leaves us with a question. What else, besides following a set of rules, is involved in diagnosis? What do

we have to fill the gap between theory (i.e. the codified rules of a psychiatric classification) and practice (i.e. the diagnostic explanations given by practitioners in individual cases)?

In the last two sessions of this chapter we will turn to one possible candidate for filling the gap between theory and practice, namely tacit knowledge built up through clinical experience and embodied in clinical practice skills. That tacit knowledge may be important in the gap between theory and practice in medical (including psychiatric) diagnosis is, as we will see, suggested by arguments from both the philosophy and sociology of science to the effect that tacit knowledge is important in the gap between theory and practice in *all* sciences.

There is of course no strict connection between the context dependence of scientific explanation and the tacit knowledge of practical skills. Tacit knowledge, though, as we will see, like the context dependence of explanation, has been shown to be a feature of the paradigm 'hard' sciences such as physics and engineering. If, then, tacit knowledge is important in physics and engineering, it is entirely consistent with the scientific status of medicine (including psychiatry) that tacit knowledge should turn out to be important in these disciplines as well. Though as we will see in the concluding session of this chapter, when it comes to diagnostic explanations specifically of *mental* symptoms, i.e. as in psychiatry, there is an added element of difficulty, foreshadowed in Chapter 11 by the tension between scientific and hermeneutic (meaning-driven) accounts of psychoanalysis, an element of difficulty arising from the fact that such symptoms may demand what Karl Jaspers (in Part II) identified as (meaningful) understanding as well as (causal) explanation.

An empirical investigation of the role of tacit knowledge in applied physics

One of the clearest demonstrations of the role of tacit knowledge in the hard sciences is to be found in the work of the contemporary British sociologist, Harry Collins. The sociology of science will be the subject of Chapter 16. As we will see, the very idea that there can be a distinctive sociological investigation of science raises important questions, both about its underlying assumptions and its relation to the philosophy of science. Investigating those issues, in a later chapter, will help shed light on the scientific enterprise generally. But one preliminary issue concerning the nature of the sociological of science is worth advertising in advance of that later discussion.

Two views of the role of sociological investigation

There have, historically, been two views of the role of sociological analysis of science. On one view, sociology complements philosophical analysis. Philosophy of the science articulates the rational structure that scientific progress ought to take. This is then mapped on to historical or sociological analysis of what has actually happened. Where there are divergences it is up to historical or sociological analysis to explain why the scientists concerned did not follow the path of rationality as dictated by philosophy. The assumption behind this approach, which was historically supported by the sociologist Carl Mannheim, is that rationality is its own explanation. Once one has pointed out that an action was rational in the light of the demands of scientific method, no further explanation is needed of why it took place. All that needs sociological explanation is failure of rationality.

On the other view, there is no trans-historical standard of rationality by which to judge the progress of science and it is the task of sociological analysis to determine just what, in any given context, was judged to be rational and why. This second view is clearly a more radical view and gives greater weight to sociological findings. Collins subscribes to this more radical view. But in what follows, while we will see some of the evidence that might be used to support the radical view, it is not necessary to assume that it is correct.

Background to the extract

The short extract linked with Exercise 8 below gives a feel of Collins' work in his book *Changing Order* (1985). The book as a whole is concerned with a key aspect of scientific process: replicating findings. Replication plays a role in science in general like reliability in medicine. Collins takes it to be the mark of scientific respectability that a finding can be replicated. His argument concerns the abilities of scientists involved in successful replication and the role of tacit or implicit knowledge.

In chapter 3 of his *Changing Order* Collins discusses the case of replicating a new kind of laser. While it might have been assumed that publishing the technical details of such lasers in an engineering journal would provide trained scientists with the ability to construct one, this proved not to be the case. In fact new lasers were built only by those who had direct practical experience of a working laser in another laboratory. Was this because insufficient details had been given? The extract below suggests some general reasons for the uncodifiability of the whole of the expertise involved.

EXERCISE 8 (20 minutes)

Read the extract from:

Collins, H. (1985). *Changing Order.* London: Sage (extracts: pp. 66–67, 69–71)

Link with Reading 14.4

- What is the connection between replication and objectivity?
- What reasons does Collins suggest for there being a tacit dimension to knowledge?

Think about whether and how these might apply to clinical expertise.

Replication and objectivity

Collins suggests that replication is a central test of the objectivity of scientific claims, especially claims based on experimental results. For an experimental result to be objectively established it must be possible for others to replicate it. (This matches the connection between validity and reliability discussed in Chapter 13.)

Scientific knowledge is both general and impartial. Its results should be reproducible because it concerns the *general* working of the world in accordance with universal principles and principles should be indifferent to the particular experimental scientists concerned. (Science does not subscribe to the personality cults of witchcraft, see, e.g. Rossi, 2003.) If an experimental result cannot be reproduced, this suggests that it is not a reliable indicator of how the world really works but a mistaken interpretation, a result of some error somewhere in the original experiment.

According to Collins, scientific practitioners generally misrepresent what is involved in replication. They assume that experimental protocol can be satisfactorily and fully recorded in the written research papers published in journals and that these contain sufficient information to enable others to replicate results. But this is not in fact so.

Knowledge cannot be linguistically represented

Collins's key claim is that the knowledge necessary for replication both is not and cannot be completely linguistically codified. Some necessary knowledge is practical and tacit. It is not conveyed in the formal representation of experiments found in both research journals and science textbooks. Rather, it can only be communicated through practical demonstrations and personal contact.

It is worth examining the reasons suggested in the extract for the presence of a tacit dimension. Perhaps the most powerful point is the suggestion that noting some and ignoring other differences between different lasers is crucial. Seeing some physical differences as playing an important role and seeing others as completely inessential presents a real problem for codifying scientific expertise. Listing all the things to be ignored, for example, would be an infinite task.

Collins's summary of his own empirical findings

Collins goes on to draw more radical conclusions than those we have highlighted. He summarizes his findings in the following six points:

1. Transfer of skill-like knowledge is capricious.
2. Skill-like knowledge travels best (or only) through accomplished practitioners.
3. Experimental ability has the character of a skill that can be acquired and developed with practice. Like a skill it cannot be fully explicated or absolutely established.
4. Experimental ability is invisible in its passage and in those who possess it.
5. Proper working of the apparatus, of parts of the apparatus *and of the experimenter*, are defined by the ability to take part in producing the proper experimental outcome. Other indicators cannot be found.
6. Scientists and others tend to believe in the responsiveness of nature to manipulations directed by sets of algorithm-like instructions. This gives the impression that carrying out experiments is literally a formality. This belief, suspended at times of difficulty, recrystalizes upon the successful completion of an experiment.

These claims go further than we have a justification for. Claims 4 and 5, for example, suggest that because an ability cannot be fully written down it is both impossible to establish ('absolutely') and is invisible in its transfer. But in general we do not think that it is impossible to assess practical skills using practical examination and there seems to be nothing particularly mysterious in transmitting such skills through practical instruction. This is part of a standard medical training.

But whether or not all his conclusions are justified, his claim about the central role of a tacit dimension is important. What makes Collins's analysis particularly striking is that it is advanced as a result of the investigation not of an interpretative or social science but of the 'hard' science of engineering. He is not merely saying that there is some tacit knowledge in some of the softer sciences but that tacit knowledge is a necessary feature of replication in the whole of science.

The status of Collins's remarks

Such a general claim deserves to be taken with some scepticism. Collins's work is based on empirical research and thus, by the same scientific standards that he articulates, stands in need of replication. What is more, a general claim of the sociology of science is that social factors—including interests and values—have a profound influence on scientific investigation. Thus one might expect a professionally optimistic sociologist to 'discover' that his subject matter—hard science—contains aspects that can only be investigated using sociology. Here, however, our purpose is not to establish the general scientific credibility of Collins's claim. Instead it is to raise Collins's claim as an interpretative tool for examining mental health care and to investigate whether there are conceptual or philosophical reasons to support it.

Replication and diagnosis

The connection between Collins's discussion of replication and our concern with explanation and diagnosis is this. Replication requires that the same experimental results are reproduced by other experimenters in other contexts. The most direct method of attempting this is to reproduce the same experiment using the same equipment and the same methods. It is for this reason that details of experimental methods are given in technical papers in scientific journals. But, according to Collins, such papers can never fully explicate what the relevant 'sameness' comprises.

The number of factors that could in principle be specified in describing the original experimental set-up is unlimited and thus could not all be set down. Instead such descriptions presuppose an unrepresented set of assumptions that have to be communicated as practical know-how.

The same general issues are repeated in the case of explanation and diagnosis. Diagnosis requires identifying particular medical conditions as the *same* as others of the same diagnostic classification. The general lesson that Collins's account of replication suggests is that no account of sameness in the case of diagnosis can be given that does not rely upon uncodifiable presuppositions. Sameness in the case of diagnosis cannot be codified. In other words, Collins arrives at a similar claim to that arrived at above through the discussion of formal models of explanation. The result of that discussion was that explanation could not be codified in a formal model because it is essentially contextual and depends on a background of assumptions about what is relevant and of interest. Collins's claims add to that the thought that formal models of diagnosis could not be successful because diagnosis presupposes an uncodifiable background of practical skills and assumptions about what is relevant.

Theory and practice

Because the example discussed is that of building a laser, Collins's claims about the role of tacit knowledge might seem to apply only to the *practical* elements of science. Those are areas where the presence of a tacit dimension is least surprising. Diagnosis also has such a practical element. It is the practical application of medical knowledge in specific cases for practical purposes. But, according to some philosophers and historians of science, tacit knowledge also plays a part in guiding the theoretical side of sciences. One such author is the historian and philosopher of science whose work we touched on in Chapter 11, Thomas Kuhn. His book *The Structure of Scientific Revolutions* (1962) has been very influential in the debate about the development of scientific theory and the possibility of growth or progress.

The central role of tacit knowledge in Kuhn's account of science

Normal and revolutionary science

To understand Kuhn's claims here it will be necessary first to review very briefly his general account of science as outlined in Chapter 11. Kuhn argues, on the basis of historical analysis, that scientific activity falls into two kinds. In the main, scientists are engaged in 'normal science'. This comprises the articulation and application of stable dominant theories and meta-theoretical assumptions to new areas. Kuhn calls this background the dominant paradigm (although he also and confusingly uses that word for many other things including, for example, worked examples). During such periods, no serious attempt is made to refute or even defend the theoretical background, which is instead simply presupposed. However, these stable periods of normal science are punctuated by brief periods of revolutionary theory change. Sparked both by the accumulation of anomalous results and by the development of rival theories or even rival meta-theoretical assumptions, the dominant orthodoxy is cast aside and a new theory or set of theories put in its place. Only during these revolutionary periods is the truth of what will become the new scientific background called into question.

Given this general account of science, Kuhn suggests in the reading linked with Exercise 9, that normal science is guided by entrenched tacit knowledge.

EXERCISE 9 (30 minutes)

Read the extract from:

Kuhn, T.S. (1962). *The Structure of Scientific Revolutions,* Chicago: University of Chicago Press (extract pp. 46–47)

Link with Reading 14.5

- What is the connection between normal science, puzzle solving and tacit knowledge?

Puzzle solving

In this extract, Kuhn (1962, pp. 46–47) suggests that the main activity carried out in normal science is puzzle solving. What is characteristic of puzzle solving is that the nature of the solutions sought is already partly determined by the dominant theoretical background. Even more importantly, puzzles are selected because they are soluble by the lights of the background theory or paradigm. Solving them is thus not a matter of great surprise but rather serves as a test of the theoretical or experimental prowess of the scientist in question. It also serves to extend and make more explicit the paradigm. (Problems that mattered to previous paradigms may be rejected as the product of bad science or bad metaphysics or they may simply be ignored until a subsequent revolutionary change makes them into important puzzles again.)

Kuhn goes on to suggest that puzzle solving highlights the role of tacit knowledge in theoretical science. One of the central skills that is acquired by puzzle solving is learning to recognize how to apply the background theories to new cases, what assumptions or approximations count as reasonable, what would constitute a satisfactory solution, and so forth. In other words, explicit knowledge of a regimented theory is insufficient to be counted as a competent scientist. One must also have the 'know-how' required to *apply* high level theories to particular cases. A key element of this is to recognize that apparently different puzzles can in fact be treated in the same or analogous ways.

Four areas of tacit knowledge

That is only one of the ways in which scientific research work is guided by tacit knowledge. Kuhn (1962) also suggests that scientific work is guided by a set of underlying assumptions

or commitments in four ways:

1. At a practical level, research is guided by commitments to particular kinds of instrument, experiment, or tests.
2. Laws and theories…
3. …and higher level meta-theoretical or metaphysical assumptions determine what is taken to be the subject of science, what sort of thing there is—atoms in a plenum or fields of force—and thus the sort of account to be developed.
4. …Finally, the values that are constitutive of being a scientist: weight placed on rationality, coherence, quantification, observation, and measurement.

Kuhn's argument for the tacit status of these commitments

It might seem that these commitments are imposed upon scientists working within a particular tradition in the form of *explicit* rules or codes. But Kuhn (1962) argues that they are, in fact, implicit and tacit. He provides two main arguments for this claim. The first is empirical. Historical inquiry has simply failed to discover evidence of sufficient numbers of *explicit* rules to explain the coherence of scientific traditions. Therefore the rules must be *implicit*. (Note, by the way, that Kuhn himself reserves the word 'rule' for explicit rules.)

Secondly, Kuhn suggests how it might be that the rules are implicit. Scientific training, from its beginnings in school-work to PhD level and beyond, is by example and application. Terms are introduced together with the theoretical context in which they have their life. Theories are introduced alongside applications in the solution of problems or puzzles. Most of a scientific education comprises learning how to apply theories to problems. If these 'finger exercises' are successful, a trainee scientist learns to see similarities between cases that permit the application of familiar puzzle-solving techniques. However, this does not require that he or she has abstracted an explicit rule about what it is that makes cases similar *except* that the same sort of solution can be applied.

This point can be put in a way that Kuhn does not. The reason why some knowledge must be tacit is that explicit knowledge of rules or principles would be insufficient. One can know a theory without knowing how to apply it to solve a problem. Similarly, one might know a general principle without being able to apply it in particular cases. But the latter ability is just what is required in normal science.

A conceptual argument for the essential contribution of tacit knowledge

Are there philosophical arguments for a tacit dimension?

While Kuhn's (1962) account broadens the issue of the role that tacit knowledge plays in science it is still, like that of Collins (1985), an empirical account albeit derived from historical rather than sociological sources. What we need now is to see whether there are philosophical reasons to think that science must be like that and that Kuhn and Collins are more than contingently correct.

EXERCISE 10 (10 minutes)

Spend a little time thinking about whether the sources of tacit knowledge in scientific practice as discussed above, establish that there *must* be an element of tacit knowledge or just that there characteristically is or has been in the past. Can all one's knowledge be put into words without remainder? Must anything always be presupposed?

Sameness in Wittgenstein's later philosophy

The key idea in both Collins' (1985) and Kuhn's (1962) work is that both practical and theoretical expertise relies on a perception of the relevant similarity of different situations. Collins highlights this by looking at the similarity and difference between working and non-working lasers and the components from which they are built. Kuhn discusses the sameness of different physical situations for the solving of scientific puzzles. Diagnosis clearly also involves seeing sameness in the form of illness between different patient presentations. Is there reason to believe that this must always involve a tacit dimension? Or could an explanation of similarity be given without any hostages to fortune?

The Cambridge-based Austrian philosopher Ludwig Wittgenstein (1889–1951) in his *Philosophical Investigations* (1953), provides just such an argument. The key argument is contained in one hundred or so central paragraphs that discuss following a rule. The general moral of this discussion is beyond the scope of this chapter, but some of its negative results are clear. (In the introduction to Collins' book from which the extract above is taken, he explicitly appeals to just these sections of Wittgenstein's work to serve as a philosophical framework for his own empirical research.)

Wittgenstein (1953) sets up the discussion by considering what it is that guides a speaker to apply a word consistently over time in accord with its meaning. One could imagine that the word was a descriptive term for a form of observable psychopathology. If so to apply it correctly would require a sensitivity to the signs and symptoms presented. But it will also require that one understands what the word means so as to know to which signs and symptoms it applies. Wittgenstein then questions how we can understand this latter element.

Understanding appears to combine two elements: what is grasped in a flash and what is applied over time.

> But we *understand* the meaning of a word when we hear or say it; we grasp it in a flash, and what we grasp in this way is surely something different from the 'use' which is extended in time! (§138)

> [I]sn't the meaning of the word also determined by this use? And can these ways of determining meaning conflict? Can what we grasp *in a flash* accord with a use, fit or fail to fit it? And how can what is present to us in an instant, what comes before our mind in an instant, fit a *use*? (§139)

The problem is thus to explain the connection between what occurs in a flash and the use one makes of a word over time. The problem now is that if one models the understanding one has as a kind of inner definition or instruction that will only guide correct behaviour if one can explain how one understands that inner definition or instruction. It appears that that requires that one knows how to interpret the inner sign and that generates a regress.

Consider, for example, how we might explain understanding the meaning of a simple signpost such as a pointing arrow. If one understands the sign—if one has grasped its meaning—then one understands, centrally, which way it points. It points towards, rather than away from, the arrowhead. So, schematically, one might attempt to represent that meaning in the mind of someone who has understood the signpost with a pointing arrow, pointing in the same direction as the signpost. But, of course, that representation will only represent pointing in the direction of the arrowhead if one already understands how to interpret it. And the problem is that specifying the correct interpretation of the inner representation—through another representation—would generate a vicious regress.

Summary of the problem

Using a word in accord with its meaning requires that one uses it in the same, or in a relevantly similar, way on each occasion. But how is one's use of a word guided by what it means? What does understanding a meaning consist in? Wittgenstein (1953) argues that it cannot consist in having any symbol in mind. Any symbol could be interpreted in a multitude of different ways. So if having a symbol in mind is to play a role in understanding it will have also to be augmented by a particular interpretation. This, however, simply pushes the question back one stage. What is it to have an interpretation in mind? Is this to possess a further symbol that redescribes or interprets the first symbol? If so then this new symbol could be interpreted in any way and will need to be augmented by a further interpretation.

Understanding a series

This point can be brought out by considering what understanding the correct continuation of the rule of successively adding two could comprise. It cannot involve the conscious entertaining of the whole of that series as it is unlimited. But any *representation* of it could be misinterpreted. In everyday life the instruction: 'write 2, 4, 6, 8, 10, 12, and so on' is unambiguous. But it could be misinterpreted to mean what we would mean by 'add 2 starting at 2 up to 1000 and 4 afterwards'. One might add the rule: 'ensure that the units are always follow the following series: 2, 4, 6, 8, 0 . . .' But even this rule might be misinterpreted because it is open ended. Of course we might protest that if one were to continue 18, 20, 23, 26 then the units are not the same as those prescribed. But the problem is to explain what this assertion of *sameness* amounts to. (Consider the case of adding 10. Here one might say that the units should always follow the series 0, 0, 0, 0 . . . Now it may seem to us bizarre that anyone might 'follow' this rule by writing a number that ends in anything other than a nought. But the question is: what about this symbol *compels* our interpretation and precludes the deviant rule follower. Each nought, after all, is different—is a different nought in a different place—from the one before even if to us they are relevantly similar.)

The negative moral

The purely negative moral that Wittgenstein (1953) draws is that no amount of adding further disambiguating clauses will escape the fact that each new clause has to be *understood in the right way* if it is to be successful and could not be misinterpreted. Clause A requires disambiguative Clause B; but Clause B requires disambiguative Clause C; and so on ad infinitum. But there is no infinite regress in practice. In practice we do know what it means to continue a series, apply a rule or use or word. Thus it seems that attempting to specify what it means to continue a series, apply a rule or use a word in the same way by using words (or other explicit symbols), always leaves something out, that 'something' in which our given knowledge must (in part) consist.

Wittgenstein (1953) summarizes this in the following way:

> It can be seen that there is a misunderstanding here from the mere fact that in the course of our argument we give one interpretation after another; as if each one contented us at least for a moment, until we thought of yet another standing behind it. What this shows is that there is a way of grasping a rule which is *not* an *interpretation*, but which is exhibited in what we call 'obeying the rule' and 'going against it' in actual cases. (§201)

A positive corollary

Wittgenstein's positive characterization of understanding links it directly to a practical ability. Understanding the meaning of a word is identified with being able to use it correctly. This practical orientation is emphasized in Wittgenstein's otherwise surprising suggestion that understanding is not a *mental* process or state at all. What this suggests is that there is a tacit dimension that underlies even linguistic representation. Even if the project of codifying all aspects of scientific method were to be successful it would still presuppose that degree of tacit knowledge.

Meaning and tacit knowledge

The claim that the meaning of a word is tacit may appear to be contradicted the following everyday fact: for normal speakers and learners of a language, knowledge of meaning *is* something that can be put into words, i.e. made explicit. One can explain what words mean in both the same and different languages. Wittgenstein, however, is by no means arguing that meaning is private and ineffable. (That is a picture of meaning he explicitly attacks.) So in that sense it is not tacit. But it is tacit in that such explanation presupposes our normal and shared responses to explanations of meaning. What these shared responses amount to are agreement in how to use words that cannot be expressed *in* words in a non-question begging way. Knowledge of meaning is tacit in that it goes beyond mere interchange of symbols.

Wittgenstein's discussion of rules has important implications for the role of tacit knowledge in scientific claims for the following reason. Whenever an activity is rule governed it will rest on a basis of tacit knowledge or know-how. This will apply at all levels of practicality or theoreticity: from correctly applying a mathematical formalism or deducing a result, to the rule governed classification and recognition of signs and symptoms in medical diagnosis. All will depend upon implicit knowledge of how to go on. Any attempt to express this knowledge will itself depend on implicit knowledge left unexpressed.

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 support do Collins and Kuhn provide for thinking that there is a role of tacit knowledge in scientific practice?
2. What specific arguments does Collins provide from his investigation of replication in engineering?
3. What is the basis of Kuhn's arguments for the importance of tacit knowledge?
4. Do Collins and Kuhn provide principled reasons for thinking that this is an essential feature of scientific knowledge?

Session 4 Tacit knowledge, diagnosis, and a possible link to phenomenology?

In this chapter, we have seen that there are reasons for believing that scientific explanations, and hence diagnostic explanation, may not be fully codifiable. We have also seen that there are good reasons to think that all scientific work—both the more theoretical and the more applied—presupposes tacit expertise and knowledge. This suggests that psychiatric diagnosis must likewise depend on an element of tacit knowledge that could never be fully captured in a formal or linguistic codification. But that conclusion as derived in this chapter applies to diagnosis in all areas of medicine. In this last session, therefore, we consider the particular nature of diagnosis in psychiatry. We ask whether the fact that psychiatry is concerned with the mental implies that psychiatric diagnosis has a further and special tacit dimension?

The following exercise links with a reading by the German psychiatrist and phenomenologist, Alfred Kraus, who takes up this suggestion in a specific way. Kraus (1994) argues that there is a *phenomenological* element in psychiatric diagnosis. What that means will become apparent shortly. But note that this is just one way in which psychiatric diagnosis might involve tacit elements. Kraus argues that it is because of the nature of psychiatry that it involves skills that can only be described using phenomenology and that a consequence of that is that they cannot be explicitly codified, as attempted in recent versions of ICD and DSM.

EXERCISE 11 (20 minutes)

Read the extracts from:

Kraus, A. (1994). Phenomenological and criteriological diagnosis. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press, pp. 148–162. (extracts: pp. 152, 154)

Link with Reading 14.6

- What arguments does Kraus advance against the explicit regimentation of symptoms that underlies diagnosis using DSM or ICD?
- Do these turn on the particular subject matter of psychiatry?

Top-down or bottom-up?

Kraus (1994) argues that diagnostic systems such DSM and ICD miss out an important element of psychiatric diagnosis. This is the result of their fundamental structure. Because the model presupposed by these manuals is one in which diagnoses are built up from a number of individual and conceptually independent symptoms they cannot capture top-down and holistic elements of diagnosis.

One criticism that Kraus makes of what he calls this criteriological approach to diagnosis, is that rather than providing a reliable foundation, the connection between individual symptoms and conditions lacks specificity. There remains widespread disagreement about the correlation between individual symptoms and underlying syndromes. By contrast, according to Kraus, a top-down holistic model is more specific because it allows a correlation between schizophrenia and *particular kinds* of catatonia or delusional structure. Correlations are not between schizophrenia and delusions in general but delusions with a specific schizophrenic colouring. But this connection can only be established with a top-down rather than criteriological model of diagnosis.

Conjoining symptoms: more than the sum of the parts

A further inadequacy of the bottom-up model is that symptoms can only be added together through conjunction. But no mere conjunction of individual symptoms—a 'Chinese restaurant menu' approach—can capture the full sense of psychological wholeness that the individual parts add up to. For that, one again needs, according to Kraus (1994), a holistic approach. This is not to say, however, that particular elements cannot be identified in a holistic

diagnosis. It is just that the individual elements have a different logic. One way of marking this distinction (not Kraus's) is to contrast parts that are independent pieces and parts that are essential aspects. The pieces of a jigsaw add up to a whole, but each piece can exist independently of the others. By contrast a musical note has both a tone and a pitch, but neither aspect can exist independently of the other. Thus according to a holistic approach, psychological symptoms are interdependent aspects of a psychological unity.

Kraus combines with these two comments on the limits of a criteriological model of diagnosis a further philosophical explanation of the difference in approach. This is why he contrasts the criteriological with a *phenomenological* rather than merely a holistic model. This concentrates not on psychiatric diseases but on the mode of being of whole persons, the 'whole of the being in the world of schizophrenics or manics'. Thus the phenomenologically based diagnosis of schizophrenia turns on an overall assessment of the patient—a 'praecox feeling'—as having a very different form of 'being-in-the-world' (see also Reading guide).

Heidegger on 'Being-in-the-world'

The term 'being-in-the-world' is taken from Heidegger's account of the nature of human beings in his early work *Being and Time* ([1927] 1962), which he describes as a piece of phenomenology. (Heidegger was introduced in Chapter 10.)

Heidegger argues there that it is an essential feature of humans that they inhabit a world that they did not choose but that has practical significance and thus meaning for them. It is through the use of 'handy tools' in temporally extended practical projects that humans make sense of themselves, their lives, and their eventual deaths. Being-in-the-world symbolizes this embeddedness in mundane activity and contrasts markedly with Descartes's view that human beings are essentially disembodied thinkers. Heidegger suggests that Descartes's account of bodies as mere objects in a three-dimensional space, rather than as things with significance and purpose, has influenced our thinking about everything we encounter and ourselves. We think of each other and even ourselves as objects whether or not we also adopt Descartes's claim that we are thinking objects. The criteriological approach to mental illness might well be thought to a phenomenologist as exemplifying this Cartesian objectivist attitude.

Phenomenology is only one kind of holism

But while a phenomenological account provides one possible explanation of the difference between a criteriological and a holistic account of diagnosis, phenomenology is not the only way to mark that contrast. All one needs for that purpose is to recognize that the symptoms one picks out may be mutually dependent aspects of a psychological whole rather than independent symptoms produced by underlying causes. No bottom-up procedure will capture a diagnosis made on the basis of an irreducibly holistic judgement in which individual 'symptoms' are, as we put it above, interdependent aspects or moments of a psychological whole.

Kraus's (1994) phenomenological approach is worth comparing in this respect with the debate between 'theory theory' and 'simulation theory' as different approaches to understanding other minds, described in Chapter 27. While theory theory supposes that the ability to 'read' another mind turns on an implicit theory that licences inferences from observable signs (behaviour) to underlying mental states, simulation theory denies this. Instead simulation theory suggests that one has access to others' minds by putting oneself imaginatively in other people's positions. As we will see, there are reasons to think that such an ability cannot be codified whether or not one subscribes to a phenomenological approach to psychiatry.

If phenomenology is not the only form of holism, however, it is one that, as we saw in Part II, has been particularly influential in the history of ideas specifically about *psycho*pathology, about, as in the title of Andrew Sims' classic text (see Chapter 3), symptoms 'in the mind'. And as we also saw in Part II, it was Karl Jaspers above all, as a phenomenologist and founder of modern descriptive psychopathology, who argued that with mental symptoms, at least, it was essential that medical explanations were concerned as much with meanings as with causes. With meanings, then, or reasons, we have at least one kind of extra element of difficulty involved in providing explanations, diagnostic or otherwise, of *mental* symptoms. In Chapter 15, correspondingly, we will examine the role that meanings or reasons play in psychiatry and contrast their structure with that of causal explanation.

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 does the reading in this session drawn from the phenomenological tradition add to the discussion of tacit knowledge so far?

Reading guide

- A good overview of explanation is provided in book length form in Salmon, W. (1989) *Four Decades of Scientific Explanation*.
- A useful collection of original essays on the philosophy of explanation is Ruben's (ed.) (1993) *Explanation* and Cornwell's (ed.) (2004) *Explanations*.
- The pragmatic aspect of explanation is discussed in Lipton's (1991) *Inference to the Best Explanation*.

Tacit knowledge

- The locus classicus for tacit knowledge is Polanyi ([1958] 1974) *Personal Knowledge: towards a post-critical philosophy*.
- And tacit knowledge is discussed by Ryle (1949) *The Concept of Mind* (especially pp. 25–61), Dretske (1991) *Explaining Behavior: reasons in a world of causes*, and Reber (1995). *Implicit Learning and Tacit Knowledge: an essay on the cognitive unconscious*.
- And in a practical context, by Luntley (2002).

Interdisciplinary work on psychiatric diagnosis

- A broader perspective is provided by Sadler (2004) 'Diagnosis/antidiagnosis' and Phillips (2004) 'Understanding/explanation', both in Radden (ed.) (2004) *The Philosophy of Psychiatry*, and Spitzer (1994) 'The basis of psychiatric diagnosis' in Sadler *et al.*'s (ed.) (1994) *Philosophical Perspectives on Psychiatric Diagnostic Classification*.

References

Brown, H. (1977). *Perception Theory and Commitment*. Chicago, IL: University of Chicago Press.

Collins, H. (1985). *Changing Order*. London: Sage.

Cornwell, J. (ed.) (2004). *Explanations.* Oxford: Oxford University Press.

Dretske, F. (1991). *Explaining Behavior: reasons in a world of causes*. Cambridge, MA: MIT Press.

Fulford, K.W.M. (2000). Teleology without tears: naturalism, neo-naturalism and evaluationism in the analysis of function statements in biology (and a bet on the twenty-first century). *Philosophy, Psychiatry, & Psychology*, 7: 77–94.

Heidegger, M. ([1927] 1962). *Being and Time* (transl. J. Macquarrie and E. Robinson). Oxford: Blackwell.

Hempel, C.G. (1965). *Aspect of scientific explanation*. London: Free Press.

Hempel, C.G. (1966). *Philosophy of natural science*. London: Prentice-Hall.

Hempel, C.G. (1999). Laws and their role in scientific explanation. In *Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambidge, MA: MIT Press, pp. 299–315.

Kendell, R.E. and Jablensky, A. (2003). Distinguishing between the validity and utility of psychiatric diagnosis. *American Journal of Psychiatry*.

Kraus, A. (1994). Phenomenological and criteriological diagnosis. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (eds J.S. Sadler, O.P. Wiggins, and M.A. Schwartz). Baltimore, MD: Johns Hopkins University Press, pp. 148–162.

Kuhn, T. (1962). *The Structure of Scientific Revolutions*, Vol. II, No. 2. Chicago, IL: University of Chicago Press.

Lewis, D. (1986). Causal explanation. In *Philosophical Papers*, Vol. II. Oxford: Oxford University Press pp. 214–240. (Reprinted in Ruben, D.-H. (ed.) (1993). *Explanation*. Oxford: Oxford University Press, pp. 182–206.)

Lindahl, B.I.B. and Johansson, L.A. (1994). Multiple cause-of-death data as a tool for detecting artificial trends in the underlying cause statistics: a methodological study. *Scandinavian Journal of Social Medicine*, 22(2): 145–158.

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

Luntley, M. (2002). Knowing how to manage: expertise and embedded knowledge. *Reason in Practice*, 2(3): 3–14.

Megone, C. (2000). Mental illness, human function, and values. *Philosophy, Psychiatry, & Psychology*, 7/1 pp. 45–65.

Phillips, J. (2004). Understanding/explanation. In *The Philosophy of Psychiatry* (ed. J. Radden). Oxford: Oxford University Press.

Polanyi, M. ([1958] 1974). *Personal Knowledge: towards a post-critical philosophy*. Chicago, IL: University of Chicago Press.

Reber, A. (1995). *Implicit Learning and Tacit Knowledge: an essay on the cognitive unconscious*. New York: Oxford University Press.

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

Ruben, D. (ed.) (1993). *Explanation*. Oxford Readings in Philosophy. Oxford: Oxford University Press.

Ryle, G. (1949). *The Concept of Mind*. Chicago, IL: University of Chicago Press.

Sadler, J.Z. (2004). Diagnosis/antidiagnosis. In *The Philosophy of Psychiatry* (ed. J. Radden). Oxford: Oxford University Press.

Salmon, W. (1989). *Four Decades of Scientific Explanation*. Minneapolis: University of Minnesota Press.

Scriven, M. (1959). Truisms as the grounds for historical explanation. In *Theories of History* (ed. P. Gardiner). New York, pp. 443–468.

Spitzer, M. (1994). The basis of psychiatric diagnosis. In *Philosophical Perspectives on Psychiatric Diagnostic Classification* (ed. J.Z. Sadler, O.P. Wiggins, and M.A. Schwartz). London: Johns Hopkins University Press.

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

Van Fraassen, B. (1999). The pragmatics of explanation. In *The Philosophy of Science* (ed. R. Boyd, P. Gasker, and J.D. Trout). Cambridge, MA: MIT Press, pp. 317–327.

Wakefield, J.C. (2000). Aristotle as sociobiologist: the 'function of a human being' argument, black box essentialism, and the concept of mental disorder. *Philosophy, Psychiatry, & Psychology*, 7: 17–44.

Wittgenstein, L. (1953). *Philosophical Investigations*. Oxford: Blackwell.

OUP Copyright