6. Evaluating Alternative Views

In the previous chapters, I developed the model-based account of diagnostic psychiatric reasoning, arguing that it fulfils the adequacy conditions as well as the desiderata for an answer to the Methodological Question: "What is the method of proper, contemporary, psychiatric diagnostic reasoning?" Following on from the presentation of my own proposal, this last chapter will be dedicated to five other philosophical attempts to understand psychiatric diagnostic reasoning. The purpose of looking at alternative proposals in this chapter is to assess how my proposal holds up against them. If these proposals are compatible with my proposal, they may exceed mine in terms of convincingly meeting the adequacy conditions and fulfilling the desiderata; if they are incompatible with my proposal, they may present a more convincing proposal that also meets the adequacy conditions and fulfils the desiderata. We have just discussed the desiderata, so they should still be fresh in our minds, but let me offer a brief recap of the adequacy conditions.

The first adequacy condition was to adequately describe the method at work behind the diagnostic process. What does this method look like? What are its operations? When are which steps conducted? The second was to explain the rationale behind this method. What purpose do the steps of the method serve? How are these steps thought to contribute to the achievement of the epistemic ends of the methods used? The third was to set out how we should consider the justificatory status of beliefs achieved using this method. How are specific aspects of the method thought to justify its outcomes? Can we say something general about how promising the method is for arriving at true conclusions, or say how we may make such judgements for specific instances of the methods used?

With these conditions reviewed, for the purpose of comparing my proposal to the most relevant alternative views I will consider 1) Cooper's (2014) case formulation as an empathetic simulation account; 2) Murphy's (2012) sketch of diagnostic reasoning; 3) Reznek's (1988) inference-to-the-best-explanation account; 4) Gupta, Potter, and Goyer's (2019) intersubjective knowing account; and finally 5) Fuchs's (2010) and Parnas, Sass, and Zahavi's (2013) phenomenological approach to diagnostic reasoning. In my discussions of each of these views, I will show why my proposal is to be preferred as an answer to the Methodological Question.

5.1 Cooper: Individual Case Histories

In the fifth chapter of her book *Psychiatry and Philosophy of Science,* Cooper (2014) addresses the role of individual case histories as a means of explaining mental disorder in clinical contexts. Case histories as she discusses them should not be confused with the diagnostic output format of a case formulation. Case formulations as discussed throughout this thesis are primarily intended to provide an explanation for why a given syndromal diagnosis was chosen. In chapter 3, I interpreted the case formulation as a synthesis of information derived from the in-depth evaluation and the resulting selection of symptom models as candidates for the best explanation for a presented complaint of a patient – a selection that enables the choice of a syndromal diagnosis. Case histories as understood by Cooper, on the other hand, are a means to making a patient's behaviour and perhaps aspects of their cognition intelligible to us by looking at their life history. Case histories of patients, as Cooper puts it, provide "the beginning of an explanation of their behavior" (Cooper, 2014, p. 69). At first glance, one might, as has been suggested by some philosophers (e.g., Murphy, 2020), think that Cooper's case histories provide an alternative proposal to my understanding of case formulations. One may think that while my proposal intends to enable diagnosis by identifying symptoms through the use of constitutive models and hence takes a constitutive approach to explaining patients' dispositions to produce the occurring symptom, Cooper's proposal uses patients' life stories to provide a causal approach to identifying present symptoms. As I will argue below, however, interpreting Cooper's account as an alternative to mine is wrong. First, though, let us explore further what kind of explanation Cooper is aiming to provide with a case history.

The very purpose of case histories, as they are usually understood according to Cooper, is "a narrative understanding, empathy, or 'verstehen'" (2014, p. 79) regarding a patient's complaint. This understanding is provided by an "explanation of why they thought as they did is some particular circumstances" – an explanation that, although we are considering a specific case, "will be an explanation of why any human being would think in that way in that circumstance" (ibid, p. 70). Cooper's proposal is that what the clinician is doing when they try to achieve this simultaneously general but also specific understanding of a patient's psychology is to wonder what they themselves would have done.

This act of self-reflection is interpreted by Cooper in line with the simulation account of folk psychology that has been put forward in varying forms by several philosophers (e.g., Heal, 2003; Goldman, 2006; Hurley, 2008). Roughly speaking, the basic idea behind the simulation account of folk psychology is that we imagine (either unconsciously or with conscious effort) being in another person's position based on what we know about them and their situation and run a simulation of what we would do or think if we were them in order to understand their current or predict

their future behaviour. Although there are many nuances to this process, this is also the level of abstraction that Cooper operates on when describing this account, so we will adopt a similar descriptive level here. In light of this background, she claims that case histories basically work in the same way:

> Case histories work by providing us with the scaffolding to simulate another. This explains why case histories focus on all that is unique to the individual. I can suppose that most of another's mental states and ways of thinking will be the same as my own (they too will think that $2 + 2 = 4$, that Paris is in France, that good food is nice, that being wet and cold is bad, and so on). As such, it is their peculiarities that I need to know about if I am to make necessary corrections to my own ways of thinking to be able to mimic theirs. Along similar lines, the more detail provided by a case history the better it will tend to be. The more information I am given about another, the easier it will become for me to think as if I were them. (Cooper, 2014, p. 69)

As an example, Cooper presents the sketch of a case history of Mary, a patient of the psychotherapist Robert Akeret (1995):

> Akeret's patient, Mary, had a Catholic upbringing. She had been brought up to believe that evil thoughts are approximately as bad as evil actions. As a child, on a number of occasions she had wished that bad things would happen to people, and they did. One day she became angry with her father and wished he were dead, and the next day he died. On the basis of this story, we can easily imagine how we would feel if we had Mary's beliefs and were in her situation. It will not come as a surprise to us that Mary suspects it is her fault that her father died, and that this leads to feelings of guilt and depression. (Cooper, 2014, p. 69)

Providing an explanation by means of simulation is, as Cooper readily admits, not a very deep kind of explanation. As she puts it:

> In so far as the target system can be simulated, the explanation of its behaviour must refer to features that are shared with the simulating system. Of course, we may still want an explanation of why it is that *any* of the systems behave as they do. When we simulate a system, this does not completely explain its behaviour, but it does at least tell us what kind of explanation we should look for. (ibid., p. 70)

More important for my purpose of discussing this account here, however, is an epistemic feature of a case histories used in this way. According to Cooper, mental states and behaviours of patients that can be accounted for in this way are not abnormal:

"insofar we can simulate them, we can conclude that there is nothing special or abnormal about the subject" (ibid.), she says.

Cooper adds to this that simulation might not be possible in all cases. I agree with her, and this will be important for my interpretation of her account as well as in later sections where I criticise other accounts that also rely heavily on empathy. But why does Cooper think so? Not all experiences or cognitive or emotional states, and therefore not all behaviours, are necessarily open to simulation. Someone who has never experienced hospitalisation may have a hard time understanding the behaviour of people seeking to avoid it. It might also be hard to understand the reactions to certain situations that are manifested by people who have experienced torture. Or, to consider the example of delusions, there might be cases in which understanding a patient is still within the realm of the imaginable – such as patients who have the delusion that spots on their face contain maggots, which we may imagine in terms of weird skin sensations causing us to want to get something out of our skin. Other delusions, however, especially concerning emotions and puerperal beliefs, might be harder to imagine – like the delusion of having a romantic relationship with the polar beer in the local zoo, as Cooper suggests (2014, p. 76). It might be even harder to imagine and therefore understand the thoughts and behaviours of someone suffering from Cotard's delusions, perhaps claiming to have rotten organs, not to have eaten or slept for years, or to have no blood and indeed be dead but still here (ibid., p. 77). So much for Cooper's account. Next, let me turn to the question of how her account relates to my proposal.

The first question to ask is whether her account is compatible with mine or not, and whether it covers any aspects of diagnostic reasoning that my account neglects. It may initially seem that there is tension between Cooper's account and mine, because one may perceive a contradiction between her proposal for how to understand the case history and my ideas about the nature and purpose of the case formulation. I do not think that this is the case.

Although the case history as well as the diagnostic case formulation draw on information about the patient's past experiences, behaviours, and social circumstances and employ them to explain something about the patient, they do so in different ways and for different purposes. Whereas the purpose of the diagnostic case formulation as part of the diagnostic proposal is to serve diagnostic classificatory diagnostic interest, the purpose of the case history is not classificatory but to enable a narrative (folk-psychological) understanding of what the patient is doing and experiencing. The case history is therefore trying to do something different from a diagnostic case formulation. The case formulation serves the aim of backing up the classificatory decisions that are ultimately expressed in a syndromal diagnosis, in my opinion by summing up the decisive evidence that led to choices for and against symptom models. The case history, on the other hand, allows the clinician to understand aspects of the patient's experiences and behaviour in an empathic way, which

might be useful for building a working relationship with the patient because the patient feels understood by the clinician, or to acquire a sense of what therapeutic strategies might be employed to help the patient. For example, if there is a plausible folk-psychological understanding of some of the patient's problems, there might be some obvious way to help – such as reducing stress reported by the patient who reports being totally stressed out in a way that is quite understandable from their situation. However, this relationship-building and potentially interventional value is not the same as diagnostics.

That case histories as considered by Cooper are not meant to play a central diagnostic role in psychiatry and therefore should not be considered a potential alternative to diagnostic case formulation. My way of understanding case histories becomes clear when we consider the limitations that Cooper herself points out. Cooper suggests that there are many non-typical mental states that are perhaps hard to grasp for a clinician *qua* mental simulation based on a case history. As one example she proposes specific delusions such as the delusion of having a romantic relationship with the polar beer in the local zoo. Given their uncommonness, we can perhaps assume that there are other psychopathological phenomena that are difficult for clinicians to simulate, such as the experiences of people who are so severely depressed that they show mutistic behaviour, stop eating, and stop getting out of bed. Another example that is perhaps hard to imagine for someone who has never experienced it would be a full-blown panic attack. If case history-based simulations were the method of diagnostics, we could perhaps not diagnose delusions diverging so far from common experience as well as other psychopathological conditions as for example panic attacks, or depressed mood seen in especially severe cases of depression, as we had a hard time simulate them. Since we do diagnose these disorders, and since in these diagnostic processes (as in any diagnosis) a formulation is expected to do the explanatory work for the resulting diagnosis, it seems that case histories cannot be an alternative approach to case formulations – at least unless Cooper expressed some scepticism towards diagnosing such empathically challenging conditions, which she does not. So, if Cooper's account is apparently not trying to provide a theory of diagnostic case formulation under another label, does her approach – and folk psychology along with it – really have no relationship with diagnostics? Not even in part? One might think this strange. Indeed, folk psychology plays a role in psychiatric diagnostics and the case formulation.

According to my own approach spelled out in Chapter 3, folk psychology plays into the process of model-based psychiatric diagnostics in the evaluation of psychological complaints. As I discussed there, complaints may be evaluated *inter alia* as non-pathological psychological problems. In this case, they are not classified as symptoms of a psychiatric or other medical disorder. This outcome will be reached if a propositional model supporting this no-symptom evaluation is best (and sufficiently well) supported by the diagnostic information about the patient – in other

words, if the occurrence of psychological complaint is constituted by circumstances that are judged to render it a normal mental occurrence. What renders a psycho-behavioural reaction normal rather than pathological, and as such occurs as a proposition in the model supporting this judgement, will be influenced by the understanding of normal psychology employed by the clinician. This understanding will in turn be influenced by psychometric knowledge and academic psychological knowledge about normal psychology, and also by folk psychology.

Take the example provided by Cooper: Mary, who wished that her father would die before he died in an accident and who believes that evil thoughts are as morally wrong as evil actions. If her father died yesterday and she reports such feelings no clinician would judge her guilty feelings to be a psychiatric symptom; rather, they would appear to be an immediate psychological reaction in line with her moral convictions. In this context, her guilt does not appear to be pathological, it is not (for example) a delusion, and since it is acute and guided by moral conviction it does not seem to be rumination. Her presentation is constituted by factors that would lead to the evaluation of non-pathologically relevant psychological distress. As this example shows, folk psychology can and will often have a place in psychiatric diagnostics, namely as a background theory based on which propositional models of psychological complaints that would render them non-pathological can be set up. Folk psychology and its uses for understanding others, however, are not the whole engine of psychiatric diagnostics.

I conclude this section by summing up some core points discovered in the discussion of Cooper's work. Although at first glance it might seem as though Cooper's proposal and mine are competing to explain how information about patients is used to provide an overarching representation of their case for the purpose of drawing diagnostic conclusions, this is not the case. I demonstrated why Cooper's case histories are different in nature and aim from the model-based account of case formulations: While case formulations aim primarily to support and back up diagnostic classification, case histories support the relationship-building and interpersonal understanding between patient and psychiatrist on a folk psychological level. This can be useful for several clinical purposes, but it is not intended or equipped to be a tool for proper clinical psychiatric diagnostics.

5.2 Murphy: A Version of Diagnostic Modelling

In his book *Psychiatry in the Scientific Image*, Murphy (2012) addresses the issue of psychiatric diagnostics. He provides a very brief discussion of his idea of diagnostics in psychiatry, which even makes reference to some of the same literature on philosophy of modelling that I discussed in previous chapters. But although Murphy talks about modelling in the context of psychiatric diagnostics, I will show that his account and

mine are vastly different. It is not straightforward to decide whether his proposal should be understood as aiming to provide a full understanding of how psychiatric diagnostics works. But no matter how one reads his proposal, be it as one that claims to provide an account of psychiatric diagnostic reasoning in general or only in some of its aspects, I would argue that his account is insufficient. It would be insufficient as an overall proposal for answering the Methodological Question because Murphy does not address all adequacy conditions and does not meet relevant desiderata. If, alternatively, we interpret his account as a proposal for only some aspects of what would be needed for a complete answer to the Methodological Question, his account would also be insufficient. In this case, it is insufficient because even the aspects of the Methodological Question that he does address – which, as we will see, are the descriptive adequacy and the justificatory adequacy condition – are addressed in an implausible manner. But before I come to argue all this, let us begin by looking at his proposal.

Murphy's approach starts from the assumption that psychiatric disorders are usually thought of as exemplars, by which he means "idealized theoretical representations of a disorder" (2012, p. 206), and that they must be differentiated from models. Models, according to Murphy, go beyond exemplars:

> An exemplar is a representation of the typical course and symptoms of a mental illness, whereas a model is a representation of those symptoms, that course, and the causal determinants of both of them. A model is an exemplar together with an explanation. (ibid., p. 206)

He also puts it slightly differently, with more emphasis on the nature of what he means by causal determinants:

> [A] model is an explained exemplar: the exemplar is the typical manifestation of the symptoms and course of disorder, and a model is the representation of the causal relations that obtain between features of the exemplar and various aspect of the organism. (ibid., p. 207)

Murphy goes on to explain his take on diagnostics considering this understanding of an exemplar of a disease:

> diagnosis works by fitting a patient to a portion of the exemplar, and the exemplar is explained by modelling the process whereby the symptoms in the exemplar express the state of neurobiological system (pathology) that depend in its turn on logically prior causal processes (etiology). (ibid., p. 206).

Murphy's brief remarks are more a sketch than a full-fledged proposal of how psychiatric diagnostics is supposed to work, but they provide enough detail to be evaluated and compared to my proposal.

A core difference between Murphy's account and mine concerns our perspectives on the role that models and modelling play in diagnostics. While my view is that models are set up based on background knowledge to be used to diagnose symptoms in a process of comparing these models to the patient, Murphy assumes modelling to play a vastly different role. In Murphy's account there are no models of specific symptoms, as I propose, but only models of disorders. Moreover, while models play a direct role in the diagnostic evaluation of the patient, in his account they are only the background from which features to look for in patients are derived. So, how should we assess Murphy's account?

I will argue that Murphy's approach has two problems. First, it does not meet the adequacy conditions for an answer to the Methodological Question. And second, the proposal he makes does not insufficiently address some of the desiderata of an answer to the Methodological Question. The proposal does not meet the adequacy conditions because among these condition (providing a description of the method at work at psychiatric diagnostics, providing a rationale for the inferential processes at work within the proposed methods, and providing an understanding of how the outcomes are supposed to be considered justified), he at least fails to meet the descriptive criterion, the rationalisation-of-inference criterion, and at least to some degree also the justification-related criterion. Moreover, his proposal does not enable us to address several desiderata in a sufficient manner, or at least does so in a less satisfying manner than the model-based proposal does.

That Murphy's proposal does not provide a rationale for the inferential operations undertaken in the process of diagnostics. What kinds of inference are made and how the inferential patterns employed are supposed to support his conclusions is essentially not discussed by Murphy. He tells us that the exemplars of disorders are compared to the patients to decide the outcome. However, what kind of inference is taking place and how exactly any specific type of input is enabling the inferential matching to work to produce its outputs is not addressed in any detail. It thus seems fair to say that this adequacy condition is simply not addressed by Murphy's proposal. Next we turn to the adequacy condition of illuminating why we should deem the outcomes of the method's inferential work justified.

Murphy makes no proposal regarding internal justification; he cannot, because he has not spelled out the structure of the inferential method he proposes sufficiently well to make claims about how it is supposed to provide justification. However, his claims about where the exemplars come from that are used in diagnostics might be considered as a proposal for where the external justification is coming from: namely, the scientific models used to set up the diagnostic exemplar. In principle, this seems reasonable. After all, when I talked about external justification in the

last chapter, all I did was gesture towards the science of psychopathology. However, there is a problem with Murphy's attempt to rely on science to obtain justification for his diagnostic proposal. He assumes that the psychiatric sciences add something to the process of diagnostics that it cannot offer. Thus he has not provided an acceptable approach to the external justification of his proposal. Let me elaborate.

Murphy assumes a support for the diagnostic exemplars that is problematic because he seems to have an inadequate picture of the state of psychiatric knowledge and its application to psychiatric diagnostics. Murphy's approach seems to presuppose that there are widely accepted explanatory models of psychiatric disorders informing us about the proximal causes (i.e., physiological processes) giving rise to certain symptoms as well as about the distal causes that brought about the changes responsible for the presence of the psychiatric symptom. This is what he assumes models in psychiatry to present us with. The exemplars then used in diagnostics are basically this model minus the explanations; they contain only information about the symptoms explained by the model, as well as the cause of their occurrence and change in the context of the disorder. This is a highly problematic background assumption. As I mentioned in Chapter 3, despite many interesting and important scientific efforts, psychiatry currently lacks full-fledged detailed models of psychiatric disorders as a whole – and even for most psychiatric symptoms – that would offer a detailed mechanistic explanation of the proximal and distal biological causes of occurring symptoms, as well as of the developmental pathological importance of various factors such as genes and social environment.

Moreover, beyond the face-value fact that there are no such models around yet, it even seems implausible that there could be anything like such a unitary model for many major psychiatric disorders according to the currently used diagnostic classifications, because many disorder are likely lumping together clusters of distinct conditions. Just think of major depression. Major depressive disorder can occur in patients with 227 combination of symptoms that are vastly different and partly without any symptomatic overlap, which, according to our best current scientific understanding, suggests that vastly different causal (e.g., neural) processes are involved in different instances of one and the same disorder (as classified in current diagnostics). This is all the more likely if we consider instances with no symptomatic overlap, which we know are not only possible according to the manual but indeed occur in significant numbers in patients (Zimmermann et al., 2015). If we assume that different symptoms and especially non-overlapping or only partly overlapping clusters of symptoms will be caused by non-identical psycho-biological processes, there cannot be one scientific model of major depression, because major depression is not a single phenomenon but seem to consist of multiple phenomena that science would have to identify and explain. One model could not comprehensively cover everything that falls under the label of major depression. Accordingly, even if we had good causal models of psychiatric disorders, the case of major depression

illustrates that, given the current diagnostic systems, we would perhaps not end up with one only. Hence, there could not be an exemplar whose components could then be fitted to the patient. In conclusion, it therefore seems that Murphy's view of the state of psychiatric diagnostics and the way it can enable clinical diagnostic work fails to make a adequate proposal for an answer to the Methodological Questions that is true to the state of psychiatric science and diagnostics.

Considering the problem with the assumed unitary background models of mental disorders that, according to Murphy, is meant to back up and justify the exemplars used in psychiatric diagnostics, his proposed method of psychiatric diagnostic seems implausible regarding the external justification condition. Internal justification is not addressed by him at all. Hence, the adequacy condition of spelling out how conclusions of the used method are supposed to be deemed justified seems to be failed by Murphy's proposal. What about the descriptive adequacy condition, to propose a method via which psychiatrists draw diagnostic conclusions that maps onto the diagnostic efforts of clinical psychiatrists? After all, one could say that the idea of comparing exemplars to patients seems to provide such a proposal and that it is not so far removed from my position that disorder diagnostics takes place as pattern recognition. This seems to be a plausible proposal for a method, and even one where we seem to agree with each other, but Murphy made this point before me. I disagree, or at least I would claim that interpreting my way of describing the intermediate steps of drawing diagnostic conclusions as just another way of putting what Murphy had in mind would be as unnuanced as the worn-out claim that Plato already said everything there is to be said in philosophy, However, this depends on how exactly we understand Murphy's proposal. Let's look at it again.

In his proposal, describing the belief-forming procedure – that is, the method by which psychiatrists arrive at diagnostic conclusions – he claims that exemplars, which consist in assumptions about sets of symptoms and the course of their development derived from a background model of the disorder, are used in a process of "fitting a patient to a portion of the exemplar". Let's accept this idea and forget for a moment that, as I argued earlier, such exemplars cannot be derived in the way Murphy proposes, instead focusing on his proposed method, the "fitting [of] a patient to a portion of the exemplar". It appears that there are at least two ways to understand this short phrase and therefore the proposed processes of diagnostic reasoning according to Murphy: one that appears to be highly problematic and should for reasons of charity not be attributed to him, as this would render his proposal a failure, and one that is indeed more plausible and closer to my own ideas, but so underdeveloped and implicit in his writing that one could hardly argue that Murphy made the same proposal as I did, given that developing the proposal to an adequate level of detail is part of the heavy lifting I undertook in the last chapter. By either of the readings, it would seem that Murphy's proposal to describe the method either fails to be adequate or is at least less adequate (because it is not worked out in any detail)

compared to mine. Let's begin with the more problematic reading of what he may mean by "fitting a patient to a portion of the exemplar".

One way to understand "fitting a patient to a portion of the exemplar" would be to assume that psychiatrists somehow evaluate patients for the presence of fixed packages of symptoms making up whole disorders that would be sufficient to provide a psychiatric diagnosis and that this is the one and only level of diagnostic evaluation. However, if we accept this, there would be no lower-level diagnostic investigation as part of the diagnostic evaluation – that is, no inferential process that evaluates the patient for the presence of specific symptoms so that patterns of symptoms required for a diagnosis can be identified in the output of such a lower-level diagnostic process. If this were what Murphy wanted to say, his approach would seem implausible. On the one hand, it would ignore all the diagnostic reasoning work of the psychiatrist that contributes to deciding whether a symptom is present or not. Moreover, it seems that there are diagnostic categorisations whose assignment to a patient could not be carried out by Murphy's approach. Think, for example, of the categories of "unspecified depressive disorder" (APA, 2013, p. 184) that allow a psychiatrist to diagnose a depressive disorder if several psychopathological symptoms of depression are present, but not all necessary criteria for another depression diagnosis are fulfilled. There is no concrete description of the exact number of combinations of depressive symptoms that need to be present for this disorder to be diagnosed. It seems hard to imagine that Murphy wants to claim that there is an exemplar that represents all depressive presentations that do not fulfil any other depression-related condition requirements, given that an exemplar, according to Murphy, is an "abstract" and "ideal" representation of the disorder. It therefore seems that a diagnosis is intended to be provided based on previous insight into the presence of psychiatric symptoms and recognition of one of many potential patterns of symptoms that do not suffice for any other depressive disorder diagnosis and thus yield this diagnosis. However, this requires a diagnostic reasoning process that identifies symptoms in the first place, which is not part of Murphy's proposal as interpreted here. Another similar point about Murphy's proposal is that it would not explain how the psychiatrist may recognise symptoms insufficient to support any disorder diagnosis but occurring somewhat disparately and not feeding into any of the disorder diagnoses given to the patient. A patient might, for example, suffer from minor depression but also experience depersonalisation. How could the psychiatrist be aware of this single symptom if it were not acknowledged by the application of a disorder exemplar? It seems again that some lower-level diagnostic reasoning process is necessary for this that goes beyond the application of exemplars to patients. However, there is a different, perhaps more plausible, and realistic way to interpreting "fitting a patient to a portion of the exemplar".

On this second interpretation of "fitting a patient to a portion of the exemplar", we could take Murphy's account to imply that the psychiatrist knows what a disor-

der would look like if all its symptoms where present and knows what combination of subsets of these symptoms, which Murphy would call "portions of the exemplar", would need to be present in order to provide the diagnosis. If we understand Murphy along these lines, his idea would indeed be compatible with my proposal, as this is basically what I also assume that psychiatrists are doing. Murphy, however, taking the first steps on the path I have taken with my proposal, did not flesh out this idea to any grain of detail comparable to the proposal I have made in the preceding chapters. Accordingly, even if we understand Murphy along these lines, it seems that my proposal exceeds his in detail and explanatory depth by a wide margin, so that again it appears fair to say that Murphy's proposal does not adequately explain in detail what goes on in the process of psychiatric reasoning, even if we are willing to grant that he intended to imply what my proposal worked out explicitly. Thus, once more it seems that his account lacks the criterion of providing a description of the method adequate to diagnostic practice because it fails to address relevant aspects (symptom diagnostics) in detail. We may assume, in this more charitable interpretation, that his account implies a more detailed explanation, but he does not say how symptom diagnostics is supposed to take place. The lack of detail in Murphy's account of symptom diagnostics, and his rather abstract way of talking about the disorder diagnostic part of the proposal, can on the most generous reading be understood to fulfil the descriptive criterion for an answer to the Methodological Question to a small degree, and certainly to a lesser degree than my proposal, which also details the steps of the method of diagnostics on the symptom level. This makes his proposal a weak substitute for mine.

If we sum up by asking how Murphy's ideas hold up against the three adequacy conditions for an answer to the Methodological Question, it seems that he scores low. The criteria related to justification and the rationale for inferential patterns were not provided or were shown to be implausible. The method description was present in an insufficient manner on the most charitable interpretation. It therefore seems that Murphy's ideas represent an inadequate attempt to understand the method of proper contemporary diagnostic reasoning. Although it is no longer needed because the proposal is already shown to be inadequate, let us nonetheless talk briefly about desiderata. What Murphy presents us with would seem also to fail many of the desiderata. His account is certainly not comprehensive, since it fails to talk about the whole aspect of diagnostics in enough detail to understand what happens there (symptom diagnostics) and leaves out whole aspects of clinical diagnostics (i.e., diagnostic co-formulations resulting from critical discussions between clinicians). Moreover, those aspects of diagnostics that are addressed in his proposal are explained in such an abstract way that they hardly seem to have the explanatory resources to provide a remotely detailed understanding of, for example, the difference between misdiagnosis and diagnostic malpractice, diagnostic

disagreements and their resolutions, diagnostic uncertainty and how to resolve it, or how good diagnostic instincts may work.

In sum, Murphy's proposal fails to meet any of the adequacy conditions for an answer to the Methodological Question, or at best meets one of them to a very limited degree. As mentioned at the beginning of this section, this means that it fails as an alternative to my model-based proposal, no matter whether we take his proposal as a full-blown attempt to address the Methodological Question or only as some ideas addressing just a subset of its central requirements. As briefly discussed at the start, it also seems that there are at least several desiderata for an answer to the Methodological Question that Murphy's account seems unable to fulfil. As I have demonstrated in the preceding chapters, the model-based proposal, by contrast, meets all the conditions and is able to fulfil the desiderata, so it seems fair to conclude that the model-based account is to be preferred over Murphy's ideas about psychiatric diagnostics.

5.3 Reznek: Inference to the Best Explanation

In his article "On the epistemology of mental illness", Reznek (1998) discusses the challenges of psychiatric diagnostics and puts forward a proposal for how psychiatrists arrive at justified conclusions about the presence of mental disorders or psychiatric symptoms in patients. As such, Reznek's proposal should perhaps be understood not as an attempt to provide a full answer to the Methodological Question, but rather as an effort to address two aspects of an adequate answer to it: what patterns of inferences are at work in psychiatric diagnostics and how its conclusions using these patterns of inference may be deemed justified. As I discuss below, Reznek's ideas about how to address these two aspects overlap to some extent with mine, but my position offers a more satisfying answer to these two aspects of the question. Furthermore, by addressing the remaining aspects of an adequate proposal and also fulfilling the desiderata of an answer to the Methodological Question, the model-based description of the psychiatric method proves preferable – regarding the specific aspects of diagnostics that both proposals addresses and also as an overall more satisfying framework. Let us begin by looking at Reznek's framework.

Reznek's starting point is the well-known Rosenhan experiments (Rosenhan, 1973). In a nutshell, Rosenhan sent supposedly mentally healthy people to psychiatric hospitals, instructing them to pretend to hear voices. These individuals were diagnosed with psychosis and admitted to treatment. Reznek treats this occurrence as a case study bringing to our attention a problem for psychiatric diagnostics that

he calls the "Rosenhan challenge":[1] "there is a logical gap between the description of subjective symptoms and the attribution of an objective disorder. We cannot deduce the presence of a disorder from a list of purely subjective symptoms or behaviours" (1998, p. 216).

After discussing alternative approaches to how one might arrive at conclusions about the mental states of others and how they might enable psychiatric diagnostics, which, according to Reznek, fail to provide sufficient support to diagnostic conclusions, he arrives at the only approach he considers promising. He calls it the scientific or hypothetico-deductive method. This method "postulates the existence of some theoretical entities to explain observable phenomena" (1998, p. 218). To map this method and its scientific use onto attempts to determine the presence or absence of a mental state in other, he presents an illustration:

> For example, when Newton observed such diverse phenomena as the tides, the motion of the planets, and the falling of apples, he hypothesized the existence of the gravitational force that explained such observations (even though Newton claimed that he never made hypotheses). We come to believe there is such thing as a gravitational force because we need such a theoretical entity to explain these observations. In the same way, we might postulate the existence of mental events – they are the theoretical entities that are needed to explain behavior of other people. Without them we cannot make sense of their behaviour. The explanatory power of such theoretical entities provides evidence for their existence (just as it does in science). This seems our most reasonable approach. (ibid.)

Interestingly, what is being described here by Reznek is not the hypothetico-deductive method as usually conceived since Popper (1935), but rather an abduction or inference to the best explanation. Let me elaborate.

The hypothetico-deductive method usually follows an algorithm that contains more elements than the one described by Reznek. Let's take the explication offered by Godfrey-Smith (2003, p. 236). According to Godfrey-Smith, the hypothetico-deductive method proceeds roughly as follows: (i) Use of experience: You consider a problem/observation you wish to explain and gather data about it. (ii) Forming a conjecture: You put forward a hypothesis whose truth would adequately explain the phenomenon of interest and the data you gathered about it. (iii) Deducing predictions: You deduce predictions that must follow from the truth of (ii). (iv) Testing: You

1 It may be worth emphasising that Reznek himself does not buy the sceptical conclusions that Rosenhan himself drew from his experiments regarding the validity of psychiatric diagnostics, but merely considers Rosenhan's work to put forward an interesting challenge. Earlier responses to Rosenhan's work challenged the power of Rosenhan's experiment to support his sceptical conclusions altogether (Spitzer, 1975), and more recent responses have presented evidence of massive fabrication of data in his studies (Cahalan, 2019).

consider observational evidence that could disprove the predictions and therefore refute our hypothesis. As is widely known, however, in a purely hypothetico-deductive framework, evidence not conflicting with the hypothesis is usually not considered evidence for the hypothesis, as this would equal the logical fallacy of affirming the consequence.

Steps (iii) and (vi) are missing from Reznek's example, where it appears that the explanation that is considered to make most sense of the observations is what is accepted as an explanation. This suggests that what is happening here matches with the pattern of inference to the best explanation, which, according to Lipton (2017), has as "[i]ts governing idea is that explanatory considerations are a guide to inference, that scientists infer from the available evidence to the hypothesis which would, if correct, best explain that evidence" (Lipton, 2017, p. 184).

One may consider this a misreading of Reznek. Maybe he intended his example to contain steps (iii) and (vi) but he did not try to explicate them because he expected his readers to be sufficiently primed by his mention of the hypothetico-deductive method to do this for themselves. Textual evidence speaks against this reading.

Later, Reznek gives another example of his preferred account to diagnostics, discussing how to diagnose whether someone is suffering from hallucinations:

> We identify genuine hallucinations by comparing two overall hypotheses of bizarre behaviour – one is that the person is hallucinating (and is deluded), and the other is that the person is malingering. The hypothesis that provides the best overall explanation of the behaviour, and is consistent with all our knowledge of ethnology, anthropology, and so on, is the one we should accept. We will have no proof, but only a good hypothesis. But since this is all we have anyway, in any discipline, we should not feel uncomfortable. (1998, p. 229)

Again, this paragraph highlights that what the psychiatrist is doing is a comparative judgement amongst different explanations, choosing the one that is supposedly most coherent with other theoretical assumptions as well as with the observations at hand rather than facing a test in other situations – that is to say, no step (iii) or (iv). Moreover, what is even more striking is that in this example it becomes even clearer that in the context of diagnostics, the generated hypothesis explains not only potential future phenomena but also the specific phenomenon at hand that led to the psychiatrist formulating and choosing among the explanatory hypotheses. This again seem to be a feature of inferences to the best explanation rather than of hypothetic deductive reasoning. As Lipton (1991, p. 67) points out, one advantage of inference to the best explanation over the hypothetico-deductive method is that while hypothetico-deductive explanatory entitlement is directed *only* to future events (i.e. the hypothesis generated in (ii) is only applied in step (iii), not the very phenomenon that inspires the hypothesis to be formulated) Inference to the best explanation has

a broader scope of explanation. Namely that "Inference to the Best Explanation suggests that explanatory considerations may apply to both the generating candidates and the selection from among them" (ibid.). In other words, in contrast to the hypothetico-deductive method, Inference to the Best Explanation not only explains future events but also the context in which it was initially conceived.

In sum, it appears that Reznek is confusing abduction with hypothetic deduction. Accordingly, his answer to the Rosenhan challenge is that although we cannot deduce mental symptoms of patients from their behaviour, we can make inferences about their presence by taking into account everything relevant we have learned about the mental phenomena we are considering attributing to the patient and assessing whether the patient's presentation makes it seem most coherent that this phenomenon is occurring in the patient or whether another explanation is more plausible. We can do so by an inference to the best explanation, which would involve the assumption of the presence of the condition in question.

How do we learn about the phenomenon in the first place if we cannot determine its presence directly through the observation of behaviour? Reznek proposes that we must start with some stipulations to carve out a phenomenon, which on his view is a matter of clinical judgement:

> We are first required to decide who is depressed, for example, and who is not. Only after this, can we find out what sorts of questions best identify those who are depressed. These questions can only be as good as the clinical skills that differentiated the two groups in the first place. And the test can only be as objective as the diagnostic process that set up those groups prior to the construction of the test itself. Far from being an objective test of psychiatric disorder, the psychological tests are as subjective as the clinical procedure on which they are based. (1998, p. 223)

Ideally, Reznek goes on, such decisions should be made according to what he takes to be the ideal case in medicine – namely, with reference to biological disease underlying an occurring disorder that is described in terms of a symptom-based syndrome. As he puts it, "In medicine, the identity of a disorder is defined by the underlying biological nature of the syndrome rather than the syndrome itself" (ibid., p. 219).

Assessing Reznek's proposals, there are several synergies between his account and mine. We both believe that psychiatric diagnostics is not a straightforward deductive inference from utterances or simple behaviours of patients to the attribution of a symptom, precisely because the simple occurrence of a behaviour underdetermines what is going on with the patient. We both assume that as part of the assessment we make an inference to the best explanation to what condition is present in the patient on the symptomatic level, given the relevant evidence we have collected about the patient. And we both believe that our psychopathological understanding

based on background knowledge about these psychopathological conditions is central to this inference.

I have no bone to pick with Reznek about the things he says. In terms of internal and external justification of psychiatric diagnostics, he tells us that psychiatrists' inferences are inferences to the best explanation about which conditions' presence should be assumed as the best explanation of the patient's presentation. He thus presents us with the inferential pattern that rationalises the diagnostic process, and he indicates how diagnostic judgements are supposed to be internally justified. He then also tells us where the credibility of judgements made this way comes from – namely, from the scientific understanding of the psychiatric condition via which we calibrated our initial judgements about what should be considered the explanation-worthy phenomenon in the first place. This provides a proposal for external justification. Although I agree with all this, I think however that the model-based account has more to offer than Reznek's, including if we look at the very topics also addressed by Reznek.

On the point of meeting the Methodological Question's requirement to provide a rationale for the method used in the diagnostic process, Reznek can only say that whatever precisely the method is (he is not proposing a concrete description of a method), it works *qua* the inferential pattern of inference to the best explanation, and then gives us some examples. This may be right, but the lack of a proposed method makes this answer rather abstract, since there are many ways in which an inference to the best explanation can take place. Reznek describes, in his examples, how information about the patient is collected and taken to point towards a diagnosis based on our understanding of what kinds of behaviours and experiences we should expect to see in a patient if he has this diagnosis. My sense is that all this tends in the same direction that I have pursued in my more detailed proposal – namely, that the diagnostic process follows an indicator (in my argument a constitutive indicator) strategy. This idea may lurk implicitly in Reznek's remarks, but the model-based account presented here has explicated this idea and laid it out in detail. Reznek provides no detail on how he would propose the inference to best explanation to be structured – for example, according to which general inferential strategy is realised in the diagnostic process. Moreover, by discussing the cognitive vehicles supposed to underlie the pursuit of the indicator strategy that is realised in diagnostic inference to the best explanation, by proposing the existence and use of propositional diagnostic models, I have added a layer of detail that is also missing in Reznek, who make no great effort to spell out in detail the process or means of comparison. Hence, it seems that the model-based provides a more detailed answer as to the rationale for psychiatric diagnostics proceeding the way it does.

When we turn to what Reznek's proposal has to offer in terms of the adequacy condition of helping us to understand how the conclusions of the method are supposed to be deemed justified, he again has offered us something. He provides an

account to help us grasp the external justification by gesturing towards the relevant science informing our clinical psychopathology, which is what I did. However, when it comes to internal justification, all he has to offer us is that diagnostic conclusions are justified since they are arrived at by an inference to the best explanation, which is also part of my answer. However, since this is all he offers, he seem to miss a relevant aspect of psychiatric diagnostics: exclusion diagnostics, or diagnostic conclusions drawn not because we have an explanation that best explains what the diagnostic condition is, but because we actually have no explanation (no model, as I would say), for the patient's presentation. Such conclusions are justified by identifying which explanations do not apply and then providing a diagnostic label that basically means that the patient has a complaint whose evaluation did not match up with any of our potential explanations of this complaint. This inference and the justification it provides for a diagnostic categorisation of the condition in question is not an inference to the best explanation; it is an inference *qua* the lack of explanation. Although Reznek intends to discuss how diagnostic judgements are internally justified, he apparently missed this aspect of diagnostic practice, or at least his proposal does not address it. By contrast, the model-based account contains an explanation of the inferential work and how it justifies the diagnostic conclusion that has been reached in terms of the inferential pattern of apophatic inferences. Here it seems that the model-based account is preferable over Reznek's as it provides an understanding of how justified diagnostic conclusions – in a class of diagnostic judgements that are not discussed by him although they seem to be present in clinical diagnostics – can be arrived at. The only inferential pattern he puts forward to explain how psychiatric diagnostic reasoning is supposed to arrive at justified conclusions – that is, abduction – cannot account for this class of judgements.

A remaining step to assess Reznek's proposal against mine would be to discuss to what extent it can satisfy the desiderata I set up and showed to be fulfilled by my own proposal. However, as I stated at the beginning of this section, Reznek's intention in his work seems not to have been to provide a full-blown answer to the Methodological Question; rather, he focused on just one aspect of it, the topic of justification. Thanks to this fact alone, his proposal will not fulfil the desiderata. Just think of the desiderata of comprehensiveness and being cognitively realistic: If there is no description of the method of diagnostic reasoning, it cannot be comprehensive and realistic. The same goes for the requirement of helping us to make sense of diagnostic disagreements or the difference between misdiagnosis and malpractice, or of any of the other desiderata I put forward as being preferable in an answer to the Methodological Question. The ideas that Reznek provides are not sufficient to address these issues in a satisfying degree of detail, because his very general point that inference to the best explanation is the inferential basis of diagnostics is not suited to telling us, on the level on which a proposed method would operate, what happens in the case of the phenomena we are interested in. One might suggest, for example,

that diagnostic disagreement arises when diagnostic experts disagree about which inference to the best explanation to make once they have gathered diagnostic information about a patient. However, to accept this level of abstractness to account for the desiderata would be a low bar to clear to account for the phenomena pointed out in the desiderata. Saying that we understand what is going on these cases with this level of abstraction would be like saying, if we ask how a biomedical researcher discovers genes responsible for a disorder, that we are satisfied by the explanation, "by induction". Intuitively, few people interested in the topic would be satisfied, and we should not be satisfied with the degree of detail that Reznek's account would provide us with to address the desiderata relevant to achieving a good understanding of psychiatric diagnostics.

In sum, it appears that Reznek's account does not provide a full answer to the Methodological Question. Furthermore, it seems that even in terms of the adequacy conditions for the answer he does provide, his proposal performs worse than the model-based account, given the lack of detail and depth in terms of helping us to understand the rationale behind the diagnostic procedures. This weakness in his proposal for understanding the internal justification of diagnostic reasoning is due to his exclusive focus on inference to the best explanation. Moreover, largely because he does not provide a description of a concrete method at work in diagnostic reasoning, he also fails to fulfil the relevant additional desiderata. Consequently, it seems that the model-based account is more satisfying, as it goes beyond the scope of Reznek's proposal. Even if we set aside the fact that no description of a method of diagnostic reasoning itself is provided by Reznek, the model-based account has substantial benefits over Reznek's account where they address common aspects required for such a proposal.

5.4 Gupta, Potter, Goyer: Interpersonal Knowing

In their paper "Diagnostic reasoning in psychiatry", Gupta, Potter, and Goyer (2019) intend to make a specific contribution to the theory of psychiatric diagnostic reasoning. Their contribution is not a proposal for how to address the Methodological Question, nor any of its aspects. Their contribution, roughly speaking, is a critique of the way that many proposals, which they call cognitive accounts, miss a crucial aspect of psychiatric diagnostics – namely, the role of second-person knowing (i.e., knowledge acquired from the second-person perspective) about the patient for the act of diagnosing in clinical practice. As I will spell out in a moment, they argue that this second-person knowledge is necessary for psychiatric diagnostics. It is necessary since without including such knowledge, a psychiatrist cannot recognise the presence of a mental symptom in a patient. Thus they argue that cognitive approaches to psychiatric diagnostics focusing on the processing of objective data of

patients (self-reports and observations) leave out the role of second-person knowing in the identification of psychiatric symptoms. Gupta, Potter, and Goyer would presumably classify my account as a cognitive one, since I do not stress the role of second-person knowledge for the use of disorder-diagnostic models, but rather imply that the relevant propositions are to be evaluated by self-report, observation, and formal testing. If I am right that they would think of the model-based account as a cognitive approach, then considering their argument is worthwhile, because if they were right, the model-based account would be missing something important and would be wrong.

I will argue that Gupta, Potter, and Goyer are not wrong that second-person knowing is crucial in psychiatric diagnostics, but that it is crucial in a different way than they believe – a way that is in fact covered by the model-based account. I will argue that second-person knowing is not necessary for any case of clinical diagnostics to assess the plausibility of the presence of a certain mental symptom in itself, but rather that the right place for second-person knowing in psychiatric diagnostics is a specific aspect of differential diagnostics. Specifically, I suggest that second-person knowing is involved in setting up and testing the diagnostic hypothesis to show that a psychiatric complaint is not a symptom, but rather an unpleasant but normal mental occurrence. In other words, I claim that we need a second-person perspective to argue that perhaps a complaint could be better understood as a non-pathological phenomenon rather than a symptom. However, this perspective is not essential to assess the plausibility of initially considering it as a symptom before comparing it to the alternative non-pathological explanation.

Gupta and colleagues on the other hand claim that the second-person perspective is already necessary to do exactly this, to assess the initial plausibility of a complaint being a symptom in the first place. But before I come to my argument, let me present the ideas of Gupta and colleagues.

Gupta, Potter, and Goyer (2019) claim that the usual understanding of diagnostic reasoning is focused solely on the cognitive evaluation of objective data about patients, which is not sufficient for the context of psychiatry, since a form of interpersonal (second-person) understanding of patients is needed to draw certain diagnostic conclusions in psychiatry. As they put it in their article, they take issue with the idea that diagnostic reasoning

> is a cognitive process involving the manipulation of objective data that takes place in the mind of the individual clinicians. Instead, we argue that psychiatric diagnostic reasoning requires the clinician to use intersubjective ways of knowing even though they are not explicitly acknowledged as sources of evidence in preeminent accounts of diagnostic reasoning. (ibid., p. 51)

They claim that to really grasp the epistemology of psychiatric diagnostic, "a grasp of the role that this kind of knowing plays is necessary." In this sense, "the process of belief formation through second-person knowing is not only what we do but is necessary to diagnostic reasoning in psychiatry because it is a central means by which psychiatrists gather evidence for diagnosis" (ibid., p. 53).[2]

Their approach to interpersonal understanding differs from the previously discussed proposal of Cooper's (2014), because in contrast to Cooper, Gupta, Potter, and Goyer claim interpersonal understanding to be relevant for diagnosing specific symptoms and so to have a proper diagnostic function. If they are right about this, my account would have missed something. Let us look at their proposal.

Their general perspective on psychiatric reasoning is that with a few exceptions – certain neuropsychiatric disorders such as Huntington's disease – psychiatrists make diagnoses by matching elements from the patient's history of illness to sets of operationalised criteria (e.g., *Diagnostic and Statistical Manual of Mental Disorders* [DSM] criteria) so that "[a]part from a clinician simply being mistaken about the correct criteria for a given diagnostic category, it is difficult to claim that psychiatric diagnoses are right or wrong" (Gupta, Potter, and Goyer, 2019, p. 50). From this very abstract commonsensical description of what psychiatrists do in diagnostics, they proceed to the following claim:

> Unlike in general medicine, diagnostic reasoning in psychiatry is less like finding a solution to a puzzle. Instead, it is more like sketching a roadmap that will enable clinicians to understand their patients' problems to identify means to alleviate their distress. The quality of psychiatric diagnostic reasoning must be evaluated in relation to the extent that it facilitates these tasks. (ibid.)

Here, Gupta and colleagues begin to mix up the intrinsic purpose of diagnostics (namely, to identify the present symptoms and disorders) with practical purposes that diagnosis serves in psychiatry, namely treatment selection. The result in the passage just quoted is that they make a statement about the *purpose* of diagnostics ("being a roadmap") rather than about its *nature* ("finding a solution to a puzzle"). Why we should believe that psychiatric diagnostics, as opposed to diagnostics in other medical fields, should be thought of along these lines remains unclear. Instead, they go on to point out that a grasp of the patient's problems that would feed

2 However, Gupta, Potter, and Goyer are inconsistent (or at least unclear) about how important second-person knowing really is in psychiatric diagnostics. While in the passages quoted here it sounds like its presence is ubiquitous and generally necessary, later in their paper they make more modest claims, such as "intersubjective knowing is not merely a helpful add-on to subjective or objective knowing, but *in some cases* forms an integral part of knowing a person" (2019, p. 57; my emphasis).

into their "roadmap" requirement for diagnostics requires an interpersonal understanding, an insight in terms of the psychiatrist's own imagination and experience of what the person is going through:

> Constructing an accurate roadmap of a patient's psychiatric problem seems to require more than the kinds of objective data about the person that serve as evidence in support of most medical diagnoses. Understanding – or even accurately describing – a person's mental state including her thoughts, feelings, and experiences is intersubjective; that is, it requires an awareness of the patient's world that is mediated by the clinician's own thoughts, feelings, and experiences when in relationship with the patient (Pauen, 2012). (Gupta, Potter, and Goyer, 2019, p. 50)

Elaborating further on the idea of the relevant kind of interpersonal knowledge, they argue as follows:

> [B]ecause the clinician does not have direct access to the patient's mental states (such as his or her beliefs, emotions, desires, motivations, and meaning making), the clinician needs to draw on resources such as imagination and empathy, and to continually confirm one's inferences with the patient while adjusting her understanding of how the patient's world is experienced by the patient himself and noting how the patient shifts and adjusts to the clinician as well (cf. Pauen, 2012). (ibid., p. 54).[3]

After presenting their view on psychiatric diagnostics and the importance of interpersonal or second-person knowing, they illustrate their case with examples that all attempt to drive home the same point in a similar fashion. Let us look at one of these examples: the diagnosis of major depression. Regarding the diagnosis of major depression, they claim that "The criteria set contains some items that can be identified strictly subjectively (e.g., diminished interest, fatigue, feelings of worthlessness) and some that can be assessed objectively (e.g., 5% weight loss). There are no

3 Gupta, Potter, and Goyer (2019) repeat the point once more in terms of Gallagher's (2009, p. 290) notion of "participatory sense making". They paraphrase Gallagher as arguing, first, that "for me to understand how you experience your world, I need to attend, imagine, empathize, and listen with openness to your ways of indicating what it is like to be you and how you make sense of your world" and, second, "that I need to respond to your communications and behaviors with an eye toward clarifying, correcting, offering possible explanations, inquiring more, and seeking opportunities for emotional connection" (Gupta, Potter, and Goyer, 2019, p. 55). They conclude that "making sense of our interactions and relations with others, therefore, seems to require second-person knowing" (ibid., p. 55), which is the kind of knowing discussed in the previous quotes.

items that are explicitly intended to be known intersubjectively" (2019, p. 57). However, they go on to claim that "if we examine certain items more carefully, intersubjective knowing must be at play in their assessment" (ibid., p. 57). To demonstrate this, they pick out the symptom of depressed mood: "Depressed mood most of the day, nearly every day, as indicated by either subjective report (e.g., feeling sad or empty) or observation made by others (e.g. appears tearful)" (APA, 2013, p. 160).

While Gupta, Potter, and Goyer acknowledge that tearfulness as an observed behaviour might be an objective indicator, the problem remains that "tearfulness may also indicate other mood states, such as anger, anxiety, frustration, or joy" (2019, p. 57). They argue that to adjudicate between these possibilities, second-person knowing is needed:

> it seems as though intersubjective knowing is required to interpret the objective observation of tearfulness. Some examples that would support the hypothesis of sadness in the presence of a tearful patient (who is not subjectively reporting depressed mood) might include the telling of life experiences that the clinician finds sad ("finding sad" requires empathy or imagination), and the clinician's own feelings of sadness in the presence of the patient and that patient's life events (which requires emotion). Additionally, behavioral gestures such as a downcast gaze may also provide evidence that the patient is depressed, but this again requires an interpretation of behavior that could be consistent with other emotional states. In other words, although depressed mood can supposedly be assessed in objective terms (seems to be tearful), intersubjective knowing is needed to act as an intermediary between the third-person observation and the first-person state of depressed mood. (ibid., p. 57)

What Gupta and colleagues are thus arguing is that it is valid to make a judgement about the presence of "depressed mood" based on introspective report of things introspectively associated with depressed mood, such as feelings of emptiness or sadness *or* observation of behavioural features such as tearfulness. However, tearfulness alone as an objective behaviour is not enough, they claim, since it may be caused by mental conditions other than depressed mood. Therefore, knowledge that in their view is second-person knowledge must be generated in order to make plausible the interpretation of tearfulness as indicating depressed mood – to assess whether, in light of biographical details and the interaction with the patient, it is plausible that the patient is indeed sad on the empathetic level. This case is supposed to show that second-person knowledge is necessary to make a supposedly possible diagnostic judgement (depressed mood, based on behaviour or tearfulness) plausible.

Another example discussed by Gupta and colleagues concerns generalised anxiety. One of the criteria of generalised anxiety is "[e]xcessive anxiety and worry (apprehensive expectation), occurring more days than not for at least 6 months, about a

number of events or activities (such as work or school or performance)" (APA, 2013, p. 222). Regarding this symptom, they claim:

> Although the state of being worried can be reported subjectively by the patient, whether or not the worry is excessive is more complex. For the physician to judge excessiveness she may first seek out some objective data (e.g., time spent worrying), but similar to the behavior of tearfulness in the depression example, the physician needs a method to make the jump from a certain quantity of worrying to a judgment of excessive. Such a judgment requires understanding of this patient given his personality as well as the context, content, and preoccupation of his worries. […] Such an assessment cannot be objective in the sense that there is no true amount of worrying that is the correct amount for a given person's situation. In other words, there can be no recourse to an objective assessment that will not eventually loop back to an intersubjective assessment. (Gupta, Potter, and Goyer, 2019, p. 58)

Regarding another potential feature of generalised anxiety, namely irritability, they claim that that although it might seem at first glance that this feature might only be known by self-report, this is not the case:

> a person may not endorse irritability, yet the clinician finds that the patient is behaving in an irritated manner in the clinical encounter. It may be that the person does not generally feel irritable, but is feeling irritable toward this psychiatrist at this point in time. However, it may also mean that the person does not understand what irritability is, or does not wish to acknowledge his irritability. To make this determination, the clinician would need to engage in a full range of strategies of knowing the patient to evaluate the credibility and plausibility of the self-report including asking for a more detail behind the subjective report (how the patient is feeling at the moment), using his imagination (how the patient is perceived by others), and trying to establish a shared language to describe the patient's feelings based on what is being discussed and interpreted between them. (ibid., p. 58)

In other words, whether the patient is irritable will again depend a complex set of information, assessing which supposedly requires second-person knowledge of the patient since otherwise one may neither judge the behaviour of the patient to indicate irritability nor be sure that self-reports of present or recent experiences and behaviours indeed indicate irritability. I offer a last quote here that, though made in the context of the depression example, seems to speak to all these examples interchangeably: "This illustrates Pauen's point, as noted, that objective knowledge (of a patient's sadness, based on the observation of tearfulness) needs to be grounded in

some prior second-person knowing" (ibid., p. 58). Now that I have presented the ins and out of Gupta et al.'s proposal, let me turn to assessing it.

Guptas and colleagues' proposal is modest. They do not attempt to provide a full approach to diagnostic reasoning, but rather claim that mainstream approaches to diagnostic reasoning focus on the cognitive processes taking place, and that there is a necessary aspect of diagnostic reasoning that all these cognitive approaches miss. This aspect is that identifying the presence of a psychiatric symptom requires the use of knowledge that can be gained only from the second-person perspective. I assume that they would classify my approach as a cognitive approach that misses this component. Given this assumption, they would claim that something essential is missing in the model-based account. Hence my account would be wrong.

My response will be to argue that Gupta, Potter, and Goyer's (2019) argument fails to establish the necessity of second-person knowledge in the identification of mental symptoms, and that there is therefore no reason to assume that the model-based approach, or any other cognitive approach, fails because of its absence. My basic argument for this is that Gupta and colleagues overstate the epistemic role of second-person knowing by exaggerating the irreducibility of the second-person perspective in a way that does not align with actual claims made in the original sources they use, namely Michael Pauen's work. This is a problem, since instead of offering their own independent arguments for their claims, they rely repeatedly on Pauen as an authority to justify their claims, and suggest that their positions are paraphrases of his. Without Pauen, there is no reason to believe them. When we clarify what Pauen's actual position is and apply it to the context of psychiatry, the irreducibility claim made by Gupta and colleagues collapses, and with it their argument for the necessity of the second-person perspective in diagnostic reasoning. Hence, they fail to show the inadequacy of cognitive accounts including the model-based account. After presenting this principal argument for why they have not established the necessity of second-person knowing in psychiatric diagnostics, I will take one of the examples they provided to illustrate their argument and show why what was shown in principle can also be shown in practice – that is, I will also show why second-person knowing is not necessary in the specific case.

To supplement my criticism of their argument for why second-person knowing is necessary to identify a psychiatric symptom, I will point out the important role that second-person knowing has – a role that does make it highly relevant in psychiatric diagnostics. More specifically, second-person knowing is required in the context of differential diagnostics. Here, however, it does not contribute to identifying whether the patient meets what is required to have a certain mental symptom. Rather, it contributes to our folk-psychologically informed considerations as to how it might be that the patient has a certain distressing mental state or disposition for reasons that are not psychopathological or in other ways medical. In other words, the second-person perspective and second-person knowing do not come in

when learning something about the patient that allows us to attribute a symptom to them; rather, they contribute to the psychiatrist's capacity to recognise when a patient's complaint *is not* a psychiatric condition. This role, however, as I will discuss, is covered by the model-based account. Thus, I show that Gupta et al.'s specific argument for second-person knowledge fails, and that the perhaps intuitively plausible idea that the second-person perspective must play a role in diagnostics is not wrong, but, if considered correctly, is also no threat to the model-based account. Let me begin by clarifying Michael Pauen's understanding of the second-person perspective and second-person knowledge.

The reference to Pauen's (2012) paper "The second-person perspective" at multiple points in Gupta, Potter, and Goyer's (2019) paper to support the irreducible relevance of interpersonal knowing in diagnostics is curious. In his article, Pauen discusses different epistemic perspectives for gaining epistemic access to mental states, which he roughly divides into first-person accounts (introspection), second-person accounts (interpersonal knowing), and third-person accounts (objective data). He argues, amongst other things, that our third-person access to mental states depends on our second-person access in an irreducible way. Crucially, Pauen sums up the irreducible relevance of the second-person perspective in relation to the third-person perspective in two regards. First, he highlights its importance for the initial calibration of our third-person access to mental phenomena:

> the second-person perspective is needed in order to *ground* third-person claims regarding mental states. This is why it cannot be reduced to the third-person perspective. If we want to identify the neural correlates of, say, pain in an experimental subject, we have to make sure that the experimental subject really *is* in a pain state in the first place. Doing this requires the application of the relevant concept, that is, the concept of pain. As we have seen above, employing mentalistic, particularly phenomenal concepts like "pain" implies that the speaker is able to simulate and ascribe the mental state in question. And this just *is* ascribing a mental state from the second-person perspective. (2012, p. 45)

In other words, we as people conducting science or at least attempting to objectify ways of attributing mental states to other need to decide in the first place to whom we are willing to attribute a certain mental state. To calibrate, for example, an MRI method or a questionnaire to recognise a certain metal state or disposition in someone, I first need to determine whom I will take to be in this state. The second-person perspective is therefore a means to calibrate, to ground, my third-person method.

The second form of relevance of the second-person perspective is in making third-person ascriptions of mental states interpretable or understandable: "the second-person perspective is also needed in order to understand third-person claims regarding mental states. Again, the reason is that using mentalistic, particularly

phenomenal concepts requires an imagination or simulation of the mental state in question" (Pauen, 2012, p. 46).

What Pauen seem to be saying here is that the other way the second-person perspective is important is in the understanding of what it means to make a claim that a person is in a certain mental state. The reason is that if we attribute a mental state, especially one that entails an experiential dimension, to someone, fully grasping what it means to attribute this mental state requires that we can imagine what it is like to be in this state in terms of experience and dispositions.

How do Gupta and colleagues employ Pauen to defend their ideas regarding the irreducibility of the second-person perspective? It seems that they believe that in *any* instance of diagnostic reasoning, both the *grounding* and the *understanding* aspects need to occur in order to support the clinician's diagnostic reasoning. This means that every time we ascribe a mental symptom to a patient, we have to do two things. First, we must attribute this mental state to them exclusively by means of our second-person ways of reading someone else's mind in interaction with them, as we do every day in a folk-psychological manner (i.e. grounding). And second, we must have simulatory access to the mental states we attribute, to fully grasp what these mental states are that we are attribute to the patient (i.e. understanding). Yet if we read Pauen carefully, it is not necessary that both aspects must be present in every instance of attributions of mental states.

As discussed above, Pauen believes that the understanding portion that comes with the second-person perspective is generally irreducible if we want to fully grasp what we are talking about in attributing mental states to others, but he does not believe that the grounding portion is irreducible in any instance of attribution. Rather, he believes that some second-person attribution of a mental state is necessary to start with, but that later on, an alternative tracking method calibrated on such attributions may well substitute for the second-person grounding of an attribution. In other words, attribution can perfectly well take place from a third-person perspective once a way to do so has been established. As Pauen himself states very clearly:

> third-person perspective taking is definitely possible, even with respect to mental states like feelings, beliefs, and desires. This is, by the way, what we have to expect given that perspectival differences are differences on the level of epistemic access, not on the level of epistemic objects. If this is so, then it should be possible to take different perspectives on one and the same object – as it is the case with respect to the third- and the second-person perspective regarding mental states. (2012, p. 46)

As Pauen emphasises, the difference between the perspectives is not their *epistemic object* but a difference in *forms of epistemic access*. The very same object (the mental state X) may be epistemically accessed by introspection, intersubjective knowing, or

a third-person method calibrated on our otherwise attributed mental states. Epistemic access *qua* third-person perspective does not presuppose the employment of a second-person perspective to ground it every time anew but only in its calibration phase. What conclusions can we draw from this first comparison between the claims of Gupta and colleagues' and Pauen's positions?

Straightforwardly, it appears that Gupta, Potter, and Goyer may rightfully base on Pauen's account their claim that the *understanding* portion of the second-person perspective (having an empathetic understanding of what it is the patient is experiencing) is indeed irreducible. But they cannot justifiably employ his approach to support the claim that any attempt to diagnose a mental symptom must be grounded in second-person understanding, because it would be perfectly coherent to use only a third-person method that was calibrated on second-person attributions. As a result, all that Gupta and colleagues can claim is that for a clinician to understand what it is like for a patient to have a certain mental symptom requires the understanding portion of the second-person perspective, and requires that at some point the method by which psychiatric symptoms are assessed has been grounded in the second-person perspective. If this is the case, however, attributions can correctly be made *in situ* when previously grounded with a second-person approach, without taking the second-person perspective into account every time they are made. Hence the irreducibility claim that was meant to extend to each diagnostic attempt crumbles to the necessity of some grounding in the past. It amounts only to the necessity of making an interpretation of this attribution *qua* emphasising, in order to know what it means for a patient to suffer from a certain symptom. However, even these two remaining necessities of the second-person perspective face problems if we attempt to apply Pauen's ideas not to normal psychological phenomena almost everyone knows from first-hand experience – like beliefs, desires, or pain – but to the context of psychiatric phenomena.

If we do not just consider how the claims of Gupta and colleagues hold up in light of Pauen´s thoughts on the second-person perspective, but also consider how applicable Pauen's approach is to psychopathological phenomena rather than normal psychological phenomena, it seems that even the remaining necessity fortresses that Gupta et al. could defend turn out to crumble. First of all, the idea that all third-person access must have been grounded at some point on second-person access has seemed plausible so far. However, if we look at psychopathological phenomena, this starts to seem problematic. In contrast to the mental states that Pauen discusses in his paper, which are common propositional attitudes and phenomenal experiences, at least some psychopathological phenomena seem hard or impossible to empathise with in the way required when taking Pauen's second-person perspective. This requires that we arrive at the attribution of the mental state by drawing on our own experiences with this very state and assuming the other to be in the same type of state. As we discussed earlier in the context of Cooper's proposal, it seems hard

or impossible to put ourselves is a valid imaginary perspective that gives us a good grasp on what it is really like to have intrusive memories, be in pre-psychotic prodromal state, or have a Capgras delusion or some other severe mental condition if we have never experienced these things ourselves.[4] For at least some psychopathological conditions, grounding plausibly must have taken place based not on the second-person perspective but on first-person information from patients acquired in third-person forms such as via verbal reports, behavioural observations, and potentially formal cognitive or biological testing. If we do not need second-person access in grounding, then it seems that the second-person perspective is not necessary at all for developing and engaging in the diagnostic reasoning required to diagnose various psychiatric mental symptoms. Therefore, the necessity claims regarding this first aspect of *grounding* and the second-person perspective in diagnostic reasoning seems dispelled. What about the other aspect, understanding?

If what I argued in the last paragraph is correct, we can derive from this another interesting point that speaks against Gupta et al.'s claims. We have already established that a second-person understanding is not a necessary part of any *in situ* diagnostic procedure, since it is possible that such procedures (however they are grounded) may be carried out based entirely on calibrated third-person methods, such as taking into account the self-reports of patients, observing their behaviour, or doing some sort of testing. However, if there are cases in which the *grounding* could not be done in a second-person form for at least some mental symptoms, it seems that there could also be no second-person understanding of these conditions in the context of diagnostics. This means that, in contrast to what we would expect according to Pauen regarding the use of mental terms, when we say that patients suffer from these symptoms without second-person grounding, we are not saying so with a clear empathetic take on what it would be for us to be in this state and therefore understanding this diagnostic label through second-person access. It would then follow that second-person understanding in psychiatric diagnostics is not only not necessary but in some cases is even impossible.

In the last few paragraphs, I have argued plausibly that Gupta, Potter, and Goyer (2019) fail to show that second-person knowledge is essential to psychiatric diagnostics based on their adoption of Michael Pauen's account of the second-person perspective. More than that, I have shown that if we take Pauen's account and attempt to apply it to psychiatric phenomena rather than typical mental states, it even

4 A problem that, as one may add, is today commonly accepted in psychiatry and one of the drivers to include individuals with the lived experience of psychiatric disorders on almost any levels of mental health care, instead of relying solely on people lacking these experiences and imagining how thing are for these patients (see, e.g., Fusar-Poli et al., 2022; Happell et al. 2022; Sunkel and Sartor, 2022).

appears that in some cases it is plausibly not even *possible* to employ the second-person perspective. The irreducible role of the second-person perspective in psychiatric diagnostics that Gupta and colleagues attempted to make plausible therefore seems a claim that remains unproven. Although their point is rebutted in theory, however, one may still think that their examples make a persuasive point that cannot be put aside by a principled argument. Do they not have a point with their examples that plausibly generalises? To address this worry, let me next take one of their cases, the diagnosis of depressed mood, to show how their examples can be deflated as well.

In their example of depressed mood, Gupta and colleagues identify two principal approaches suggested in the DSM to ascribe a depressed mood to a patient. The DSM states that depressed mood is "indicated by either subjective report (e.g., feeling sad or empty) or observation made by others (e.g., appears tearful)" (APA, 2013, p. 160), and they assume that according to the DSM, depressive mood might be diagnosed based on either self-reports or observations. However, in discussion of diagnosing depressed mood by observation, through tearfulness, they seek to find an implicit route via which to back up the necessity of the second-person perspective: "tearfulness may also indicate other mood states, such as anger, anxiety, frustration, or joy" (Gupta, Potter, and Goyer, 2019, p. 57). Thus, to really determine whether tearfulness indicates depressed mood requires more – for example, "telling of life experiences that the clinician finds sad" and "behavioral gestures such as a downcast gaze" (ibid.). Although this information can be assessed objectively, it is lent support only thanks to the second-person perspective, since "intersubjective knowing is needed to act as an intermediary between the third-person observation and the first-person state of depressed mood" (ibid.). Though initially plausible, there are severe problems with this approach.

The first problem is that Gupta and colleagues employ an artificial interpretation of tearfulness. It is true that someone may cry if they are angry, sad, or happy, but just having tears in your eyes crying is not all that the usual thought be entailed by tearfulness. Just as the word *jubilatory* does not only entail that is uttering a laconic "YEY!" but also evokes expectations about other behaviours, tearfulness evokes a certain overall expectation. This expectation would include certain body language (e.g., drooping shoulders, shakiness, downward gaze, motor retardation) and speaking behaviours (speaking more quietly, slowly and hesitantly or with a shaking, raspy voice in an almost logorrheic manner). Of course, there is ambiguity, and it is fine to say that someone is tearful if, for example, she has just won Wimbledon, raises her arms, and screams 'Yes!' with tears in her eyes. However, it seems that this additional qualification (screaming 'Yes!', raising arms, and having won Wimbledon) is necessary to prevent the initially described associations we have with tearfulness from coming to mind when the word is used. If this is true and tearfulness, despite its ambiguity, commonly has a primary meaning (the one I proposed above) in the sense that it is the first thing we commonly think of when we think of "tearfulness,"

it appears fair to also assume that this unqualified meaning is also intended in the DSM, which attempts to be pragmatic and concise in most of its descriptions.

In other words, on a common interpretation of tearfulness, everything that Gupta and colleagues claim to be implicit aspects of what a psychiatrist looks for thanks to a second-person grounded approach is already entailed in the use tearfulness in the DSM. No personal interpretive grounding work based on empathy would then be necessary, just a proper look at the patient. However, not everything that is important for interpreting tearfulness as an objective indicator of depressed mood is covered in this way. What about the relevance of empathy with the patient's sad life experiences, which arguably do not fall under "tearfulness" but are an object of second-person interpretation? Even if all these behavioural aspects are captured by "tearfulness", this dimension is not, and it may well be crucial to interpreting the patient's tearfulness. This brings us to the next problem.

The second problem with Gupta, Potter, and Goyer's proposal is that they as quoted earlier proclaim that to identify patients' tearfulness as an indicator as an objective sign of depressed mood the clinician would need to have to use information about occurrences in the patient's life that the clinician themselves finds sad. However, if the patient's emotional reaction appears to be fully intelligible because it apparently is the result of an event that would have made sad almost anyone (i.e. you loved mother died three weeks ago) this rather seem to speak for the tearfulness to be an expression of a normal state of sadness that is easy to emphasize with and not a sign for a pathological state of depressed mood, so that being well explainable in the context of a sad life event would rather (or at least as well) be point for the differential diagnose of normal sadness rather than depressed mood. That these two things are different, and therefore should also be kept distinct, seems apparent if we judge by existing phenomenological work on depressive mood (e.g., Ghaemi, 2007; Ratcliffe, 2015). To show this let us look at an exemplary description of the depressed mood, in an extract of a description provided by a patient:

> All connections are lost. One feels or is like a little stone, lost in the endless grey of a fading landscape. The sensation of smallness, insecurity and loss can become so strong, that one almost has a feeling of a dream world in which even being oneself is anything more than an abandoned point, like a dried leaf moved here and there in a lifeless autumnal world. […] The solitude of the depressed is different from every other solitude and from every other state of abandonment. One is not alone in a house, in a city or country. For the house is like lost, it does not mean protection anymore; the city is not a familiar city, the country is not homeland anymore, the starry sky burnt by the ice… However, now one is not humans in the flesh, with heart, strengths and spirit to bear solitude–one is a stone. A stone that suffers and thinks; something like that exists. So to speak, one is retro-evolved in stone. Sometime I have thought, "Now I know what is like to be a stone". It is even too clear that this little stone in the cold universe, this enigmatically afraid and doubting man

> strives to grab himself, with ineffable, fervid effort, and find a hold in everything on which he can in some way grab himself (human, animals, things)… […] What is left of the human, when he is deprived of the rational capacity, the intuitive force, the capacity of transmitting and receiving love? A little intellect is left…it is nothing but the bed of a dried stream, a binary on which nothing travels anymore. It is in himself a poor dried leaf. […] It does not matter which fuel you put into the furnace of suffering and for which reason the fire develops. In a sense it is a good that objects are found, even though this sharpens the suffering; because the true and horrible essence of anguish, in the depression, is its lack of an object. (Tellenbach, 1980, pp. 250–252)

It appears that such an experience goes well beyond and is very different from normal sadness. In line with Cooper's (2014) considerations discussed earlier, we would not expect someone unfamiliar with such an experience to be able to properly simulate it in their mind as the expected mental consequence of sad life events. Thus, against Gupta and colleagues, it seems that using the second-person perspective in the context of diagnosing depressed mood is an epistemically problematic move. A clinician following the ideas of Gupta and colleagues, who has never experienced depressed mood and models what he attributes based on their experience of sadness, would, if they meet a depressed patient, wrongly attribute to him sadness and just call it "depressed mood". Also, if they meet someone who experiences something sad and whose tearfulness is fully intelligible in the context of their experiences, they will end up telling them that they are depressed. This will not always be wrong, as many people who experience depression have had sad experiences; however, often they will be wrong because many of us have sad experiences and are tearful, but seemingly few of us at the same time make the experience of depression described above. Many people are simply sad. Hence, the clinician would often end up wrongly telling people they have a depressed mood when there is actually only sadness. As a result, assessing the presence of depressed mood based on evaluating behaviour focusing on the patient's life story, as proposed by Gupta and colleagues, seems to be mistaken and should be discarded.[5] Given the two problems with the

5 One objection I might predict is that it seems that if one indeed assumes that the term "tearfulness" is meant to cover all the observable aspects taken to be associated with depressive mood, and the understanding approach to depressive mood based on life events fails, would that then mean that in psychiatric diagnostics the appearance of tearfulness with all its aspects is indeed treated as sufficient to diagnose a mood as rich and multifaceted as that described by Tellenbach's patient? That seems to be quite an epistemic leap.
My response to this worry is twofold. First, although the manual intends the diagnosis of depressed mood to be possible by self-report only or by observable behaviour only, its seems that what we would want for a diagnosis is self-report and behaviour both suggesting this symptom. And indeed, in a clinical context we will almost always have both kinds of information: most patients tend to speak about their experiences and suffering when they enter

example of depressed mood provided by Gupta and colleagues that I pointed out in the last few paragraphs, it appears that the second-person perspective is not needed to assess depressed mood. Even worse, it might lead the clinician to wrongly assume that they would be able to adequately empathise with what the patient is experiencing. Having provided principled reasons why Gupta and colleagues fail to establish their claim regarding the irreducibility of second-person knowing in diagnostics, and having also provided an exemplary demonstration how one of their examples in support of their case fails, let me come to something more constructive. I will now point out where second-person knowing is indeed crucial in psychiatric diagnostics, though in a very different way than that argued for by Gupta and colleagues, and in a way that is covered by my own account.

While Gupta, Potter, and Goyer's (2019) argument aimed to show the necessity of second-person knowing in diagnostics, more precisely its necessity in assessing specific psychopathological symptoms, there is another area of psychiatric diagnostics covered by my approach in which it plays a role. Second-person knowing comes into the picture when the psychiatrist starts to consider potential diagnostic evaluations of the presented complaint in which, rather than being a psychiatric symptom or a non-psychiatric medical symptom, it could also be a psychological complaint without any symptom value – that is, a psychological phenomenon falling into the scope of normal psychology rather than psychopathology or other areas of medicine.

---

a diagnostic setting. If a clinician finds himself forced to make the diagnosis based on only one information source, be it on self-report (e.g., because he works for a telehealth service) or only by behaviour (e.g., because the patient suffers from mutism and so cannot speak to the psychiatrist), he may have to consider the tearful appearance of the patient alone. However, to justify using tearful behaviour as valid evidence to diagnose depressed mood, he will make sure to have a better basis than just the momentary assessment of the patient;s appearance. First, the psychiatrist will ensure that the preamble of the diagnostic criterion is met: that there are reliable reports that the patient has been in this behavioural state most of the time for at least two weeks. And second, the psychiatrist will consider possible differential-diagnostic options. For example, he will assess whether the patient may have started to take medication that is associated with side-effects such as the development of psychological complaints of depressed mood (e.g., some hormonal contraceptives; Skovlund et al., 2016; Mu and Kulkarin, 2022), such that the timing of the onset of the complaint may better be understood as a psychological side-effect of medication rather than as the symptom of a psychiatric disorder. Only if the criterion is fully met regarding the timespan of tearfulness, and the information gathered about the patient does not better support a differential-diagnostic reason for the patient's presentation, may the psychiatrist provisionally conclude that the patient suffers from depressed mood. However, it should be noted that even with all these aspects in check, my sense is that most psychiatrists would be rather uneasy about making this diagnosis without self-report, and would be eager to get such self-reports from the patient as soon as possible.

If we recall my examples in the second chapter of this thesis, we may think of the example of the complaint alogia. I presented a range of potential ways in which it may be assessed, leading to various outcomes. I showed that the initial complaint may turn out to be a psychiatric symptom under some circumstances but may also turn out to just be a normal psychological phenomenon, such as a hesitation to speak to the psychiatrist out of worry about receiving a diagnosis. In the latter case, the diagnostic procedure relies on a propositional qualitative model containing a set of propositions that, if they all applied to the patient, would together indicate that the reluctant speech of the patient is not a pathological problem. This model, however, was not based on any scientific background knowledge, but on the folk-psychological belief–desire–motivation psychology that we use in everyday contexts. Unless this model is acquired by learning it from another clinician or via the literature (which is the case often enough throughout clinical education), it may be that the clinician comes up with such a model based on considerations using their own capacity to empathise. In this case, they might ask themselves, "Based on my initial idea of who they are, what might be a reason for this person to be so reluctant to talk openly to a clinician?". But again, no case of *in situ* grounding is necessary, even though plausibly it stands at the beginning of all such models and may be entertained to initially develop them. So, the modest role of second-person knowledge, according to my account, is as the original basis, and perhaps sometimes the *in situ* grounding, for the assessment of how plausible it would be to consider a patient's complaint to be a non-pathological psychological phenomenon.

I conclude my discussion of Gupta, Potter, and Goyer (2019) by saying that the second-person perspective does not seem to be irreducible in psychiatric diagnostics in the sense they claim. Hence, they fail to show that cognitive accounts to diagnostic reasoning (a label under which my proposal might fall) miss something crucial in the assessment of symptoms if they do not acknowledge the centrality of the second-person perspective. Gupta and colleagues therefore do not endanger the plausibility of the model-based proposal. Moreover, I have indicated why I believe that the second-person perspective is indeed central to a different aspect of diagnostics, namely the clinician's attempt to understand a patient's experiences and behaviour in a non-pathological sense, which I discussed in more detail in Chapter 2 and Chapter 3.

5.5 Fuchs and Parnas, Sass, and Zahavi: The Phenomenological Proposal

The final alternative philosophical position that I will discuss is one held by researchers in the field of phenomenological psychiatry, a tradition stemming from the ideas of Husserl (1900) and first applied to psychiatry by the philosopher-psy-

chiatrist Jaspers (1913). Among authors inspired by this tradition, some directed their attention towards psychiatric diagnostics. Some well-known figures amongst them are Fuchs (2010) and Parnas, Sass, and Zahavi (2013). For reasons of simplicity, I will call their related positions "the phenomenological proposal".[6]

The basic idea of this proposal is that psychiatrists, when encountering the patient directly, recognise the gestalt of the present disorder in the patient's presentation, unmediated by previous recognitions of symptoms. This proposal is presented in several short remarks. There is no explicit indication as to whether it is intended as a comprehensive philosophical account to psychiatric diagnostics or whether, as in the case of Reznek, it is intended to limit itself to only some aspects of diagnostics. Be that as it may, if one decides to read them as attempting to provide a full proposal, my criticism would be that the proposal is incomplete and that what they supply is inconsistent with my proposal, which is no problem for my account, since I will show that their proposal is implausible. One may also read them as intending only a limited account, most plausibly providing the infantile pattern at work in psychiatric diagnostics, and thus aiming to provide something that meets the adequacy condition rationalising the diagnostic procedure. In this case, their claim would still be incompatible with my proposals regarding this point, and I will argue that their account is implausible. Moreover, I will argue that if their proposal were right, any full-blown account of psychiatric diagnostics would deprive itself of the possibility of fulfilling several of the enumerated desiderata, since their assumed diagnostic pattern makes it impossible to address them. Before I provide my argument, however, let me present their account. To outline their proposal, I will begin with the remarks of Fuchs.

In his article "Subjectivity and intersubjectivity in psychiatric diagnosis", Fuchs (2010) presents a general and affirmative approach to what he thinks experienced psychiatrists do when they diagnose psychiatric disorders. He claims:

> experienced clinicians do not diagnose and practice by ticking off the diagnostic criteria of the manuals. They work with the prototypal approach to diagnosis […] that help[s] to grasp the essence of a phenomenon as an organizing and meaningful "gestalt" over particular details. (ibid, p. 271)

Fuchs does not provide details of why and how the process of the direct recognition of a clinical gestalt is supposed to take place in diagnostics. Parnas, Sass, and

6 My interpretation of the phenomenological proposal, as well as two of its problems (its relationship to pattern recognition and critical reasoning) discussed in this subsection, have previously been developed by me in Kind (2023). I reiterate these points here as part of my extensive discussion on the phenomenological proposal. Without including them, I would not be able to present a comprehensive picture of the phenomenological proposal and its weaknesses for comparison with my model-based account.

Zahavi (2013), however, provide an account of why diagnostics is supposed to directly address the whole gestalt of a disorder in their paper "Phenomenological psychopathology and schizophrenia: Contemporary approaches and misunderstandings", where they discuss the nature as well as the epistemic access to the clinical gestalt in more detail.

In discussing the nature of a disorder's clinical gestalt and the epistemic constraints it puts on the possibilities of how one may recognise a patient's psychopathology, they state that the clinical gestalt of a disorder is "not a simple aggregate; [as] the 'whole is more than the sum of its parts.' This unity [of the gestalt] emerges from the relations between component features and is influenced by the whole (part-whole relations)" (ibid., p. 275). Here, the "components" are symptoms of mental disorders. In other words, the occurrence of whole clinical gestalt is the result of some sort of interaction effect (therefore "more than the sum") of the presence of all the relevant components (i.e., symptoms) at once, which gives rise to the clinical gestalt of the disorder.

With regard to epistemic access to the clinical gestalt of the disorder and its symptoms, they claim that "[a]spects of a Gestalt […] may be focused on in diagnosis or research; but one must remember that these aspects are interdependent in a mutually constitutive and implicative manner" (ibid.). They go on: "What, then, defines a given individual experience/expression as a specific symptom or sign, […] articulates itself from within an experiential expressive whole [of the gestalt]" (ibid.). What Parnas, Sass, and Zahavi seem to be saying here is that while it may be possible to focus on single aspects of the clinical gestalt, this is possible only if at the same time the whole clinical gestalt is also recognised. Therefore, while the clinical gestalt and its components are mutually constitutive in their presence, it is the gestalt enjoys epistemic primacy in that, according to Parnas and colleagues, it is only in the context of this gestalt that symptoms "articulate" themselves – that is, can be singled out. Next, let's try to put together what Fuchs and Parnas and colleagues have offered us.

According to Fuchs, the psychopathological feature recognised first and attributed to a patient is the whole gestalt of a disorder, existing over and above any of its details. This means that the psychiatrist does not first discern symptoms and signs, but rather directly recognises a disorder based on the prototypical gestalt as it shows up in the patient's behaviour and reports. Hence, the first step of the diagnostic reasoning process is that the psychiatrist directly recognises the disorder (e.g., a major depression). This point of Fuchs's recurs, though with a little more explanation of why this has to be the case, in Parnas and colleagues' argument. In the first quoted passage quoted above, they tell us that the gestalt emerges with its components (the symptoms) to the clinician. But at the same time, they make clear in the second quoted passage that there is no way to get a valid grasp of these components other than the gestalt. With this claim, they attribute a kind of epistemic

primacy to the disorder gestalt in relationship to the symptom, as there seems to be no way around starting by grasping this gestalt if one is attempting to grasp the single symptoms. While Fuchs claims the epistemic primacy of the gestalt merely descriptively (this is just what psychiatrists do), Parnas, Sass, and Zahavi provide an explanation of why this is the case.

Merging Fuchs's and Parnas and colleagues' remarks provide us with an outline of how the phenomenological account of diagnostic reasoning is supposed to work, but details about how the initial recognition of the clinical gestalt is supposed to take place are sparse. However, one interpretation of what the phenomenologists have in mind suggests itself in Fuchs's and Parnas and colleagues' use of the terms *prototype* and *gestalt* – namely, that psychiatrists engage in a form of pattern recognition, prototype processing, that leads to the recognition of the clinical gestalt of a disorder. Let me explain.

The prototype theory of pattern recognition in cognitive psychology is a model of pattern recognition according to which different prototypes of objects are memorised by the system:

> in the process of pattern recognition, outside simulation only needs to be compared with the prototype, and the sense to objects comes from the matching between input information and prototype. Once outside simulating information matches best with a certain prototype in the brain, the information can be ranged in the category of that prototype and recognized. (Pi et al., 2008, p. 435)

An essential feature of this kind of pattern recognition is that it contains top-down processing and no bottom-up processing (ibid., p. 436). Recognising the relevant *object begins with the matched prototype itself*. There is an immediate matching between information input and prototype, rather than an intermediate step in which aspects of what will be identified as a prototype are first recognised independently and then found to constitute a prototype, which would be a bottom-up process. This description seems to match well with the idea of a direct and unmediated recognition of the disorder gestalt, which may later be discerned in its constituent elements. But does the notion of a prototype understood along these lines fit with the idea of the gestalt? We might just take Fuchs's use of this term as an indication that it does. But we can do more than this.

For the notion of prototype employed in the context of top-down pattern recognition to map onto the notion of a gestalt, a gestalt would need to be a complex entity consisting of in-principle separable elements that together form the prototype. It certainly sounds like a promising fit, if we remember that Parnas, Sass, and Zahavi (2013, p. 257) talk about how the "unity [of the gestalt] emerges from the relations between component features". However, we can back up this link even more strongly if we consider the notion of the gestalt from other sources. Ehrenfels, one

of the founders of gestalt psychology, tells us that having a gestalt representation of something means having a

> content of presentation bound up in consciousness with the presence of complexes of mutually separable (i.e., independently presentable) elements. That complex of presentations which is necessary for the existence of a given *Gestalt* quality we call the foundation of that quality. (Ehrenfels 1890, in Smith, 1988, p. 93)

The gestalt (and then also the clinical gestalt of a disorder), understood in this way, is a whole consisting of related elements. Thus, understanding the phenomenological proposal along the lines of a prototype-based top-bottom pattern recognition process appears plausible.

As a result, the following picture emerges. According to Fuchs, the psychiatrist directly perceives the disorder as complex or gestalt in the patient after being confronted with diagnostic information and without further explicit cognitive efforts. This interpretation of Fuchs's general idea also matches with Parnas and colleagues' elaborations. As we saw, they claim (in the first quotation I offered above) that while the disorder and its symptoms are *ontologically* mutually constitutive, the clinical gestalt nevertheless enjoys *epistemic* primacy. As we saw in their last quote, they seem to believe that signs and symptoms are epistemically secondary insofar as the psychiatrist determines them after identifying the disorder. This interpretation follows from their statement that only once the gestalt of the disorder is recognised can a psychiatrist proceed to identify the symptoms and signs of that disorder in the patient. The gestalt must be recognised first, since only the clinical gestalt of the psychopathology allows for a symptom or sign to "articulate itself" and therefore become epistemically accessible to the psychiatrist. Reinterpreting this idea considering research in cognitive science, we may say that the phenomenological proposal for understanding psychiatric diagnostic reasoning is an automated form of prototype-based pattern recognition. This form of prototype-based pattern recognition leads psychiatrists to form cognitively unmediated assumptions (i.e., assumptions without explicit inferential reasoning) about the presence of a disorder in a patient that occurs to the psychiatrist as a gestalt quality of their perception of this patient. Any details of the psychopathological state of the patient are thereby epistemically secondary.[7] Particular features of a disorder can be accessible and become relevant

7 To interpret the phenomenologists' proposal as the idea that psychiatrists can directly access patients' overall psychopathological mental condition via a quasi-perceptual process fits with other views held by authors from the phenomenological tradition. Zahavi (2019), for example, defends a similar position, not regarding psychopathological mental conditions but for our overall interpersonal access. In his view, our everyday knowledge about each other's minds (e.g., about whether someone is angry) is also acquired in a direct quasi-perceptual manner without cognitive mediating processes.

to the psychiatrist only if the disorder is already recognised. The phenomenologists' proposal therefore has the feature of being epistemically top-down. It is a *disorder first, symptoms second* account of psychiatric diagnostic reasoning. Now that I have presented the phenomenological proposal, let me come to discuss it.

If we understand the phenomenological proposal as attempting to providing an adequate description of the method that underlies clinical psychiatric diagnostics, the first problem occurring is descriptive adequacy. This problem occurs in two forms. First, it results from the use of a very abstract, almost nonexistent, description of the diagnostic process itself onto which the phenomenologists map their proposal. The second version of the problem results from implausibilities concerning the proposed method itself. Let me discuss both in turn, starting with the aspect of the descriptive adequacy problem arising from the abstract basic picture of psychiatric diagnostics.

The phenomenological proposal provides only a rather abstract picture of what it takes to be the diagnostic process it is mapping onto. There is no mentioning of the steps of the diagnostic process, no talk about the screening taking place at the start of diagnostics, and no mention of the case formulation. It appears that either they presuppose a basic description of the process of psychiatric diagnostics that is so abstract (i.e., psychiatrists see patients and then diagnose them) that the interesting details of the diagnostic process are not explicitly discussed to any degree of detail, or they consider that good psychiatric diagnostics does not follow an approach that is more structured than this, for example as presented in my first chapter. In the first case, it seems that they assume an extremely abstract picture of psychiatric diagnostics, which, if we explained it by proposing a method that comprehensively covered it, would nonetheless offer only a very vague understanding of the actual diagnostic process in all its details – a rather unsatisfying result. If the second option is the case, then they simply seem to have an idiosyncratic understanding of psychiatric diagnostics, which also would disqualify their approach as relevant to understanding how what we would consider proper contemporary psychiatric diagnostics works. This would render their proposal uninteresting for the scope of this investigation, but also not opposed to my view. The more charitable interpretation, which also keeps the phenomenologists in the game as proposing an alternative to my account, would be to assume that they do wish to address what is considered proper psychiatric diagnostics, and not some rather totally different way of diagnosing patients. We should therefore interpret them as intending to adhere to professional standards rather than as considering a form of diagnosis that violates professional standards. If we do so, and thus assume the first case, their proposal nonetheless seems to be an unsatisfyingly abstract way to present a method of psychiatric diagnostics, due to their mostly nonexistent description of the process of diagnosing itself and the fact that, drawing on my discussion above, it appears that the phenomenological proposal makes no effort to be in touch with what is commonly as-

sumed good contemporary psychiatric diagnostic practice. In its presentation, the phenomenological account lacks the connection to clinical reality, and thus seem unfit to be considered the presentation of a method that maps satisfyingly onto the aspects of what psychiatric clinicians do. But as mentioned, this is not the only point to consider. Even if we were satisfied with the vague picture of the actual diagnostic process that they operate with, there would be a problem with their proposal itself.

The second problem concerns the inconsistency of the phenomenological account with widely regarded empirical research on clinical diagnostics. In research conducted by psychologists and medical education researchers, two types of cognitive processes have been identified as relevant in diagnostic reasoning: bottom-up pattern recognition (e.g., Conderre et al., 2003; Groves, O'Rourke, and Alexander, 2003) and analytic reasoning (e.g., Croskerry, 2009). Since these two types of reasoning are widely recognised as being involved in diagnostic reasoning, any theory of diagnostic reasoning should either be coherent with the assumption that they are present, or if not, provide good reasons why – going against common sense and research – this is not the case. However, neither cognitive process has a place in the phenomenological account, nor does this account provide reasons why not to expect the presence of this type of reasoning. Let me elaborate.

At first glance, one might be inclined to interpret the phenomenological approach to gestalt recognition along the lines of bottom-up pattern recognition. However, it is not understood as such, at least in the context of research on medical cognition. In this context, bottom-up pattern recognition is considered a highly automatic, cue-based, feature-outcome associating process, whereas the cues are the signs and symptoms of the disorder, while the pattern is identified with the syndromal disorder diagnosis (Loveday et al., 2013). This, however, is not what is suggested in the phenomenological proposal as worked out earlier. While in bottom-up pattern recognition, symptoms and signs must be individuated and identified first, and only based on them is there an automatic detection of the disorder, the phenomenological proposal turns this process upside down. The phenomenological proposal, as a *disorder first, symptom second* approach, grants epistemic primacy to the disorder gestalt (i.e., the pattern). In their approach, the disorder must be recognised prior to the discerning of symptoms. It therefore appears that pattern recognition in the sense typically espoused by researchers in not included in the phenomenological proposal.

Analytic reasoning also plays no role in the phenomenological account. Analytic reasoning involves the explicit and careful consideration of the patient's presentation, identifying symptoms given certain background knowledge, and carefully weighing which diagnostic options are most plausible based on the available evidence. While pattern recognition is often used in simple diagnostic tasks (e.g., diagnosing a flu), analytic reasoning is commonly employed when medical experts face complex or ambiguous diagnostic scenarios (Croskerry, 2009). Such complex-

ities and ambiguities often appear in psychiatric cases. On the phenomenological account, by contrast, the disorder is first recognised as a whole gestalt, and symptoms are individuated only after the gestalt of the disorder has been recognised in the patient. It therefore seems that analytic reasoning plays no part in the actual diagnostic reasoning process that identifies a disorder. If such reasoning is exercised at all, it would provide only a circular form of post-hoc justification for the diagnostic intuitions by which the clinician recognised the patient's disorder in the first place, since it is this initial diagnosis that forms the basis on which (rather than on any independent grounds) the confirming symptoms would be recognised. As the phenomenologists claimed in their quotes symptoms are epistemically individuated only in the context of the previously recognised gestalt. If psychiatrists really diagnosed in the manner described by the phenomenological account, it seems that they would not engage in analytic diagnostic reasoning.

Neither type of reasoning occurs in the context of the phenomenological proposal, and no reason is provided to explain why they should not occur. It therefore seems that the phenomenological proposal goes against what we should expect to be present in the context of psychiatric diagnostic reasoning, without any reasons that could support the rejection of the expectation that a proposal should cohere with research insights into the usual presence of pattern recognition and analytic reasoning in diagnostic reasoning. If the burden of making such an argument is not met, this seems to be a problem for the proposed method of the phenomenological account; its proposed method simply does not seem to be in line with what we should expect from a method of diagnostic reasoning. Hence, the description for their proposed method of diagnostic reasoning seems to be inadequate. It is inadequate because it is too abstract to qualify as a satisfyingly detailed understanding mapping onto the actual steps of psychiatric diagnostics, and also because in itself because it seems to be inconsistent with some well-founded expectations we can hold regarding a proposed method.

While the previously discussed point would apply to the phenomenological proposal no matter whether it intended to be a comprehensive answer to the Methodological Question or only an aspect of what would provide such an answer, there are additional problems if we assume for a moment that the former is true. If it wanted to present a full answer to the Methodological Question, the phenomenological proposal would fail to address two adequacy conditions for such an answer. First, it would not provide us with any rationale for the method they propose. There is no discussion of the rationale, the inferential strategy, or the inferential patterns at work in the *disorder first, symptom second* gestalt approach that would support its procedure, and this kind of discussion is needed for a methodology of the proposed method. Moreover, the topic of justification remains unaddressed. Due to the aforementioned lack of a rationale presented to back up their method, they cannot spell out the internal justification of their method – that is, what the method's internal

principles are that ensure it justifies drawing a conclusion accruing to the method. Nor did they make plausible how the employment of a disorder gestalt could be supported *qua* external justification, because they provided no information on where these disorder gestalts used by psychiatrists come from and why they should be considered valid guides for diagnostics. In sum, it seems that the phenomenological proposal, wither we take it to be a full or only a partial attempt to address the Methodological Question, ends up being either an unsatisfying or an unsatisfying and incomplete proposal, respectively. Next, as indicated, I want to look at the problems the phenomenological proposal produces if we consider the desiderata for an answer to the Methodological Question.

There are several desiderata that the method proposed in the phenomenological proposal seem to be incapable of addressing. First, it does not address the intra- and interpersonal critical diagnostic reasoning that leads to revising one's own earlier diagnostic conclusions as well as critically discussing diagnostic conclusions among colleagues and rationally resolving disagreements. Second, the phenomenological proposal seems unable to identify diagnostic malpractice or to support its differentiation from mere misdiagnosis.

Critical diagnostic reasoning, as already discussed in the previous chapter, is used by clinical professionals who are trained in it and expected to practice it, no matter their specialisation (e.g., Marmaden, Schmidt, and Riekers, 2007; Harjai and Tiwari, 2009). Engaging in critical diagnostic reasoning means critically examining one's own or another's diagnostic judgements in order to avoid making mistakes in diagnostics due to biases or other errors in reasoning. Questions like "Why exactly should I draw this diagnostic conclusion?", "What could be an alternative explanation?", "Did I consider all available and potentially relevant information?" are typically asked when engaging in this kind of reasoning. Critical diagnostic reasoning can take place intrapersonally (by critically evaluating one's own diagnostic judgements) or interpersonally (by evaluating the diagnoses of others, as a clinician who supervises or works on a team might). To engage in critical reasoning about the justification of one's diagnosis in a non-circular way, however, analytical diagnostic reasoning is a prerequisite.

The problem here for the phenomenological account is that if a psychiatrist were to diagnose in the manner it prescribes, this intra- and interpersonal critical diagnostic reasoning would be impossible, or at least unnecessary. Intrapersonal critical reasoning would not be required, since considerations of a more plausible alternative diagnosis, given the symptoms and signs of disease, could not be found. In the phenomenological proposal, it is the initial diagnosis that determines what signs and symptoms the clinician will be able to individuate in the patient. It follows that any attempt to evaluate one's own diagnosis will, by the logic of the phenomenological account, lead to a necessarily self-confirming result. To get out of this vicious

circle, the identification of signs and symptoms has to be achieved analytically as described above.

There is a related problem when it comes to interpersonal critical evaluation. Part of the critical interpersonal discussion of diagnoses involves explaining to other clinical experts why one has given a particular diagnosis. These experts suggest potential alternatives in order that they may eventually come to an agreement on the best diagnostic decision. If, however, all symptoms and signs that the clinicians recognise depend on their initial diagnoses, then pointing out other symptoms or signs to them would be hopeless because they would not be able to individuate those symptoms or signs independently of their original diagnosis. Interpersonal disagreements about diagnoses would become unresolvable and farcical, since any one participant could never rationally convince the other participants who disagreed with him. This is because it would be impossible for both sides to recognise the symptoms and signs that could serve as counterevidence to their own diagnostic proposal, given that those symptoms and signs would not fit the gestalt they recognised. The symptoms and signs individuated by each side in the disagreement would, at least in principle, be epistemically inaccessible to the other. It appears that in sum, the phenomenological proposal not only fails to provide an explanation for the intra- and interpersonal correction of diagnostic judgements, but moreover it is set up in a way that arguably makes it impossible for critical diagnostic reasoning – which we usually see and expect in the context of clinical diagnostics – to take place. Next up is malpractice.

To identify malpractice and distinguish it from mere misdiagnosis, what we need to be able to do is to identify what went wrong in the diagnostic process. We need to decide whether the wrong diagnosis was given due to missing, insufficient, or wrong information available to the clinician by reasonable information-gathering efforts, or whether the clinician themselves has done something wrong with the in-principle sufficient information base in the context of their diagnostic reasoning efforts. In the first instance, we would have a case of mere misdiagnosis; in the latter, it would be a case of malpractice. However, if we look at the phenomenological proposal, it is not clear how we should make this distinction.

We do not know what information is supposedly crucial for diagnostic decision-making according to the phenomenological approach, so we cannot evaluate when sufficient or insufficient information was attained and whether this information (or lack of it) should be considered responsible for a wrong diagnosis. Moreover, since the phenomenological approach provides us with no guidance on how the disorder gestalt is discovered by the psychiatrist, we have no way to assess whether, in the process of coming up with one's diagnostic conclusion *qua* the recognition of the supposed disorder gestalt, any mistake has taken place. Diagnostics according to the phenomenological approach remains a black box regarding the relevance of different types and tokens of information in any given diagnostic process and regarding

the very process by which the diagnostic conclusions are drawn. The immunity towards being an object of meaningful critical diagnostic reasoning, as discussed in the previous paragraphs, therefore has the secondary effect that any result achieved by the recognition of the disorder gestalt also seems to be unfit to be evaluated as potentially being a case of misdiagnosis or malpractice if it turns out to be wrong.

Having pointed out these problems of the phenomenological account, let me now compare how the model-based proposal holds up against it in all these problem domains. First, regarding descriptive adequacy, it seems that the model-based approach performs better than the phenomenological approach. As briefly discussed at the start of the last chapter, my proposal meets the adequacy requirement of being cognitively realistic, which entails that its proposed explanation of psychiatric diagnostic reasoning is true to a detailed description of the steps of the diagnostic procedure and ensures that the assumed inferential steps make sense of these procedures. Second, my discussion of how the model-based proposal explains intra- and interpersonal critical diagnostic reasoning also shows that it is able to put forward a plausible and helpful proposal on this front, which, as discussed, is fully blocked for the phenomenological proposal, which even undermines the possibility of such reasoning taking place. Thirdly and finally, regarding the inclusion of the empirically supported types of reasoning that are commonly encountered in the context of diagnostic reasoning, but that do not seem to play a role in the diagnostic proposal of the phenomenologists, again the model-based account holds up well. As I proposed in Chapter 3 and repeated in Chapter 4, the inference from symptoms to disorders can potentially (in well-trained diagnostic experts) be conceptualised as a rule-based pattern recognition process. Moreover, analytic reasoning plays a prominent role in the model-based account. This account assumes that the decision as to which complaint should be evaluated as constituting which kind of psychiatric or medical symptom is a detailed and thorough process that is carried out in the context of diagnostic reasoning, and then again when it is explicated in the context of the required case formulation that puts together the diagnostic conclusions at the level of symptoms as well as the disorder level, and supports them by the information considered crucial to support the diagnostic conclusions thus drawn.

In sum, the phenomenological proposal has at least two significant problems. The first major problem is its detachment from actual clinical diagnostic practices. Its proposed method seems to be inconsistent with plausibly expected features of a method of psychiatric diagnostic reasoning. The second problem is its inability to explain critical diagnostic reasoning and to help us to understand and discern the differences between diagnostic mistakes and malpractice. If we evaluate it as a full-blown proposal to address the Methodological Question, we would have to add that it does not address two of the relevant adequacy conditions – namely, providing the rationale for the method's operations and demonstrating how we should consider the results of the method to be justified, both internally and externally. All these are

points in which the model-based account I have presented over the preceding chapters performs better. It is intimately close to actual diagnostic practice, as shown in Chapters 1 and 3; it can make sense of intra- and interpersonal critique and revisions of diagnostic decisions as we see them every day in the clinic, as shown in Chapter 4, and it can help us to understand the difference between malpractice and misdiagnosis and provides guidance on how to assess which of the two took place, as also discussed in Chapter 4. Considering these problems of the phenomenological approach that the model-based approach does not encounter, it seems the model-based proposal is preferable.

5.6 Conclusion

In this chapter I selected several philosophical contributions to the topic of psychiatric diagnostics that *prima facie* presented alternatives to or problems for my account to psychiatric diagnostic reasoning. I showed that on closer inspection these accounts variously turn out to not actually not concern psychiatric diagnostics itself (Cooper); to concern it, but in a way that is in principle compatible with my approach, though my approach does a better job of providing detailed discussions of the overall process and how to use this understanding to address relevant topics in the context of psychiatric diagnostics (Reznek and Murphy); or to concern aspects of psychiatric diagnostics that my proposal also deals with in a way that seem incompatible with my own approach, but when putting forward criticism or alternatives to my account, to be plagued by problems that make their proposals less plausible than my model-based account (Gupta, Potter, and Goyer). Finally I discussed the phenomenological proposal (Fuch, Sass, Parnas, Zahavi) which I showed to fail several adequacy conditions for a proper answer to the Methodological Question and to be detached from the clinical reality of diagnostics, making the model-based account I defend preferable over it. In the end, it seems that my account is the best candidate – one that, as demonstrated in the previous chapter, meets all adequacy conditions, allows us to address several interesting sub-questions regarding psychiatric diagnostics, and does so better than any of the candidates discussed in this chapter. The model-based account of psychiatric diagnostics seems to be the most well-rounded candidate to provide an answer to the Methodological Question.