The Indeterminate Present
Essays on Quantum Mechanics and the Open Future

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A mia madre.

CONTENTS

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« When you think
Your toys have gone berserk
It's an illusion
You cannot shirk »

Spellbound, Siouxsie & the Banshees

INTRODUCTION

The dissertation is a defense of the following conditional claim: if there are objective collapses of the wavefunction, then the future is genuinely open. Although this is no radically new idea, the strategy I shall use to defend it is a new one. It proceeds in two main steps. First, building upon the recent literature on metaphysical indeterminacy in quantum mechanics, I argue for the view that systems in superposition have be interpreted as objectively indeterminate state of affairs. Second, I propose an alternative way to think of openness, according to which the future is open as of *t*, if and only if there is an indeterminate state of affair *S* at *t*, and *S* becomes determinate at *t'* (with *t'* later than *t*). To argue for the second step, I will give an analysis of the objective collapses of the wavefunction as the becoming determinate of previously indeterminate systems. Furthermore, in developing my arguments, I will also make some remarks concerning the ontology of objective collapse interpretations of quantum mechanics, the issue of whether metaphysical indeterminacy can be at some derivate level of reality, and the possibility of the openness of the future being an emergent phenomenon.

We are often told that modern physics favors a B Theoretic conception of time[1]. The main reason for this, is that Relativity implies that it is impossible to specify a preferred frame of reference. This, in turn, makes the notion of absolute simultaneity between any pair of events, as needed by the A Theory, straightforwardly inconsistent[2]. However, we know that Relativity is not the whole story. Last century's physics gave us another amazingly well-confirmed theory, Quantum Mechanics (henceforth, QM), and arguably, the two theories cannot both be true at the same time. Thus, some philosophers have found vindication for some A Theoretic conceptions of time in certain features of QM (Popper 1982, Shimony 1993, Lucas 1998, Christian 2001, *inter alia*).

As to the how they have done so, we can broadly distinguish two different lines of reasoning.

(1) Non-Locality — It has been argued (Popper 1982, Shimony 1993, Lucas 1998) that non-local interactions between space-like separated systems, by occurring *instantaneously*, and in a frame

[1] The labels 'A'/'B Theories' have been introduced by Richard Gale in that anthology *The Philosophy of Time* (1967), where he analyses the famous argument for the unreality of time made by John McTaggart Ellis McTaggart (1908). Roughly, the A Theory of time maintains that the properties of 'being past', 'being present', and 'being future' (the so-called A-properties) are irreducible features of reality, whereas the B Theory claims that those properties can be fully explained in terms of B-relations such as 'later' and 'earlier'. For an overview, see: Le Poidevin (1998), and Maudlin (2007, ch. 4).

[2] Although the incompatibility between A Theory and Relativity was rather acknowledged from the very first formulation of the theory, the firsts who explicitly formulated the objection are Putnam (1967), and Stein (1968). Others who followed this line of reasoning include: Savitt (1994), Sider (2001), Wüthrich (2013). There are, of course, many replies to the objection: Craig (2001), Markosian (2004), Zimmerman (2011), *inter alia*. For a general overview on this topic, see: Saunders (2002). It is worth noticing though, that in its first formulation also the B Theory would be incompatible with relativistic theories. The crucial difference between the two, in fact, is that the B Theory can relativize 'earlier' and 'later' relations to frame of reference without changing the spirit of the view. It is highly controversial that any such move can be made by the A theorist.

independent way, force to the reintroduction of a preferred foliation of space-time, thus providing a notion of absolute simultaneity as it is needed to formulate the A Theory[3].

(2) **Collapse of the Wavefunction** — Some physicists and philosophers (Stapp 1977, Whitrow 1980, Christian 2001, Lucas 1999, Gisin 2016) have argued that objective collapse interpretations of QM, such as the well-known Ghirardi Rimini & Weber approach (GRW in short, 1986), if true, would support the view that time genuinely passes, and that the future is open not only from an epistemic perspective.

To be clear, notice that (2) can be taken as independent from the A/B Theory distinction, as I will show more clearly in the fourth chapter.

In his recent book *What Makes Time Special?*, among many other things, Craig Callender (2017) has spent a tremendous effort to show why both options above need be rejected[4]. While I will leave the discussion about (1) to another occasion, this dissertation is, in a way, an extensive response to Callender's arguments against (2). So, before getting started, it will be useful to have a look at his view on these issues.

The core claim behind (2) might be expressed by the following conditional:

[3] Here is Popper (1982), for instance, arguing from non-locality to a Lorentzian interpretation of Relativity: « If there is action at a distance, then there is something like absolute space. If we now have theoretical reasons from quantum theory for introducing absolute simultaneity, then we would have to go back to Lorentz's interpretation ». For an insightful and updated discussion, see Baron (2017).

[4] The chapter from Callender's book I am commenting here is a reprinted version of a previous paper from (2008). Although there are some few differences between the two, I will use both versions almost interchangeably.

> **Quantum Open Future** (QOF) — *If* there are objective collapses of the wavefunction (as described by GRW and the likes), *then* the future is open in a genuine, not only epistemic sense.

In the face of QOF, the opponent of the open future thesis can simply reject objective collapse interpretations altogether. For instance, she could appeal to the fact that (as argued in Albert (1992), *inter alia*) it is not yet clear whether they solve the measurement problem. Though attractive for whomever is unsatisfied by this particular interpretation of QM, this move is philosophically of little or no interest. First of, because it does not seem acceptable (at least to many of us) to reject a physical theory in order to defend a metaphysical view (no matter how plausible the view is). And secondly, to those who are not moved by such general considerations, I can simply suggest they take QOF as an interesting thesis *per se*, thus simply conditionalising on the truth of GRW type of interpretation.

Instead of arguing directly against the antecedent, Callender aims at denying that QOF expresses a true entailment, and he does so by considering, and eventually rejecting, three ways in which the entailment could be justified. First, Callender notices that the connection between collapse theories and the open future could be found in the role played by objective probabilities in QM.

> Perhaps the link with openness and transience arises instead from the single-case objective probabilities needed for a collapse theory? Shimony and Popper stress throughout their work the benefits of a truly probabilistic process, seeing in it an open future, the flow of time, and even freedom. The intuitions underlying these links are clear enough. Suppose at time t there is an objective chance of 0.5 that a radium atom will decay tomorrow. For this to be true, some believe, there must not "already" at t be a unique determinate future with (say) a decayed radium atom in it tomorrow. That

> would entail, in one way of understanding objective chances, an objective probability of 1, not 0.5. Since the tenseless theory of time entails that there is a unique determinate future – in a sense – the existence of non-trivial objective probabilities requires the tensed theory of time. (2008, p. 69)

Although this view could take different forms, the core idea is that a satisfactory *explanation* of the truth of propositions referring to non-trivial probabilities (those other than 0 and 1), requires the denial of the existence of a unique future. Callender's rejection of this idea starts with the consideration that, even if true, it would be so only according to certain interpretations of *chance*—such as the Popperian propensity interpretation —but not for all of them—for example, the Lewisian one. Furthermore:

> […] the justification for the line that a "fixed" future implies trivial values of objective chance is similar if not identical to the famous argument for fatalism. The sea battle tomorrow spoils freedom today just as the radium atom's decay tomorrow spoils non-trivial values of chance today. But if one believes, as I do, that the argument for fatalism is flawed […] then the existence of the sea battle tomorrow doesn't undermine freedom today; one therefore needn't see the tenseless view and its implications of either a sea battle or not tomorrow as a threat to freedom. Nor need it be a threat to non-trivial chances. The actual world may contain our radium atom in it decayed tomorrow, yet today it still may have a one-half chance of decaying. (2008, p. 70)

I think Callender is right in reminding us that any appeal to objective probabilities, along with the indeterminism of the laws that justifies the true claims about those probabilities, is generally misplaced when used to derive conclusions about the openness of the future. As I will say with more details in chapter 4, section 4.2, indeterminism of laws is neither necessary nor sufficient for the open future (see also, Barnes & Cameron 2009). Thus, I grant Callender that the defender of the conditional expressed by QOF should not take this way to argue for her view.

A rather different, and at least *prima facie* more proposing strategy, is to argue for the existence of a mapping between states of *superposition* and objective openness of the future. In fact, in order for QOF to get correctly what a collapse is in the first place, as Callender correctly points out, we would need the *superposition/eigenstate* distinction to map sharply the *open/fixed* one. Roughly, through this mapping what you would get is that whatever system is in a superposition state will somehow indicate facts about *openness*. And similarly, everything that is represented by the theory as being in an *eigenstate* is meant to reproduce *fixed* facts.

However, according to Callender, it is not so clearly the case that *superpositions* and *eigenstates* can be mapped to the *open/fixity* distinction, for the following reason:

> […] the symmetry of Hilbert space implies that we can write out our wavefunction in any of an indefinite number of bases, e.g., position, momentum, spin. A wavefunction that is a superposition in one basis may not be a superposition in another; for instance, the wavefunction of *x*-spin down is a superposition of up and down spins in the *z*-spin direction. Here a collapse to fixity in *x*-spin buys openness in *z*-spin. (2017, p. 95)

The mechanism of collapse, by itself, does not pick up a preferred basis. Therefore, whenever a superposition on a certain basis disappears due to collapse, what happens is that the observable corresponding to another basis will immediately turn into a new superposition, provided it is incompatible with the first one (as it is in the example given by Callender of the *x* and *y* spin, and in virtually every case of superposition). Callender is here thinking of the *collapse* of the wavefunction as it appears in textbook quantum mechanics, wherein no basis is privileged over the other. I must say that am not completely certain whether this objection goes through even for the textbook quantum mechanics case, since one could stress that that what makes a certain basis privileged in this case is the fact that someone performs a measurement. And once the measurement is performed, there is a basis which gives the right way to map *openness* and *fixity*. Be that as it may, what is more important is that in the case of spontaneous collapse models there always is a preferred basis—as I will show in chapter 1, section 1.3, this is mostly *position*—and we should only care about position superposition states and position eigenstates when we speak about the asymmetry between fixed past and open future.

Callender grants that much, but then immediately responds with a final objection to defenders of QOF by reminding that « in any realistic collapse theory such as GRW one doesn't get collapses onto eigenstates, but only near eigenstates » (2017, p. 95). Hence, even granting, by focusing only on position states, that there is a mapping between *superposition/eigenstate* and the *open/fixed* distinction, in GRW type of interpretation collapses are never to *eigenstates*, but only *near* ones. And without *eigenstates*, the thought would go, we will not end up having openness properly. As I will show in chapter 1, section 1.3, the lack of eigenstates is a general problem for GRW (Lewis 1995), and one that is independent from any discussion on the open future. It is known in the literature as the *tails problem* (Albert & Loewer 1990), and it is actually considered by many philosophers and physicists to be the main flaw of spontaneous collapse models. It is fair to point out, however, that many solutions to the *tails problem* are on offer (for a recent review of the literature, see Gao ms), and either Callender is willing to reject all of them altogether (along with with the viability of spontaneous collapse models) or the proponent of QOF could simply defend their view, once again, by stressing that it is true only conditionalising on the solutions to the *tails problem*. That is the path I will follow in this dissertation, since I believe that if the tails problem is solved, collapse models can be taken as an ideal starting point to argue for the open future *via* the mapping between *superposition states* and *openness*.

What will take a central stage in developing my arguments, is the intuition that we can reinforce our understanding of the difference between both dichotomies I aim at connecting—the *superposition/eigenstate* one on the one side, and the *openness/fixity* one on the other—by first understanding another dichotomy, the one between *indeterminacy* and *determinacy*. Indeed it is no chance, I believe, that *indeterminacy* understood in a robust, metaphysical way, recently made his appearance in discussions about both quantum mechanics and the open future. However, it did so in quite different ways, and we should avoid the risk of confusing two kinds of indeterminacy that might share, after all, nothing but the name. In one case, the indeterminacy about the open future is unsettledness between incompatible options. In the case of quantum mechanics, as I will show in the first and second chapters, it is not so clearly the case.

For these reasons, I will spend a large amount of time examining what sense we should make of quantum indeterminacy (chapter 1), how to account for it with metaphysical tools (chapter 2), and whether it is a part of the fundamental furniture of the world or rather a somewhat emergent phenomenon (chapter 3). Only when a better understating is made about all these crucial issues, will I eventually get back to connect quantum mechanics and the open future (chapter 4).

CHAPTER 1

Quantum Indeterminacy

1.1 A New Perspective on Quantum Mechanics

This chapter is meant to introduce the reader to the topic that will be at the core of the following discussion, namely *quantum indeterminacy* (henceforth, QI). Many, if not all the things that have been said about what the world would be like according to quantum mechanics, eventually became with the years a matter of harsh philosophical dispute. What I will be saying here about the theory is admittedly no exception at all. However, a huge merit I believe of the recent debates on QI, is that it gives us an interestingly new perspective on many old issues. Roughly, and setting aside certain crucial differences (which will be largely discussed), the defenders of QI believe that quantum mechanics displays a radical kind of indeterminacy, one that we cannot understand as linguistic or epistemic in character. By properly interpreting the theory, according to some scholars we have discovered that the quantum world sometimes lacks definiteness or determination. This kind of indeterminacy could intuitively remind of what philosophers call *ontic vagueness* or *metaphysical indeterminacy.*

The core idea of Metaphysical Indeterminacy (MI) is rather straightforward: at least in some cases, vague or indeterminate things (objects, properties, facts), are indeterminate not in virtue of the ways we refer to them, but because of the ways themselves are. Until the early 2000, few people took MI seriously, and many even thought that the very idea of the world lacking determination is, to use David Lewis' (1993, p. 27) and Michael Dummett's (1975, p. 413) word, « unintelligible ». A large part of the justification for such an attitude towards MI lay, on the one hand, on the argument by Gareth Evans (1978) against vague identity, and on the other in the success of the linguistic explanation of vague or indeterminate phenomena (see Fine 1978, *inter alia*). When we say that a cloud or a mountain lack determinate boundaries, we intuitively tend to blame the linguistic tools we use to refer to them. On this, as well as on many other respects, quantum mechanics seems to have radically changed the old point of view. In fact, if quantum mechanics really entails any indeterminacy (as defenders of QI maintain), then this would be rather hard to interpret as a mind-dependent feature of the world.[5]

In recent years the literature on QI has grown exponentially[6]. Different ways of accounting for the phenomena have been put forward, and despite the disagreement on many crucial issues—whether QI is fundamental or not, whether it is independent from the way we interpret the theory or instead affects only some interpretations, what specific feature of the theory generates it, and so on—a lot of scholars today take the possibility of QI very seriously. In this chapter I will touch upon the main topics discussed

[5] Though see Ch.3, section 3.7, for discussion of the competing view.

[6] A list of papers include: Lowe (1994, 1999, 2001), French & Krause (1995), Darby (2010, 2014), Skow (2010), Bokulich (2014), Wolff (2015), Lewis (2016), Torza (2018), Calosi & Wilson (2018, manuscript), Glick, (2018), Darby & Pickup (2019).

so far in the literature, and will thereby settle the discussion for some new topics which I will explore in what will follow.

Section (1.2) starts by analyzing what features of QM—independently from how we interpret the theory—might be the source of the indeterminacy. Section (1.3) builds upon the previous discussion, and adds considerations about the main interpretations of the theory, Bohmian, Everettian, and the Spontaneous Collapse Models.

1.2 The Sources of Quantum Indeterminacy

The idea that quantum mechanics displays a deeper sense of indeterminacy has been discussed in the literature since the very birth of the theory. Different authors have focused on various quantum phenomena, including *entanglement*[7], the *lack of space-time trajectories*[8], *superposition*[9], *incompatible observables*[10], and *quantum statistics*[11], arguing that they give an empirical ground for metaphysical indeterminacy. However, it is only in the past few years that a certain agreement on the meaning of such indeterminacy has been reached. Many authors (Darby 2014, Bokulich 2014, Wolff 2015, Calosi & Wilson 2018, *inter alia*; for criticisms, see Glick 2018) have reflected on the pervasiveness of a feature known as *lack of value*

[7] Lowe (1994), French & Krause (2003), Bokulich (2014), Wolff (2015), Calosi & Wilson (2018). See Darby (2014) for a discussion.

[8] French & Krause (2006), and Bokulich (2014) for a criticism.

[9] Bokulich (2014), Wolff (2015), Calosi & Wilson (forthcoming).

[10] Darby (2010), Skow (2010), Bokulich (2014), Wolff (2015), Calosi & Wilson (2018).

[11] French & Krause (2003, 2006). For criticism see Smith (2008) and Berto (2016). For discussions see Darby (2014).

definiteness (henceforth, LVD). We shall immediately notice that LVD is largely independent of how we interpret quantum mechanics. Of course, different interpretations will account for LVD differently. Nonetheless, since it depends on the very mathematical formalism we use to formulate any interpretation of quantum mechanics, namely the Hilbert space formalism, this feature is at the heart of the quantum mechanics.

The properties of a physical system are described in quantum mechanics by operators in the Hilbert space (a complex vector space). Since the beginning of the last century, many experimental results have suggested that we just happen not to be able to know, jointly and with absolute certainty, the values of certain microscopic properties (the ones so-called *incompatible*). The most common example is that of position and momentum; the more accurate the value assigned to the one, the less accurate that of the other. This is an experimental fact. And the reason why the Hilbert space is such a powerful tool for describing quantum systems is, at least in part, because it provides a well defined mathematical way of talking about the systems we experience at the microscopic level.

To understand what LVD consists in, let us consider a very simple example stemming from quantum mechanics. To make things easier, we will discuss a quantum property known as *spin*. Very roughly, a way to visualize this property is by thinking of the classical angular momentum. If microscopic particles were billiard balls, their spin would be the direction at which they rotate on a certain axis. To simplify, we can take microscopic particles like electrons to have spin in three orthogonal directions, x y and z (each corresponding to a well defined operator on the vector space). Furthermore, spin is a *quantized quantity*, meaning it is discrete. Thus, for each orthogonal direction, electrons (which are fermions, and so have spin-½) can only have two possible values, that we call *up* and *down*. It is again an empirical fact that different spin components are, like position and momentum, incompatible observables. That is, if we know, say, that the electron *e* has spin-down on the *x*-axis (suppose because we measured it), we thereby also know that its spin on the other two axis cannot in principle be assigned. Slightly more formally, we can use Dirac's *bra-ket* notation to write the state of the electron as follows:

$$e = |\downarrow x> \quad [1]$$

$$e = |\downarrow x> = 1/\sqrt{2}|\downarrow z> + 1/\sqrt{2}|\uparrow z> \quad [2]$$

$$e = |\downarrow x> = 1/\sqrt{2}|\downarrow y> + 1/\sqrt{2}|\uparrow y> \quad [3]$$

[1] express the fact that the electron is in an *eigenstate* of having the value 'down' for the observable *x*-spin. [2] and [3] express the fact that being in an *eigenstate* of the observable *x*-spin implies being in a superposed state of the observables that are incompatible with *x*-spin, namely *z*-spin and *y*-spin respectively. What is allegedly the main interpretative issue in quantum mechanics is related to what we should say about states of superposition like the above. If a system is in superposition of a certain observable, this seems to suggest that it does not posses a definite value (it is neither 'this' nor 'that', although 'being this' and 'being that' exhaust the possibilities). How are we supposed to make sense of this?

The first, quite natural reaction is to stress that everything we just said merely indicates our *epistemic* limitations—we just happen not to know which is the value possessed by the system, yet it does always posses one. Historically, this option has been proven not viable. Many foundational results in quantum theory, such as the Bell inequalities and the Kochen-Specker theorem, have shown that supplementing the theory with such *hidden variables* (which roughly are the values we do not know) does not help completely eliminate states of superposition[12], which need to be taken more seriously from an ontological perspective. As I shall show now, *indeterminacy* is one way of doing so.

Let us get back to LVD. Since to every eigenstate there are some corresponding superposition states, LVD in quantum mechanics is both pervasive and unavoidable. More generally, we can consider the standard way of reading physical properties off of the quantum formalism—that is a way to translate what the equations say about the world we live in. In QM, this role is played the so-called Eigenstate-Eigenvalue Link (EEL), which can be expressed as follows:

EEL — A quantum system *s* has a definite value *v* for the observable *O iff* it is in an eigenstate of *O* having eigenvalue *v*.

From EEL, we can straightforwardly derive that systems that *are not* in eigenstate of having value *v* for the corresponding properties, *do not possess* a definite value for those properties. And given that, as we saw earlier, to each eigenstates there always are certain corresponding superposition states, QM along with EEL describe a world in which properties are often instantiated indefinitely. This is, in a nutshell, LVD in quantum mechanics.

A quite natural question to ask now, is why suppose that EEL is such an undeniable principle. EEL is at the core of the standard, orthodox

[12] Slightly more technically, the results show that there is no way of assigning values to certain sets of incompatible observables that is consistent with the experimental results. Thus, either quantum mechanics is false, or some values simply cannot be assigned in principle. And given the astonishing amount of predictive power of the theory, nobody would seriously consider the first option.

interpretation of the theory[13]. Other interpretations though, either revise this principle or even drop it altogether[14]. In the discussion about metaphysical indeterminacy in QM, however, it is common to assume EEL as a good starting point, so as to give what David Wallace calls an « interpretation-neutral discussion of the ontology of QM » (2016). Everything else will then be build upon this by then taking into account the many relevant features that are not intepretation-neutral. By referring to EEL, we can give the following classification of the sources of QI[15]:

Incompatible observables — All the observables that obey to Heisenberg's uncertainty principle are called *incompatibles* (those that fail to commute). Examples include *position* and *momentum*, or distinct *spin* components in mutually orthogonal directions. If two observables *O*' and *O*" are incompatible, and a system *S* is in eigenstate for, say, *O*', it follows from EEL that *S* does not have a definite value for *O*".

[13] For an insightful philosophical discussion on EEL, and on how it relates to orthodox QM and to the other interpretations, see the recent paper by David Wallace (2016). Wallace argues extensively for the view that EEL is nothing but a 'philosophical tool', while the theory itself does not mention at all of it. Be that as it may, something like the EEL is needed whenever we start posing ontological questions to QM, which is clearly the business of those who reflects on quantum indeterminacy.

[14] See, e.g., Albert & Loewer (1996) for spontaneous collapse models, and Goldstein (2001) for hidden variables theories.

[15] I am following Calosi & Wilson (2018) in giving this three-fold classification. There is however no general consensus in the literature concerning which phenomena we should count as genuine source of MI, about what is the catalogue of them, and more importantly about what relation (of priority, for instance) there is between them. Just to make an example, Bokulich (2014), and Wolff (2015) present a classification slightly different from the one above, either by removing one of the sources (usually superposition and incompatible observables are considered two faces of the same coin), or by adding *quantum statistics* as a further source (for more on this, see French & Krause 2006).

Superposition — We can formulate the principle of superposition (PS) as follows:

PS: If *a* and *b* represent pure states of a system *S*, then their linear sum *c* is such that it also represents a possible state of a system.

Although it is not in an eigenstate, *c* does represent a possible state of the system *S*, and from EEL it follows that the system *S* lacks a definite value for the corresponding observable.

Entanglement — The component of the two entangled systems each lack determinate value for some observables *O*1 and *O*2, though the observable *O*12 corresponding to the sum of *O*1 and *O*2 is in an eigenstate. LVD is implied by the failure of the components of the entangled systems to be in eigenstates. Without being in an eigenstate, from EEL it follows they both lack definite values.

The three feature of quantum mechanics listed above are distinct from each other in at least three respects: (i) *mathematical*, (ii) *epistemological*, and (iii) *conceptual* or *metaphysical*.

As regards to (i), as argued by Calosi & Wilson (2018), the mathematical features underpinning each source of QI are distinct: the *non-commutativity* of the operators in the case of incompatible observables, the *linear sum* in the case of superposition, and the laws of the *tensor product* in the case of entanglement.

As regards to (ii), we shall notice from an epistemological perspective, that the *linearity* of the Schrödinger equation and the *non-commutativity of operators* cannot be derived from the mathematical facts underpinning the other quantum phenomena. Furthermore, without the non commutativity of the operators, a theory with the principle of superposition cannot be distinguished epistemically from a theory without it (see Hughes 1989).

As regards to (iii), the metaphysical differences between the three sources, we could notice that indeterminacy in the case of superposition affects a single system, while in the two other cases we need to refer to a plurality, either of at least two different systems (case of entanglement)[16] or of two different parts of the same system (incompatible observables). This means, I believe, that QI in the case of superposition is somehow metaphysically privileged, at least in that it gives us a clearer perspective on many issues.

The many differences between the three sources of QI invite a further consideration. Although we should expect, at least in principle, that a theory of QI would take care of each case, it is realistic to always remind us that this might not be the case in practice. In the next chapter, I will discuss different metaphysical approaches to QI, and show the difficulties for some of them to account for some features, and for some others to account for others. In the end, the intuition that QI is the *same* phenomenon in each case can surely be used as a guiding principle, or as a starting point, yet it is also one that we should be ready to revise at each step.

A concluding remark, connected to the previous one, concerns more generally whether there is any relation of dependence or priority between the sources of QI. Indeed, were there any such relation, one could argue that an account of QI should first focus on whatever turns out to be more fundamental. Perhaps, in order to find a promising strategy we should start looking at how quantum theories differ from classical mechanics. After all, classical mechanics makes no mention of indeterminacy in the same way as QM does, which also means that once we understand the main distinction

[16] See Hasegawa (2012) for a case of entanglement in a simpler system. I notice however, that in this case it would apply what I say about the plurality needed in the case of incompatible observables.

between classical and quantum mechanics, we also discover what makes the latter special with respect to indeterminacy. However, this is not as easy as it might seem. In the history of quantum mechanics, we find at least three views, and it would be hard to decide which is to be preferred. Paul Dirac (1930, 10-18) famously maintained that the biggest difference between classical and quantum mechanics is to be found in the way in which the principle of superposition puts constraints on the sets of possible states to be modeled in the Hilbert Space. While Dirac took the principle of superposition to be crucial for QM, others have focused on the uncertainty principle in instead. For instance, Hanson (1967, p. 45) reminds us that « John von Neumann generated all of quantum mechanics from an operationally suitable statement of the uncertainty relations alone ». Finally, we find a third view in those who believe that both principles, also taken separately, are enough to explain the main difference with classical mechanics. Here is, for instance, Hughes (1989):

> The principle of superposition tells us something about the set of admissible states, the uncertainty principle something about the set of observables encountered in the theory. *Any theory which includes either of these principles is, we may say, inherently probabilistic*; that is, each principle entails that there are pure states which assign to the outcomes of certain experiments probabilities other than one or zero. When the principle of superposition holds we can construct such states from any pair of states which assign a probability of one to different outcomes of a given experiment. (p. 83)

As I mentioned, it would be very hard to establish which view should be preferred. It is however interesting to point out that there is no overall consensus on this issue, and therefore no preferred route one could take while in the business of providing an account of QI.

1.3 QI Meets the Measurement Problem

So far the discussion about QI has been quite straightforward. I have started from how the theory would work as a description of the physical the world, and shown that given some of its features, such description entails a peculiar kind of indeterminacy. We all know, however, that this whole story cannot be that simple, since we are talking about a theory, QM, on which philosophers and physicists have been disputing over almost a century. And most importantly for us, it is not that simple since a large part of those disputes were mainly concerned with the status of the theory *qua* representation of the physical world—which is, once again, something that I was deliberately assuming to be true in the previous section. My aim in this section is to provide a more nuanced analysis of QI *vis à vis* the issue of how the theory can be taken as representing the physical world, that is how we interpret quantum mechanics. I will focus on the three main realist[17] interpretations, the *Everettian*, the *Bohmian*, and the one I am more interested in here, the *Spontaneous Collapse Models*.

Let us start by considering *Spontaneous Collapse* interpretations, the most famous of which is GRW (from Ghirardi-Rimini-Weber, 1986). In order to solve the measurement problem, GRW supplements the standard deterministic dynamics of the Schrödinger equation with a new equation. While the former equation, being linear, gives rise to superposition states, the latter is meant to break the linearity by introducing a fundamentally

[17] By 'realist' (see Miller 2014) I mean those views that take the quantum state as representing something objective in the world, rather than a representational device. I set aside the possibility that 'orthodox' QM could be interpreted realistically, and consider only those theory that are enough developed to make reasonable to discuss them without ambiguity. Although many participants to the debate on quantum indeterminacy have tried to argue that MI could be use to make sense of orthodox QM in a realist fashion (see Bokulich 2014), I do not think this is a promising way to go, for the measurement problem would still remain a big obstacle, untouched by any consideration about indeterminacy.

stochastic physical process, the *collapse* of the wavefunction. According to this new dynamics, every system has a certain probability per unit time of undergoing a *hit*, thereby collapsing into a more definite state. And that is the first reason why GRW is a fundamentally *indeterministic* theory. If we take two perfectly indistinguishable quantum systems, the new dynamics is such that even if the probability for both systems of undergoing a *hit* is the same, they might evolve differently. In other words, whether or not a collapse occurs, and where the collapse is centered, for a system *S* at a time *t* cannot be derived from the status of *S* at *t* plus the initial condition. However, GRW is fundamentally indeterministic also in a second, much more radical sense. When a collapse occurs, the wavefunction is multiplied by a narrow Gaussian function that has tails stretching to infinity in all directions in 3d space. This means that collapses in GRW are never to eigenstates, but only near ones. This is known in the literature as the *tails problem*, and it is certainly the main drawback of GRW. There are two types of solution to this problem, both with interesting consequences regarding quantum indeterminacy, so I will spend some time on them[18].

GRW Link approach — Substitutes the Eigenstate-Eigenvalue Link with a new principle that ascribe definite properties to quantum systems. Two main ways:

- GRW Fuzzy Link (Albert & Loewer 1992) — A quantum system has a definite value v for a particular observable *O iff* the square projection of its state into an eigenstate of *O* is greater than 1-*P*, for some (arbitrarily chosen) *P*.

[18] I should mention that Peter Lewis (2006) has argued that the differences between the approaches to GRW I mention is less substantial than what it first seems, and that eventually there only is one GRW. Though I believe his view deserves much attention, I will not discuss it in this occasion.

- GRW Vague Link (Lewis 2016) — A system has a determinate value for a given determinable *to the extent* that the square projection of its state onto an eigenstate of the corresponding operator is close to 1.

GRW Primitive Ontology approach — Provides an underlying ontology that is always determinate, and regards the residual indeterminacy as derivative. Two ways:
- GRWm —Mass Density (Ghirardi & al. 1995, Allori 2013);
- GRWf — Flash (Bell 1987, Tumulka 2006a).

The main different between the approaches above, lies in whether they allow for indeterminacy only at a derivative level or also at the fundamental one. I will discuss this issue further in chapter 3, but for now I shall notice that the different approaches to GRW provide many equally good examples of how quantum mechanics could imply some indeterminacy[19].

Let us now consider the Everettian, or many worlds interpretation of QM. This family of interpretations introduces the branching process which multiplies the worlds and eliminates macroscopic superposition states within a branch. The superposition *across* the branches is then interpreted as an emergent feature of reality, since the worlds themselves are « emergent entities » (see Wallace 2012). However, it is crucial to notice that, in Everettian interpretations, there still remains superposition *within* the ranches before the decoherence is imposed on the system. Furthermore, there also seems to be a further kind of indeterminacy that concerns the number of branches (which is generated by the relevant probabilities). Schematically, in Everett type of interpretation we find three kinds of indeterminacy.

[19] For criticism, see Glick (2018), and the discussion of his view in chapter 3.

1. Emergent indeterminacy *across* branches;
2. Epistemic indeterminacy in the *number* of branches (see Wilson, ms);
3. Metaphysical indeterminacy *within* branches before decoherence.

As for GRW, a final remark here should be made regarding whether the type of indeterminacy in (3)—which is the only one we are interested in here—is to be considered fundamental or not. Many view the Everettian interpretation as describing, at the fundamental level, only a multi dimensional wavefunction. Everything else, on such views, is thus considered derivative (see Ney Albert 2013). However, a main problem with interpretation is precisely how we are supposed to recover, from the wavefunction, the ordinary 3d space (see Lewis 2004, 2013). And no matter how we do this in detail, there will still remain the issue of how we interpret the indeterminacy within a branch of quantum systems before decoherence. Fundamental or derivative, there seem to be some MI (see Calosi & Wilson ms).

Let us finally consider Bohmian mechanics. In this view, the positions of the fundamental ontological items, say the particles, are always determinate (Bohm 1951). We do not know the precise values for all observables, but the fundamental ontology is made by particles that always possess determinate positions. Thus, it seems that indeterminacy in Bohmian mechanics is simply epistemic. What about those properties that, unlike position, are *not* fundamental? Asking this question, Peter Lewis (2016) invites us to consider *spin* as a good example, and observes that « although the Bohmian strategy arguably makes all the properties we directly observe determinate, it does not thereby make *all* properties determinate » (p. 101). This means, I believe, that we could map the possibilities into the following three views:

Eliminativist Bohmian View (Miller 2013, Esfeld 2014) — Positions are the only properties full stop: merely epistemic indeterminacy.

Hierarchical Bohmian View — Spin is a real physical property, but it is not a fundamental one: derivative metaphysical indeterminacy.

Egalitarian Bohmian View — Spin is as fundamental as position; fundamental metaphysical indeterminacy.

Although nowadays the first position is probably the most common, the other two can also be attractive for various reasons. The main one worth mentioning, is that eliminativism is generally considered to be problematic when it comes to recovering the 3dimensional macroscopic world, since it is populated by properties other than position. This means that although for the Bohmian type of interpretation indeterminacy can be seen as merely an epistemic phenomenon, there also are views in the vicinity on which QI is a worldly feature.

In conclusion, in this chapter I have shown that QI emerges from many features of quantum mechanics, and, in one way or the other, it affects each of the three main interpretations of the theory. In chapter 3 I will consider an objection, made by David Glick (2018), against part of my above conclusions. Before that, however, I will now turn to chapter 2 and to how metaphysics could help us getting a better understanding of QI.

CHAPTER 2

The Metaphysics of Quantum Indeterminacy

2.1 Metaphysical Indeterminacy

Our representations of the world, our thoughts and linguistic expressions can often be imprecise, or such that they partially lack determination. While this is uncontroversial, it is however controversial whether the world itself can lack determination, or in other words whether there can be worldly, metaphysical indeterminacy (MI for short). There is a growing literature in philosophy discussing whether and how we can make sense of MI or, as the received view has it in the spirit of David Lewis's claim that '[t]he only intelligible account of vagueness locates it in our thought and language » (1986, p. 212), all indeterminacy has a semantic source. As I mentioned in the first chapter, one question which has taken central stage in this discussion is whether certain interpretations of quantum mechanics entail the existence of MI. This question is particularly important, since if they do, then this appears to give us a good reason to abandon the received view that indeterminacy is a purely semantic phenomenon and to accept the existence of MI.

In the previous chapter I gave some initial support to the idea that quantum indeterminacy is a serious challenge to the received view on indeterminacy being never a metaphysical matter, and the next chapter 3 will further defend QI against some objections. Here I will momentarily assume that QI is a worldly issue, and one that needs be taken care by metaphysics. Therefore, I will be exploring what are the options we have on the table in order to give a systematic account of it. I will show in details what appears to be the two main contenders, the *object-level* approach (see Wilson 2013, Calosi & Wilson 2018), and the *meta-level* approach (see Barnes & Williams 2011) and shall argue that the former is to be preferred. In short, the *object-level* approach on MI maintains that, along with determinate state of affairs, the world is also constituted by indeterminate ones. The difference between the two families of state of affairs is to be found in how properties are instantiated. The *meta-level* approach on MI, instead, locates the indeterminacy at a higher level, in that while state of affairs are always determinate, they sometimes do not obtain determinately.

The most developed version of the *meta-level* approach is the theory developed by Elizabeth Barnes and Robert Williams in a number of papers (Barnes 2010, Barnes & Williams 2011, Barnes 2013, Williams 2008). Their account, also known as the *metaphysical supervaluationist* theory, has been criticized for being unable to account for QI (as already argued by Darby 2010, Skow 2010, Calosi & Wilson 2018). Although I agree with the spirit of this critique, in this chapter I will elaborate a new version of the *meta-level* approach, which I believe does better than the previous ones in accounting for QI. I will then conclude by explaining some potential drawbacks of the *object-level* view, and yet argue that it still is our best shot to account for QI.

2.2 Metaphysical Supervaluationism

The fundamental idea of supervaluationism (Barnes & Williams 2011) is that metaphysical indeterminacy can be modeled as indeterminacy in which of a range of admissible metaphysical precisifications of the actual world correctly represents it. In this section, I will first introduce Barnes & Williams' version of the theory and then give a rationale for why one should adopt a supervaluationist theory along these lines, instead of one of the rival theories of MI which have been proposed in the literature.

Barnes and Williams' theory is modeled on the supervaluationist theory of vagueness. Vagueness is a particular sort of indeterminacy which affects predicates and is closely linked to the sorites paradox[20]. According to, as one may call it, *semantic* supervaluationism, vagueness is unsettledness regarding which of a range of admissible precisifications of a particular language is correct, where a *precisification* is a complete valuation of this language. Here is how this account of vagueness roughly plays out.

A characteristic feature of vague predicates is that they do not allow us to draw a sharp line between objects to which they definitely apply and objects to which they definitely fail to apply. Take for example the predicate 'is bald'. There are some people to which the predicate definitely applies (think of a person with absolutely no hairs on their head) and others to which it definitely fails to apply (think of a person which a full head of hair). But then there are also some people who are neither definitely bald, nor definitely not bald (think of someone who is in the process of balding, but has a significant amount of hair, like probably myself in few years). This feature of vague predicates is highly problematic, since it renders them susceptible to the sorites paradox. This problem could easily be avoided by

[20] See Fine 1978, Keefe 2000, ch. 7 & 8, and Varzi 2007, for influential defenses of the theory.

introducing a sharp delineation between e.g. the bald and the non-bald by *fiat*. If we do this for each predicate of a language, the result will be what supervaluationists call a *semantic precisification* of the language, a complete classical evaluation of the language. Semantic supervaluationism is based on the idea that languages containing vague predicates always admit of a multitude of admissible semantic precisifications, semantic precisifications which are not *a priori* ruled out by established conventions governing the meaning of the expressions of the language. The core idea of supervaluationism is that vagueness can be understood as unsettledness regarding which of these admissible semantic precisifications gives us the correct interpretation of the language.

Metaphysical Supervaluationism (MS for short) adopts this idea and applies it to MI. The idea is that in cases of MI there are different admissible precisifications of the actual world, and it is unsettled which of them corresponds to the actual world. Let us unpack this claim. The three concepts which need explaining here are that of *unsettledness*, that of a *precisification of the actual world*, and that of *correspondence* between such a precisification and the actual world.

There are fundamentally two ways to understand 'unsettled'. First, one may take the concept expressed by this predicate to be distinct from that of indeterminacy. This reading turns the core idea of supervaluationism into a reductive claim. A semantic supervaluationist may for example treat unsettledness as a form of ambiguity, and then claim that vagueness is a special kind of ambiguity. In the metaphysical case, it is not quite clear whether this reading of the notion of unsettledness applies. At any rate, I will not further discuss this option for the simple reason that I will instead follow the second approach, also taken by Barnes & Williams (2011). According to them, the notion expressed by 'unsettled' in our schematic statement of the idea should be replaced by a pre-theoretical notion of indefiniteness, which is, crucially, not further analysable (see Barnes & Williams 2011, pp. 108ff.). This means that the account of MI on offer in Barnes and Williams's variant of MS is non-reductive in the sense that it relies on a primitive notion of indeterminacy, which cannot be explained in terms of other elements of fundamental ontology.

Barnes and Williams identify precisifications of the actual world with *ersatz* possible worlds which stand in a particular relation to it, i.e. to our reality (Barnes & Williams 2011). Let us first clarify what an ersatz world is. In contrast to the possible worlds posited by modal realism which are exactly the same sort of maximally connected (analogous) spatiotemporal wholes (cf. Lewis 1986), ersatz worlds are abstract entities which are posited to play certain theoretical roles in philosophical theories. For the purposes of illustration it might help to think of them as maximally consistent sets of propositions, but we can remain neutral regarding questions of what sort of entities ersatz worlds are, as long as they are fit to play these roles. As I have just made clear, Barnes and Williams take MI to be metaphysically primitive, which means that their reliance on ersatz worlds in their theory should not be confused with a commitment to a substantive claim about the nature of MI. Ersatz worlds are in no sense the building blocks of MI, they are just the elements of a machinery that helps us specifying the semantic behavior of the primitive notion.

Now we still have to say what it takes for an ersatz world to qualify as a precisification of the actual world. Barnes and Williams's idea is that the precisifications of the actual world are those ersatz worlds which do not determinately misrepresent reality, i.e. the actual world (see Barnes & Williams 2011, 115).

This leaves the third notion, that of correspondence between the actual world and an ersatz world. Since, unlike the actual world, ersatz worlds are abstract entities, correspondence here cannot simply mean identity. No ersatz worlds can be actual. However, leaving MI aside, one of the ersatz worlds can be *actualized*, which means that it is the one ersatz world which correctly represents all the actual truths, i.e. the truths which hold in the actual world.

Putting these notions back together, we get the following core claim of Barnes & Williams' version of metaphysical supervaluationism:

> **Core claim of MS** — It is metaphysically indeterminate whether A if, and only if, there is an ersatz world w which is a precisification of the actual world in which p and an ersatz world w' which is a precisification of the actual world in which ~p and it is indefinite which of w and w' is the actualized ersatz world.

As I will discuss later, Barnes and Williams 2011 somewhat modify this idea when spelling out the formal details of their theory, but these modifications are clearly meant to stay faithful to the core claim. MS is an elegant metaphysical theory, and does justice to the idea that metaphysical indeterminacy is an intelligible notion, contrary to what David Lewis thought. In this dissertation I will deliberately set aside some metaphysical objections one could move against MS (see e.g., Akiba 2015), and rather I will only focus on the objection according to which the model cannot account for quantum indeterminacy. The next section shows what it consists in.

2.3 The QM-based objection to MS

This section is dedicated to the arguments against Metaphysical Supervaluationism based on quantum mechanics. I start with some general remarks about the scope of the objections, with the aim of avoiding certain confusion that is often in the literature. In the second part I then focus on Skow (2010) and Darby (2010), who both independently argued against MS based on the Kochen-Specker no-go theorem of QM.

As I mentioned, the literature on MI in quantum mechanics has been growing exponentially in the past few years, and there still is large disagreement on many issues. Nonetheless, a seemingly widespread consensus is that MS is not able to capture what Skow (2010) calls *deep* metaphysical indeterminacy, that is indeterminacy in QM. Given that many (including Barnes & Williams (2011) themselves) seem to take QM to be the main motivation for pursuing a theory of Metaphysical Indeterminacy, if MS is unable to account for it then the account would lose a great deal of its appeal, and would thereby become less motivated. A plausible explanation for this is that the sort of MI arising from QM cannot easily be explained away as merely semantic, while it has been argued that some other purported examples of MI given in the literature can[21].

However, when asking whether MS is compatible with quantum indeterminacy, two issues need be addressed before even starting any further analysis. First, what feature of QM, if any, really forces us to accept metaphysical indeterminacy? Second, do all interpretations of the theory imply the same result? David Glick (2018), for instance, has recently argued that QM does not force us to accept MI, and even suggested (p.c.) that it is a main desideratum for any interpretation that it avoids metaphysical

[21] See in particular the discussions of a semantic supervaluationist (dis-)solution of the problem of the many in Eklund 2008, Lewis 1988, Lopez de Sa 2008, 2013.

indeterminacy (I will largely discuss this objection in chapter 3). Notice that if none of the main interpretations of QM involve MI, as Glick argues, then MS is still (although trivially) a valid option as an account of MI. However, it would also lose one of its motivations (perhaps the main one). A similar consideration would follow from taking quantum indeterminacy to be merely of an epistemic kind (as for the Bohmian interpretation of QM). No matter what the details about which interpretation is correct, or which interpretation is a viable option and which is not, MS would be more solidly motivated if at least some interpretations entail indeterminacy. And more generally, the more endemic quantum indeterminacy is, the more MS would be motivated. Therefore, MS (i) has to accept the existence of quantum (metaphysical) indeterminacy, and (ii) needs the resources to account for it. Darby (2010), Skow (2010), and Calosi & Wilson (2018) argues that MS does not have resources to account for quantum indeterminacy, thus violating (ii). As to the why (ii) is violated, as I mentioned Darby and Skow disagree with Calosi and Wilson, and so I will treat them separately. According to Darby and Skow, the reason why MS fails should be found in the Kochen-Specker no-go theorem (KS for short). Calosi and Wilson, instead, believe that there is no need to mention KS, because the structural dependencies between quantum observables is enough to show why MS cannot account for quantum indeterminacy. Although I agree with Calosi & Wilson, I will first spend some time on the KS based objection, given its importance in the literature on QI.

To show why, according to Skow and Darby, MS fails as an account of QI, let us first see how it *would* work. What I shall call the *naive implementation* of MS, in the quantum case, is to interpret each side of a superposition state as a precisificational possibility, and construe an example of MI thereby. However, this implementation fails because in some cases (namely, cases when some of subspaces corresponding to the properties are not independent) we *cannot* assign values to all properties of a quantum systems. Here I will first consider how the *naive implementation* might work —by considering a quantum case where it might seem to work—and then I will show why it does not work (using KS).

First, the *naive implementation* at work. Suppose we perform a spin measurement (with a Stern-Gerlach) on a quantum system *S* in the direction z, and find the system in eigenstate of spin-up:

S = |up *z*>

Different spin components in QM are incompatible observables, which means that *if* a system *S* is in eigenstate of a certain orthogonal direction (z, in our example), *then* the system *S* is in a superposition in both the other directions (in the example: x, y). Thus, we can write the state as follows:

S = |up *z* > = 1/r2 |down *x* > + 1/r2 |up *x* >
S = |up *z* > = 1/r2 |down *y* > + 1/r2 |up *y* >

In this case, according to MS, the following are true:

i. DET (*S* = up *z*)
ii. DET *not* (*S* = down *z*)
iii. IND (*S* = up *x*)
iv. IND (*S* = down *x*)
v. IND (*S* = up *y*)
vi. IND (*S* = down *y*)

(1)-(2) are true because according to every precisificational possibility—possibilities that do not determinately misrepresent the system—the system has spin-up in *z*. (3)-(6) are true because according to some precisificational possibilities the system is in a different state.

Second, why the *naive implementation* does not work. In the above simple example the set-up seemed to be such that the value assignments to properties of the system were independent one to another. This simply means that assigning up in one direction allows for either up or down in the other one. (Only 'in part' independent, however, because if we pick each precisificational possibility, either up or down for the *same* direction has to be the case). Thus, we would have 4 values assignments, each corresponding to a precisificational possibility (*pp*):

*pp*1: (S = up z) & (S = down y) & (S = down x)
*pp*2: (S = up z) & (S = up y) & (S = down x)
*pp*3: (S = up z) & (S = down y) & (S = up x)
*pp*4: (S = up z) & (S = up y) & (S = up x)

Recall that every *pp*, according to MS, has to be perfectly precise (*pp*s are maximally complete set of sentences). As correctly argued by both Darby and Skow, the assumption that each *pp* is precise (has values for all properties, as it is in (*pp1*)-(*pp*4)) is crucial for MS. Besides, as they both notice, assigning precise values to all properties mirrors the strategy that the *naive* 'hidden-variable' strategy tried to pursue. According to this strategy, all properties possess a definite value at all times, but we are ignorant about those values. The Kochen-Specker theorem goes against the naive hidden-variable theories, by showing that it is impossible to assign values to all the properties[22]. Hidden-variable theorists can pick up a preferred basis (usually position), and claim that only position assignments are always definite (even when we do not know which). This option, however, would undermine the motivations for MS, for it would give up quantum indeterminacy altogether (at least at the fundamental level).

The question before us is whether the states described in (*pp1*)-(*pp*4) are possible states in QM. The answer is *no*, as first was shown by Kochen and Specker (67)[23]. To write down the conjunction of the states in each of (*pp1*)-(*pp*4) would require to write a state like the following (let's take for instance (*pp*1)):

$$(S = \text{up } z) + (S = \text{down } y) + (S = \text{down } x)$$

This state however, makes no sense in QM, for it goes against the constraint that assignment of spin values to different spin components has to be equal to 2 (because $Sx + Sy + Sz = 2$). In other words, it must be the case that one of the three conjuncts is assigned value 0.

Observables (like spin) in QM are represented by operators in a multidimensional vector space. When an operator gets value 1 (the vector with which it is associated is an eigenvector), the subspace corresponding to it has value 1. However, different operators (corresponding to different properties, such as different spin components) may share the same subspace (and so the same value associated with it). KS type of theorems exploits this

[22] Certain interpretation of QM bites the bullet and reject non-contextuality. This means that assignment of values might depend on the experimental set-up, and thus the very same observable can be assigned 0 or 1 depending on the context. I will not explore this option further, because as I said I am explicitly interested in those interpretation of the theory that accept instead value indefiniteness.

[23] In fact, KS requires a much more complicated set of vectors to get to their result. I am here over simplifying this point to make it conceptually more salient.

feature of QM to generate examples in which sharing subspaces puts constraints on value assignments, so that: either we cannot always assign values, or we run into the contradiction of assigning to the same observables two incompatible values (0 or 1). To avoid contradictions, this naturally leads to value indefiniteness of QM.

Some concluding remarks are in order. MS requires the precisificational possibilities to be completely precise, but according to QM we cannot always assign precise values to quantum properties (value indefiniteness). Therefore, MS cannot account for quantum indeterminacy. In the next section (2.4) I will first consider some possible ways out for MS and show why they fail, before turning my attention, in section 2.5, to a version of MS which I believe effectively escapes the objection.

2.4 Ways out for MS

In this section I will briefly evaluate some possible ways of escaping the QM based objection to MS. Before getting into the details, let me first set aside a possible response that a defender of MS could give, which I believe does not deserve much scrutiny despite its initial appealing. The idea would be simply to reject quantum indeterminacy, for instance by appealing to the recent paper by David Glick (2018), who extensively argues for eliminativism about QI. In chapter 3 I will spend more time on Glick's view, but what is important here is to stress that proponents of MS should not take this route, because it would basically throw the baby out with the bathwater. After all, as I mentioned, MS needs QI in order to give more support to the view.

A much better line of defense for Barnes & Williams' MS could be to insist that their model is *ersatzist*. Thus, rather than concluding from KS that there is no actual world, since in MS all actual worlds are precise, we should say that no possible world can correctly represent reality (i.e., no world can be actualized). But this conclusion is what we should expect, given that possible worlds are representations that are precise in all respects, and reality is assumed to be indeterminate. In the MS account, however, it is unsettled which possible world is @, but each possible world is precise. So, given that KS rules out the possibility that a precise representation of the world is correct, it is *determinate* that no world represents correctly the actual world. And this is why MS account is incapable of capturing the indeterminacy at the heart of QM.

Another way to escape to the objection, would be for Barnes & Williams to simply take off orthodox QM from the set of the acceptable interpretation of quantum mechanics. Skow (2010) objects to this by noting that it does not matter whether or not orthodox QM is *the* correct interpretation:

> Let me emphasize that it is not part of my argument that the orthodox interpretation of quantum mechanics is, in fact, correct. There are many other interpretations of quantum mechanics (Bohmian mechanics, for example, and the many Everettian interpretations) that make no use of the notion of metaphysical indeterminacy. If we reject the orthodox interpretation and accept one of them instead then we will not have to say that there is *actually* any deep metaphysical indeterminacy. But it will still be true that the metaphysical indeterminacy in the orthodox interpretation of quantum mechanics is a *possible* kind of metaphysical indeterminacy. (2010, p. 8)

Recall that MS aims to capture *all* possible cases of MI. Orthodox QM might not be the correct interpretation, but it is still a possible case of MI. Therefore, MS fails at capturing all possible cases. First note that, in order for this argument to work, we need the further assumption that orthodox QM is metaphysically possible. However, a supporter of MS might simply deny this. Where Skow sees a Modus Ponens, Barnes & Williams might see a Modus Tollens.

There are however other good reasons for not rejecting orthodox QM. One reason is that, in the light of KS theorem and of Bell's inequality, it does seem a well-confirmed hypothesis. Moreover, anyone who shares a certain naturalistic attitude towards physics, would probably find the rejection of orthodox QM not well motivated.

Another response to the objection, recognized by Skow himself, is for MS to allow for *partial precisifications.* Skow is however skeptical about this for the following reason:

> For suppose we keep their framework but replace perfectly precise possible worlds with imprecise possible worlds (sets of sentences from a language that suffers from semantic indeterminacy). Even when there is no metaphysical indeterminacy we can expect it to happen that several imprecise possible worlds do not determinately fail to correctly represent reality. So using imprecise worlds would give us multiple actuality even when there is no metaphysical indeterminacy.

Skow's idea here, as I understand it, is that if we use use imprecise worlds as ersatz to represent quantum indeterminacy, we would also need to allow for imprecise world when representing standard case. After all, many imprecise representation will not determinately fail to correctly represent reality (that is the definition of the metaphysical precisifications) even when there is no indeterminacy. However, defenders of MS can simply bite the bullet here[24], or even find a way to distinguish two kinds of metaphysical precisifications.

[24] See for instance Torza (2017, 2018) and Darby & Pickup (2019) for developments of this view. I will not discuss these views further, since my main interest here is to provide a version of MS that stays faithful to the idea that the representations need be fully classical, rather than imprecise or incomplete.

So I do not think Skow's point actually goes through. There is, however, another worry with partial or imprecise precisification, that is that they imply a significant departure from the spirit of MS. In its spirit, MS aims at capturing indeterminacy as unsettledness between classical, complete options. If this main tenet of the theory is dropped, I feel that other accounts of MI would be preferable.

Finally, an option for MS would be to provide a more hybrid view and allow for some indeterminacy at the level of objects[25]. Recall that the main problem with MS is that in the model, even though it is indeterminate which possible world is the actualized world, each possible world is precise. Given KS, it follows that no possible world can be the actualized world. This is a collapse of the model, if we understand the indeterminacy at issue as unsettledness with respect to which precise representation is the correct one. In a sense, the problem with the model is that, given how it is constructed, it is impossible that any of the available representations be a correct representation of its *target*. In other words, there is something wrong with trying to capture the indeterminacy of QM as unsettledness concerning which precise representation of the world is correct, given that it is determinate that none is.

However, one could still coherently keep the machinery of *ersatz* worlds as precise representations if one does not locate the indeterminacy merely at the level of unsettledness concerning which representation is the correct one, but also more directly at the level of the target of the representation. The idea is then to understand the actual world @ as a *concrete entity* (or at least not as an inherently representational one) constituted by indeterminate state of affairs (along the lines of Jessica Wilson's (2013) view, for instance). Between the actual world @ and the ersatz possible worlds W = {w^1, w^2 … w^n}, we could introduce two kinds of representation relations.

[25] I thank Giuliano Torrengo for this idea.

'R', which express the idea of an entirely correct representation, and 'R^{QM}' which express the idea of a representation correct modulo claims concerning attribution of properties based on QM.

If we want to capture the indeterminacy of QM, we can then proceed as follows. It is quite trivial that, given KS, we cannot say much with 'R', basically just that it is not the case that there exists a possible world w in W such that Rw@. However, 'R^{QM}' is much more interesting, since it allow us to reconstruct truth conditions for claims about indeterminate matter in a way that is analogous to that of MS, without succumbing to KS-based objections. Rather than having a set of Multiple Actuality, here we define a set A of worlds that are representations of @ that are correct in so far as we bracket quantum mechanics. In other terms, in A goes any representation w such that R^{QM}w@. The idea is that each world in A represents a distribution of quantum mechanics relevant properties in a precise manner, together with all the macroscopical facts about @. No world in A represents the quantum mechanics relevant aspects of the world right—they cannot, given KS—but they all get the macroscopic ones right, as it were

It should be clear that if we make this move there is no risk of running into the complaint that in this model there is no actual world. It is true that none of the ersatz possible worlds represent correctly the actual world, and hence no possible world is *actualized*. But this is because of the deep difference between the actual world and the merely possible worlds in this model. While the merely possible worlds are representational entities whose target is the actual world, the actual world is a non-representational entity. Given that the representations in the model are all precise, while the actual world is indeterminate, the actual world cannot be represented correctly. However, given the possibility of exploiting (in the meta-language) representations relations which require only partial correctness, such as 'R^{QM}w@', we can model the behavior of claims about indeterminate matter by resorting to the truth value of such claims relative to the ersatz worlds in A.

I think there are three objections one could move against the above view, two of which are particularly worrying. First, it might seem *ad hoc* to simply make the theory blind to quantum indeterminacy, if after all we are in fact in the business of accounting for it. I do not think this is a big worry though, because a good response is to stress that the model provides precise conditions through which we distinguish cases of quantum indeterminacy from classical cases, and this is enough as a defense for MS. The second worry starts precisely when we ask what are the conditions for quantum indeterminacy, that is how we define the relation R^{QM}. We cannot simply say that everything that QM says is represented through R^{QM}, because we want to keep many statements from the theory, some of which are perfectly determinate in truth value. And neither can we put in R^{QM} only what counts as indeterminate. First, because it would be weird, to say the least, for a theory of MI to account for everything but indeterminacy. And second, because it is not clear whether we can actually do it. QM should be taken as a whole, and the parts of the theory which allow for determinate states are the very same that display indeterminacy in some other cases.

I have argued extensively that many of the responses MS could give to the objection based on QM are unsatisfactory for various reasons. I now move to what I believe is the best version of MS, what I call Plural Metaphysical Supervaluationism, which I think is the only one that effectively responds to the objection moved by Skow (2010) and Darby (2010).

2.5 Plural Metaphysical Supervaluationism[26]

The main point of this section is to introduce a version of MS which avoids an important objection to Barnes and Williams's version of the theory, namely the objection that it cannot account for a particular sort of MI to which certain interpretations of quantum mechanics give rise. My version shares the core claim of their theory: MI can be modeled as indeterminacy regarding which of a range of precisification is actualized. However, it also departs from it in an important way. This departure, which I will discuss in detail later on, is not only motivated by the quantum mechanics based objection to their theory, but is also independently motivated by an internal tension in it. To bring out the tension, we need to say a bit more about the formal part of their theory.

Barnes and Williams's theory consists of two components. First, a primitivist account of the nature of MI and an account of how to reason about MI which consists of a logic characterized by a model theoretic semantics plus a definition of logical consequence. The second component is based on the core idea of their theory, the idea that MI can be characterized as indeterminacy regarding which precisification of reality is correct. In Barnes & Williams 2011, three different semantics for three different object languages are developed. The first for the language of first-order quantified modal logic, a language which contains a propositional modal operator which can be used to express modal claims like "it is necessary that *p*", the second for a language which in addition contains a definitely-operator which allows one to formulate claims about MI in the object language. This operator is however only allowed to take widest scope

[26] The option I explore in this section has been developed in a joint work with Robert Michels and Giuliano Torrengo.

over sentences not containing the operator[27]. The third semantics is a semantics for a language in which this syntactic restriction on the sentences containing the D-operators is lifted and in which the operator is allows to freely combine with itself, the logical constants, and the modal operators. These three semantics are accompanied by definitions of logical consequence which are designed to be classical in the sense that the resulting logic contains all theorems of classical first-order logic and that these notions of consequence obeys the same rules of inference as the standard notion of consequence of that logic.

The tension concerns a particular feature of the models used to interpret the three different languages. These models contain a set of ersatz worlds and in addition single out one of these worlds as the actualized world of the model. This designated world of the model plays a crucial role in their logic, as they define logical consequence in terms of truth in all models, where the notion of truth in a model is in turn defined as truth at the actual world of the model (see Barnes & Williams 2011, 124, 129, 133).

The crucial point is that each model of Barnes and Williams's model theory contains one world which is singled out as the actualized world of the model. But if each model explicitly specifies the actualized world, how can such a model reflect the core idea of the theory, the idea that MI is indeterminacy regarding which precisificationally possible ersatz world is actualized? There is no such indeterminacy to be found in their models.

To be fair, Barnes and Williams adopt a modification of their core idea when moving from the semantics for the first of their three languages to that for the second and third. Leaving the technical details of the changes in their modal theory aside, Barnes and Williams define a notion of truth

[27] So if A is an arbitrarily complex closed or open sentence of the language of first-order quantified modal logic, the second object language will contain the sentence DA. Note that it will not contain any logically complex sentences involving sentences of this form as sub-formulas.

simpliciter as truth in the *intended* model, where this intended model as before singles out one world as the designated actualized precisificationally possible world of the model[28]. They write: « Our guiding conception of metaphysical indeterminacy has it that one among the possible worlds 'gets matters right », but it was indeterminate which world that was. To this point I have specified three of the four elements of the intended model: the space of worlds, domain, and interpretation. So the candidates to be the intended model are the various models containing those three elements together with some specified ersatz world. Some of these models will designate as actual worlds that are determinately non-actual. These models will be determinately unintended. Some models will designate worlds that are neither determinately actual nor determinately non-actual (what we called the ontic precisifications). These models will be neither determinately intended nor determinately unintended. (Barnes & Williams 2011, 125-6.) In this new setting, MI is in fact no longer directly modeled as indeterminacy regarding which precisifically possible world is actualized in a model, but rather as indeterminacy regarding which *model specifying an actualized world* does this correctly.

The result of this change is a shift from a theory which models MI as a particular modality to a theory which models it as what one might call a meta-modality. Conceived of as a modal notion as in the initial core idea of SM, that it is metaphysically indeterminate whether p is reflected by there being, in the intended model, two precisificationally possible worlds, one in which p is true, another in which ~p is true. In this case, the definiteness-

[28] With respect to the third language, things are a bit more complicated. In the corresponding model theory, the intended model contains the designated selection function which precisifies all halos of indeterminacy (sets of worlds) including in particular the one designated halo of indeterminacy in the correct way, i.e. picks out one world among the worlds which are actually precisificationally possible as the one world which is actualized. See Barnes & Williams 2011, section 6.1 for a full explanation of the model theory and the definition.

operator of the second and third language is interpreted via quantification over precisifications and it allows us to directly expresses claims about MI. According to Barnes and Williams's modified core idea, that the world is metaphysically indeterminate regarding whether p is reflected by there being two models which both correctly represent reality, save for that one contains an actualized world in which p, the other one in which ~p is true. Claims about MI are understood in terms of quantification over models. As a consequence, they are not longer directly expressible in the object language using the Definiteness-operator, since that operator has its interpretation fixed with respect to a single model.

There are several problems with this new version of the core idea. Briefly: first, the object-language D-operator can no longer be said to directly express claims of MI, for the reason just given. This means that the theory de facto introduces a second, distinct kind of MI, even though it is supposed to model a single phenomenon.

Second, earlier in their paper, Barnes and Williams insist that « our 'precisifications' will be worlds rather than interpretations of a language » (Barnes & Williams 2011, 115). While this claim of course still holds for MI as expressed by claims involving the D-Operator, the same cannot be said about MI as reflected by indeterminacy in which model is intended. Models are mathematical structures which are used to interpret formal languages, so it seems that Barnes and Williams claim no longer holds if the new version of the core idea is adopted. The theory has earlier been criticised by failing to model a distinctively metaphysical, rather than a semantic kind of indeterminacy in Akiba 2015 and this point appears to corroborate this criticism.

Third, another criticism due to Wilson is that Barnes and Williams's theory of MI is a meta-level account and as such fails to genuinely locate MI in the world. The new core idea can be seen to support this criticism, as it delegates MI to an even higher level of abstraction in the model theory.

I will not discuss these problems in more detail here, since my aim is not to argue that Barnes and Williams' modification of MS' core idea is inadmissible. The relevant point for me is just there is a tension between the original core idea, and the fact that Barnes and Williams's model contains designated actual worlds. Their modification to the core idea deviates significantly from the spirit of the original core idea in ways which may be contested.

The core difference between my version of MS and that of Barnes and Williams is that it takes the relation which holds between reality (or the actual world, our universe, as opposed to the abstract ersatz worlds) and the ersatz worlds in a model which qualify as precisifications of reality to be a plural relation. Barnes and Williams take this relation, call it R_p, to be a singular relation which holds between reality and one ersatz world.

My proposal is to replace this singular relation by a relation which relates reality to a plurality of worlds. Importantly, I claim that the plurality of these worlds is irreducibly plural in cases of MI. Irreducibly plural instantiations of properties are properties which are instantiated by a plurality of objects *oo*, but neither by any of the single objects among the *oo*, nor by any sub-plurality, i.e. any plurality consisting of some objects among the *oo*, but not all of them. There is nothing unusual about such cases. Think for example of the relational property of joining hands together to form a line around the base of the Empire State Building. We can imagine a group of protesters which forms a very tight line around the building, so that none of them could leave without breaking the line[29]. I claim that the

[29] Contrast this to a cases in which the protesters form a less tight line, so that one or two of them could leave without breaking the line, or think of a rather fantastic scenario in which someone has such enormously long arms that they alone can form a tight line around the building by joining their two hands.

relation of being a precisification of reality, it R_{pp}, as applied to a plurality of ersatz worlds in cases of MI is of this kind: each of the worlds contributes to precisifying reality in such a case, but none of the worlds alone counts as a precisification of reality on its own and neither do just some, but not all of the relevant worlds.

So consider a case in which reality is metaphysically indeterminate regarding whether *p* is the case. In this case, there are two ersatz worlds which correctly represent all the actual facts if we disregard *p*. One of the two will represent reality as being such that *p*, the other as being such that *~p*. But, metaphorically speaking, both of them have to work together to count as precisifications of reality, so neither of the two alone does. This is the basic idea of Plural MS.

As a contrast case, consider the case that there is no MI in reality. Just like Barnes and Williams's version of MS for the language involving no Definiteness-operator, Plural MS will model these cases as such that there is a single ersatz world which is actualized. Note that this is perfectly compatible with the move to R_{pp}. R_{pp} accepts single ersatz worlds as limiting cases of pluralities of ersatz worlds and in such cases, the requirement that the relevant worlds together and only together as a whole precisify worlds is trivially met.

The move to R_{pp} has several consequences for the overall theory. One consequence concerns the intuitive interpretation of what it means to count as a precisification. Barnes and Williams characterize their relation R_p indirectly as holding between reality (or, since they generalize their theory to possible worlds, a possible world) and an ersatz world which « does not determinately misrepresent reality » (Barnes & Williams 2011, 115). One might straightforwardly adapt this characterization to the plural relation R_{pp} by simply describing it as the relation which holds between reality and a plurality of ersatz worlds which only taken together as a complete plurality are such that they do not determinately misrepresent reality.

This is one option, however there is another: we might instead characterize the relation as that of holding between reality and a plurality of ersatz worlds which only taken together as a complete plurality are such that they do give us a precise representation of reality. The second characterization gives us, to use a term introduced by Calosi & Wilson (2018), a glutty view of MI: only if we take all the single worlds which together qualify as being precisificationally possible into account do we get the full picture of what MI is like (according to MS).

The second characterization of R_{pp} also suggests an answer to an open question about MI, namely whether there is higher order MI. This question is inherited from the discussion about vagueness, i.e. about a particular kind of indeterminacy which affects predicates and is closely tied to the sorites paradox. Most contributors to this discussion take it at as a datum about vague predicates that they are not only vague, but also higher order vague. A vague predicate like "is bald" not only fails to draw an exact boundary between the bald and the non-bald, but also fails to draw an exact boundaries between the definitely bald and the not-definitely bald, and the definitely definitely bald and the not definitely definitely bald, and so on[30]. However, MI should clearly be distinguished from metaphysical vagueness (see e.g., Eklund 2008), so the near consensus about the existence of higher-order vagueness should not compel us to to accept the existence of higher-order MI. The second characterization in fact can be taken to suggest that there is no higher-order MI: If it is metaphysically indeterminate whether reality is such that p, Plural MS models this as there being two ersatz worlds which together count as precisifying reality. The resulting picture is perfectly precise. One of the worlds is such that p, the other such that ~p

[30] See e.g. Sainsbury (1996), and Wright (1992, 2009) for a dissenting voice.

and these two worlds together both localize the MI and furthermore indicate all the ways the world could be, were it precise with respect to *p*. Likewise for more complex cases in which the determinacy involves a range of mutually exclusive alternative states of aspects of reality.

The discussion about MI contains significantly different purported examples of MI. Some for example claim that the openness of the future should be understood in terms of MI, others link MI to material objects with fuzzy boundaries, and there are of course those who argue that quantum mechanics give rise to MI. It is not clear whether these examples all involve the same sort of MI. We believe that Plural MS is flexible enough to accommodate these different examples of MI, but there might be differences between them regarding whether they involve not only first-order, but also higher-order MI. Accordingly, the reading of the second characterization of R_{pp} which rules out higher-order MI might suit some types of examples better than others. Importantly, it seems that it suits the particular sort of MI which can be argued to arise from quantum mechanics which will take center stage in the following section.

A core question about any version of supervaluationism is how it handles the notion of truth. The standard approach in semantic supervaluationism is to identify truth with super-truth, i.e. truth in all admissible precisifications, but the framework offers the resources to define different notions of truth. In the application of the theory to quantum indeterminacy, I will rely on the notion of super-truth to give us a notion which tracks what the quantum state tells us about the states of the world. In cases of quantum MI, there will as a consequence be propositions which fail to express super-truths. Accordingly, I will, in one sense, not have a classical, bivalent object language. I will however make use of the flexibility of the framework to define classical notions of logical truth and consequence. This bi-furcated approach to truth will be introduced and motivated in the following subsections.

Let us return to the tension in Barnes and Williams' theory. If we go plural and if we want to model MI using a model theoretic semantics at the model-level, rather than at a meta-model level, it no longer makes sense to include a designated actualized world in the models. No ersatz world alone qualifies as a precisification of reality, so no ersatz world can be singled out as being the actualized world of the model, the one world which correctly represents reality.

These are the core ideas of Plural MS. Since this version of MS was introduced, at least in part, in order to overcome the objections based on QM, I shall now analyse how Plural MS does indeed the job.

2.6 Plural MS and the QM objection

To see how PMS accounts for quantum indeterminacy, let us consider again the example from section 2.4, where we had *S* = |up *z*>, from which we also known that it is indeterminate whether S = |up x> or S = |down x> and likewise that it is indeterminate whether S =|up y> or S = |down y>. According to PMS, there are four worlds which are *together and only together as a whole* precisificationally possible, and which represent the following states as obtaining:

*pp*1: (S = up z) & (S = down y) & (S = down x)
*pp*2: (S = up z) & (S = up y) & (S = down x)
*pp*3: (S = up z) & (S = down y) & (S = up x)
*pp*4: (S = up z) & (S = up y) & (S = up x)

This gives us a model in which S = |up z> is supertrue, but neither of the attributions of up or down spin to *x* or *y* have this status. Supertruth is here the relevant notion of truth concerning what goes on in the quantum system.

A crucial feature of the model is that it considers the *pp*s to be *classical*, in the sense that quantum laws do not hold in them. Calosi & Wilson (2018) object against a move close to my own:

> Taking precisifications to be ones in which classical laws are operative violates supervaluationist constraints on admissible precisifications—namely, that precisifications cannot be determinately incompatible with (cannot determinately misrepresent) the actual world. In particular, the true claim that 'the position and momentum of a system cannot be jointly fully precise' is determinately true if the actual world is, as we are assuming, a quantum world; but classical worlds in which every system has determinate position and momentum will be worlds in which this claim is false, not true; hence any such world would fail to be an admissible precisification. (p. 18)

In response, we shall notice that, contrary to standard MS, here admissibility should not be taken as applying to single precisificational possibilities. Admissibility is rather a notion that in our model makes sense for a plurality. Quantum laws are rather to be taken as meta-laws, laws that provides constraints on possible precisifications. Although this might seem *prima facie* unusual, I believe there are at least two reasons, one from physics and from metaphysics, why it is only superficially so. First, recent works on quantum theory aims precisely at defending the view that quantum theory is to a great extent *a priori* (D'Ariano et al. 2017). Second, we could appeal to the distinction, to be found in Maudlin (2007) between dynamical laws—FLOTEs, Fundamental Laws of Temporal Evolution—and adjunct principles, and maintain that the latter only are those that provide meta-level constraints on possible precisifications.

Although there is still a lot of work to be done for PMS to account for quantum indeterminacy, I believe the view is on the right track to respond to Darby and Skow's kind of objection. Furthermore, allow for classical (non quantum) representations seems to be something we should expect if we want our theory of MI to respect the main tenet of the *meta-level* accounts. The intuition here is that QI is a non classical phenomenon, and yet the only thing we have to understand it are our representations, which are after all classical.

Nonetheless, in the next section of this chapter I will show that the situation for *meta-level* approaches gets even worst than what Skow and Darby imagined. I will therefore considered, in the final section, the *object-level* as the main alternative, and show why I take it as our best shot as an account of QI.

2.7 Calosi & Wilson's objection to MS

I now consider another, even stronger objection to MS, from Calosi & Wilson (2018). The main difference with the objection based on the Kochen-Specker is that Calosi and Wilson consider all the main interpretations of QM, while Skow and Darby seem to rely on possibility of the orthodox interpretation of QM being a genuine one. This is a crucial issue here, so let us what they mean. First, let us recall that Skow (2010) believes the objection only needs that orthodox QM is at least a genuine possibility. On a similar vein, in a footnote Alisa Bokulich (2014) writes, even more explicitly:

> One might object that the standard interpretation of quantum mechanics is only an instrumentalist theory, and that it only makes sense to inquire into the metaphysical implications of a "realist" theory of quantum mechanics, such as Bohm's hidden-variable interpretation. I think this objection is a mistake: For any theory one can take either a realist or instrumentalist attitude towards it […] In this paper I am taking a realist attitude towards the standard interpretation, and asking what the world would be like if this interpretation were true. Those who think that there can only be realist interpretations of theories such as Bohm's, conflate "realist" with "resembling classical mechanics". (fn 460)

I agree here with Calosi & Wilson (2018), however, in noticing that this kind of considerations about the orthodox interpretation can be easily overcome. Many have indeed claimed that the notion of *measurement* in the standard, orthodox view of QM is so ill-defined to make the question of whether or not this view is consistent hard to even pose correctly (just to mention one, see Bell 1987). Therefore, it would be quite a small problem for MS if the model would not be able to account for this possibility.

For these reasons, Calosi & Wilson claim to be able to provide a much stronger line of reasoning concerning why quantum indeterminacy should lead to the rejection of MS. Instead of focusing on the Kochen-Specker theorem, and on the Orthodox interpretation only, they consider a number of crucial features of QM—namely, the ones we met earlier in chapter 1, *superposition*, *incompatible observables*, and *entanglement*—and argue that they are, in one way or the other, present in each of the main live interpretations of QM. The main reason for this is, more generally than the KS, the structure of dependencies between properties (observables) in the formalism(s) of quantum mechanics. Thus, their main claim is that, for each interpretation, either there is no metaphysical indeterminacy, or if there is, MS cannot account for it.

Let us start considering GRW type of interpretation. Having in mind what has been said in chapter 1, section 1.3, the conclusion we should draw is two-fold. First, on the primitive ontology approaches, although there is some residual indeterminacy, it only concerns the derivative level of reality. At the fundamental level, both the flashes and the mass distribution are entirely determinate (see also, Glick 2018). As per the link approaches, instead, the indeterminacy can be seen as fundamental, as also Lewis (2016) notices. Calosi & Wilson (2018) consider both strategies, and concludes that the possibility of residual indeterminacy in the primitive ontology approach « at least renders unclear the compatibility of this interpretation with a supervaluationist approach » (p. 14). The reason is again based—and, to be clear, independently from results like the Kochen-Specker theorem—on the dependencies between property ascriptions to quantum systems. In the case of the primitive ontology approach to GRW, this is true *at least* for certain complex, macroscopical systems, and even granting that the fundamental ontology is free from any indeterminacy. As for the linking principles approaches on GRW—although Calosi & Wilson are not explicit on this—the rejection of MS is also based on dependencies between quantum properties, but here it would be even more problematic given that it would be at the fundamental level.

Turning to Everettian QM, the situation might seem more promising at first, for one could think that each branch represents a metaphysical precisification. However, recall from chapter 1 that the *residual* metaphysical indeterminacy in Everettian interpretation is the one *within* each world before decoherence, and not the one *across* the different branches. And although one could argue that MS is able to account for the emergent indeterminacy *across* the branches, this effort would be rendered useless since there would still remain the other type of indeterminacy left unexplained.

Finally, similar considerations apply to Bohmian mechanics, where, as we saw in the first chapter, either the indeterminacy is merely epistemic—as for eliminitivist bohmians—or it is such that MS cannot account for it. Indeed, if it is not only epistemic, then properties like *spin* are considered to be real, even if only derivative, so that arguments like the ones above would work.

A general remark is here in order. As we saw, Calosi & Wilson do not appeal to the Kochen-Specker no-go theorem in their objections, mainly because they maintain, and rightly so, that the structure of dependencies between quantum properties (in many live interpretation of the theory) would be enough to rule out MS. This is certainly true, unless the rejection of the Kochen-Specker type of objection is motivated on a more general rejection of MI altogether, which again should not be case for defenders of MS. In this sense, we shall conclude that appealing to the Kochen-Specker theorem is misleading, unless motivated by a rejection of MI - which, we notice, is not the case in both Darby (2010) and Skow (2010).

In conclusion, Calosi & Wilson (2018) argue that on many interpretations of QM, the structure of properties dependencies is such to rule out the core assumption of MS according to which single precisificational possibilities are always precise. Therefore, they suggest that we reject MS in favor of a different account of MI, the *determinable-based* approach.

2.8 Object-Level Indeterminacy

In this last section my focus will be on the other approach on MI, on how it relates to quantum indeterminacy, and finally on some of its potential flaws. Alisa Bokulich (2014) has been the first to connect explicitly quantum indeterminacy with this other approach to metaphysical indeterminacy, namely Jessica Wilson's (2013) *object-level* approach. In Wilson's view, indeterminacy is treated in terms of the distinction between *determinables* properties and *determinates* properties, and MI obtains when there is an object having a determinable property without a determinate value for that property:

> What it is for an SOA [state of affairs] to be MI [metaphysically indeterminate] in a given respect *R* at time *t* is for the SOA to constitutively involve an object (more generally, entity) *O* such that (i) *O* has a determinable property *P* at *t*, and (ii) for some level *L* of determination of *P*, *O* does not have a unique level-*L* determinate of *P* at *t*. (Wilson 2013, 366)

So, for instance, an object will always have a determinable property such as *being colored*, whilst it could lack a determinate property like *being red* or *being green*. Bokulich (2014), Wolff (2015), and, more systematically, Calosi and Wilson (2018), use this machinery in the case of QM. The idea is that quantum particles are not vague objects but rather that they have vague properties. The spin is again a good example. An electron will always have the determinable property of *having x-spin* while lacking, in specific situation, the determinate property of *having x-spin-up* or *having x-spin-down*.

Wilson's account, unlike MS, locates indeterminacy at the level of objects and properties. Indeed, Wilson's view is different to this respect from almost every other approach to MI. The only account that shares some similarities with Wilson's is the one developed by Nick Smith and Gideon Rosen (2004): «Our paradigm for the indeterminate object is an object that possesses length, but no determinate length, or color, but no determinate color » (p. 198). In Wilson's view there are indeterminate state of affairs (SOAs), and which of them obtains is always a *determinate* matter. The indeterminate SOAs are those in which a determinable is instantiated, even though either no unique determinate (for some level of its determination) is instantiated ('glutty' MI), or any determinate is instantiated ('gappy' MI). In contrast, on MS, as we have seen, it is unsettled which among many *precise* SOA obtains, since it is unsettled which possible world is actualized. Furthermore, object-level accounts of metaphysical indeterminacy do not run the risk of attributing properties in a way that violates the constraints of KS theorem, since they explicitly allow for indeterminate state of affairs.

Bokulich (2014), Wolff (2015), and more extensively Calosi & Wilson (2018) all argue that Wilson's *determinable based approach* can be used to understand quantum indeterminacy. A major issue is what specific implementation of the account is to be preferred in the quantum case, whether the *gappy* or the *glutty* one. Although Bokulich (2014) and Wolff (2015) suggest that a *gappy* implementation would be preferable, Calosi & Wilson (2018) show a potential problem with this approach, thereby arguing for the *glutty* implementation instead. The first major problem is that the lack of determinates would make it impossible to distinguish different probability weights in the theory. Take a case in which a system *S*' is in a superposition in the *x*-spin of 70% up and 30% down, and another system *S*" which is also in superposition in the *x*-spin, but instead it is 71% up and 29% down. From a metaphysical perspective, the *gappy* approach does not possess the resources to distinguish the two cases, since in both of them the situation is exactly the same: both *S*' and *S*" possess the determinable *x*-spin, and both lack the corresponding determinates. A *glutty* implementation seems to be in better position here, because the different statistical weight can be accounted for by the existence of *both* determinates. Of course, here as well, we need to say more about why the existence of both determinates will eventually result in a different statistical weight. Calosi & Wilson suggest in a footnote that maybe the only way to go here is to allow for degree-theoretic instantiation of properties. Roughly the idea would be that, to take the example above, the difference between *S*' and *S*" lies in the difference between the degree in which the determinate *x*-spin up and *x*-spin down are instantiated. Even then though, there would still is the issue of why degrees of instantiation are so related to measurement results[31]. By itself, to say that a certain physical system instantiates properties in different degrees, does not say much about why, when performing measurement, the system picks up one property over the other. I take this to be the major challenge for the object-level *glutty* approach, and I believe that future developments of the theory will need to account for it.

Another problem with the *gappy* implementation, which is also a problem for virtually any account of the *meta-level* type, is how to explain the well-known phenomenon of quantum interference. In the standard experimental set-up, quantum particles produce an interference pattern when fired through a double slit. If one of the main *desiderata* for a theory of quantum indeterminacy is to understand, from an ontological perspective, what a superposition state consists in, then clearly the double-slit experiment is a crucial test. Suppose one says that (before decoherence, objective collapse, the experimenter influencing the apparatus, of what you have) the particle is in superposition of 'passing through the upper slit' and 'passing through the lower slit'. If we want to explain how system shows an interference pattern, we clearly cannot say that the particle did not pass

[31] A problem recognized by Claudio Calosi himself (p.c.).

either of the slits (the system lacks both determinates corresponding to the determinable position). If it lacks both determinates, the interference would be left unexplained. And as I was saying, this is a problem for the *meta-level* approach too. In this case, it is not even clear how worldly unsettledness between two options (the electron passing through the upper and passing through the lower slit) can interfere. Once again, the *glutty* implementation can solve this problem by claiming that the interference is produced by the interaction between the determinates properties.

The object-level approach, and in particular the *glutty* implementation of it, is preferable over MS to account for quantum indeterminacy. However, this by itself does not mean that the account has no problems. For instance, as Wilson herself points out, her view is incompatible with an assumption concerning the determinable/determinate distinction that is often regarded as central and non-negotiable. That is, the assumption that every time an entity instantiates a determinable properties, there is exactly one determinate (for each level of determination) that the entity instantiates. As Funkhauser (2006) has it:

> An object instantiating a determinable must also instantiate some determinate under that determinable. Colored objects must be red or yellow or blue, etc. No object is merely colored simpliciter. (Funkhauser 2006, p. 549)

In Wilson's view, determinable properties are *non-reducible* (2013, p. 382). The distinctive link between a determinable and a determinate is that instantiating a determinate is tantamount to an increase of determination in the SOA. But an entity O does not possess a determinable *in virtue of* instantiating exactly one determinate, and indeed if O is in a indeterminate SOA, then it may fail to instantiate *any* determinate while instantiating the relative determinable[32]. To stick with the previous example, an electron may instantiate the determinable property of *having spin direction* while lacking, in specific situation, the determinate property of *having spin-up* and *having spin-down* in that direction.

The idea that instantiating a determinable requires instantiating exactly one determinate (for each level of determination) is indeed very natural and Wilson argues at length to defend her somewhat unorthodox alternative. One of the strongest argument against Wilson's view that I can think of, is that in order to distinguish the determinable-determinate distinction from the genus-species distinction, we need to appeal to the fact that the instantiation of the determinable is *metaphysically explained* by the instantiation of the determinate. This is different to the genus-species case, in which the instantiation of the species-related properties are at least partially explained by the instantiation of the genus-related properties. For instance, we can explain why a certain apple is colored by appealing to a certain determinate shade of red it exemplifies. While we (at least partially) explain properties that are possessed by my cat by appealing to the fact that she is a feline. If so, it is in the very nature of the determinable-determinate distinction to respect the constraint that Wilson's account jettisons.

However, perhaps this criticism is too quick. After all, Wilson may agree that the determinate has usually a certain primacy of explanatory role, but the story need not to be so simple in every case. Even when no specific determinate is instantiated, the determinable does not 'float free' with respect to the determinates that the entity *could* instantiate (and maybe does instantiate in different circumstances – for instance, after the measurement). Be that as it may, there is an alternative view to Wilson's,

[32] Wilson considers also another situation in which an object could enter an indeterminate SOA, when it instantiates a determinable, but instantiates more than one determinate, since it instantiates determinates only in a relive manner. I will not dwell upon this alternative here, since it is less relevant for QM (as also Bokulich 2014 notices).

which seems to catch just as much as her view, without having to abandon the assumption that instantiating a determinable requires the instantiation of a determinate. I briefly considering this alternative and present it is as a sort of a challenge to Wilson's view.

If one accepts Wilson's idea that it is determined which SOAs obtain, and the indeterminacy is rather in the SOAs themselves, one may think that indeterminate SOAs are those whose determinables are possessed in virtue of instantiating the determinate *being indeterminate with respect to a given property* (to stick again to the previous example, the determinate *being indeterminate with respect to the property of having spin-up direction* or *having spin-down direction*). Unlike Wilson's view, this proposal does not give up the assumption an entity O instantiates a determinable in virtue of O instantiating precisely one determinate (of a given level). Rather, besides "determinate" determinates such as *having spin-up direction* or *having spin-down direction*, there are also "indeterminate" determinate such as *being indeterminate with respect to the property of having spin-up direction* or *having spin-down direction*.

This proposal is incompatible with Wilson's. However, even if not particularly attractive, it is coherent and has the advantage of being more "conservative" than primitivism about determinables. It may also have some explanatory advantages. For instance, instantiating the "indeterminate" determinate (as *being indeterminate as to having spin-up direction* or *having spin-down direction*) excludes the instantiation of other determinates of the same level (such as *having spin-up direction* or *having spin-down direction*). This is as we should expect, and the result is a consequence of a general fact about the determinable-determinate relation. I do not wish to defend this position here, though. Rather, it suffices to point out that there may be 'object level' alternatives to Wilson's proposal that also escape the problem raised by KS.

Another potential threat to Wilson's view concerns whether indeterminacy of properties is related to indeterminacy of identity conditions. Since his famous paper from (1976), nobody who wishes to account for worldly indeterminacy has aimed at challenging Evans' argument, and rather tried to circumventing it. The result is roughly that whatever MI is, it should not imply indeterminacy in identity conditions. It would be worrying of course if Wilson's account does. Note that, according to the view, even if the electron lacks definiteness relative to a certain level of determination, it is not itself vague, in the sense that identity claims concerning it have determinate truth values. For instance, it is determinately the case that the electron is *not* identical to any 'precisified' version of it, rather than being indeterminate to which it is identical to. However, this consideration looses some strength when we consider that there could be a difference between fundamental entities and common objects with respect to the determinable-determinate distinction. Intuitively, an electron seems to be nothing more than the sum of very few properties. Once one of them is indeterminate, it is not clear what it mean to say that the electron is still something determinate. Let us assume that electrons are fundamental entities. An argument to the conclusion that, if a certain fundamental entity *e* has some indeterminate properties, then *e* itself is indeterminate, could be on the following lines. Suppose *e* possesses exactly 5 properties: color, shape, position, mass, and spin. Now also suppose that, for each of these 5 properties, the object could instantiate them only in 2 ways or degrees—the object can only be colored in 2 ways, can only be located in 2 different places, can only have 2 different shapes, and so on. Now suppose that *e* determinately possesses 4 out of 5 properties, but it is indeterminate as to which determinate of the fifth determinable it instantiates. Say it is indeterminate in its color. Now one can imagine that, even though *e* possesses an indeterminate property, the object itself is not a vague one. But suppose now, that instead of just one indeterminate property, *e* has 4 indeterminate properties out of 5, say only its shape is still determinate. Is the object still a determinate one? Although one could answer positively for the same reasons as before, it is also clear that doing so implies accepting a certain view of individuals as something over and above their properties. To strengthen this claim, we could after all even imagine that all the 5 properties are indeterminate. It seems that the account needs to presuppose a specific view of what individuals and properties are. If one thinks that individuals are nothing but the sum of their properties (as for the bundle view of properties), then the indeterminacy of some properties will entail in some cases indeterminacy of individuals as well. Although this is no problem *per se*, it is definitely one that requires a further analysis.

To conclude, I argued that the *object-level* approach to MI is not free from objections. However, as I have shown, it seems able to handle many of them, and furthermore it is certainly the best account for quantum indeterminacy.

CHAPTER 3

Quantum Indeterminacy & Fundamentality

3.1 Eliminativism on Quantum Indeterminacy

As shown in the previous chapters, many have focused on quantum mechanics to provide motivations for developing an account of metaphysical indeterminacy. The most recent discussions, however, show that quantum indeterminacy has to be understood not in isolation, but rather by looking at its status in each of the main interpretations of the theory. David Glick (2018) has recently done so, and eventually argued that quantum indeterminacy ends up disappearing from the fundamental level, from which he concludes that it would be 'eliminable'. In order to provide an extensive response to Glick's eliminativism on QI, in this chapter I will be focusing on the relationship between MI and the topic of *fundamentality*. I shall start by showing that, according to the main views on the relationships between the derivative and the fundamental levels, inferring 'eliminable' from 'derivative', as Glick does, is generally mistaken. However, there is a more charitable reading of Glick's view on which I will be focusing on. Roughly, the idea is that, despite not being eliminable, if quantum indeterminacy would turn out not be fundamental, it would not be metaphysical either. One way of doing so is by claiming that MI (independently from any consideration about quantum mechanics) cannot be a derivative phenomenon—a position famously defended by Elizabeth Barnes (2014). I will argue extensively that Barnes' reasoning can be resisted. Finally, I will consider another way of defending the view of Glick and the likes, namely that of exploiting standard semantic or epistemic resources to account for derivative QI. My conclusion will be that quantum indeterminacy, though derivative, still is a mind-independent phenomenon, and thus, fundamental or not, it should still be taken as motivating an account of metaphysical indeterminacy.

Proponents of metaphysical indeterminacy (MI) have recently been focusing on quantum mechanics to motivative their view. Standard arguments for the existence of MI were mostly concerned with macroscopic phenomena, such as the vague boundaries of clouds and mountains. However, this kind of vagueness is notoriously hard to understand as metaphysical in character, and many semantic accounts are on offer which seem to capture quite nicely what goes on in these cases—for instance, by insisting that the meaning of the words we use to refer to those macroscopic objects fail to determine their instances unambiguously. Since it does not seem to dependent on our language, quantum mechanical indeterminacy, also named 'deep' MI by Skow (2010), is considered to be of a special kind. On these lines, many have argued[33] that by establishing its existence we would provide strong motivations in favor of MI.

David Glick (2018) has recently challenged the above line of reasoning. While previous attempts of arguing in favor of quantum indeterminacy were mostly focused on the general mathematical structure of the theory,

[33] See footnote 7 above.

Glick invites us to look at the different interpretations in order to establish what quantum indeterminacy really consists in. Glick considers the three main live interpretations of the theory—Everettian, Bohmian, and GRW—and after a quick analysis (p.2), he concludes that in each of them, quantum indeterminacy disappears from the fundamental level, and therefore that it can be eliminated (p.3).

> If […] one took the properties to be ontologically derivative and quantum states to be fundamental, there would be little room for metaphysical indeterminacy […] any indeterminacy would occur at the non-fundamental level and hence may be viewed as eliminable.

This chapter argues that Glick's view, as it stands, is not tenable. As a start, in section (2) I will discuss Glick's analysis of the status of indeterminacy in the three main realist interpretations of quantum mechanics. In section (3) I will then show that according to the main views on the relationships between the fundamental and the derivative levels of reality, the inference from 'derivative' to 'eliminable' is generally mistaken. In section (4), I propose a more charitable reading of Glick's claim, according to which quantum indeterminacy, being merely derivative, is not metaphysical, and should not therefore be taken as motivating an account of MI. To strengthen such new reading of Glick's view, in section (5) I will then consider an argument by Elizabeth Barnes (2014) to the conclusion that MI has to be fundamental, but eventually I will reject it in section (6). I will finally argue, in section (7), that Glick's view does not go through, for the standard ways of understanding non-metaphysical indeterminacy are deeply unsatisfactory in the quantum mechanical case.

3.2 Is QI a derivative phenomenon?

A striking feature of quantum mechanics, recognized since the very birth of the theory, is that it challenges the standard way of thinking about property attribution. When we think about objects instantiating properties, we are intuitively lead to assume that they always do so in a definite way. Quantum objects, instead, sometimes lack definite values for their properties. This feature, known as lack of value definiteness, is often taken to motivate MI. Here are, for instance, Calosi & Wilson (2018):

> […] the property dependencies characteristic of quantum phenomena […] are present not just on the orthodox interpretation but also on (common understandings of) all the main non-orthodox interpretations conceiving of quantum indeterminacy in metaphysical terms. [p. 27]

Since it depends on the very algebraic structure of quantum observables, according to Calosi & Wilson quantum indeterminacy (QI) is pervasive. However, as noted by Peter Lewis (2016), *inter alia*, we should not in general derive overall metaphysical conclusions from quantum mechanics, given the large disagreement between different interpretations. Indeterminacy, as correctly claimed by Glick (2018), is no exception. Let us ask then, once the different interpretations of the theory are taken into account, what must be said about quantum indeterminacy. Following the literature on this, as well as my own discussion in chapter 1, section 1.3, I will now consider only the three main realist interpretation of the theory.

Let us start by considering *Spontaneous Collapse* interpretations, and in particular GRW (from Ghirardi-Rimini-Weber, 1986). Recall that, in GRW, when a collapse occurs the wavefunction is multiplied by a narrow Gaussian function that has tails stretching to infinity in both sides. This means that collapses in GRW are never to completely definite states (eigenstates), but only near ones. To solve this problem, known as tails problem, we find in the literature two main ways. To recap from the discussion in chapter 1:

- **GRW Link**. The EEL is substituted with a weaker link, one that allows systems to possess definite values for their properties even if they are not in the corresponding eigenstate, but are only near one. Two ways:
  - GRW Fuzzy Link. (Albert & Loewer 1996) Fuzzy Link: A quantum system has a definite value v for a particular observable *O iff* the square projection of its state into an eigenstate of *O* is greater than 1?-*P*, for some (arbitrarily chosen) *P*.
  - GRW Vague Link. (Lewis 2016) Vague Link: A system has a determinate value for a given determinable *to the extent* that the square projection of its state onto an eigenstate of the corresponding operator is close to 1.

- **GRW Primitive Ontology**. We provide an underlying ontology that is always determinate at every time, and we regard the residual indeterminacy only as derivative.
  - Two ways: GRW Mass Density (Ghirardi & al. 1995);
  - GRW Flash (Tumulka 2006a, 2006b).

The conclusions I draw from considering the different approaches on GRW with respect to quantum indeterminacy, are: (i) in link approaches, as also noticed by Lewis (2016), the indeterminacy is somehow fundamental; (ii) for the primitive ontology approaches, although there is some residual indeterminacy, it only concerns the derivative level of reality. At the fundamental level, both the flashes and the mass distribution are entirely determinate, as David Glick (2018) also maintains. Thus, given that it is safe to claim that the *primitive ontology* approaches are nowadays the received view on GRW, Glick is correct in saying that the received view on GRW sees the indeterminacy as a derivative phenomenon.

Let us now turn to Bohmian mechanics. According to this interpretation, it is definitely true that the positions of the fundamental ontological items, say the particles, are always determinate (Bohm, 1951). And given that these are the only properties we directly observe, it follows that all the properties we directly observe are perfectly determinate. Therefore, indeterminacy in Bohmian mechanics turns out to be simply epistemic. We just do not know the precise values for all observables, but the fundamental ontology is made by particles that always possess determinate positions. However, this still leaves us with the issue of what we should think about properties that are, unlike position, not fundamental, as I was mentioning in the first chapter. Recall, for example, Peter Lewis (2016): « … although the Bohmian strategy arguably makes all the properties we directly observe [positions] determinate, it does not thereby make *all* properties determinate » (p. 101). Contrary to position, spin can have indefinite values in Bohmian mechanics. What should we say, from a metaphysical point of view, about the status of those derivative properties? Depending on the details of the different takes on this particular interpretation of the theory, indeterminacy is either epistemic or, if metaphysical, only derivatively so. So, once again, Glick (2018) is correct in saying that Bohmian mechanics does not suggest fundamental indeterminacy.

Finally, let us consider Everettian interpretation (Everett, 1957). At first glance, the Everettian interpretation seems to avoid metaphysical indeterminacy by postulating the process of branching. Every branch seems free from indeterminacy. On a closer inspection, however, *before* decoherence is imposed on the quantum systems, indeterminacy is still present *within* each branch. The status of this residual indeterminacy is highly problematic. Is it fundamental or derivative? Recent discussions on Everettian interpretation have focussed on a view known as *wavefunction realism* (Albert & Ney, 2013). According to it, what is the fundamental just is the multidimensional wavefunction, everything else supervenes on that. And the wavefunction does not contain or entail any metaphysical indeterminacy, or so it is argued. Even admitting that, on the derivative level, superposition states within each branch before decoherence cannot be eliminated, the fundamental ontology of Everettian QM does not include indeterminacy. Therefore, also here, Glick is correct that quantum indeterminacy is not a fundamental feature of reality.

As noted by Calosi & Wilson (manuscript), there also are ways of interpreting QM on which indeterminacy is fundamental[34]. However, I believe Glick is correct in establishing that the received views are those according to which QI is not fundamental. QI seems to appear, in many views on QM, only at some derivative level, emerging from the particles spatial distribution, the wavefunction, the mass density distribution (GRW-Mass), or the distribution of the flashes (GRW-Flash).

The question before us now, is what kind of relation is there between the fundamental (fully determinate) level, and the derivative (sometimes indeterminate) quantum properties. In the next section I will briefly consider some possible options, and eventually conclude that none of them justifies the claim that the derivative level is eliminable.

[34] The clearest example is 'standard' QM, that is extensively discussed by Glick (2018) as well. However, Calosi & Wilson (ms) also considers ways of interpreting Bohmian, Everettian, and GRW, according to which the indeterminacy turns out to be fundamental, though they are not the received views. In response, Glick (ms) has recently pointed out that it might even be a desideratum for a physical theory that of avoiding fundamental indeterminacy. I shall notice however, that in general imposing such metaphysical constraints on physical theories is a mistake. Furthermore, even if Glick were correct, perhaps what justifies the development of fundamental-indeterminacy free interpretations is precisely the lack of a coherent account of MI. Given that we now have many ways of developing such an account…

3.3 Quantum Indeterminacy is not Eliminable

How to spell out exactly what is the metaphysical relation at work in the cases just discussed is something that goes beyond the scope of this paper. However, a quick review of the main positions will be enough to show that inferring 'eliminable' from 'derivative', as Glick does, is generally erroneous. For reasons of space, I will only be considering what I take to be the three main notions that can be used in this context, namely reduction, emergence, and grounding[35].

As a preliminary remark, I shall notice that, as regards to reduction and emergence, we can set aside a certain type of relation that is widely discussed in the literature, namely the inter-theoretic one. In the case at hand, it is obvious that we are not talking about reduction/emergence of one theory to the other, for both levels of discourse—the fundamental wavefunction and the derivative quantum properties—belongs to the very same theory, quantum mechanics[36]. To argue for the contrary would be, to say the least, highly revisionary of the way we think about what it means to provide an interpretation of quantum theory. So I will not consider these options, and instead focus only on the ontological way of spelling out the above relations, which I assume to be the correct one.

Ontological reduction can be of two types, *eliminativist* and *conservative*. The question to ask is whether we have any reasons to believe that the former type is apt to our case study. I do not think it is, as a quick review of the literature will show. Here are, for instance, van Riel and Van Gulick:

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[35] Calosi & Wilson (ms).

[36] A similar point is made by Alyssa Ney (2013) when she discusses the reduction of 3d space to the multidimensional wavefunction: « What I am most interested in here is the project of facilitating an ontological reduction. When I talk about *ontological* reductions, the contrast is with the classical notion of inter-theoretic reduction in the philosophy of science» (p. 173).

> Reductivists are generally realists about the reduced phenomena and their views are in that respect *conservative*. They are committed to the reality of the reducing base and thus to the reality of whatever reduces to that base. Though conservative realism is the norm, some reductionists take a more anti-realist view. In such cases the reducing phenomena are taken to replace the prior phenomena which are in turn *eliminated* […] The oxygen theory of combustion replaced the phlogiston theory and phlogiston was eliminated. Whether to count such *eliminativist* views as a variety of reduction is a matter of theoretical choice. Some might argue reduction entails realism about the reduced phenomena. If so, elimination is not reduction. (2003)

Even accepting that some reduction can be eliminativist—as perhaps it is in the case of phlogiston—the claim that the reduction of derivative quantum properties to the wavefunction is one of those cases lacks motivations. First, because contrary to the case of phlogiston, derivative quantum properties are not *replaced* by new properties. Second, because there also seems to be good reasons for being *conservative* reductionists in the QI case. A major problem faced by all those who are realist about the wavefunction is how we get to our ordinary 3-dimensional space, if all there is fundamentally to the world is the wavefunction living in a multidimensional space. Indeterminacy is no exception here, since the quantum properties that we usually take to be indeterminate—such as *spin*—only appear in the 3-dimensional space. To solve this problem, Albert (1996) famously proposed an error theory according to which for the wavefunction realist, the 3-d space is nothing but an illusion. Accepting Albert's view is perhaps one way to go for the eliminativist on QI. However, very few people today share this view, and many are trying to come up with a better story. If the 3d space is not entirely eliminated from our ontology, then neither are the properties that we find in it, along with their indeterminacy.

Another idea that is usually associated with reduction, to which defenders of Glick's claim could appeal to, is that of the possibility of a *translation procedure*. Once a reduction is successfully achieved, it should come with an in principle way of properly translating the objects of one level of discourse (the target of reduction) into those of the other level (the reductive base). In the quantum case this can easily be done by showing that every quantum property, whether indeterminate or not, corresponds to (can be translated into) properties of the wavefunction. Once again though, it is not clear why such translation procedure would imply any eliminativism. Translation procedures are clearly symmetric. In order to argue that one side of the reduction is ontologically privileged, we need further constraints. For instance, one way to provide such constraints can be seen in the Churchland (1986) style reduction of the mental to the physical[37]. Churchland argues that purely qualitative mental properties are problematic, and thus should be eliminated. However, she argues for this claim on independent ground, and it is unclear whether such independent grounds can be provided in the case of QI. To assume without independent ground that indeterminate properties are problematic is clearly question begging[38].

Turning to emergence, I believe it is even more clear that Glick's claim is too strong. Metaphysical emergence of new features or properties can be of two types, weak and strong, neither of which allows for eliminativism. The reason is, in both cases, that emergence is usually associated with two

[37] I thank Jessica Wilson for suggesting this example.

[38] Glick (ms) argues that such independent ground can be methodological, in that physical theory should be such to avoid any reference to indeterminacy. As already noted in footnote 2, however, I am suspicious of any such strong metaphysical constraints on physics. After all, last century developments in physics have shown how damaging can be to assume too much metaphysics when interpreting physical theories. I believe that a good naturalistic attitude towards physics suggests we do not take determinacy as a desideratum as Glick suggests.

components, *synchronic dependence* and *ontological/causal autonomy* (for an overview, see Wilson (forthcoming)). The latter component is crucial to see why eliminativism cannot be applied to emergence. No matter how is spelled out[39], *autonomy* cannot be understood if the emergent entities/ properties are dispensable and eliminated from the ontology.

Let us finally consider grounding as a way to understand the relationships between derivative quantum properties and the fundamental wavefunction. Alyssa Ney (2013) is the most prominent example of such view. Nonetheless, as regards to Glick's view, a few examples will suffice to show that grounding is generally introduced *in contrast* to eliminativism, rather than in accord with it. Fine (2001), for instance, explicitly introduces the notion of *ground* to provide a middle way between the realists and the skeptics about ontological dispute (p. 3). Schaffer (2009) is even more explicit, when he claims that « [m]y sort of neo-Aristotelian will also be permissive about existence, in that she will not toss many candidate entities into the rubbish bin » (354), and « I am invoking the one and only sense of existence, and merely holding that very much exists » (360).

A lot more should be said about each of the options I have been quickly considering here. Nonetheless, the above is enough to show that there is in general no interesting sense in which derivative entails eliminable. As it stands, Glick's conclusion is simply mistaken, unless a reason is given to believe that standard ways of talking about the above notions should be revised when we talk about indeterminacy. Lacking such independent

[39] Ways of understanding ontological autonomy for emergent entities include: nomological supervenience (van Cleve 1990, *inter alia*), non-fundamental or fundamental novelty (Humphreys 1996, Wilson 2002, *inter alia*), non additivity (Bedau 1997, *inter alia*), multiple realizability (Aizawa & Gillet 2009, *inter alia*), symmetry breaking (Morrison 2012), elimination in degrees of freedom (Wilson 2010), in principle failure of deducibility (Hempel & Oppenheim 1948, *inter alia*), *inter alia*. For a discussion, see Wilson (forthcoming). None of the aforementioned characterization allows for eliminativism.

reason, we shall conclude that eliminativism about derivative quantum properties should be rejected.

3.4 QI: Derivative yet Metaphysical

Although Glick's position is untenable as it stands, there is perhaps a more charitable reading of his view in the vicinity that we might consider instead[40]. The idea, roughly, is that although not eliminable *tout court*, QI is eliminable *qua* metaphysical. By going this way, nothing need be added about the status of the derivative ontology, or about the nature of the relationships between fundamental and derivative levels. There still is indeterminacy in quantum mechanics, but the fact that it is not fundamental as we might have thought, implies that it is not metaphysical either. And, therefore, that it should not motivate accounts of MI. I will argue in this section that, also on this reading, Glick's view is not as straightforward as it might seem.

In order to argue that derivative quantum indeterminacy is not metaphysical, two strategies seem available. The first strategy is to insist that, independently from QI, derivate MI is not consistent *tout court*. As I will show, proponents of this strategy might appeal to an argument, given by Elizabeth Barnes (2014), to the conclusion that MI has to be fundamental. The second strategy, instead, is to focus on QI only, and claim that it can in principle be explained as a mind-dependent phenomenon (either epistemically or linguistically). This can be achieved by showing that epistemic or semantic accounts of indeterminacy can be applied to quantum indeterminacy, provided it is merely derivative. Before

[40] Glick (p.c.) also admits that this is the view he had in mind.

considering each strategy though, a general worry needs be addressed. Thus far, in this chapter I have been talking about MI quite liberally and without specifying what it is meant by it. However, in order to better address whether MI can be merely derivative, and whether QI can be taken as non metaphysical, a few reminders about the different approaches on MI are in order[41].

In recent discussions, a consensus has been reached that two quite different accounts of MI can be put forward. Jessica Wilson (2013) has named them *meta-level* and *object-level* approaches, and I will follow her on this. On the former approach, MI has to be understood as worldly unsettledness between fully precise options: there is MI when it is indeterminate which (determinate) state of affairs obtains. On the latter, *object-level* approach, MI consists in the (determinate) obtainment of indeterminate state of affairs. The first approach will generally be accompanied with a logic and semantic for the sentential indeterminacy operator, usually mimicking modal logic (Akiba 2004, Barnes & Williams 2011). The second approach does without the operator, and instead will need certain further metaphysical assumptions concerning how to distinguish determinate states of affairs from indeterminate ones. For instance, Wilson (2013) works on the assumption that determinable properties can be as fundamental as determinate properties, and then reduces MI to the obtainment of state of affairs composed by objects instantiating determinable without unique determinate properties. Given the substantial differences between the two approaches, to avoid confusion I will discuss them separately from now on.

In his paper, Glick (2018) explicitly focuses on Calosi & Wilson (2018) approach to QI, which builds upon Wilson (2013) *object-level* approach. The main reason for this, as I have argued in chapter 1, is that *meta-level*

[41] For a more extensive discussion, see Chapter 1, section 4.

approaches might be ill suited to treat QI (Skow 2010, Darby 2010, Calosi & Wilson 2018, *inter alia*). However, I have also pointed out that many have been recently responding to this worry (Torza 2017, Darby & Pickup 2019, Fletcher & Taylor, ms), and it is fair to say that the debate looks pretty much open on this issue. I will therefore also consider the *meta*-level approach in what follows.

Let us start by asking whether, on an *object-level* approach on MI, merely derivative MI is consistent. Although Glick (2018) does not say explicitly, while introducing Calosi & Wilson's (2018) account he writes the following:

> For one who adopts Wilson's approach to metaphysical indeterminacy, this situation can be understood as a particle with the determinable *position* but lacking a (unique) determinate of position. If this is the correct understanding of QM, it follows that there is widespread indeterminacy at the fundamental level of reality. (p. 2)

If we assume, for the sake of the arguments, that QM is a fundamental theory (as Glick does in a footnote), then Wilson's approach would entail a 'widespread indeterminacy at the fundamental level'. This means that even if merely derivative MI might be consistent, according to Glick the quantum case is one in which the indeterminacy has to be fundamental for proponents of Wilson's approach. I think, however, that Glick's mistake here is to confuse fundamentality of theories with fundamentality of entities or facts. Even on the assumptions that (i) QM is a fundamental theory, and, crucially, that (ii) the fundamental entities are described only by fundamental theories, the argument does not go through. The reason is that (ii) leaves open (and rightly so!) the possibility for fundamental theories to also entail the existence of derivative entities. In order for Glick to argue that QI has to be fundamental, he needs to assume that, *qua* fundamental theory, QM only describes fundamental entities. This assumption, however, is highly problematic and probably question begging.

Is there a better argument for the claim that MI has to be fundamental on an *object-level* approach? I do not think there is, since the very metaphysical structure of this view is silent about whether the indeterminate states of affairs obtain at a derivative or at a fundamental level. It simply does not say. The confusion might arise from thinking that, for instance according to Wilson's (2013) account, determinable properties are as fundamental as determinate properties. But the notion of fundamentality at work in this claim is a merely *relative* one. To make an example, to say that the determinable *color* is as fundamental as its determinates *red* or *blue*, is not to say that *color* is as fundamental as, say, *spin* or *mass*. On an *object-level* view, fundamentality does not enter into the definition of indeterminacy. For the indeterminacy to be fundamental on such view, we need to claim that the state of affairs that are indeterminate are fundamental.

Turning to the *meta-level* approach, the situation changes radically, and looks more promising for those who share Glick's intuition. First of all, because as argued by Barnes (2014), the *meta-level* approach needs to assume in their ideology an indeterminacy operator. Of course, having an operator *per se* does not mean having a primitive (just think of Lewis' reduction of the modal operator). It is true, however, that according to Barnes & Williams (2011)—which is probably the most developed *meta-level* account—indeterminacy is in fact a *primitive* notion. There is a sense in which, then, having indeterminacy as a primitive notion entails something about its being fundamental. The connection between *primitiveness* and *fundamentality* is a very intuitive one (see Benovsky 2013 for discussions), and a detailed discussion of this issue goes beyond my aim here. Notice, however, that at least *prima facie*, there is a big difference between saying that there is indeterminacy in our fundamental ontology, and saying that indeterminacy *itself* is fundamental (see Barnes 2014).

What is needed is an argument for the claim that if indeterminacy is in our ontology, then it is in the fundamental ontology. To my knowledge, the only such argument has been given by Elizabeth Barnes (2014), so it is worth spending some time on it.

3.5 Barnes' Argument Against Derivative MI

Elizabeth Barnes (2014) has influentially argued for the following conditional claim: if there is any metaphysical indeterminacy, this must be at the fundamental level of reality. In a nutshell, Barnes' argument relies on the following two principles:

> **Bivalent Completeness** (BC) — A complete description of a world *w* is a bivalent assignment of truth values to every sentence at *w*.

> **Determinate Link** (DET-L) — The determination link between more and less fundamental levels of reality is determinacy preserving.

From these two principles, and assuming metaphysical indeterminacy at some derivative level of reality, but no indeterminacy at the fundamental level of reality, Barnes derives a contradiction, thus establishing the truth of the following conditional:

> Fundamental Metaphysical Indeterminacy (FI) — If there is metaphysical indeterminacy, it cannot be only at the derivative level of reality (p. 341).

Benjamin Eva (2018) has recently provided reasons to reject FI by challenging BC. Despite my agreement with Eva's rejection of FI, I believe his reasoning fails. As I will show, however, a stronger case against Barnes' conclusions can be found by focusing on DET-L. My conclusion will be that derivative metaphysical indeterminacy is consistent across a large part of the logical space. In this section I introduce the argument by Barnes (2014) in favor of FI, discusses Eva's (2018) critique of BC, and suggests a way to resist it. In the next section I argue that, even granting BC, Barnes' argument still does not go through, for DET-L can be rejected.

Barnes (2014) starts her paper by inviting us to consider the following intuition about how fundamentality relates to indeterminacy:

> If the world really is indeterminate, then it must be in virtue of indeterminacy in fundamentals…if you've got determinate components and combine them in determinate ways, there's nowhere for indeterminacy to come from. (p. 341)

If she is correct[42], then either there is no metaphysical indeterminacy at all, or if there is any, it has to be fundamental[43]. To argue for this view, Barnes provides a *reductio* of the possibility of metaphysical indeterminacy only at the derivative level. Starting from the assumptions that there is indeterminacy in how the world is at the derivative level, but no indeterminacy in how the world is at the fundamental level, she derives a contradiction. Her argument runs as follows (*f* and *d* are variables ranging respectively over descriptions of the world at its fundamental and derivative level, while *F* and *D* are names of particular descriptions):

1. For any complete true description of how things are fundamentally, *f*, and any complete description, *d*, of how things are derivatively, either *f* entails *d* or *f* is incompatible with *d*. (Assumption)
2. Entailment is determinacy preserving. (Assumption)
3. For some complete description, *D*, of a way for things to be derivatively, it is indeterminate whether *D* is true. (Assumption)
4. For some complete description, *F*, of a way for things to be fundamentally, it is determinate that *F* is true. (Assumption)
5. Either *F* entails *D* or *F* is incompatible with *D*. (From 1)
6. If *F* entails *D*, and *F* is determinately true, then *D* is determinately true. (From 2)

[42] I shall notice that, even at this stage, it is unclear how many people would actually agree with Barnes' way of expressing her intuition. To give an example, those sympathetic with Jonathan Schaffer's monism (2010) would not grant the idea that 'the fundamentals' are the components which make everything else. As I shall make clear in this section, here as well as in many other passages, Barnes seems to make many unwarranted assumptions, both on fundamentality and on indeterminacy.

[43] In the paper I am considering here, Barnes does not argue for the consistency of metaphysical indeterminacy, but simply assumes it in order to discuss whether it then has to be fundamental or not. In other works, however, she also argues for the view that metaphysical indeterminacy is consistent (2010, Barnes & Williams 2011).

7. If F is incompatible with D (i.e., F entails not-D), and F is determinately true, then not-D is determinately true. (From 2)
8. Either D is determinately true or not-D is determinately true. (From 4, 5, 6 & 7)
9. Contradiction. (From 3 & 8)

The assumptions (1)-(4) lead to a contradiction, and therefore cannot be all true. Assumptions (1) contains the expression 'complete true description'. Notice that—given the way in which Barnes uses the assumption for the argument—we could take this expression to be implicitly defined as BC above. Assumption (2), instead, is an instance of the principle DET-L. Finally, assumptions (3) and (4) taken together simply express the view, respectively, according to which there is metaphysical indeterminacy at some derivative level of reality, but no indeterminacy at the fundamental level. Thus, in order to maintain the consistency of metaphysical indeterminacy only at the derivative level, one has to reject at least one of premises (1) or (2).

Assumption (1), as Barnes has it, « is intended to be the unpacking of the thought that the fundamental facts fix the derivative » (2014, p. 342). A first remark to be made here concerns Barnes' use of *entailment* in order to express the relationship between the fundamental and derivative levels of reality. Indeed, one could think that although the fundamental facts do indeed fix the derivative ones, this is not enough for the former to *entail* the latter. Perhaps, instead, a better way to express the idea behind (1) would be by appealing to the notion of *grounding*. Barnes quickly considers this option, and concludes that « … if the relationship between the fundamental and derivative—whether entailment or grounding or 'in virtue of', and so forth—is determinacy preserving, the argument will still go through » (2014, p. 342). Notice *en passant*, however, that taking into account here the different ways of defining *what it is* to be fundamental would require a much more detailed analysis[44]. I will set this complication aside for now, and shall assume that there is a way to cash out the relation between the fundamental and the derivative in (1) so as to come out correct for each all the main views on fundamentality.

Benjamin Eva (2018) has recently argued that premise (1) can be rejected. According to him, the completeness mentioned in (1) is ambiguous between BC and the following different reading:

Maximal Completeness (MC). A complete description of a world *w* is a full and consistent specification of which sentences are determinately true, which are determinately false, and which are indeterminate at *w*.

While BC yields Barnes' conclusions, MC does not. Eva then notices (p. 36) that assuming BC is question begging, for it would seemingly require a 'complete description' to be entirely free from indeterminacy. As Eva has it:

> Clearly, this assumption is no longer justified once one countenances the possibility of metaphysical indeterminacy. Indeed, the claim that the world admits of metaphysical indeterminacy is itself equivalent to the claim that states of affairs need not always be complete. (p. 37)

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[44] In fact, I am here convinced that Barnes assumes a certain account of fundamentality, the so-called *independence* view of fundamentality—that is the idea that the fundamental is what is not dependent on anything else, and so it is *ungrounded*, or *unbuilt* (Bennet 2017). This reading is also supported by her quick discussion on the possibility of substituting *entailment* with *grounding*. However, the *independence* account of fundamentality is not the only one on offer, and a much more careful analysis is needed in order to establish a result like Barnes' for each of the main views on what it is to be fundamental (such as, just to mention, the *minimal complete basis* view, the *naturalness* account, or the *primitivist* account). I take such analysis to be highly worth pursuing, though it goes beyond the scope of this paper.

Eva's idea here, as I understand it, is that once we assume metaphysical indeterminacy (no matter at what level), we should not rule out MC as the correct way of cashing out what a 'complete description' amounts to. However—and putting the details of Eva's discussion aside—we could wonder why Barnes cannot simply require to read the completeness in premise (1) as BC for fundamental level, yet granting MC for the derivative level. After all, she is explicitly assuming for the *reductio* that the fundamental level is completely determinate, which seems to actually suggest such a reading.

I shall stress that I do not mean to consider the above consideration conclusive. Rather, I only aim at suggesting that Eva's critique might be resisted, and I take this as the main motivation for pursuing a different strategy. My claim is that instead of questioning MC, we rather focus on DET-L and assumption (2) thereby. And I should also point out that Eva is explicit in granting Barnes' defense of (2) (see, p. 34), which I take as further reasons to provide further analysis of it.

3.6 The (In-)Determinate Link

Premise (2) can be seen as an instance of the general principle DET-L, repeated below:

> **Determinate Link** (DET-L) — The determination link between more and less fundamental levels of reality is determinacy preserving.

One could immediately wonder why requiring the link between what is fundamental and what is derivative to be 'determinacy preserving', given that—for reasons that I already mentioned regarding premise (1)—after all we are in fact *assuming* the possibility of metaphysical indeterminacy. Perhaps, if there is metaphysical indeterminacy, it might turn out that its source lies precisely in the *link* between what is more and less fundamental. And, if so, there may be indeterminacy that emerges at some derivative level without it being inherited from the more fundamental level. Such a line of reasoning would straightforwardly deliver a way to reject Barnes' argument, so let us see how it works in details.

In the presence of indeterminacy, the thought would go, one cannot infer 'Determinately *q*' from 'Determinately *p*' and '*p* entails *q*', for instead what is needed is 'Determinately, *p* entails *q*'. In this way, however, the argument would be blocked by requiring the modification of premise (5) into the following:

5*. Either *F* determinately entails *D* or *F* is determinately incompatible with *D*.

(5*), contrary to (5), does not follow from (1), and can be rejected if we allow for the presence of some indeterminacy regarding which derivative facts are *entailed* by the fundamental ones. Barnes grants that much (2014, p. 343), but then considers the implications of allowing for such indeterminacy in the *link* between the fundamental and the derivative facts:

> …suppose the link between the fundamental and derivative can itself be indeterminate. If the facts about that link are *themselves* fundamental then this is simply another route to fundamental indeterminacy. If the fundamental facts entail the derivative ones but it's indeterminate what derivative facts the fundamental facts entail then if the facts about what entails what are fundamental then there is indeterminacy in what fundamental facts obtain. So if the link between the fundamental and the

> derivative is determinacy preserving, then there cannot be indeterminacy at all without there being indeterminacy in the fundamental facts. If the link is not determinacy preserving then that is because it itself can be a locus of indeterminacy. And so if the facts concerning the link are fundamental facts, then there is indeterminacy in the fundamental facts. (p. 343)

Behind Barnes defense of premise (2) is the idea that the facts about the link between what is fundamental and what is derivative are *themselves* fundamental. As I mentioned, Eva agrees (2018, p. 34-35)—but why should we accept this? There is a growing literature regarding the issue of whether the facts about the link between what is fundamental and what is derivative are themselves fundamental. As a matter of fact, very few people share Eva and Barnes view on this, at least when the link is understood as a grounding relation. A quick survey of literature on this proves that Barnes' assumption (2) is (at best) unjustified.

The question before us is the following: are the facts about the link between what is fundamental and what is derivative themselves fundamental? To address this issue, I will now focus on grounding. Recall that Barnes herself admits that the appeal to entailment in premise (2) might be a mistake, and that perhaps grounding would be a better notion to express the thought that fundamental facts fix the derivative facts. Be that as it may, it is quite clear that what she has in mind here is precisely a relation of determination and priority between facts. I myself believe that the appeal to entailment is misleading, but I will not argue for this and, for reasons of space and simplicity, I will simply assume that the discussion on grounding can be properly translated into whatever Barnes has in mind, be that entailment, a building relation, 'in virtue of', and so forth[45].

While discussing whether the facts about the link are fundamental or not, Barnes also makes reference to a paper on this topic, namely Karen Bennett's *By Our Bootstraps* (2011). Curiously enough, in this very same paper Bennett argues for the view the the facts about the link *are not* fundamental, *contra* what Barnes needs to assume for her argument to work (Barnes 2014, p. 343). However, there are also other options on the table on this issue, so let us quickly consider them. Three main views have been defended in the recent literature, which we can schematically group as follows:

I. The facts about what grounds what are fundamental.
II. The facts about what grounds what are derivative.
III. The facts about what grounds what are neither fundamental nor derivative (Trialism).

There are, of course, reasons in favor and against each of the above. As regards to (I), the main motivation is that if grounding facts *are not* fundamental, they would in turn be grounded in some other facts in a way that might seem to involve some vicious regress. As regards to why (II) should be preferred, different views on fundamentality differ. According to Ted Sider's (2011) principle of *purity*, for instance, fundamental truths involve only fundamental notions. This principle clearly implies that facts

[45] One might object her that this is too quick, and that entailment should rather be the focus, for that is what Barnes uses after all. I shall stress, however, that an argument very close to the one I will provide using grounding can be given for entailment as well, by using the generalized version of a principle of *purity*. If, the idea is, the fundamental facts cannot contain any reference to non fundamental facts, then facts about what derivative facts are entailment by the fundamental facts are straightforwardly not fundamental. This is at least a possible view, and it is one that Barnes does not even take into account.

about what grounds what cannot be fundamental, for they make a reference to the non-fundamental level[46]. As I mentioned, Karen Bennett (2011) extensively argues in favor of (II), mainly by responding to the regress concern. Finally, in the recent discussion, option (III) is gaining a lot of consensus. The first to introduce this view is deRosset (2013), although he does not endorse it. We then find an extensive defense in Dasgupta's (2016) distinction between facts that are *apt* and *not apt* for being grounded. On such view, the facts about what grounds what are *not apt* for being grounded, and so they are neither fundamental nor derivative.

Once again, it should be clear that I am not concerned here with assessing which of the above options is to be preferred. Rather, what is enough is to show that Barnes needs to assume (I) for her argument, and this is unjustified provided that Barnes does not provide an independent argument in support of it.

Thus, the defender of derivative indeterminacy can claim that facts about what grounds what are not fundamental, either because they are neither fundamental nor derivative (as for Dasgupta), or because they are derivative (as for Bennett, and Sider, *inter alia*). By doing so, she is able to claim that the fundamental level is free from indeterminacy, and that the derivative level can be indeterminate insofar as some facts about what grounds what are also indeterminate.

To conclude my analysis, I will now turn to a recent paper by Ryan Wasserman (2018), that could be taken as a way to characterize the idea that grounding facts can sometimes be indeterminate. Wasserman (2018)

[46] Here I am using Bennett's (2011) reading of Sider's reasoning. However, once Sider's account is taken more seriously, the issue gets more complicated. In fact, Sider's naturalness view on fundamentality is sub-propositional, which implies that we cannot even translate so straightforwardly the problem Bennett and I are discussing. I thank Claudio Calosi for making this point. On a separate note, see also Schaffer (2010) for a different reasoning, based on free recombination, in order to defend (II).

considers a view that, as he himself recognizes, though there is « much to say on its behalf », is « undeniably strange » (p. 66). Strange or not, I think the view can be useful to better understand how certain grounding facts can be indeterminate, as it is required by the modification of premise (2) I suggested in the previous section[47]. According to Wasserman, we could provide a novel theory of vagueness, and thereby solve the sorites paradox, by allowing for a metaphysical counterpart of the indeterministic laws of nature. The idea of an analogy between laws of nature and laws of metaphysics has been recently discussed by Kment (2014), Schaffer (2016), *inter alia*. The analogy is first suggested by the explanatory power of laws of nature, a feature that is also meant to characterize the laws of metaphysics. As Wasserman has it, the laws of metaphysics are « generalizations about *what grounds what* ». Now, we could be tempted to consider whether, as in the case of laws of nature, laws of metaphysics as well can be *indeterministic*. In a footnote, Schaffer (2016) considers this option, but immediately rejects it:

> Note that the restriction to the deterministic case is reasonable insofar as one wants a template for grounding, since "indeterministic grounding" seems impossible. Grounding seems to imply supervenience: fix the grounds and one fixes the grounded. The status of the grounded thus cannot be open to chance. By way of illustration, it seems impossible that, given a fixed physical ground, the biological status of the system remains open to chance. (p. 61)

On the contrary, according to Wasserman allowing for indeterministic laws of metaphysics would help us solving the puzzles related to the notion of

[47] I shall say in advance that a detailed discussion of such view would require a much longer analysis, one that might be interesting for further developments.

vagueness, and so deserves more consideration[48]. Let us consider an instance of the sorites paradox in order to see how this idea is supposed to work. Take a young man, Robert, who has at time *t* 1 billion hairs. We should agree that a man with 1 billion hairs is not bald, and thus:

1. Robert is not bald at *t*.

Now suppose that at a later time *t'* Robert starts loosing his hair at the rate of 1 hair per second, until he becomes completely bald. The sorites paradox can be formulated by adding the following, rather innocuous premise:

2. If Robert is not bald at *t*, then he is not bald at *t* + 1 second.

By reiterating (2), we will paradoxically conclude:

3. Robert is not bald at *t* + 1 billion seconds.

Standard responses to the sorites consist in denying the possibility of reiterating (2), and adding a *cutting point* where the reiteration is blocked—a precise number of hairs after loosing which one becomes bald. Wasserman's suggestion is instead to accept that there is no fact of the matter about what is the precise number of hairs that makes someone not bald—it is, in other words, metaphysically indeterminate when someone is bald and when is not, and it is so *because* the relevant law of metaphysics leaves this indeterminate.

[48] Though in a different context, also Emery (manuscript) contemplates the idea of *indeterministic grounding*. However, as she does not develop this idea further, I will only consider Wasserman's paper here.

> The basic idea behind the law-based theory is simple: *Vagueness consists in indeterminacy in the laws of metaphysics*. suppose that the fundamental facts, together with the laws, determine that a particular man has a thousand hairs on his head. In that case, we can suppose that the facts fail to determine whether or not the man is bald— in other words, the laws leave us with a borderline case of *baldness*. (2018, p. 76)

According to Wasserman, the laws of metaphysics that are relevant for cases of vagueness are indeterminate. And recall that, as I mentioned above, such laws basically are *generalizations about what grounds what*. Therefore, a possible way to characterize how the link between more and less fundamental can be indeterminate is to allow for indeterminate laws of metaphysics.

Barnes' argument against the possibility of derivative metaphysical indeterminacy relies on the principle that I called the *Determinate Link*. This principle, however, can be resisted in a context where we assume the possibility of metaphysical indeterminacy—be it derivative or not. Allowing for indeterminate grounding facts would not imply the impossibility of derivate metaphysical indeterminacy, as far as the facts about *what grounds what* are not fundamental. I have shown that according to many views on this topic, such facts are either derivative, or neither fundamental nor derivative (Trialism). Finally, having granted that facts about what entails what might not be fundamental, I have sketched a way, following Wasserman (2018), to allow for indeterminacy with respect to those facts. Thus, my conclusion is that Barnes' (2014) argument against derivative metaphysical indeterminacy does not go through.

3.7 QI is not Semantic or Epistemic

As I mentioned, there is a final strategy for those, like Glick, who wish to defend the view that QI is not metaphysical. The idea would consist in showing that standard epistemic or linguistic tools would suffice to account for QI, once we have established it being merely derivative (as we are assuming here). To see how this strategy could work, we first need to distinguish the two main approaches on QI—the *meta*- and *object*-level ones—and treat them separately.

Let us first consider the meta-level approach on MI. Elizabeth Barnes (2010) has provided the following counterfactual test (CT) that serves precisely as a tool for recognizing whether a sentence is MI or not:

> CT — Sentence S is metaphysically indeterminate iff: were all representational content precisified, there is an admissible precisification of S such that according to that precisification the sentence would still be non-epistemically indeterminate. (Barnes, p. 604)

The main strength of this definition, as correctly noticed by Barnes herself, is that both the epistemicist (who thinks that all the indeterminacy can be explained as a lack of knowledge) and the semanticist (who thinks that all the indeterminacy can be explained as semantic indecision) will agree on the truth of the above counterfactual. Disagreement, if any, would arise when evaluating single sentences. Let us consider, then, what an evaluation of a sentence expressing QI would amount to in this setting.

Let us take the sentence S to express the proposition ⌜the electron *e* is spin-up in *x*⌝, and let us assume that *e* is in eigenstate of being spin-down in *y*, so that the spin component in *x* is indeterminate according to QM. We can now suppose to run Barnes' counterfactual test for S. First, we should imagine that the language in which S is expressed has all its content precisified. In this case, the obvious candidate for the content that needs to be precisified is that corresponding to the predicate 'spin-up in *x*'. Clearly though, no matter how precise is the content corresponding to the predicate 'spin-up in *x*', the indeterminacy at issue is not eliminated. In fact, the predicate is already fully precise and unambiguous. Think of a vague predicate, such as 'bald', to understand where the difference lies here. 'Bald' is a vague predicate because we can easily build a Sorite's paradox with it. However, the precisificational strategy helps us avoiding the paradox. We can imagine an ideal language with a predicate for each number of hairs, instead of the predicate 'bald', and no paradox would arise. Even for such an ideal language though, the predicate corresponding to 'spin-up in *x*' is still perfectly precise. So the precisificational strategy is of no help here, and the sentence S is still indeterminate. To conclude the counterfactual test, we only need to add that such indeterminacy is not epistemic in character, as we already extensively discussed in chapter 1. Thus, sentences expressing QI (no matter whether fundamental or derivative) satisfy the counterfactual test for MI.

The counterfactual test just used, however, does not work for MI of the object-level kind. The reason is simply that, on this approach, sentences are not the kind of things we believe are indeterminate. Quite the opposite, in fact. According to Wilson (2013), every sentence is perfectly determinate (and this is why there is no need for an indeterminacy operator on that approach), while some of them might express, or indicate an indeterminate state of affairs. Thus, in this context the counterfactual test would be simply meaningless. Perhaps, what is needed is a sort of modified counterfactual test like the following:

> **CT-Modified** — The state of affairs expressed by a sentence S is metaphysically indeterminate iff: were all representational content precisified, there is an admissible precisification of S such that according to that precisification the state of affairs expressed by the sentence would still be non-epistemically indeterminate.

According to the (**CT-Modified**), the precisificational strategy is still available for the semanticist (who can therefore accept the truth of it). Furthermore, the test leaves open the possibility of the resulting indeterminacy being epistemic (so that also the epistemicist would accept it). Nonetheless, this new test also allows for the possibility of an object-level reading of metaphysical indeterminacy. What the test says, is that if the content of the sentence cannot be further precisified, and if it is non-epistemically indeterminate, then it has to be MI.

If we run this new test, however, we find that quantum indeterminacy of the object-level type would not be metaphysical. To see why, we can go back to the ideal language containing one predicate for each precisification. In the quantum mechanical case, the object-level approach could understand those predicates as each corresponding to a degree of instantiation (e.g. '50% spin-up in *x*'). In other words, the idea is that if our language were powerful enough to possess a predicate for each degree, the indeterminacy would eventually disappear. Although this is allegedly the most powerful way of defending Glick's view, I believe there at least two responses, which I will now consider in turn.

First of, we shall notice that the idea of having a predicate corresponding to each degree of instantiation is not as simple as it might first look. Recall that the degrees of instantiation in the quantum mechanical case map the real numbers, so that the language expressing them would need to posses infinite predicates. Secondly, and more importantly, the above strategy relies on an erroneous interpretation of how the object-level approach actually works. In fact, we have been assuming that a predicate like '*x*-spin' behaves precisely like predicates such as ‘bald’, while this is not what the approach tells us about quantum indeterminacy. In the ‘bald’-case, we can construct different predicates corresponding to the possession of certain numbers of hairs. In the case of *spin*, instead, this is not clearly the case. The predicate *spin* has rather to be taken as incomplete, when without the degree of instantiation. In other words, the degree-theoretic approach to QI takes the predicate spin as a three place predicate.

3.8 Conclusions

In this chapter I have focused on the relationship between quantum indeterminacy and the topic of fundamentality. My main goal was to show that a certain objection to QI does not stand scrutiny. Although QI might not fundamental—as for the majority of the live interpretation of the theory —*pace* Glick (2018), it still is a mind-independent phenomenon that escapes our attempts of regimenting it as due merely to our language or knowledge.

CHAPTER 4

The Indeterminate Present and the Open Future

4.1 Introduction

The relationship between contemporary physics and the philosophical debate on the open future are notoriously intricate. On the one hand, the theory of Relativity clearly imposes some constraints on our theorizing about the openness of the future. On the other hand, it is far less obvious what story quantum mechanics (QM) has to tell on this. On top of that, we all know how difficult it is to come up with a satisfying theory that unifies both quantum and relativistic phenomena, a so-called quantum theory of gravity. Thus, as a matter of fact, both philosophers and physicists have looked at some features of QM in order to argue for the objective openness of the future, thereby claiming that Relativity might not have the last word on this issue. My first aim in this chapter is to show that these attempts to argue for openness *via* QM failed because they focussed on *indeterminism*, whereas the right phenomena to look at is the lack of *determinacy* suggested by quantum theory. Some of the main interpretations of QM are *fundamentally* indeterministic—the evolution of quantum systems through time is such that two perfectly indistinguishable systems can evolve differently. Furthermore, such indeterminism is taken as an objective feature, in that it is totally independent from our epistemic limitations. However, plenty of philosophical arguments show that indeterminism fails as a sufficient condition for openness (see e.g. Pooley 2013). After having briefly evaluated these arguments, in this chapter I will suggest a way to provide necessary and sufficient conditions for openness by focusing on metaphysical indeterminacy in QM.

Metaphysical explanations of the genuine openness of the future often appeal to objective indeterminacy. However, according to the received view on this issue, as I will show, such indeterminacy only pertains to *future-tensed* state of affairs that obtain at the present. I will here put forward a different, original view on the relationship between metaphysical indeterminacy and openness of the future, which I shall call the *strong indeterminate present* to distinguish it from the received view, which I dub the *weak indeterminate present*. According to the approach I develop, unsettledness of future contingents is explained by the indeterminacy of certain relevant *present-tensed* state of affairs. In order for an indeterminate present-tensed state of affairs to explain the unsettledness of a future-tensed state of affairs, there has to be a connection between the two. I will argue that this connection can only be provided if we look at the internal structure of the state of affairs. I will then suggest that the best background theories to explain the connection are the so-called *spontaneous collapse models* of Quantum Mechanics, like GRW, which we already briefly met in chapter 1.

In section (2) and (3) I introduce the standard argument for openness based on *indeterminism*, show why it does not go through, and I then analyse the received view on the relationship between openness of the future and indeterminacy in reality, along with the notion of the *weak indeterminate present* (WIP) on which it is based. Section (4) gives the general outline of a theory of openness of the future based on the notion of the *strong indeterminate present* (SIP). In section (5), I focus on those features of the spontaneous collapse models of quantum mechanics that make them the best candidates for a background theory to explain the openness of the future in terms of SIP. Section (6) provides the theoretical framework for using spontaneous collapse models of QM as a theory for SIP.

4.2 Openness, Indeterminism, Indeterminacy

Among our ordinary intuitions about time, the idea of an asymmetry between past and future is perhaps the most pervasive, and yet a very mysterious one. We think at the past as fixed, no longer available to modification, and at the future as open, not yet settled. This pre-theoretical notion of *openness* is supported by a large number of phenomena. For instance, the openness intuition is somehow suggested by our sense of agency and by our feeling that we can influence the future while being impotent with respect to what happened in the past. Furthermore, we tend to think that the structural differences between memory and expectation as different intentional states might be grounded on the openness of the future. The correctness of a memory report is linked to its past target in a way that expectation is not: in order for me to remember something the content of my memory has to represent correctly what happened in the past. Contrariwise, expectation does not require a determinate connection with the future, and expectation claims allows for a non-factive reading: in order for me to expect that *p* will be the case, obviously I do not need to represent the future correctly as containing the truth of *p*. Finally, although philosophers have tried to explain the temporal asymmetry between past and future in terms of the asymmetry between cause and effect (e.g., Reichenbach 1956), it is quite natural to think of the difference between the cause and the effect as grounded in their difference in temporal location: effects are less determinate because they are not yet there, when the cause is (see Miller 2005).

We all seem to agree that the notion of openness is somehow related to these phenomena. Disagreement comes when we try to provide a theoretical explanation of the notion. For instance, one could think that openness can be explained ontologically by certain theories of time. According to presentism (Tallant 2009a, 2009b, 2012, 2014 Tallant & Ingram 2018, Ingram 2016, 2018) and to the growing block view (Correia & Rosenkranz 2019), the future simply is not there, and that is the reason why it is open. Or we can think that openness is explained by there being a multiplicity of options and no fact of the matter at present with respect to which will be the actual one (Belnap et al. 2001). Other views seek an explanation of openness in the metaphysics of passage. If there is genuine becoming, then we can ground the asymmetry between fixity of the past and openness of the future on the directionality of flow. And we can do this within an eternalist framework as well, as with the moving spotlight views (Cameron 2015, Deasy 2015), or with theories according to which the present and future exist, but not the past ones (Casati & Torrengo 2011, Norton 2015).

Finally, and more importantly for our purpose, certain views try to look at the laws of physics for an answer. If laws of physics are fundamentally indeterministic—once again: as it is indeed maintained by certain interpretations of quantum mechanics—then perhaps openness is just a result of the fact that nature does not determine or fix all the future facts (Le Poidevin 1991, 38). This approach in particular has been very common in the literature on the philosophy of quantum mechanics. Many physicists and philosophers in the past decades have argued, in one way or the other, that this theory supports the open future intuition, since some of the quantum mechanical laws (at least on some interpretation) are fundamentally stochastic. However, as argued by many scholars the above line of reasoning is not as straightforward as it might seem, at least for two reasons. First, as argued by Lewis (1986), and Markosian (1995), *indeterministic* laws would not just make the future open, but would make the past open as well, thereby in fact rejecting the asymmetry intuition. Second, and perhaps more importantly, there is also the issue nicely summarized by Oliver Pooley (2013)[49] in the passage below:

> Suppose that the laws of nature are indeterministic in the sense that specification of the world's history up to a certain time, together with those laws, does not fix all future facts. To say that the future is open might only be to say that the future is not nomologically determined in this sense. But that the past and present, together with the laws, do not fix all future facts does not entail that there are no such facts. In tenseless terms, there can be a unique actual continuation of the world to the future of some time *t*, but this continuation need not be the only one compatible with the actual laws and the way the world is up to and including *t*. (p. 322)

The mere fact that indeterministic laws of nature—together with all the facts about what is present and what is past—do not determine a unique future, does not mean that *there is no such future*. Whether or not the laws entail a unique future, from a metaphysical perspective there still remains

[49] See also Torrengo (2013), Borghini & Torrengo (2013), for similar conclusions, though with a focus on the the so-called *thin red line* view.

the problem of the different ontological status of the future with respect to the present and the past.

In order to assess whether QM supports the open future thesis, perhaps a better starting point would be to find what could be a minimal theoretical consensus on the notion of openness, one that is shared by everybody who participates in this debate. If the future is open, and the universe shows some qualitative diversity through time, contingent matters about the future are *unsettled*. Traditionally, this idea is captured by appealing to the notion of *future contingents*, that is future-tensed claims that are neither necessarily true (as "tomorrow either it will rain or it won't" is) nor necessarily false (as "tomorrow both it will rain and it won't" is). A *mark* or symptom of the openness of the future is thus that future contingents lack a (determinate) truth value. I take such a mark to be a crucial *desiderata*, and a sort of adequacy condition for any theory of openness, along the lines expressed by AC below.

> **(AC)** Assuming that there is some qualitative variation across time, *future contingents* are unsettled with respect to their truth value.

In other words, we could assume that in order for a theory of the openness of the future to be adequate, it has to entail that the truth or falsity of propositions like <There will be a protest in front of Wall Street tomorrow> is not at present settled.

But what do I mean by unsettledness here, and what is precisely the link between unsettledness of future contingents and openness of the future? This question is particularly pressing since, after all, one could have a merely epistemic take on unsettledness, and claim that the only reason why future contingents are unsettled is because we just *do not know* yet whether a certain proposition is true or false. However, if our aim is to characterize the idea of the openness of the future as something not *entirely* due to our ignorance, clearly an epistemic take on the notion of unsettledness is uninteresting. If the future is open in a more robust and genuine sense, the unsettledness of future contingents have to be interpreted *metaphysically*, as an objective feature of the external world. That is to say, the *world itself* has to be unsettled with respect to the truth of future-tensed claims. What does an explanation of the temporal asymmetry in terms of a theory of metaphysical unsettledness of future contingents look like? And more generally, what sense, if any, can we make of the notion of *metaphysical unsettledness*?

The best way to think of unsettledness in a metaphysical way is by referring to the notion of *indeterminacy*, which appears to be more well regimented. This is a rather safe assumption, since notice that for those already committed to accepting *metaphysical unsettledness*, metaphysical indeterminacy would do no harm. The use of the notion of metaphysical indeterminacy just is the best way to spell out a prior commitment towards taking unsettledness in a robust way. Barnes & Cameron (2009), for instance, admit that the acceptance of metaphysical indeterminacy in order to defend the open future might be taken as a cost. However, they correctly rejoin that their proposal « is concerned with someone who has already accepted the open future thesis [and is] therefore *already* committed to making sense of robust, non-representational indeterminacy (indeterminacy in how things are, rather than how they are described). In her case, she is committed to worldly indeterminacy concerning the future » (p. 304). Therefore, once we grant this, the openness of the future can be understood as a form of *objective indeterminacy*. More precisely, a natural idea is to think that a certain kind of objective indeterminacy of the future explains why future contingents are *now* unsettled. Thus, the order of explanation goes from an objectively indeterminate future to the unsettledness of future-tensed claim when evaluated at an objectively determinate present. Future contingents are 'oriented' toward the future (their representational aims lie in the future), but are evaluated as unsettled *in the present*. What is unsettled is in the present (the truth value of future contingents, or the result of their evaluation), what is indeterminate is in the future (its openness), and future indeterminacy explains present unsettledness.

Notice, though, that indeterminacy of the future cannot mean that when the future arrives we will find that it is indeterminate. The indeterminacy of the future is *resolved* when the future becomes present. So, it seems *prima facie* contradictory to say that there is indeterminacy *in* the future, since the future is not indeterminate *when it becomes* present. Rather, the indeterminacy is in the future only until it does not arrive—it is from the point of view of the present, so to say, that the future lacks determination. *Per se*, the future is as determinate as the present.

To sum up. The unsettledness of future contingents is the mark of the openness of the future. Such a mark is explained by the indeterminacy of the future, which is however indeterminate only *as of the present*. The question before us is then the following: if the future is indeterminate *only* as of the present (because when the future arrives, it won't be any longer indeterminate), does the openness of the future require also some form of indeterminacy *in the present*?

In what follows I will argue that according to what I call the *received view*, the present is indeterminate only in a *weak* sense, and I will thus dub this view the *Weak Indeterminate Present* (WIP). WIP is a weak form of indeterminacy of the present because no present-tensed states of affairs are indeterminate on this view, nor it is indeterminate which present-tensed state of affairs presently obtain. According to WIP, the openness of the future requires only a weak form of indeterminacy in the present, one concerning only which *future-tensed states of affairs* presently obtain. In the following sections, I will put this view in contrast with the view I call *Strong Indeterminate Present* (SIP), according to which the future is open *because* there is indeterminacy in certain *present-tensed* state of affairs. The connection between the indeterminate states of affairs in the present, and the indeterminacy of the future is given by the structural relations between the states of affairs. Roughly, the present-tensed indeterminate state of affairs are such that they can be "resolved" in more than one way in the future.

To give an example, we could think that the indeterminacy of <There will be a protest in front of Wall Street tomorrow> is grounded on the indeterminacy of some relevant present-tensed states of affairs that obtain at present, such as the uncertainty among the members of the congress relative to the upcoming vote, and the swaying feelings of the protesters. The reason why there is such a connection is that protests in front of Wall Street usually require a certain kind of motivation and attitudes.

In the next section I will provide a more careful analysis of WIP, before moving to my own approach in the rest of the chapter.

4.3 The Locus of Indeterminacy in the Received View

A future contingent is a proposition expressed by a claim about some future contingent matter. If now, in 2019, you utter the future-tensed sentence "There will be a space battle around Terok Nor in four hundred years", you express a future contingent. In the standard analysis, the proposition expressed by your utterance is composed by a kernel present-tense proposition <There is a space battle around Terok Nor>, the future sentential operator *It will be the case that*, and the metric determination (attached to the sentential operator) *in four hundred years*. In other words, by claiming that there will be a space battle around Terok Nor in four hundred years, I express the following proposition.

> SPACE-BATTLE: <*It will be the case* (in four hundred years) *that* there is a space battle around Terok Nor>.

SPACE-BATTLE is clearly, in some sense, *unsettled.* For once, we do not know yet if it a battle around Terok Nor will take place in four hundred years or not. Besides, nobody would think that if SPACE-BATTLE is unsettled, then when four hundred years elapse, it is indeterminate whether a space battle is going on or not around Terok Nor. As already stressed, the future is not unsettled in *this* sense. Indeterminacy *about* the future is not indeterminacy *in* the future, for the indeterminacy will be resolved when the future arrives. Rather, everybody agrees that, if SPACE-BATTLE is unsettled, then it is *now* indeterminate what *will be* the case with respect to space battles around Terok Nor in four hundred years. Openness of the future requires indeterminacy of the future as of the present: the future is indeterminate *as of now*, not *as of then*.

Thus, openness of the future *requires* some form of indeterminacy in the present. However, in the received view, such indeterminacy does not require the present to be *constituted* by any indeterminate state of affairs. We can introduce the notion of constitution as explained below.

> **Constitution** — A state of affairs partially constitutes a time t if and only if it obtains at t and it is exclusively 'about' what is going on at t.

The notion of 'being about' is notoriously a vague one, but for our purposes suffice it to say that *no* future-tensed state of affairs that obtain at the present time is about the present time; they are all about what *will be* the case at future times. Thus, no future tensed state of affairs can constitute the present.

Since according to the received view the present is not constituted by indeterminate state of affairs, the openness of the future does not require indeterminacy in presently obtaining present-tensed state of affairs. Rather, the received view is that there is in the present indeterminacy with respect to future-tensed state of affairs. More precisely, it is at present indeterminate which of two incompatible state of affairs about a contingent course of event will take place. For instance, let us assume that time *t'* comes four hundred years later than time *t*. At *t*, it is indeterminate whether the state of affairs that [It will be the case in four hundred years that there is a space battle around Terok Nor] or the state of affairs that [It will be the case in four hundred years that there is no space battle around Terok Nor] obtains. The indeterminacy, at *t*, as to which state of affairs will obtain, explains why SPACE-BATTLE is in turn unsettled.

Now, assuming that time *t* is present, we can ask whether the indeterminacy as to which of the two state of affairs obtain makes the *present* indeterminate in some robust sense? There are good reasons to answer negatively. In order for the present to be indeterminate in some robust sense, it has to be constituted by indeterminate state of affairs, namely certain *present*-tensed state of affairs have to be indeterminate, or there has to be indeterminacy with respect to which present-tensed state of affairs obtain. Future-tensed state of affairs « point beyond themselves », as Sider (2001) puts it. Whether they obtain or not in the present *depends* on what will be the case later on, and not—not entirely at least—on what is going on in the present. Thus, the indeterminacy concerning which future-tensed state of affairs obtain at *t* in the case of the space battle is not due to indeterminacy in *t*. This suggests that according to the received view the locus of indeterminacy should be in the future of *t*. Yet, the future is indeterminate only insofar as it is still future: it is *at present* indeterminate what *will be* the case.

We can make more rigorous the notion of weak indeterminate present by appealing to two roles times can play in claims concerning the obtaining of state of affairs, which we will refer to through the expressions "at *t*" and "as of *t*", respectively. Intuitively, if a state of affairs obtain at *t* then *t* is the temporal position of its obtainment, and if as of *t** a state of affairs obtain at *t*, *t** contains "all that it takes" for the state of affairs to obtain at *t*. We then take the first notion as a primitive that can be characterised as in (at t) below, while the second notion is explicitly defined in (as of t) below.

> **(at *t*)** If a state of affairs F obtains/fails to obtain at *t*, then the proposition P that expresses F is true/false at *t*. If it is indeterminate whether F obtains at *t*, then P is unsettled at *t*.

> **(as of *t*)** As of *t*, a state of affairs F obtains/fails to obtain/is such that it is indeterminate whether it obtains at *t** if and only if the states of affairs that constitute *t* entail/fail to entail/neither entail nor fail to entail that F obtains at *t**.

If, along with state of affairs talk, we also allow for the notion of a state of affairs' *internal negation*, indeterminacy with respect to the obtainment of F can be construed as being indeterminate which of F and its internal negation not-F obtains. Thus, if it is unsettled whether there will be a space battle around Terok Nor in 400 years, it is undetermined which of the two states of affairs there will be a space battle around Terok Nor in 400 years and there will *not* be a space battle around Terok Nor in 400 years obtains.

A weak indeterminate present (WIP) is a time that is constituted only by determinate states of affairs, but for which it is indeterminate which contingent future-tensed states of affairs obtain, namely which between some future tensed states of affairs and their internal negations obtain. By resorting to metrical tense operators ('it will$_n$ be the case that' to be read as 'it will be the case in *n* units of time that'), we can put it schematically as follows.

> WIP — a time *t* is a WIP iff *t* is present, and as of *t* it is determinate which state of affairs constitute *t*, and for some present tensed proposition φ and positive number *n* it is indeterminate which of the future-tense state of affairs [it will$_n$ be the case that φ] or its internal negation [it will$_n$ be the case that not-φ] obtains at *t*.

The received view explains the openness of the future via the *weak* indeterminate present. If it is unsettled whether there will be a battle around Terok Nor in four hundred years, then as of now, it is indeterminate whether the state of affairs that [there is a space battle around Terok Nor] or the state of affairs that [there is no space battle around Terok Nor] obtains in 2419. But when 2419 comes, it will no longer be indeterminate which state of affairs obtain in 2419. More generally, for any time *t*, some time *t** (with *t** > *t*), some positive number *n*, and every proposition of the form <*It will be the case that* φ> (where φ is present tense, and does not contain hidden references to the future) that is unsettled at *t*:

(i) As of *t,* it is indeterminate which of the two future-tensed state of affairs [it will be the case that φ] and [it will be the case that not-φ] obtains at *t*.
(ii) As of *t*, it is indeterminate whether [φ] obtains or [not-φ] obtains at *t**.
(iii) As of *t**, it is determinate which between the two future-tensed state of affairs [it will be the case that φ] and [it will be the case that not-φ] obtains at *t*.
(iv) As of *t**, it is determinate whether [φ] obtains or [not-φ] obtains at *t**.

We can now pinpoint more precisely the received view as the thesis that the locus of indeterminacy is the future insofar as it is still future. The future is open because there is indeterminacy with respect to what *future*-tensed state of affairs presently obtain, and such indeterminacy in the present is in turn (given that the present is otherwise constituted only by determinate state of affairs) explained by the indeterminacy with respect to which "corresponding" present-tensed state of affairs *will* obtain. More schematically, below.

> **(Received View)** The future is open if and only if for any times *t* and some time *t** (with t*>t), and for some φ, (i) to (iv) holds, and (i) because (ii).

For instance, assume that as of *t*, there is indeterminacy with respect to which future-tensed state of affairs obtains at *t*, namely whether [it will be the case in four hundred years that there is a space battle around Terok Nor] or [it will be the case in four hundred years that there is no space battle around Terok Nor] obtain. The present time, thus, is indeterminate, but only because as of the present there is indeterminacy with respect to what state of affairs constitute future times. But when the future times become present, the indeterminacy will be resolved: there is *no* indeterminacy as of at t^* with respect to which one of the two (mutually exclusive) state of affairs [there is a space battle around Terok Nor] and [there is no space battle around Terok Nor] obtains at t^*. Finally, as we should expect, Received View complies with AC. Claims about future space battles are claims about contingent future matters. Given (as of t), if the contingent matter of a future space battle around Terok Nor in four hundred years is indeterminate—as the Received View entails—then it is at present unsettled whether it will occur.

4.4 *Weak* versus *Strong* Indeterminate Present

Although consistent, the received view is somewhat unstable. It explains the openness of the future in terms of the present being weakly indeterminate and the weak indeterminacy of the present in terms of the indeterminacy of the future *relative to the present*. One may think that this is how things should be: future times are open only until they are future. The locus of the indeterminacy that grounds the openness of the future can be neither in the present *per se*, nor in the future *per se*, but it must lie somewhere in the middle. I do not mean to provide any knock-down argument against such a train of thoughts. However, I believe that once we assume that the aim is to model a metaphysically *robust* idea of openness, the received view is unsatisfactory. Even if there is indeterminacy now with respect to which present-tensed state of affairs will obtain, given that the indeterminacy will resolve, there is a mapping between the propositions that are unsettled as of a time *t*—the future contingents—and certain *corresponding* state of affairs that will be *determinate* when the future arrives. But then, what is it *in reality* that makes the relation between the present and the future the locus of indeterminacy, given that they are both constituted only by determinate state of affairs? It is very tempting to reply that what makes the relation between the present and the future an indeterminate matter is just an epistemic factor. From the point of view of the present we *can't see precisely*, as it were, what the future is like. In other words, one might think that if all the state of affairs are determinate, both those in the present and those in the future, then the weak indeterminacy in the present could be explained away epistemically.

There are several reactions one can have to this criticism of the Received View, assuming it is on the right track. One can insist that if we take on board some further metaphysical assumption, such as the lack of existence of the future (cf. presentism or growing block), or a substantive notion of passage as a shift from the indeterminate to the determinate, the objection is blunted. As I have made clear, though, I am mostly interested in what is *shared* among many views about the openness of the future. Insofar as this rejoinder is based on auxiliary substantive thesis, which someone who accepts the Received View can reject (stick to the two examples above: a B-theorist can accept neither), then I will simply just discard it.

Another reaction is to revise the Received View by inverting the order of explanation and claim that it is indeterminate what the future is like because the present is indeterminate, as in the Received View-revisited below.

Received View-revisited — The future is open if and only if for any times *t* and some time t^* (with $t^*>t$), and for some φ, (i) to (iv) holds, and (ii) because (i).

If things stand as the Received View-revisited has them, the future can be said to be the *locus* of indeterminacy (to be indeterminate with respect to which present-tensed state of affairs obtain) only in a derivative way, as a consequence of being indeterminate which future-tensed state of affairs obtain in the present. An instability very similar to the one pointed out for the Received view shows up in the revised version too: the openness of the future is explained in terms of the present being weakly indeterminate, but the indetermination of the present concern only *future*-tensed state of affairs. Indeed, the suspicion is that it may be historically indeterminate which one of the Received view and the Received view-revisited is the actual received view. But more to the point, insofar as the only notion of indeterminacy we have at our disposal is the WIP, the openness of the future looks very much like a brute fact. *As of itself*, no time is constituted by indeterminate state of affairs, the indeterminacy of the future is only from the point of view of the present, and this relation between the present and the future—as suggested—is more naturally construed in epistemic terms, rather than as a feature of reality, given that both the present and the future, *as of themselves* are fully determined.

In this section, I will explore an alternative in which the indeterminacy of the future is explained in terms of a more robust idea of indeterminacy of the present. The hypothesis is that the present is partly constituted by state of affairs that entail indeterminacy about the future. Those present state of affairs ground the indeterminacy concerning the constitution of future times, and thus also the indeterminacy concerning which future-tensed state of affairs obtains in at the present time.

Let us start by considering what strong indeterminacy in the present consists in. Recall from chapter 1 that in the debate on what is the best way to characterise metaphysical indeterminacy, the main divide is over whether indeterminacy has to be captured at the level of state of affairs—which state of affairs obtains, whether a certain state of affairs or its negation obtains—or rather at the level of objects—as for objects failing to determinately instantiate properties. In the former case, it is sometimes indeterminate that a certain state of affairs obtains, while in the latter it is always determinate whether or not a state of affairs obtains, but sometimes an indeterminate state of affairs (determinately) obtains. In the case of the *weak* indeterminate present, the indeterminacy as to which future state of affairs obtains is clearly of the first type—it is indeterminacy as to which state of affairs obtain. What about the *strong* indeterminate present? Although both options seem viable, I will assume here (for reasons presented in earlier chapters) that the latter approach is preferable. Indeterminacy of presently obtaining present-tensed state of affairs is then to be understood as the obtainment of an indeterminate state of affairs—and *not* as whether a determinate state of affairs or its internal negation obtains.

How are we to characterise an indeterminate state of affairs? Although I will pass over some of the details here, it is useful to provide some characterisation. I will piggyback on Jessica Wilson's (2013) account, already introduced in chapter three, to give an idea:

> **Wilson-style Indeterminate State of Affairs**: a state of affairs is (Wilson-style) indeterminate *iff* a certain object instantiates a determinable property, but more than one determinate of that determinable.

An indeterminate state of affairs, on this view, occurs if the relationship between an object and its determinate and determinable properties is of a certain type. The account explicitly provides an explanation of metaphysical indeterminacy by moving at the second-order. Although other features of the account are not relevant for the present discussion, this certainly is. In order to have an indeterminate state of affairs occurring at the present (instead of having indeterminacy as to which state of affairs occurs), we need to look at the structure of these state of affairs—namely, we need to move to the second-order in some way. We can now provide a characterisation of the *strong* indeterminate present (SIP).

SIP — a time t is a SIP *iff* iff t is present and t is constituted by at least one indeterminate state of affairs.

In order for SIP to provide an explanation of the unsettledness of future-tensed propositions, it should comply with the following adequacy condition.

(AC-SIP): for any times t and some time t^* (with $t^*>t$) and some φ, if t is a SIP, then as of t, it is indeterminate whether [φ] obtains or [not-φ] obtains at t^*, and it is so because of some indeterminate state of affairs that constitutes t.

Why should one think that robust metaphysical indeterminacy in the present can explain the indeterminacy that characterise the future? Think again of the example of the protest that I provided in the fist section. It is because the members of the Congress and the protesters have *indeterminate mental contents* that it is still open whether there will be a protest tomorrow or not. To simplify the example, consider Senator Bambo. At noon, he is still utterly undecided whether he will vote yes or no in the 18:00 voting. As of noon, it is thus undetermined whether at 18:00 he will vote yes or no. The relevant state of affairs about Senator Bambo that constitute noon here can be thought of as composed by a determinable (being a mental state, or something like that) and more than one determinate of that determinate (intend to vote yes / intend to vote no). Those states of affairs ground the present indeterminacy of the 18:00 voting with respect to Bambo's behavior. Of course, the example does not cut much metaphysical ice, since the kind of indeterminacy at issue here is clearly mere representational indeterminacy (we do not need to posit an indeterminate reality in order to have indeterminate thoughts). What matters here is to give an idea of the structural connection between the indeterminate states of affairs that constitute the present and the state of affairs with respect to which the future is indeterminate.

In general, we can say that state of affairs have as constituents individuals (s_1, $s_{2, \ldots}$) exemplifying properties. Those properties are determinable properties (D_1, D_2, …) , and each determinable property has its own corresponding pool of determinate properties ($A_{D,}$ $B_{D, \ldots}$). In order to allow for indeterminate state of affairs as characterized above, we can introduce also complex properties, whose constituents are other properties. A complex property constituted by properties A and B, for instance, will be labelled A/B. Importantly, we also need to allow that some complex properties are composed by determinates of the same determinable. For instance, $A_{D/}B_D$ is a complex property composed by two determinate properties of the determinable D. In line with Jessica Wilson's idea introduced before, we can say that when a state of affairs exemplify a determinable property D and some such complex property constituted only by D's determinate property is an indeterminate state of affairs. What interests us is to express the relation between a present state of affairs [s, D, $A_{D/}B_D$] that is constituted by a system *s*, a determinable D, and some complex property $A_{D/}B_D$ and the state of affairs [s, D, A_D] and [s, D, A_D] concerning the same system *s* and determinable D, but only one of the determinate property A_D or B_D. Such a relation can be expressed by the following two clauses. For any times t and some time t* (with t*>t) and some individual s, determinable D, and some of D's determinate A_D and B_D:

(1) As of *t,* an indeterminate state of affairs [s, D, A_D/B_D] obtain at *t*.
(2) As of *t*, it is indeterminate whether [s, D, A_D] obtains or [s, D, B_D] obtains at *t**.
(3) As of *t**, it is determinate which between the two future tensed state of affairs [it $will_n$ be the case that [s, D, A_D]] and it $will_n$ be the case that [s, D, B_D]] obtains at *t*.
(4) As of *t**, it is determinate whether [s, D, A_D] obtains or [s, D, B_D] obtains at *t**.

According to what I call the SIP-Openness view, the indeterminacy with respect to what state of affairs constitute the future is explained in terms of a strong indeterminate present.

(SIP-Openness) The future is open if and only if for any time *t* and some time *t** (with *t*>t*) and some individual s, determinable D, and some of D's determinate A_D and B_D: (1) and (2) holds, and (2) because (1).

SIP-Openness compiles with AC-SIP. It is also easy to see how it can be used to account for the unsettledness of future contingents and so how it complies with AC too, once we accept the following plausible principle Link.

(Link) If as of a time *t* there is indeterminacy with respect to which present-tensed state of affairs F_1, F_2, … obtain at a future time *t**, then as of *t* there is indeterminacy with respect to which future-tensed versions (it will be the case that F_1, it will be the case F_2, etc.) of the state of affairs F_1, F_2, … obtain at *t*.

Given Link, SIP-Openness entails that if a time is a SIP, then it is also a WIP (as we should expect). Given (at *t*), SIP-Openness entails the unsettledness of the future contingents. Note, however, that by appealing to SIP-Openness rather than the Received view (or the Received view-revisited), we can bottom out the explanation of the unsettledness of the future contingent in a radical form of metaphysical indeterminacy. In so far as the instability of the received view with respect to the issue of the locus of the indeterminacy, and the suspicion of a hidden form of epistemic indeterminacy are to be avoided, SIP-Openness is preferable.

SIP-Openness tells us something about the internal structure of present indeterminate state of affairs, and their connection with the structure of the state of affairs for which it is indeterminate whether they will obtain or not. Of course, SIP-Openness does not tell us *why* indeterminate state of affairs in the present can explain indeterminacy *as of now* of what state of affairs *will* obtain, it just gives us an idea of the structural connection between those state of affairs that is required by such an explanation. To answer this question we need to put more metaphysical flesh on the bones of SIP-Openness, by introducing a background theory to tell us what s, D, A_D, B_D, and $A_{D/}B_D$ are.

In the next section, I will introduce the basics of the *spontaneous collapse* approach to quantum mechanics, which will be then used, in section (6), as a background theory for SIP-Openness.

4.5 Objective Collapse Interpretation of QM

The *Spontaneous Collapse Models* are a family of interpretations of Quantum Mechanics that were first introduced by Gian Carlo Ghirardi, Alberto Rimini, and Tullio Weber (GRW; 1986), building upon suggestions made by Pearl (1976), and Gisin (1983). As with all other interpretations of QM, such as the Bohmian (1952), or the Everettian (1956), the main reason why GRW was first formulated was to address the *measurement problem* of quantum mechanics. Roughly, this problem comes from the assumption that the standard (uninterpreted) formalism of QM is *universally valid*. A crucial feature of this formalism is that the temporal evolution of quantum systems is governed by the Schrödinger's equation, which, among other features, is deterministic, and linear. The linearity of this equation allows the formalism to describe states of *superposition*. These states are practically impossible to visualize, and yet very easy to describe mathematically. To give an example, you can think of an electron *e*, and one of its properties (observables); for instance, its spin component along a certain axis. This property can be instantiated by the electron only in two ways: it can be either spin-up, or spin-down. When this happens, we say that the electron is in an eigenstate of the relevant observable (property), and we write *e* = |up> or *e* = |down>, respectively. Now, the linearity of the Schrödinger's equation implies that, for every two solutions to the equation, the linear combination of them (their sum) is also a solution. Thus, we do not only have eigenstates, but also states of superposition of having, in the above example, both spin-up *and* spin-down: *e* = |up> + |down>. These states are pervasive in QM, given the existence of *incompatible observables*—spin components in mutually orthogonal directions are a famous example. Whenever a quantum system is an eigenstate of a certain observable, it will always be in a superposition of the observable that is incompatible with the one in the eigenstate. The *measurement problem* emerges from the simple consideration that, apparently, macroscopic objects are never in superposed states (cats are never both alive and dead at the same time). And therefore, as nicely summarized by Bell, « either the wavefunction, as given by the Schrödinger equation, is not everything, or it is not right » (1987).

The idea of the collapse of the wavefunction would solve the measurement problem simply by *breaking* the Schrödinger's dynamics. According to von Neumann's (1932) *collapse postulate*, the dynamics breaks whenever a measurement is performed. Therefore, there are two different physical processes according to von Neumann: one is linear, deterministic, and described by the Schrödinger equation; and the other one is stochastic, and it is described by the collapse postulate. Almost everyone agrees today that this proposal is untenable, for it does not explain what a measurement is supposed to be, and for it is not clear why measurement should play such a crucial role.

In order to address these issues, GRW proposes a realistic collapse model that is very close in spirit to von Neumann's proposal. However, instead of occurring through measurement, in GRW collapses occur *spontaneously* and are governed by a new law. This law is completely different from Schrödinger's equation: it is *not* linear, it is *not* deterministic, and it is *not* time reversal invariant. The collapse is explained by the assumption that every particles has a certain probability per unit time of undergoing a *hit*, and every hit makes the particle jump to a definite state. The wavefunction that describes the particle after a hit gets multiplied by a narrow Gaussian function stretched to infinity in both sides. The exact point on which the hit is localized within the Gaussian is determined randomly. However, given that we want to recover the statistics of standard QM, the probability distribution (within the Gaussian) is given by the square of the wavefunction amplitude *before* the hit, exactly as for the Born rule.

Another crucial feature of objective collapse interpretations of QM is worth mentioning. It is usually assumed that collapses occurs in a preferred basis—in GRW, for instance, it is position. The requirement of a preferred basis is necessary insofar as one wants to take collapses as real physical processes. The choice of position is usually justified by the fact that macroscopic superposition states seem the least acceptable.

GRW model and the like seem a promising way to go in order to solve the main conceptual difficulties of QM. However, it also has some drawbacks. For instance, it has been argued that the introduction of a new dynamical law is *ad hoc*, or that the model has certain problems with dimensionality, and with locality (see Lewis 1997, 2003, 2006, 2013, 2016). It is not my aim here to address these issues, but there is one problem of GRW that is crucial for my arguments, so I will spend few words on it before moving to the next section.

Recall that, after collapse, the amplitude is not localized in a finite region, because the Gaussian function is stretched infinitely in all sides. This means that in GRW we never have collapses to position eigenstates, but only *near* eigenstates. This issue is known in the literature as the *tails problem*. If we require that a quantum object possess a determinate property if and only if it is in an eigenstate of the relevant observables (as with the Eigenstate-Eigenvalue link introduced in chapter 1), it is easy to see how from GRW it would follow that objects *never* possess definite properties. In order to respond to this problem, proponents of GRW have tried to somehow relax EEL (Albert & Loewer 1996; Lewis 1995). For instance, one can admit that objects posses definite properties also when the relevant observable is *near* an eigenstate. This proposal is called the *fuzzy link* (Albert & Loewer, 1996; Clifton & Monton, 1999):

> **Fuzzy Link —** A system has a determinate value for a given property *iff* the squared projection of its state onto an eigenstate of the corresponding operator is greater than 1 - *P*, where the determinate value is the eigenvalue for that eigenstate.

P is just a parameter that can be chosen as expressing what counts as *definite*. The bigger it is, the less the state needs to be close to eigenness in order to count as possessing the property. Notice that the *fuzzy link* is not fuzzy as to whether or not a property is instantiated, rather, it is explicitly fuzzy as to what is the border (the choice of *P*).

Or, instead of the *fuzzy link*, one can say that objects instantiate properties *vaguely*, and with degrees that corresponds to the relevant probability. This proposal has been recently put forward by Peter Lewis (2016):

> **Vague Link** — A system has a determinate value for a given property *to the extent* that the squared projection of its state onto an eigenstate of the corresponding operator is close to I, where the determinate value is the eigenvalue for that eigenstate.

Here instantiation of properties does not come as *all or nothing*. For instance, my laptop instantiates the property of being on my desk now only to a certain degree, that can be calculated by considering the probabilities given by every Gaussian function associated with all the laptop's and desk's particles. And of course, whatever this probability is, it is enough close to 1 to allow us to say, *for all practical purposes* (FAPP), that the laptop is definitely on the desk.

Each of the above proposals has drawbacks. As for the *fuzzy link*, what is allegedly the main problem is that it implies that properties sometimes behave as if they are not instantiated as the link says. For instance, according to the *fuzzy link,* an object possessing the property of being on my desk can sometime behave as if it is on my carpet. Although this would happen with a very small probability, and *for all practical purposes* (FAPP) we can avoid countenancing this possibility, from a conceptual point of view this is highly problematic. The point is that it would imply the falsity of those theories according to which properties are individuated only by means of their dispositions. As for the *vague link*, it might seem odd to many to posit such a pervasive vagueness in the physical world. After all, from the *vague link* it follows that we are always wrong in attributing determinate properties to objects.

The *tails problem* is not only particularly pressing for anyone who attempts to interpret spontaneous collapse models as entailing metaphysical indeterminacy. It becomes even more pressing for those, like myself, who wish to provide a theory of openness based on such an interpretation of QM. In his recent book, Craig Callender (2017) seems to recognize this problem very well, although he does not talk directly about quantum indeterminacy. While in the business of rejecting the various attempts of arguing for the open future based on QM, Callender considers the possibility to argue that the *superposition/eigenstate* distinction maps directly the *openness-of-future/fixity-of-the-past* one. He then notices the following:

> […] the symmetry of Hilbert space implies that we can write out our wavefunction in any of an indefinite number of bases, e.g., position, momentum, spin. A wavefunction that is a superposition in one basis may not be a superposition in another; for instance, the wavefunction of x-spin down is a superposition of up and down spins in the z-spin direction. Here a collapse to fixity in x-spin buys openness in z-spin. (2017, p. 95)

As he himself recognizes though, to this objection one can respond that in theories such as GRW, collapses always occurs in a preferred basis—as I said above, in the *position* one—and that we should only care about position superpositions states and eigenstates when we speak about the asymmetry between fixed past and open future. However, Callender immediately rejoins a second objection by reminding us that « in any realistic collapse theory such as GRW one doesn't get collapses onto eigenstates, but only near eigenstates » (2017, p. 95). Hence, even granting, by focusing only on position states, that there is a mapping between *superposition/eigenstate* and *open/fixed*, in GRW collapses are never to *eigenstates*, but only *near* ones. And without *eigenstates*, the thought goes, we will *not* end up having openness properly. This latter objection makes clear why the *tails problem* is particularly pressing.

Notice however, that Callender here seems to ignore the possibility of solving the *tails problem*. Although he might well be right that no solution to this problem is satisfactory, for dialectical reasons we need to say a lot more about why this is so. As far as I am concerned here, if the *tails problem* has no solution, then together with my proposal, the whole project of the spontaneous collapse models will not be a viable option. Therefore, I am perfectly happy to conditionalise the rest of what I say on the possibility that the *tails problem* can be solved.

4.6 The Strong Indeterminate Present in QM

In order for the SIP-Openness model to be true in our world, we need the underlying theory to meet two conditions. First, we need the theory to possess the resources to distinguish between determinate and indeterminate state of affairs, roughly in the way described in section (3) above, and to entail the existence of the latter. Second, the theory we are looking at has to provide instances of the schema given by SIP-Openness. In this last section, I argue that these conditions can be met if we interpret spontaneous collapse models of quantum mechanics as entailing quantum indeterminacy.

First though, let us recap some basics notions about quantum indeterminacy and the *lack of value definiteness* (LVD). We shall recall that LVD is largely independent of how we interpret quantum mechanics, although different interpretations will account for it differently. Let us consider once again the simple example introduced in chapter 1. If we know, say, that the electron *e* has spin-down on the *x*-axis (suppose because we measured it), we thereby also know that its spin on the other two axes cannot in principle be assigned. The then have the following states:

$$e = |\downarrow x> \quad [1]$$

$$e = |\downarrow x> = 1/\sqrt{2}|\downarrow z> + 1/\sqrt{2}|\uparrow z> \quad [2]$$

$$e = |\downarrow x> = 1/\sqrt{2}|\downarrow y> + 1/\sqrt{2}|\uparrow y> \quad [3]$$

Where [1] means that the electron is in an *eigenstate* of having the value 'down' for the observable *x*-spin, and [2] and [3] express the empirical fact that being in an *eigenstate* of the observable *x*-spin implies being in a *superposition* of the observables that are incompatible with *x*-spin, namely *y*-spin and *z*-spin respectively. Since to every eigenstate there are some corresponding superposition states, LVD in quantum mechanics is pervasive. More generally, let us recall the so-called Eigenstate-Eigenvalue Link (EEL), already introduced in chapter 1:

(**EEL**) A quantum system *s* has a definite value *v* for the observable *O iff* it is in an eigenstate of *O* having eigenvalue *v*.

From EEL, we can derive that systems that *are not* in eigenstate of having value *v* for the corresponding properties, *do not possess* a definite value for those properties. This is, in a nutshell, LVD in quantum mechanics. And the interpretative issues that behind EEL and LVD are the main reasons for proposing an account of quantum indeterminacy in the first place.

Now using the Calosi & Wilson (2018) account of QI, through the EEL, we derive that:

1. *e* has the definite value 'down' for the property '*x*-spin';
2. *e* does not have a definite value for the properties '*y*-spin' and '*z*-spin'.

Being instances of LVD, Calosi & Wilson (2018) suggest that we read the states corresponding to (2) as indeterminate SOAs. Take, for example, the state corresponding to '*y*-spin'. We have an individual, the electron *e*, a determinable, the observable corresponding to '*y*-spin', and the determinate of that determinable, '*down*' and '*up*'. Thus, at the time at which the state occurs, call it *t*, we have the following indeterminate SOA:

$$[e, \text{`}y\text{-spin'}, \text{down}_{\text{'}y\text{-spin'}}/\text{up}_{\text{'}y\text{-spin'}}] \quad [3]$$

This can be easily generalized. Take a system *s* and one of its observables *O*, with distinct eigenstates $|\psi>$ and $|\varphi>$. Any linear combination $|\omega> = a|\psi> + b|\varphi>$ is a superposition. The resulting quantum state $|\omega> = a|\psi> + b|\varphi>$, obtaining at a certain time *t*, is then to be interpreted as $[s, O, \psi O/\varphi O]$.

Another major interpretative issue in quantum mechanics is that we do not directly experience superposition states like the above. Whenever we perform measurements, the system we look at picks one of the superposed terms. In other words, after a measurement on *y*-spin, from [3] we get to one of the following:

[*e*, '*y*-spin', $\text{down}_{\text{'}y\text{-spin'}}$] [4]

[*e*, '*y*-spin', $\text{up}_{\text{'}y\text{-spin'}}$] [5]

Explaining why and how this happens is, in large part, the business of providing an *interpretation* to quantum mechanics. The orthodox view, first formulated by von Neumann in 1932, is that measurements induce the wavefunction that describes the dynamical evolution of quantum systems to *collapse* into eigenstates, thereby eliminating superpositions. Of course, it is not my aim here to discuss which approach to quantum mechanics is to be preferred. Rather, I want to point out that if (a) we interpret LVD as Calosi & Wilson (2018) suggest, and if (b) we assume that the *collapse* of the wavefunction can be described satisfactorily, then quantum mechanics might be seen as a candidate underlying theory for a model like SIP-Openness. As regards to (b), as I said in the previous section, it is no secret that von Neumann's view on this issue has been criticized on many respects. Above all, the worry is that the notion of *measurement* is not well defined in theory. However, *spontaneous collapse* QM was introduced mainly in order to overcome such difficulties, and yet agreeing with von Neumann's view about the idea that the linearity of dynamics has to supplemented with a non-linear component.

So let us see how in details how *spontaneous collapse models* can be used to model SIP-Openness. In the case of [4] and [5], the two clauses introduced in section (4) are met as follows.

1*) As of *t*, an indeterminate state of affairs [*e*, ‘*y*-spin’, down$_{\text{‘}y\text{-spin’}}$/up$_{\text{‘}y\text{-spin’}}$] obtains at *t*.

2*) As of *t*, it is indeterminate whether [*e*, ‘*y*-spin’, down$_{\text{‘}y\text{-spin’}}$] obtains or [*e*, ‘*y*-spin’, up$_{\text{‘}y\text{-spin’}}$] at *t**.

Suppose a measurement occurs at *t** and instantaneously collapses the wavefunction into one of the superposed terms. This means that, as of time *t* before the collapse, an indeterminate state of affairs obtains (the superposition), and it is indeterminate which term of the superposition will obtain at *t** after the collapse.

Recall now, that SIP-Openness is meant to provide an explanatory schema. It is not enough, then, that the two clauses are satisfied—we also need a motivation for believing that (2*) happens because (1*) does. My suggestion is that the notion of collapse does indeed play the required explanatory role. What is crucial here, is that collapse is an intrinsically temporal notion—we cannot make sense of collapse without distinguishing between the time before its occurrence, and the time afterwards. This idea is not a radically new one. Many physicists seem sympathetic with the view we are defending here, according to which the collapse mechanism is apt to explain openness (Lucas 1999, Christian 2001, Gisin manuscript). In fact, part of my goal in this chapter is precisely to clarify the metaphysics behind these intuitions. Here is, for instance, Lucas:

> There is a worldwide tide of actualization—collapse into eigenness—constituting a preferred foliation by hyperplanes (not necessarily flat) of co-presentness sweeping through the universe—a tide which determines an absolute present […] Quantum mechanics […] not only insists on the arrow being kept in time, but distinguishes a present as the boundary between an alterable future and an unalterable past (1999, 10).

And here is Nicholas Gisin:

> Admittedly, time is a complex notion, or series of notions with many facets, time may be relative, difficult to grasp, etc. But time exists. Moreover, time passes. With spontaneous collapse theories, time exists and passes, the world out there exists and undergoes a stochastic evolution (manuscript, p. 7).

If we take the temporal component of collapse at face value, we shall conclude that what the notion does is to explain how physical systems change through time. It is natural, then, to take instances of (2*) as explained (at least in part) by instances of (1*): it is the fact that there is a superposition that explains why a collapse will occur.

Conclusions

A different metaphysical explanation of the openness of the future can be given by considering what I call the *strong indeterminate present*. I have given a precise characterization of how this model works, and how it explains the openness of the future. Finally, by looking at quantum mechanics, I have provided reasons to believe that this model might apply to our own world, if a certain interpretation of QM is the correct one. Standard metaphysical explanations of openness are usually silent about the physical description of the underlying phenomena. Openness understood in terms of the collapse of the wavefunction provides a radically new, naturalistic account of openness. This, I believe, is an interesting result *per se*. Whether or not a similar model is true of our world, crucially will depend on whether or not the underlying physical theory (upon which we conditionalise) is itself true or not.

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