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THE METAPHYSICS OF SPIN

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Contents

Chapter 1
Introduction

Non-relativist quantum mechanics is one of our current best scientific theories. Although the theory was formulated almost a century ago, there is no consensus on what it tells us about the phenomena it describes. The common lore is that the standard formulation of the theory cannot be understood in realist terms: the theory is a mathematical recipe trustworthy for predicting with high accuracy experimental results but devoid of any metaphysical meaning. As a consequence, physicists tried to modify its formalism to propose realist alternatives to standard quantum mechanics. The reason non-relativistic quantum mechanics have been condemned to an instrumentalist reading has been the apparent impossibility of explaining the phenomena it describes in realist terms. An introductory example is spin along different directions. Spin is supposed to be an intrinsic property of quantum systems. Furthermore, spin can be measured along different spatial axes by letting the system passes through an inhomogeneous magnetic field. We learned from classical mechanics and our everyday experience that measuring a property of some entity means discovering the precise value of that property. Measuring, say, the height of a statue reveals how distant the statue's base is from its top. In the classical world, measurements are compatible. If, say, I measure the statue's height and then its mass, I do not bother measuring the former again. Indeed, I am one hundred per cent sure that its height is the same as before. The reason is measurements reveal the value of the measured properties which is independent of everything else. This independence guarantees that measuring the mass of the statue does not affect its height.

Alas, the contrary is exactly what happens with (some) quantum properties. Suppose I firstly measure the spin along, say, the x-axis, and, secondly, along the z-axis. After the second measurement, there is no guarantee that the system has the same spin value along the x-axis I measured at first. The mathematical reason for that is that a definite state of spin along an axis is a superposition, i.e. a linear combination, of the spin values on the other axes. After a spin measurement along an axis, a quantum system has a definite spin value along that axis. Under the idea that measurements reveal the properties of the systems, many thought that having a definite value of spin along an axis has to be understood in terms of the system having a definite spin property along that axis. But what could be told about spin along different axes before measuring them? At first, that spin is represented as a superposition of definite values suggested that quantum systems only partially instantiated the properties corresponding to these values. That is, the straightforward explanation of spin superposition consists in supposing that these properties are, before measurements, indefinite. However, several formal theorems proved that such indeterminacy does not concern what we know of these systems or how we represent them. That is, if one tries to 'cheat' by supposing that these properties have definite value before measurements, then one ends up with conclusions in tension with experimental, well-confirmed results. The fact that quantum mechanics suggested this kind of worldly indeterminacy leads many to accept it as a mathematical recipe and avoid bothering metaphysical questions. Rather than trying to make sense of the world depicted by the theory, many embraced a 'shut up and calculate' stance toward it.

Interestingly enough, the idea that indeterminacy might be a feature of reality itself has been considered for ages as incoherent even among philosophers. However, several philosophers recently reconsidered such a possibility. The taboo of an indeterminate reality started to be slowly accepted as a genuine possibility between metaphysicians, who started to propose different models of metaphysical indeterminacy. A natural application of them has been quantum mechanics. Such application is natural because quantum mechanics, on the one hand, seemed an incoherent theory if understood in realist terms. Metaphysical indeterminacy, on the other, needed a genuine example to convince those that considered it a taboo that they were wrong. As Darby (2010, p. 228) more elegantly puts it: "With a metaphysical thesis crying out for an indisputably worldly example, and some physics in apparent need of metaphysical clarification, making a connection promises mutual benefits."

Since I was a master student, I have been fascinated by the question 'How the world is, if quantum mechanics were true?". As such, it has been natural for me to carry on my research on this topic during my PhD. Yet, I have always been concerned about my research's foundational aspect. Despite it being one of the main branches of contemporary analytic philosophy, metaphysics is considered suspicious by many non-metaphysicians. How can we obtain metaphysical knowledge if there is no empirical assessment of metaphysical theses? The history of science taught us that the a posteriori method of empirical disciplines is reliable, whereas the a priori method of 'armchair' metaphysics is not.

Consequently, I have been worried to give meaning to the research I carry out by justifying that, by doing metaphysics, I aspire to understand something about the concrete world in which we live, rather than proposing abstract models with no apparent connections to it. The thesis is a reasoned collection of part of my work on these topics. It is divided into two parts. The first one is a foundational work in meta-metaphysics, the branch of philosophy that investigates whether and how metaphysical knowledge can be obtained. The second part is a concrete example of how metaphysics should be practised, according to the meta-metaphysical view I defend in the first. In particular, the second part aims to investigate whether spin superposition offers a genuine case of metaphysical indeterminacy. The thesis is thus structured.

In chapter §2, I present three meta-metaphysical approaches that are widely shared by contemporary analytic metaphysicians. These three approaches differ greatly in their details, but they share the following three theses: (i) metaphysical enquiry is conducted a priori, (ii) metaphysics is more fundamental than empirical sciences (either epistemically or ontologically), and, as a consequence, (iii) metaphysics is independent of other branches of knowledge. I critically discuss the values and the drawback of these three views before presenting a fourth approach to metaphysics, often called *naturalised* metaphysics that has increasingly gained popularity in the last years. The core of this approach is that metaphysics can aspire to give us knowledge about reality only if it is motivated and informed by our current best scientific theories. I. discuss several ways of understanding naturalised metaphysics in order to present my own understanding of it. In particular, I argue that naturalised metaphysics has, on the one hand, to retain the positive virtues of the non-naturalised approaches above and, on the other hand, to inherit methodological principles from empirical sciences. In particular, naturalised metaphysicians have to accept that their investigations are limited to the phenomena investigated rather than concerning maximally general structures of reality. General results have to be shown, starting from the analysis of simple cases. Finally, I argue that naturalised metaphysics is in tension with one of its main assumptions: scientific realism. The latter is the view according to which our current best science gives us a true description of reality. However, two 'arm chair' metaphysical theses are part and parcel of standard formulations of scientific realism: metaphysical realism and the correspondence theory of truth. Given the naturalised metaphysics' dictum that metaphysics is sound only if it is motivated by our best science, one should conclude that this thesis is not worthy of a serious commitment. Alas, scientific realism is the view that motivates naturalised metaphysics in the first place: doing metaphysics from our best science gives us knowledge of reality only if these theories are actually reliable sources of knowledge about the world. I argue that there are two ways of solving the tension: either weakening the naturalised metaphysics' dictum or providing a form of scientific realism devoid of the problematic theses above.

In chapter §3, I address the tension between naturalised metaphysics and scientific realism outlined at the end of the first chapter by taking the second horn of the dilemma. In particular, I propose a form of scientific realism that rejects metaphysical realism without ending in antirealism. To do so, I metaphorically distil the minimal claim that scientific realists share, and I argue that such a claim is independent of metaphysical realism. Then, I outline a form of scientific realism according to which metaphysics and science are part and parcel of the same effort of understanding reality.[1] They are, collectively, our best chance of providing a true model of how the mind-independent reality is. However, given their epistemic capabilities, whether our theories really 'carve Nature at its joint' is something that human beings will not be able to discover. This chapter ends the first foundational part of the thesis.

In chapter §4, I motivate a particular approach to the metaphysics of quantum me-

[1] An earlier presentation of the view has been published in (Corti, 2020).

chanics as well as my choice of focussing on spin. I explain why the metaphysics of quantum mechanics is underdetermined, and I analyse possible ways of addressing such underdetermination. In particular, I argue that adopting an interpretation-neutral approach to the metaphysics of quantum theory is a way out of the underdetermination and a reasonable route for investigating the metaphysics of quantum mechanics. Furthermore, I argue that spin is a central concept in current science. On the one hand, from a theoretical point of view, spin is necessary for the formulations of theories more fundamental than quantum mechanics and one of the properties that distinguish different particles according to the Standard model. On the other hand, spin is fundamental in explaining phenomena that belong to empirical sciences other than physics – such as chemistry and biology –, and it has uncountable technological applications. After briefly presenting the formal way spin is modelled in quantum mechanics, I engage with a recent debate concerning the reality of spin. Indeed, it has been argued that there are not sufficient epistemic reasons to consider spin a real property of quantum systems. As such, scientific realists must not be committed to the reality of spin. I show that the arguments provided for these conclusions are unsatisfactory and that a commitment to spin is due, given its explanatory power, manipulability and theoretical relevance in contemporary physics.
In chapter §5, I present a critical and original survey of the debate about quantum indeterminacy. I start by analysing the assumptions necessary for arguing that quantum mechanics offers a genuine case of metaphysical indeterminacy, and I briefly show that almost all of them are controversial. Following Wilson (2011), I divide the accounts of metaphysical indeterminacy into two families: meta-level and object-level. Then, I present an in-deep summary of the most influential accounts of each family, and I show how they are supposed to explain quantum indeterminacy. Finally, I propose an argument alternative to the one already presented in the literature by Darby (2010) and Skow (2010), that shows that the champion of the meta-level approaches fails to account for quantum indeterminacy.

In chapter §6, I explain how the failure of the account above has been taken by many as a challenge to develop meta-level accounts of quantum indeterminacy. By focussing on one of them, I argue[2] that meta-level approaches are unable to account for quantum indeterminacy. The reason is that the core of this approach is that of understanding indeterminacy in terms of 'worldly indecision' between determinate states of affairs. However, this is in tension with one of the main principles of quantum mechanics, viz., the eigenstate-eigenvalue link. According to this principle, the mathematical and the metaphysical description of quantum systems are tightly connected so that propositions expressing them are equivalent. By considering propositions with a metaphysical content as indeterminate, supporters of meta-level accounts must either accept that also propositions with mathematical content have indeterminate truth values or reject the eigenstate-eigenvalue link. However, both the horns of the dilemma are problematic for supporters of metaphysical indeterminacy, insofar as accepting one of them entail renouncing to one of the assumptions necessary for arguing that quantum mechanics offers a genuine example of metaphysical

[2] An earlier presentation of the argument against Darby and Pickup's view has been published in (Corti, 2021).

indeterminacy. Finally, I discuss several objections to the argument I present, and I conclude that its conclusions are solid, thus reaching the negative result of how quantum indeterminacy must not be understood.

In chapter §7, I investigate spin in terms of the determinable-determinate relation. This relation is a metaphysical relation that explains how general properties are connected to more specific ones. Investigating spin in terms of the determinable-determinate relation has been started by Wolff (2015), and it is now part and parcel of object-level approaches to metaphysical indeterminacy (Bokulich, 2014; Calosi and Wilson, 2018). After providing an introduction to the determinable-determinate relation, I argue that the eigenstate-eigenvalue link can be naturally interpreted to distinguish between determinable and determinate properties. Furthermore, I propose how these properties can be individuated in the formalism of quantum mechanics. Then, I put forwards several models of spin in terms of the determinable-determinate relation by grouping them in two main families: *Spin Determinable Monism* and *Spin Determinable Pluralism*. By considering the recent literature, I argue that there is no evidence about what family is endorsed by most philosophers and thus be considered the standard. However, I show that considering 'having spin along the x-axis', 'having spin along the $z-$axis', and so forth for each possible axis, as determinable properties is widely shared across physics, philosophy of physics and metaphysics of quantum mechanics. I then present two arguments whose conclusion is that accounts that belong to the *Spin Determinable Monism* family have to be preferred over their rivals. Finally, I present a new account[3] of spin in terms of determinable-determinate relations, according to which a quantum system's spin is always determinate even before measurement. I argue that such a view inherit the spirit of an orthodox understanding of quantum mechanics and, at the same time, it gives us a realist understanding of the theory. I show that such an account can explain what happens during spin measurements along different axes and that it has several advantages over other accounts of spin superposition.

Conclusions end the thesis by outlining possible future research that the thesis started.

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[3] A more sketchy taxonomy, and the account presented in §7, have been published in Corti and Sanchioni (2021).

Part I
The Scientific Foundations of Metaphysics

Chapter 2
Metaphysics and Science

2.1 Metaphysics is dead!

The branch of philosophy in which my thesis sits is 'metaphysics.' However, such a claim is not informative insofar as 'doing metaphysics' is not univocal, since philosophers mean different things with it. Such an ambiguity, however, does not lie in the word 'metaphysics.' Since Aristotle indeed, 'metaphysics' is considered the branch of knowledge that deals with structures that cannot be empirically observed and yet, they are responsible for nature's existence as it is. These structures involve some of the most general concepts of our thought, whose generality lies in the fact that they are – either explicitly or implicitly – employed in every branch of knowledge (as well as in everyday life).[1] All of this is relatively standard and well accepted. What is controversial with the term 'metaphysics' is the answer implicitly assumed to its main meta-metaphysical question: 'how can we obtain any knowledge of these empirically opaque structures?' Such a question is connected to many other relevant issues, such as the proper methodologies for conducting metaphysical inquiries, its relationship with other sciences, etc., that are highly controversial. The controversial part of metaphysics does not concern what such a discipline is but how we should practise it.
In the introduction to the second edition of the 'Critique of Pure Reason,' Kant famously wrote some memorable passages concerning the legitimacy of metaphysics. Since the writing of Kant is instrumental in giving a quick depiction of the status of contemporary meta-metaphysics, I want to start by quoting him at length:[2]

> Metaphysics – a wholly isolated speculative cognition of reason that elevates itself entirely above all instruction from experience, and that through mere concepts (not, like mathematics, through the application of concepts to intuition), where reason thus is supposed to be its own pupil – has up to now not been so favored by fate as to have been able to enter upon the secure course of a science, even though it is older than all other sciences, and would remain even if all the others were swallowed up by an all-consuming barbarism. For in it

[1] Examples of these concepts are properties, relations, causality, individuality and so forth.
[2] The english translation is taken from (Kant, 1998).

> reason continuously gets stuck, even when it claims a priori insight (as it pretends) into those laws confirmed by the commonest experience. In metaphysics we have to retrace our path countless times, because we find that it does not lead where we want to go, and it is so far from reaching unanimity in the assertions of its adherents that it is rather a battlefield, and indeed one that appears to be especially determined for testing one's powers in mock combat; on this battlefield no combatant has ever gained the least of ground, nor has any been able to base any lasting possession on his victory. Hence there is no doubt up to now the procedure of metaphysics has been a mere groping, and what is the worst, a groping among mere concepts.
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> Now why is it that here the secure of science still could not be found? Is it perhaps impossible? then has nature afflicted our reason will, the restless striving for such a path, as if it were one of reason's most important occupations? Still more, how little cause have we to place trust in our reason if in one of the most important parts of our desire for knowledge it does not merely forsake us but even entices us with delusions and in the end betrays us! Or if the path has merely eluded us so far, what indications may we use that might lead us to hope that in renewed attempts we will be luckier than those who have gone before us? (BXIV-BXV) [. . .]
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> For there has always been some metaphysics or other to be met with in the world, and there will always continue to be one, and with it a dialectic of pure reason, because dialectic is natural to reason. Hence it is the first and most important occupation of philosophy to deprive dialectic once and for all of all disadvantageous influence, by blocking off the source of the errors. (BXXXI)

The relevance that the quotation above has for the contemporary status of metaphysics is, at least, threefold.

Firstly, Kant is correct in claiming that metaphysics springs from a human desire to inquire about reality in its most general structures. Metaphysics is the kind of dream that has always haunted, and probably always will, our species' reason to the point that it 'would remain even if all the others [sciences] were swallowed up by an all-consuming barbarism' (BXIV). This partially explains why metaphysics remains an important branch of contemporary philosophy.

Secondly, if metaphysical ambitions are intrinsic to human beings' thought, then one of the most important tasks of philosophy should be, so to speak, to limit the damage of this mirage. To again use Kant's more elegant formulation, 'to deprive dialectic [i.e. metaphysics] once and for all of all disadvantageous influence, by blocking off the source of errors' (BXXXI). We can read the whole contemporary debate concerning meta-metaphysics as an attempt of drawing some clear limits between what can and what cannot be susceptible to metaphysical investigations. And such a task is more pressing than ever, given the fact that metaphysics is a huge part of contemporary philosophy.[3]

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[3] I did not find specific quantitive studies concerning how contemporary philosophy is divided. However, we can obtain a rough idea about the relevance of contemporary metaphysics by consulting one of the most used online repositories for philosophical published papers, that is, Philpapers. On its home page, it is possible to browse papers by topic. The four main topics are 'epistemology and metaphysics' (with 395,912 papers, as of 1/01/2022), 'value theory' (612,967), 'science, logic and mathematics' (474,616) and 'history of western philosophy' (358,046). A total of 48,559 papers are labelled under the topic of 'metaphysics.' By comparing such a number to other subtopics, one can see that metaphysics is a relevant part of contemporary philosophy. Moreover, there are likely more papers concerning metaphysics than those labelled. Indeed, some sub-branches of metaphysics – e.g., metaphysics of mind and metaphysics of science – are classified by Philpa-

Kant's preface's third element of interest is its partially misguided reception. Many have considered Kant, starting from its contemporaries, the 'all destroyer' of metaphysics – the one who single-handedly brought down metaphysics. But such a view is too simplistic.[4] Rather than destroying metaphysics, Kant tried to address what he takes to be 'the first and most important occupation of philosophy,' namely, that of giving solid grounds to metaphysics. Indeed, his three critiques are – firstly, if not only – books on metaphysics. The relevance of such a fact for the contemporary state of the art is soon explained. An intense debate is raging in meta-metaphysics, and those trying to give metaphysics solid ground are accused of being 'all destroyers' themselves. The current revolution in metaphysics is a civil war, and what is worse is that disagreement rages even among the revolutionaries (McKenzie, 2020).
Now, methodological and foundational revolutions are not entirely new to metaphysics. Indeed, the history of metaphysics can be seen as an eternal return: it has been declared dead many times by past philosophers[5], only to be seen arising from its ashes later on. What has to be learned from the history of metaphysics is a matter of interpretation. The detractors of metaphysics derive from its history a pessimistic meta-induction: since metaphysics has proven unreliable, give its cyclic deaths, in its attempts of reaching the fundamental structure of reality, what are the reasons that make us hope that we will achieve, this time, such an ambitious aim? (Stanford, 2017), for instance, proposes an analogous argument, based on the bearing that empirical sciences had on philosophy: scientific developments repeatedly proved past metaphysical systems wrong. Thus, there is a considerable gap between how reality is and how it can be conceived a priori; to the point that there seems to be no distinct metaphysical knowledge to chase. If scientific discoveries proved past metaphysical systems wrong – making us doubt that there is even any metaphysical knowledge – why should we follow once again metaphysics' mirage, rather than trusting those authors that in the history of philosophy tried to bring reason down to earth, on more stable ground? To the detractors of metaphysics, its history reveals that such a discipline is just a ghost that stubbornly refuses to vanish (Price, 2009).
On the other side of the debate, the supporters of metaphysics conclude that the discipline is like the mythological phoenix, which dies only to resurrect from its ashes later on. Indeed, metaphysics resurfaced each time it has been sunk; each time improved, they could add, since metaphysics changed, taking over the meta-metaphysical challenges of its detractors. Moreover, the supporters notice that the alleged death of metaphysics has been proclaimed by philosophers bound to philosophical systems that – as history revealed – failed much more than metaphysics itself; logical Neo-positivism and Kantism are often examples invoked. Therefore, the supporters conclude, if the critics of metaphysics turned out to be unreliable, why should we trust them more than the mirage itself?
Historical arguments are seldom conclusive. However, the meta-pessimistic induc-

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pers.com directly as metaphysics (in the examples above, they are classified under sub-topics of 'philosophy of mind' and 'science, logic and mathematics').

[4] See for instance §1.3 of Williams (2018).

[5] Notorious examples being Wittgenstein (1994); Ayer (1936); Reichenbach (1951); van Fraassen (2002).

tion above is a worry that friends of metaphysics should keep in mind when dealing with their discipline's foundation. Especially nowadays, since the last decades have been witnesses of a methodological "renaissance" (Stanford, 2017) which seems to have brought an end to a metaphysics' vital cycle and the beginning of a new one. The cycle that appears to be ending is that of a priori analytic metaphysics, or as Ladyman and Ross (2007) call it, 'neo-scholastic metaphysics.' The common lore attributes to Quine's (1951) answer to Carnap (1950) the born of this cycle, whose peak has been reached in the works of David Lewis (1983). The grounds of this approach to metaphysics seems to be threatened by many criticisms. Despite the polemic tones of (Ladyman and Ross, 2007, Ch. 1) against contemporary metaphysics, their aim is not that of arguing that metaphysics is impossible[6]. Their work has indeed started a deep debate[7] in meta-metaphysics that hinges on the question 'how should we, if it is possible in the first place, do metaphysics'? Among the many criticisms, the most relevant accusations moved against 'neo-scholastic metaphysics' have been its epistemic unreliability, i.e., the impossibility of justifying how a priori methods could deliver knowledge of the world[8] – and its self-imposed separation from empirical sciences. As a result of the criticisms moved by Ladyman and Ross (2007), a new approach to metaphysics has risen and gained significant popularity. This approach is often called with different names, such as *naturalised metaphysics*, *metaphysics of science* and *scientific metaphysics*.[9] To me, this is the beginning of a new metaphysics' vital cycle.

The aim of what follows is to defend such a claim. To do so, I first present what I take to be neo-scholastic metaphysics (§2.1). Contrary to what Ladyman and Ross (2007) seems to assume, neo-scholastic metaphysics is not, itself, a unitary approach. Even by keeping fixed three relevant theses which are shared throughout neo-scholastic metaphysics,[10] one can trace substantial differences in how contemporary metaphysics is pursued. In particular, I discuss three conceptions of neo-scholastic metaphysics that are the most widespread in contemporary literature. They are, respectively, 'Neo-Quinean metaphysics' (§2.1.1), 'metaphysics as the science of possibilities' (§2.1.2) and 'metaphysics as the quest for fundamental structures of reality' (§2.1.3). After their discussion, I turn to naturalized metaphysics (§2.2). In the first

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[6] The tones of the first chapter of (Ladyman and Ross, 2007) are so polemic, indeed, that their work has been initially recognized as an all-destroyer (Dorr, 2010). However, their target is a particular way of doing metaphysics rather than metaphysics itself. Indeed, the second half of their work is a defence of a metaphysical thesis known as 'structuralism.'

[7] A good introduction on the debate, is (Wasserman et al., 2009).

[8] Note, however, that the possibility of a priori knowledge is a widely accepted thesis among philosophers. According to Bourget and Chalmers (2014)'s survey, 71.1% of the philosophers involved claimed to believe in a priori knowledge. In a new study they conducted in 2020 (Bourget & Chalmers, manuscript), the percentage increased to 72.8.

[9] Some authors, e.g.(Stanford, 2017), use them to refer to slightly different views. Instead, in what follows, I use them as synonymous since they are different views of a common meta-metaphysical stance.

[10] Which are, as we will see below: (i) metaphysical enquiry are conducted almost exclusively a priori, (ii) metaphysics is more fundamental than empirical sciences (either epistemically or ontologically) and, as a consequence, (iii) metaphysics is independent of other branches of knowledge.

part, I discuss naturalized metaphysics in general terms. Then (§2.2.1), I discuss the differences between metaphysics and science, which are instrumental for putting forward my own characterization of naturalized metaphysics (§2.2.2). Finally, I discuss some problems within my account (§2.2.3). Conclusions follow suit (§2.3).
Before starting, a note is due. As it is clear from the rough depiction of the history of metaphysics, I am not a historian of philosophy. As such, the categorization that follows is not – and does not want to be – a historically accurate characterization. The chapter aims to summarize in four approaches the main meta-metaphysical views available in the literature to extract what I take to be the most interesting features and problems of these approaches. Hence, the taxonomy does not make justice to the views of the philosophers I quote below, which are much more complex and articulated than the sketches I propose, to the point that many would not even fall neatly under a single category.

2.1.1 Metaphysics, the Quinean legacy

Whereas ontology is the field of philosophy that tries to answer the question 'what does exist?', meta-ontology faces whether it is possible (and if so, how) to address the ontological question in the first place. The common lore attributes to Quine the meta-ontological framework implicitly assumed by the majority of contemporary analytic philosophers. That this is the case can be inferred by the fact that analytic philosophers often sum up the meaning of 'existence' with a quotation from Quine (1953): ''to be is to be the value of a bounded variable." Quine's dictum is based on the idea that 'existence' has to be modelled with classical first-order predicate logic's existential quantifier (∃). A consequence of such regimentation is that any true existential statement – such as "there are prime numbers"[11] – implies the existence of the entity mentioned in the statement (prime numbers, in the example above).
This criterion establishes a necessary condition for which existential debates are meaningful. However, it is not enough to establish which ontologies have to be accepted among the possible ones. There are two commonly invoked ways of doing so.
The first possibility is sticking to Quine's view as closely as possible.[12] According to Quine, scientific and philosophical attempts of investigating reality are parts of the same endeavour. Hence, an ontological commitment is due only to those entities whose existence is necessary for the truth of our (currently) best scientific theories. Quine uses such a criterion as a premise of the "Indispensability argument" in favour of the existence of mathematical entities (cf. Colyvan (2019)). As it is employed as a premise, it is reasonable to assume that not only Quine accepts it, but

[11] Such a statement is regimented in first-order predicative classic logic as '$\exists x(Px)$' where P stands for 'Being a prime number', and it means 'there is at least an x such that x is a prime number'.
[12] See, e.g., Quine (1980); Janssen-Lauret (2016).

that he would have also used such a criterion to choose between rival ontologies.[13] A second possibility is using the same super-empirical virtues scientists use to choose between rival empirically equivalent theories: simplicity, elegance, conceptual parsimony, explanatory power, etc. It might be that, from a pragmatic point of view, these criteria are advantageous. However, they are not guaranteed to be good criteria for choosing between different ontologies. Indeed – as argued extensively by (Ladyman and Ross, 2007, p. 17 *ff.*, p.81 *ff.*) – there is no reason to think that these criteria are truth-conductive. That is, we cannot know whether the world is simple, elegant, etc., rather than the opposite. If ontology aims to establish what exists, there is no reason to believe that super-empirical virtues are good ontological guides.

The Quinean meta-ontology above is so widely accepted in contemporary analytical philosophy that the first way of understanding metaphysics – which we could label as 'Neo-Quinean' – consists in establishing what exists.[14] Metaphysics and ontology, in this conception, are not different disciplines, since the aim of metaphysics is that of, so to speak, creating an "universal catalogue" of everything that exists, existed, and it could well possibly exist in the future (Varzi, 2001). I argue now that there are several reasons to be dissatisfied with such a conception of metaphysics.

First, even accepting a Neo-Quinean meta-ontology, there is no reason to think that the scope of metaphysics coincides with that of ontology. Many philosophers think that metaphysics' aim is wider than that of ontology. Indeed, metaphysicians do not only care about what exists. Rather, they also care about how existing entities are related and the structures they are part of.[15] For example, metaphysicians are interested in how identity is preserved through time[16] (Costa, 2018), how parts compose wholes (Cotnoir and Varzi, 2021), how language and truth are related (Fine, 2017), and so on. Moreover, some believe that some facts require a peculiar explanation, that is, a 'metaphysical explanation', which is a non-causal, non-scientific explanation.[17] If one believes in metaphysical explanations, then metaphysics' aim cannot be just that of listing which entities exist; rather, it must also be that of putting forward this kind of metaphysical explanation to account for some phenomena.

Second, one may doubt that the criteria of ontological commitment above must be held without further qualification. Indeed, even by accepting that it gives us a necessary condition for determining what exists, it might not be sufficient. I will belabour this point later on when I put forward a positive characterization of naturalized metaphysics as I intend it.

Third, one might doubt that ontology and metaphysics coincide according to the Neo-Quinean criterion above. The reason is that the core concept of ontology – 'existence' – is indeed a metaphysical concept. Hence, connecting the scope of

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[13] See also (Quine, 1948, p. 35 *ff.*).

[14] See, e.g., Van Inwagen (2009).

[15] See, e.g., Correia and Schnieder (2012).

[16] That is, how I may be now the same entity I was when I started my PhD, although the properties I have now are different from those I had years ago.

[17] On this, see, e.g., Rosen (2010). An example of this kind of metaphysical explanation is the one given by truth-maker theorists about how states of affairs make true sentences in ordinary language, cf. Fine (2017).

metaphysics to that of ontology seems to invert the relationship between the two disciplines illegitimately. Certainly, one's metaphysical view on existence will determine one's ontology, rather than the other way around.[18]
Finally, Price (2009) argues that Quine would not have accepted such a methodology of doing metaphysics. In particular, Price (2009) argued that Quine's critique of Carnap's frameworks does not imply that Quine would have been favourable to the kind of analytic metaphysics which populates the current literature.[19]

2.1.2 Metaphysics, the science of the possible

The following is a common understanding of metaphysics that became popular in the last decade: The aim of metaphysics is that of determining – a priori – what *could* exist, whereas empirical sciences discover what *actually* exists.[20] According to this understanding, metaphysics' field of enquiry and its methods are independent of other sciences: metaphysics investigates what is possible – rather than what is actual –, and it does so utterly *a priori*.
The claim that metaphysics' role is that of investigating what is possible has to be qualified insofar as there are different criteria according to which something might be possible. The two most used notions of possibility – in the philosophy of science, at least – are that of *nomological* and *logical* possibility. The former concerns what could be the case according to laws of nature.[21] For instance, it is nomologically possible for the train I used to take to go to Geneva from Milan to be on time, rather than late as usual, insofar as no law of nature prevents the train to be on time for once. Conversely, building a sphere of enriched uranium weighing exactly a tonne is nomologically impossible. According to the laws of nature, the uranium would decay long before such a weight is reached (causing a deflagration of the sphere). The latter concerns what is logically possible. Given a formal system – a language built from primitive symbols, syntactic and consequence rules – every well-founded formula represents a logical possibility.
In between these notions of possibility, one could think that there is room for a third notion, a 'metaphysical possibility,' as it is commonly called in the literature. We can further distinguish metaphysical possibility into two kinds: a 'higher-order nomological possibility,' so to speak, and an 'absolute' possibility. The first kind of

[18] On this point, see Fine (2009). There are two ways of regimenting the concept of existence in the current literature: the existential quantifier above and a predicate in free logic. A significant difference between the two is that 'existence' turns out to be a property when it is modelled with a predicate.

[19] For a reply to Price (2009)'s arguments, see Deng (2020).

[20] (Lowe, 1998) is one of the most influential books defending such a view.

[21] It is highly likely that we do not know all the laws of nature yet, and that some regularities which we believe to be laws of nature will lose such a status in future theory. Hence, one should clearly distinguish two notions of nomological possibility. That is, an epistemic one ('what is possible according to the laws of nature we know so far') and a metaphysical one ('what is possible according to the laws of nature *simpliciter*').

metaphysical possibility concerns what could happen if the laws of nature were different from the actual ones. For instance, one can wonder how a Newtonian world would be if we assume that gravitation is governed by an inverse-cube law rather than the familiar inverse-square one. Instead, the second kind of metaphysical possibility concerns what could exist – in the most general sense possible. For instance, one may wonder whether a human being could be a turnip in a different possible world or if there are essential properties that are instantiated in every possible world in which an entity[22] exists. This second kind of metaphysical possibility is broader than the first one. The first kind of metaphysical possibility is between the nomological and the logical possibilities. It is trivially broader than the nomological one because it concerns the non-actual laws of nature. Yet it is narrower than the logical one. If we assume, for example, that 'water' is a rigid designator, then it is metaphysically impossible that 'water' does not refer to H_2O. Yet, it is logically possible insofar as there is no logical contradiction in " the word 'water' does not refer to H_2O." Furthermore, such a notion of metaphysical possibility is tightly connected to the nomological one insofar as actual laws of nature usually determine it. That is, we keep all the laws of nature fixed but one, and we create models of what could happen if the unfixed law were different. In constrast,, the 'absolute' metaphysical possibility is independent of the laws of nature, and it is debatable whether such a notion is broader or narrower than logical possibility.

Be that as it may, the understanding of metaphysics as the 'science of possibilities' is based on the notion of metaphysical possibility. The debate about the legitimacy of metaphysical possibility hinges on its epistemology rather than on its ontological characterization. It revolves around questions concerning whether we have epistemic access to this kind of possibility, and if so, how.

In the literature up to date, the metaphysical possibility is tied up with the notion of conceivability[23]: if a human being can conceive a state of affair, then the latter is possible.[24] For instance, the fact that one can imagine a world inhabited only by two identical spheres,[25] is sufficient to claim that such a world is possible. Metaphysicians sympathetic to the 'science of possibilities' view see thought experiments as an important methodological tool. As such, their epistemic reliability is crucial for this conception of metaphysics.[26] Yet, there is a second methodological tool often employed by metaphysicians interested in possibilities: formal languages. In a nutshell, the methodology consists in taking a generic concept – that of, say, 'being part of' (Simons, 1987) or 'being located in a region *x*' (Parsons, 2007) – and to regiment it in a formal language. By starting from different axioms which rules these notions, one can put forward different models representing as many metaphysical structures to which they might refer. In this way, one can explore possible worlds in which the same concept refers to different metaphysical structures, each represented by as

[22] Or one of its counterparts, depending on one's theory of identity across possible worlds (Lewis, 1968).

[23] On this, see (Gendler and Hawthorne, 2002).

[24] Such a view is defended, among others, by Chalmers (1996).

[25] The example is taken by the well-known (Black, 1952).

[26] For a primer on thought experiments, see (Brown and Fehige, 2019).

many models of the chosen formal language. If one is, say, interested in the concept of parthood, one can model it to explore a possible world in which two objects are always parts of a third one, or a possible world in which the parthood relation might be indeterminate.[27]

Insofar as metaphysics so conceived consists purely in an *a priori* endeavour, it is considered by its practitioner as 'more fundamental' than empirical sciences. Two reasons are usually put forward to sustain such a claim. First, what is actual is just a subset of what is metaphysically possible. Hence, the domain of empirical sciences is a subset of that of metaphysics. Second, the metaphysical concepts investigated are also used – yet never properly defined – by scientists. It follows that to properly understand scientific theses, an appropriate understanding of the structures investigated by metaphysicians is necessary. Consider, as an example, mereology. Scientists often claim that some entities are part of others. Physicists say that atoms are parts of molecules, biologists that tails are parts of some animals, geographers that regions are parts of a country, and so forth. In the conception of metaphysics to which the subsection is dedicated, metaphysics investigates all possible concepts of 'parthood'. Scientists, instead, employ a particular concept of parthood which is only one of the many possibilities. Concepts of parthood are assumed and used by scientists but defined and regimented by mereology. Modelling these concepts so that scientific statements involving parthood became clear is part and parcel of metaphysics' aim.[28] Analogously, metaphysics studies all other concepts which are essential to describe the structure of reality, often used by scientists as well. Some believe that metaphysics so conceived is epistemologically prior to empirical sciences for the reasons above. Empirical sciences tell us what the world is actually like. But to fully understand the metaphysical concepts often invoked by scientists, a detailed analysis of them is necessary.

The idea that metaphysics is the science of what is possible was widely shared in the last decades. Yet, I think there are some serious epistemic concerns with such a view. Let us see some of them.

First, one could doubt that these general notions that metaphysicians are interested in have an actual referent. To stick to the example of mereology, as argued by Ladyman and Ross (2007), it is reasonable to think that how scientists use the notion of 'parts', and 'wholes' is so varied that one might be sceptical about the possibility that they refer to a single notion of parthood. There is no single parthood relation that captures the real way parts compose wholes; instead, parthood is used variously in different contexts to refer to different metaphysical relations. Hence, a unique 'true' metaphysical notion of parthood is in contrast with the scientific understanding of reality, which suggests instead that different entities entertain different parthood relations with their respective wholes. Note that the issue is not, contra Hofweber (2009), that metaphysics employs an 'esoteric' language, that is, a language distinct from the one used by scientists.[29] Rather, the point is that there might not be a gen-

[27] For a quick primer of mereology, the formal system used for regimenting the notion of parthood, see (Varzi, 2016).

[28] On mereology and empirical sciences, see (Calosi and Graziani, 2014).

[29] On this issue, see (French and McKenzie, 2012, 2015).

eral metaphysical structure to which a concept refers to such that such a structure capture each instance in which the concept applies. In the example above, that there might not be a single concept of parthood that models each example of the part-whole relation.[30]

The second epistemic worry is the following. The fact that the concepts themselves are not the direct subject matter of empirical sciences does not imply that metaphysicians can investigate them only through a priori analyses. In other words, the fact that metaphysically relevant concepts are not explored by empirical science cannot ground the legitimacy of a purely a priori enquiry. Yet, it is far from trivial to explain how purely a priori conclusions might be relevant for actual or possible entities without any comparison with empirical structures and data. Without comparing metaphysical models with concrete, empirical cases in which the relevant concept plays a role, it seems that we can extract no information about the actual world. And if this is so, then one might worry that even knowledge about what could be the case is prevented. Logical and nomological possibilities have a sound epistemic ground: the rules of formal systems for the former, empirical data (and an optimistic meta-induction, so to speak) for the latter. However, what could ground our knowledge of metaphysically possible structures? Some authors[31] think that there are 'metaphysical laws' which, similarly to laws of nature, determine necessary conditions on how metaphysical concepts might behave. Metaphysical laws would, indeed, furnish an epistemic ground for metaphysical possibilities. Yet, the epistemic status of metaphysical laws themselves is shaky and needs to be defended, bringing us back to the problem above in a new guise: how can we obtain knowledge about these metaphysical laws, and what justifies our belief in having discovered them.

Third, we have seen above that there are two kinds of metaphysical possibilities: the 'higher-order nomological possibility' and the 'absolute possibility'. The former seems to be tightly connected to nomological possibility. As mentioned above, the starting point to imagining scenarios in which different laws of nature hold is starting from actual laws of nature. What grounds our knowledge of these possible worlds is that we know what happens in the actual world, and we can thus model possible worlds in which a single law behave differently from how it does in the actual one. Hence, this kind of metaphysical possibility is far from independent from the nomological one and, as such, not much problematic. The absolute one, instead, is difficult to pinpoint, mainly because its most straightforward characterization is problematic. Indeed, one could claim that metaphysical possibilities are dictated by the formal models employed to characterize metaphysical structures. To stick to the example above, any formal system of mereology is a metaphysical possibility of how the parthood relation might be. Yet such a characterization should be worrisome for the friends of metaphysical possibility, as it seems to collapse it to logical possibility. If metaphysical possibilities can be modelled in a formal system, then metaphysical and logical possibilities seem to coincide. This would be a problem

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[30] Note that the idea that there is a unique part-wholes relation which describes every instance of parthood has been strongly criticized in a recent paper by Simons (2021), one of the founding fathers of mereology.

[31] E.g., (Barker, 2019).

for the friends of metaphysics as the science of possibility because their view needs that the domain of metaphysical possibilities is different from the domain of logic and empirical sciences. Alas, such independence is yet to be justified.

One might raise concerns similar to those raised above about the epistemic significance of formal models against the methodology of thought experiments. Even granting their prima facie intuitive appeal, there is no reason to think conceivability is a secure guide to possibilities. On the one hand, people often disagree about what can be conceived or not, and there is no objective way to side in these debates if not by appealing to introspection. On the other, scenarios claimed to be conceivable sometimes are – after a more careful analysis – incoherent.[32]

So far, I have criticized metaphysics as the science of possibility view by stressing conceptual and epistemic problems concerning the notion of metaphysical possibility. I will conclude the section by putting forward a fourth critique, which relies on historical data. One may view it as a kind of 'pessimistic meta-induction', similar to the one put forward by scientific antirealists (Laudan, 1981). Like many other arguments based upon history, it is far from conclusive. Yet, together with the problems raised above, it is just one more nail in the coffin. The argument goes as follows. Metaphysics' aim should be that of determining what is possible. Moreover, what is nomologically possible must also be metaphysically possible. Yet, many phenomena declared metaphysically impossible in the past turned out to be nomologically possible.[33] The reasons why they have been judged as impossible were either for a priori reasons – e.g., the existence of indeterminate properties was considered to be incoherent – or based on conceivability –e.g., it could not be imagined a non-Euclidean world able to explain human beings' perception of reality. However, the fact that these phenomena turned out to be possible allows one to doubt a priori methods' reliance to determine what is, and what is not, possible. Insofar as our current epistemic condition is no better than those of past philosophers and scientists, we should conclude that our imagination and formal modelling capabilities are no secure ground for inquiring about possibilities.

2.1.3 Metaphysics, the quest of fundamentality

The third meta-metaphysical view I will discuss is, somehow implicitly, quite widespread in contemporary literature, especially among those metaphysicians whose work also concerns empirical sciences' findings. According to this approach, metaphysics is the discipline that aims at discovering what the fundamental structures of reality are (Fine, 2009; Schaffer, 2009; Sider, 2011; Bigaj and Wüthrich, 2015; Schrenk, 2017). As such, metaphysics has a precise domain, and it is independent

[32] Chalmers (1996)'s zombie thought experiment being a paradigmatic example. See Dennett (1995)'s discussion of the experiment.

[33] Examples being non-Euclidean geometries, time travels and '*creatio ex nihilo*', which are possible in some space-time models of general relativity; multi-location, indeterminism, indeterminacy, which are possible in some interpretations of non-relativistic quantum mechanics.

of other empirical sciences. Metaphysics' domain consists of the fundamental and most general structures responsible for reality as we can know it, from everyday experience to scientific explanations. Metaphysics' subject matter is more fundamental than the subject matter of other disciplines (Paul, 2012). Hence, it is thus independent of different forms of knowledge about reality. These metaphysicians do not deny that the metaphysical structures they are interested in are also (implicitly) assumed by scientists. Yet, what distinguish metaphysics is that it must put forward an analysis of these structures – which is seldom carried out by scientists, as the case of the parthood relation discussed above shows – and it explains how these more general structures are connected with those investigated by scientists. For example, a common view among scientific realists is that science discovers laws of nature: 'rules' that determine the time evolution of physical systems. Metaphysicians aim to explain precisely what these laws of nature are – the three more defended views account them in terms of mere regularities (Lewis, 1994), properties of physical objects (Dorato and Esfeld, 2014), primitive entities (Maudlin, 2007) – and if they are part of the fundamental structures or reality, or grounded in more fundamental ones. The view here discussed shares many similarities with the approach outlined in the previous paragraph (§2.1.2).[34] From a conceptual point of view, these views share the idea that metaphysics' field is ontologically prior to that of empirical sciences. From a methodological point of view, they share the use of formal languages, conceptual analysis and thought experiments. Yet, the views differ radically about what is metaphysics' *aim*. The view discussed in this paragraph does not care about metaphysical possibilities; what matters instead is which fundamental metaphysical structures there are in the actual world. Hence, regimenting the concept of interest within a formal system is a preliminary of the inquiry, rather than the aim – as friends of metaphysics as the science of possibility claim. The purpose instead is to find which formal model describes the actual world. And to achieve such an aim, two criteria are often invoked: (a) *compatibility* with our best scientific theories and (b) *super-empirical virtures*. The first criterion limits the scope of metaphysical models. Since metaphysical structures should be fundamental and general, metaphysical theory cannot conflict with empirically adequate scientific theories. Indeed, a contradiction with our best scientific theories would imply that a metaphysical view does not concern the fundamental structure of the *actual* world. The second criterion concerns how the field as a whole pursues research. In a nutshell, progress is made in the following way. Philosophers put forward arguments in favour of their view and against rival ones by putting forward examples of phenomena that can be accounted for by their theories but cannot according to others. Those who defend a view that cannot account for the phenomena above can (i) bite the bullet, admitting that they can present no good answer, (ii) modify their view to account for the phenomena, or (iii) show that the argument is unsound – perhaps some premises are unjustified, or one step does not follow from another, etc. The process of argumentation and answer is carried out for a significant amount of time until the literature

[34] Indeed, most metaphysicians would most likely accept a hybrid of the views here discussed, rather than neatly embracing one of them. As mentioned in the introduction, the chapter aims not at giving a detailed historical reconstruction of meta-metaphysical views.

reaches a kind of consensus about which theory is better. Usually, the theory which draws more consensus is the one that has the most super-empirical virtues – simplicity, intuitiveness, can explain more phenomena, intuitiveness, and so forth.

Let us turn to some problems with the conception of metaphysics as the quest for the fundamental structures of reality. There are three worries: one connected with the field of enquiry that metaphysics should have and one with the criteria above.

Metaphysics is not the only discipline that describes the fundamental structures of reality since it seems to be also part of what physics aims for (Morganti and Tahko, 2017). However, the fact that physics' and metaphysics' respective fields of study overlap, raises several worries. Why do they reach so different results if they share the same phenomena of interest? And given the empirical success of physics, a legitimate question is why one should practice metaphysics rather than physics. In other terms, it has to be explained the epistemic content that metaphysics adds to physics. Moreover, if metaphysics aims to describe the most general structures of reality, circumscribing its aim only to the *fundamental* structures of reality seems to be unnecessarily restrictive. Many metaphysicians' works concern non-fundamental entities – tables, species, emotions – and the structures in which they partake. And it seems they are doing metaphysics rather than phenomenology, biology or psychology. Hence, such a view about metaphysics seems to be a view about metaphysics of fundamental physics, rather than a view about metaphysics.

Also, the criteria used by metaphysicians to claim to have reached fundamental structures of reality are problematic. Almost nobody argues that a philosophical thesis could be incompatible with our best scientific theories among currently active philosophers. However, the compatibility request is too weak to define scientifically informed metaphysics. First, the epistemic status of a priori methods has yet to be justified (as also stressed in §2.1.2). Second, scientific theories strongly underdetermine metaphysics. That is, a scientific theory might be compatible with many metaphysical interpretations that, although they do not contradict the theory in question, cannot be empirically tested. All things being equal, a choice can be put forward just by an appeal to super-empirical virtues. However, they are problematic themselves. One must note, indeed, that they are not, as argued extensively by Ladyman and Ross (2007), truth-conductive. They are *pragmatic* criteria to choose among rival theories, rather than secure guides toward *true* theories. We have no reasons to think that the world is simple, elegant, unified, etc., rather than the opposite. Especially because an often invoked virtue of a metaphysical theory (Simons, 2021) – if not of philosophical views in general (Mulligan et al., 2006) – is its accordance with common sense. However, common sense is historically and socially determined by a set of prejudices with an unavoidable pragmatic character. Hence, it is unclear how it could guide us in discovering how the world is, independently from our epistemic perspective (Ladyman and Ross, 2007, Ch. 1).

To conclude, it is worth discussing a recent paper that defends an approach to metaphysics close to the one described in this section. Morganti and Tahko (2017) argued that metaphysics and physics share their domain of interest – the fundamental structure of reality – and that they should work closely together to put forward a comprehensible description of the actual world. However, they claim that the two disciplines strongly differ in their methodologies. Metaphysics, indeed, works exclusively *a priori*. Indeed, if only a posteriori elements are considered important, metaphysics would not play "a role in our enquiry into the nature of reality" (Morganti and Tahko, 2017, p.2662). As remarked above, investigating reality only from an a priori perspective is epistemically problematic. Our best scientific theories must be, in my view, the starting point – rather than the indirect testing ground – for metaphysical inquiries. That is, metaphysics' methodology must have an a posteriori part, which consists of feedbacks from our best theories and experiment. The following section is devoted to the articulation of such a view.

2.2 Metaphysics, a project to understand reality

Foundational questions concerning a given discipline are more often than not tackled by those who practice such a discipline. Metametaphysics is no exception. Questions concerning the legitimacy of metaphysical enquiry are felt as more pressing by metaphysicians that feel an urge to justify their philosophical approaches. On the one hand, philosophers uninterested in metaphysical debates do not spend much time thinking about the legitimacy of metaphysics (Chalmers, 2009). As a result, criticism like the ones exposed above about different approaches to metaphysics are often shared by many philosophers but seldom adequately spelt out in print. Those that find the arguments above convincing, indeed, conclude that metaphysical debates are 'shallow or trivial' (Chalmers, 2009, p. 3) and dedicate their philosophical efforts in another direction. On the other hand, those who felt meta-metaphysical questions as urgent believe that metaphysics is worth to be pursued. I am no exception. In my view, metaphysics is part and parcel of the scientific attempt of understanding reality. In a nutshell, there is not a neat line of demarcation between metaphysics and empirical sciences, insofar as they are parts of the same project to understand reality. This approach to metaphysics has been defended by many in the literature (Ladyman and Ross, 2007; Hofweber, 2009; Ross et al., 2013; Wilson, 2013b; Bigaj and Wüthrich, 2015; Esfeld, 2018), and it is often called *naturalized metaphysics*, *scientific metaphysics* or *metaphysics of science*. According to this approach, metaphysics shares its subject and methodology with empirical sciences. The subject is the actual world and the attempt to explain the reality surrounding us. The methodology is a mixture of a priori and a posteriori work. The a posteriori part consists in gathering data and information from our best scientific theories. The a priori part consists of the construction of metaphysical models, starting from the data above. These models are a priori since they are built through conceptual analysis and formal languages. Yet, they must be confronted again with our best scientific theories to see whether they can play an explanatory role for concrete phenomena.

Most friends of naturalized metaphysics would agree on the rough and ready description of it I gave above. Alas, the devil is in the details. Indeed, even among naturalized metaphysicians, there is a lot of space for disagreeing on how empirical sciences have to bear on metaphysics. For example, some features of Morganti and Tahko (2017)'s account – e.g., the claim that metaphysics is prior to empirical sciences (Cf. §2.1.3) – are such that I would not classify it as a 'naturalized' approach, even though they would (quite explicitly, insofar as the title of their paper is 'Moderately Naturalistic metaphysics'). Or Stanford (2017) dissects naturalized metaphysics by distinguishing 'scientistic metaphysics' – represented by Ladyman and Ross (2007) – 'complementary metaphysics' – represented by Paul (2012) – and 'metaphysics of science.' By doing so, Stanford (2017) notes some interesting differences in how one could understand naturalized metaphysics. In what follows, I try to articulate my own view of naturalized metaphysics.[35] To do so, I start by explaining in what metaphysics and empirical science differ (§2.2.1); then, I give a positive characterization of naturalized metaphysics by highlighting its main features (§2.2.2). Finally, I discuss some problems of the approach to metaphysics here characterized (§2.2.3). Conclusions follow suit (§2.3).

2.2.1 On what science and metaphysics differ

Naturalised metaphysics shares with empirical sciences both the object and the methodology of its inquiry. As such, it detaches itself radically from the other conceptions of metaphysics discussed in §2.1. Yet, metaphysics and empirical sciences are kept apart by substantial differences. The difference between naturalised and non-naturalised metaphysicians is that, according to the formers, the boundaries between metaphysical and scientific questions are blurred rather than neat. For sure, there are metaphysical questions that will never receive an answer appropriately backed up by our best scientific theories (Thomasson, 2009). But there are also metaphysical questions that already have a scientifically informed answer, or we are optimistic it will have in the near future. Analogously, as there are scientific questions that concern only pragmatics, there are many others with a substantial metaphysical import.

What separates metaphysics and empirical science does not concern their methodology or the kind of questions posed as their aims and terminology. Empirical sciences seek to discover new theories able to explain phenomena that happened in the past and to predict future ones. New theories are necessary to increase the number of phenomena explained and forecasted. The predictive part is essential because it allows scientists to test their theories and confirm or modify them based on the results obtained. To achieve that, scientists build new mathematical models and design empirical experiments to test them. The world's scientific understanding consists of a mathematical characterisation whose trustworthiness is testified by a trail of successful experiments. Instead, naturalised metaphysics aims to explain how the world

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[35] Which would be rather called 'metaphysics of science', in Stanford (2017)'s terminology. However, 'metaphysics of science' is employed elsewhere with a different meaning, e.g., (Mumford and Tugby, 2013), which is closer to the approach outlined in §2.1.3 rather than to the one outlined in this section. I am not interested in these terminological issues, and I use 'naturalized metaphysics' and 'metaphysics of science' as synonyms.

must be like if our best scientific theories are (at least approximately) true. That is, to convert the mathematical models into more familiar concepts to obtain a clear description of reality. As such, metaphysics follows empirical sciences rather than being prior to them. Scientific theories are thus a prerequisite of metaphysics, insofar as the aim of the latter is exactly that of giving a conceptually polished description of what the mathematical content of the formers tells us about reality.
Other than a partial discontinuity in their aims, metaphysics and empirical sciences differ also in their terminology. This is somehow inevitable insofar as metaphysics, to describe the world as our best scientific theories depict it, has to analyse those concepts that scientists implicitly assume as meaningful. Causality, individuality, parthood, emergence, to name a few. Scientists take these concepts as primitive – that is, they are assumed as meaningful even though no definition is proposed. However, they are far from being obvious or clear.[36] As a result, the language used by metaphysicians might sound as 'esoteric' (Hofweber, 2009) from the scientists' viewpoint. The fact above – metaphysicians analyse concepts given for granted by scientists by using proper terminology – partly explains why some questions metaphysicians pose themselves might sound as shallow or confounding to scientists.
However, the esoteric language is not the only reason why adequate communication between metaphysicians and scientists is difficult. On the one hand, the way academia works and the enormous pressure to publish it exerts on researchers is such that they have little time to engage with disciplines different from theirs.[37] On the other, scientists and philosophers receive a completely different training during their graduate studies, so that they acquire a completely different way of thinking about the theoretical problems they face.[38] To put the difference in a few lines, it seems to me that philosophers are trained at keeping almost identical concepts apart by applying them to different possible contexts and with careful argumentation. A high capability of abstract thought and logical rigour is mandatory to do so. In contrast, scientists are trained at solving more pragmatic problems, which requires an incredible familiarity with mathematical modelling to be resolved. As such, scientists often fail to appreciate subtle differences between different concepts as philosophers – often bored by prosaic issues – tend to ignore the philosophical import of experimental results by preferring its theoretical counterpart.

2.2.2 Toward an account of naturalized metaphysics

Even if the rough and ready picture presented above of what naturalized metaphysics consists of is quite standard, philosophers disagree on the details. This subsection

[36] Individuality in quantum mechanics being a paradigmatic example (French and Krause, 2006).

[37] I am referring here to the so-called 'publish or perish' culture. There are not as many research papers to quote on this theme as newspaper articles. I advise the interested reader to read Eisen's and Buranyi's articles published by *The Guardian* which can be found, respectively, here and here.

[38] This is exacerbated by the fact that, in almost every University, no course in philosophy is mandatory for STEM students, as scientific disciplines are not for philosophical degrees.

aims to elaborate on some relevant features of my own view.

I will start by noticing that with ‘metaphysical structures’ and ‘metaphysical explanation’, I do not mean something different from what ‘neo-scholastic’ metaphysicians mean. Some naturalized metaphysicians stress the fact that metaphysics’ domain is the empirical world itself and that the entities it studies are part of this world. Esfeld (2018), for instance, argues that “by ‘metaphysics’, one does in this context [i.e., naturalized metaphysics] not mean a theory that claims to refer to a domain of being beyond the empirical realm, but, in the Aristotelian sense, a theory that seeks to achieve a general and fundamental understanding of the empirical world itself.” Whether or not my view is different depends on how one interprets the quoted sentence. If it just means that metaphysics and natural sciences investigate the same phenomena – the concrete phenomena which are part of the actual world – then there is no genuine disagreement. But many non-naturalized metaphysicians would agree on that as well (Cf. §2.1.1; §2.1.3). However, if it means that only empirically testable concepts are allowed in scientific metaphysics, then a genuine difference can be traced. Empirically unobservable concepts and entities still mediate part of our understanding of the empirical world. This is so because scientific theories employ these concepts, and metaphysics aims to give them a coherent and clear foundation. Laws of nature, physical properties, fields, space-times, etc., are metaphysical concepts. For sure, they can be mathematically modelled to give us empirically confirmable predictions. However, they are *posited* by our best scientific theories and they are not susceptible to direct empirical evidence.[39] Hence, I, as a naturalized metaphysician, do not think that I mean something different when I speak of ‘metaphysical structure’ from what a ‘neo-scholastic metaphysician’ would mean. Instead, the peculiarities of naturalized metaphysics lie mostly on what questions are found interesting and deserving of being inquired.

Moreover, my view is that not every aspect of the approaches above has to be jettisoned. Rather, substantial elements of these approaches have to be incorporated into naturalized metaphysics. However, that this is the case, it is not evident from the first characterization of naturalized metaphysics proposed by (Ladyman and Ross, 2007). They argue, indeed, that a metaphysical thesis has any value only if one can show that it unifies two or more scientific theories so that it can explain more than what the two theories could explain by themselves. Furthermore, they dedicate a considerable part of the introduction to a harsh critique of mereology. Their criticism seems to imply that the whole attempt of formalizing the relation of parthood is nothing more than a game, useless for the scientific endeavour. I disagree with both claims. The criteria imposed by (Ladyman and Ross, 2007) is overly restrictive. It would, *de facto*, ban as meaningless almost every part of the contemporary literature on the metaphysics of science. If the aim of metaphysics is that of elucidating the ontological import of our best scientific theories, then unification of more scientific theory is not a reasonable criterion to separate good and bad metaphysics.

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[39] This is, in a nutshell, what scientific realists and antirealists debate about, as we are going to see in §3. I mean that we can test the existence of everyday objects empirically by going where they are and using our senses to perceive them. No analogous empirical proof of their existence can be given for the concept I am referring to in this section.

Furthermore, one can argue that even the metaphysics of outdated theory has some value (Monton, 2008, 2013), especially if one agrees that metaphysical speculation might help scientists in inspiring scientists in their researches. Metaphysics has to be motivated by empirical sciences, yet its aim is not just to propose their unification. Let us turn now to a positive characterization of naturalized metaphysics, starting from what has to be retained from the non-naturalized approaches.

I accept the meta-ontological criteria from the neo-Quinean approach – under some further qualification. The qualification is that such a criterion is *necessary* yet not sufficient. That a scientific theory quantifies over an entity is necessary to take its existence seriously, yet not sufficient by itself. The reason is that no empirical science has yet a 'final theory,' able to explain every phenomenon of interest of that discipline. Rather, scientists have complex and different theories that work in different domains that are often in contrast with each other. Hence, a theory that works in a given context quantifies over an entity is not enough to guarantee the entity's existence insofar as such entity must also be compatible with the existence of the other entities postulated by empirically tested theories in their respective domains. Moreover, I accept a weak form of empiricism, according to which most of the empirical information we receive passes through our senses. As such, that a scientific theory posits an unobservable entity is not enough to guarantee its existence if there is not a scientific explanation of why either we do not perceive it, or we do not perceive it as it is. To sum up, the meta-ontological criteria needs 'compatibility' and a 'phenomenological' (Matera, 2014) conditions that render the Quinean meta-ontological criteria a necessary condition for the existence of the unobservable entities posited by our best scientific theories.

From the metaphysics as the science of possibility view, I inherit formal systems and conceptual analysis's relevance. Indeed, they are useful a priori tools that allow us to obtain an essential terminological clarity – for instance, we can distinguish prima facie identical concepts – and examine the logical implications of a given metaphysical model. This is essential when the a posteriori part of the metaphysical enquiry is conducted. That is when the metaphysical models are applied to individual scientific theories. By checking which metaphysical model of a concept is the correct one in a given theory, we are thus able to understand what the theory tells us about the world (for what concerns such a concept, at least). By doing so in the context of different theories, we are thus able to check whether the same concept has a different meaning in different theories – or not – and whether it is possible to have a metaphysical model which can unify the usage of this concept across theories. Finally, if a particular model of a concept correctly describes how a scientific theory uses it, and such a theory is well-confirmed in a given domain of phenomena, we find an indirect empirical justification in our belief that that object domain is characterized metaphysically by such a concept. Hence, the a priori part of the metaphysical enquiry has an intrinsic value, even for naturalized metaphysicians.[40]

Finally, from the metaphysics as enquiry of the fundamental structures of reality

[40] The claim that there is some intrinsic value in the a priori part of metaphysics' methodology has been vastly defended in the literature. See, e.g., (Dorr, 2010; French and McKenzie, 2012, 2015; Bihan and Barton, 2018).

approach, I accept that metaphysics and sciences aim to describe the same phenomena – the concrete reality that surrounds us. Metaphysics is part of the scientific endeavour rather than an independent discipline. However, since the aim of empirical sciences is not just to understand what happens at the fundamental level, one cannot reduce metaphysics' scope to it either. Otherwise, a huge part of the metaphysical literature would stop – by definition – to count as metaphysics. But if metaphysics is part and parcel of the scientific attempt of understanding reality, it does not make sense to exclude metaphysics of, say, biology (Dupré, 2021), chemistry (Scerri, 2005) or even social sciences (Hawley, 2018) from the range of the metaphysical enquiries – on the base that they do not deal with fundamental structures.
Furthermore, the fact that metaphysics has to be motivated by our best scientific theories and that they both aim to describe the same phenomena does not imply that metaphysics is – somehow – subordinate to empirical sciences. In other words, the work of naturalized metaphysicians is not that of mindlessly repeating what scientists tell us about reality. This is partially explained by the fact, argued above, that science and metaphysics have different aims. Since putting forward proper definitions and models of the concepts assumed by scientists is (one of) the aim of metaphysics, metaphysicians' work is something over and above scientific claims.
Moreover, metaphysics can offer – so to speak – revisionist interpretations of scientific theories, which conflict with how they are usually understood. Explore different possible metaphysics and propose radically new ones is an important part of naturalized metaphysics.[41] Revisionary metaphysics might help other metaphysicians in developing models for more fundamental theories (Monton, 2008) and have a heuristic value for scientists themselves. The idea that philosophy plays a fundamental heuristic view for scientists have been heartily advocated by Popper (1934), and defended in the more recent literature (Haack, 2008; Dorr, 2010; French and McKenzie, 2012). Even among leading scientists, some argued explicitly that not only philosophy play such a role for empirical science, but that such a role is more needed for contemporary science than ever (Pigliucci, 2012; Rovelli, 2018a; Laplane et al., 2019); moreover, some scientists goes as far as engaging directly with metaphysical issues connected with their scientific researches (e.g., Bassi and Ghirardi (2001); Rovelli (2018b); Oriti (2021)). History of science also reveals cases in which reflections on metaphysical implications lead to scientific progress – Einstein's idea of considering ether as a superfluous posit being a paradigmatic example; and many theoretical novelties have been introduced in different sciences through a partially scientific partially philosophical enquiry (Stein, 1989; Sklar, 2000; Friedman, 2001). That being said, one could doubt the value of metaphysics discussed above. For instance, Stanford (2017) argues that, even if there were nothing wrong with this conception of applying metaphysics to science, the heuristic

[41] One might object that if this is so, then naturalized metaphysics is not different from metaphysics understood as the science of possibility. However, there is a fundamental difference. They share the idea that exploring possibilities is part and parcel of the metaphysicians' job. However, naturalized metaphysicians do not claim that these possibilities are metaphysically significant for describing the actual world. Moreover, they do not take their work to be just an exploration of these possibilities.

value above is almost meaningless. Indeed, Stanford argues that there is no way to say when, how, and why a given metaphysical inquiry will advance our scientific understanding of the world. In the sarcastic conclusion of his paper, he remarks that if this is so, then most of the work of naturalized metaphysicians is almost useless – and this should somehow justify the scepticism that philosophers of science and scientists harbour toward metaphysics. Stanford might be right in pointing out that the value of metaphysical enquiries is unpredictable, and, as a result, most contemporary metaphysical works might be useless from the perspective of human history. Yet, this is no reason to stop doing metaphysics. For one, the same could apply to the thousands of experiments and models put forward by scientists themselves in their respective fields. Among the (roughly) million of currently working physicists,[42] it is impossible to say how many of them will advance the scientific understanding of reality. However, no one would conclude on this basis that physicists should stop doing their work.

Let us turn back to the account of naturalized metaphysics I am proposing. So far, we have seen some important features of the account concerning its aims and methodologies and its value as part of the scientific understanding of the world. To conclude, I want to discuss two additional features that naturalized metaphysics should have. They are consequences of taking seriously the idea that metaphysics' methodology follows that of empirical sciences. The first aspect I want to discuss is 'locality.' Empirical sciences' analyses often start from a few phenomena. They are formally modelled through idealization, which consists of ignoring some features of the target phenomena to keep the model under control. Then, the simplistic model is perturbed: small variations in the fixed features and/or new properties are added to the model to check how its predictions on the evolution of the systems change with the introduction of new parameters. Analogously, metaphysicians should apply their metaphysical models to simple cases before moving to more complex ones. However, the fact that a metaphysical structure explains the simplest cases of a scientific theory should not conclude that such a structure is apt to describe the whole range of phenomena accounted for by the theory itself. Metaphysical conclusions are bound to the phenomena investigated rather than being general.

Moreover, it is often the case that one can formulate even a single scientific theory in different mathematical ways – e.g., Heisenberg's and Schroedinger's formulations of non-relativistic quantum mechanics – or that it supports different mathematical models – e.g., the theory of general relativity. If one takes seriously the idea that naturalized metaphysics consists of applying metaphysics to science, then not only metaphysical conclusions are bound to individual scientific theories. They are also bound to the models and the mathematical formulation chosen. Indeed, there is no guarantee that different mathematical formulations will support the same metaphysical structures.[43] On the one hand, it is true that the idea that reality is homogeneous – that is, that metaphysical structures are the same for each scale of energy and

[42] Such estimation is taken from a review (doi: 10.1063/PT.5.010310) conducted by the online journal Physics today in 2015, available here.

[43] For instance, Dirac's formulation of non-relativistic quantum mechanics is such that it seems that non-commutative operators represent physical properties. However, in the algebraic approach

widely different phenomena – is quite widespread, as it is intuitively appealing. On the other hand, our contemporary scientific knowledge of reality is too fragmented to justify such a homogeneity. We have different theories able to model phenomena at different scales and velocities, yet we lack a global vision of reality that would allow us to subsume them under a single theory. What I have in mind here is not just a 'theory of everything,' able to explain every phenomenon whatsoever. Rather, I have in mind the incredible difficulties scientists find in putting together different theories whose target phenomena partially overlap. One can think, for instance, at the problems found in explaining in quantum mechanical terms complex chemical reactions.[44] Insofar as many aspects of reality are still missing for a unified picture of reality, we cannot request to a metaphysics applied to empirical sciences the discovery of the most fundamental and general structures of reality. Rather, naturalized metaphysicians must accept that their analyses are bound to the scope of the scientific theory of interest. The metaphysical conclusions drawn from a scientific theory are true only for the phenomena that the theory itself describes. The generalisation process to other scientific theories follows the metaphysical investigation of individual theories. In other words, that nature has homogeneous structures is something that has to be discovered rather than presupposed.

The second aspect that naturalized metaphysics has to inherit from empirical sciences is its fallibilist trait. As argued already by some metaphysicians (Ladyman and Ross, 2007; Monton, 2008; McKenzie, 2020), it is highly likely that the metaphysical conclusions we can draw from contemporary science will turn out to be false in the future. This is so because our current best scientific theories will (most probably) be replaced by more fundamental theories – thus causing a revision of the metaphysics drawn from them. The succession of scientific theories is part and parcel of scientific progress. Hence, the fact that our best metaphysics drawn by our best scientific theories will be superseded in the future has to be accepted as an intrinsic consequence of metaphysics being part of the scientific endeavour.

2.2.3 Long live the Metaphysics?

So far, I have presented different meta-metaphysical views, and I argued in favour of one of them, i.e., naturalised metaphysics. As presented so far, it seems that the view I proposed is far better than the others. However, this is far from the truth. Indeed, naturalised metaphysics presents some problematic features which potentially undermine the whole approach. I discuss here what I take to be the most relevant ones, in the form of objections to the view. To be fair, I do not think there are knock-

to quantum mechanics, they do not play a role in the formalism (Cf. Cinti, Corti & Sanchioni, 2021).

[44] To explain complex chemical reactions in quantum mechanical terms, a great deal of approximation schemes and computer simulations are involved. And yet, their efficiency depends on the accuracy required. See (Baiardi et al., 2021), for a review on new approaches to modelling chemical reactions paths with quantum mechanics.

down answers able to address the objections fully, as is often the case in philosophy. Nonetheless, they are sensible meta-metaphysical points that naturalised metaphysicians should address – in a way or in another – to propose a convincing account of their discipline.

Underdetermination. A first objection to naturalised metaphysics can be put forward by simply noticing that the metaphysics of a scientific theory is always underdetermined. That is, a scientific theory is often compatible with more than one metaphysics. If this is so, then the very project of understanding what scientific theories tell us about the world in precise metaphysical terms appear to be bound to fail from the very beginning. If there are no objective ways of choosing between different rivals metaphysics of a single theory, how can we know what is the real metaphysical import of that theory?
In my view, there is not much room to address such an issue but to bite the bullet: the metaphysics of scientific theories is underdetermined, and there are no objective ways of choosing between different rival theories. We can put forward some criteria, as argued by some authors (Benovsky, 2016), or use the super-empirical virtues – as scientists do. However, these criteria are not truth conducive, and there is no objective way of establishing a hierarchy among them. That being said, underdetermination is not a problem of *naturalised metaphysics*. Rather, it is a problem of any approach to metaphysics: there are always many metaphysical accounts of a given phenomenon of interest, and it is not possible to choose among them by empirical means. This is true of metaphysics in general, no matter which meta-metaphysical view is held. Moreover, underdetermination plagues empirical sciences as well.[45] The same phenomena can be modelled by different scientific theories which explain it in radically different terms. Therefore, one can argue that underdetermination is a problem of trying to explain phenomena *in general*.[46] That being said, metaphysical underdetermination is not always equal. In some cases, the fact that there are two theories that are able to describe the same phenomena is really problematic, as those theories actually tell us something different about the world. In other cases, it might well be that the theories are equivalent and that they differ in their description only on the basis of modelling choices. These choices reflect the preferences of those who crafted the theory rather than genuine differences in the description of the target phenomena. That is, there might be alternative ways of describing the same reality exactly because the languages we use – mathematics, formal and natural languages – are such that things can be described in equivalent ways. For instance, in metaphysics, it has been argued that some debates are just verbal disputes (Jackson, 2014). Perhaps this is true of some debates and false for others.
Now, a proper characterisation of theoretical equivalence and its implication would bring us too far from the topic of the thesis since it is a really controversial issue in

[45] See (Ladyman, 2012, Ch. 6) for an introduction on underdetermination of scientific theories.

[46] For sure, genuine cases of established rival theories aimed at explaining the same set of phenomena are rare among empirical sciences yet almost ubiquitous in metaphysics. However, the problem of underdetermination concerns the epistemic foundation of a discipline. As such, how widespread such a phenomenon is, does not make it less or more problematic.

the philosophy of science.[47] What I just wanted to highlight in this paragraph is just that underdetermination differs from case to case. Hence, one cannot put forward general guidelines on how to solve it.

The absence of a theory of everything. I argued above that an important lesson metaphysicians have to learn from empirical sciences is fallibilism. We must be aware that in doing metaphysics out of scientific theories, it is highly likely that our current best metaphysics is false. This is so because the scientific theory from which we draw metaphysical conclusions is highly likely false, and a better theory will replace it in the future. As mentioned above, this is a well-known and accepted aspect of naturalised metaphysics. However, one might argue that the lesson we should draw from fallibilism is different: naturalised metaphysics is only worth practising when we will have a theory of everything. The idea is that if metaphysics aims to explain how reality is according to our scientific models, we should wait for a scientific understanding of the whole reality before doing metaphysics. In the absence of such a theory, or so the argument goes, the metaphysical conclusions we infer are only partial and do not reveal anything about how reality is.

By taking seriously the idea that metaphysics is part and parcel of the same inquiry to reality in which empirical sciences take part, worries along the lines above seem to be less pressing. If the aim of metaphysics is to tell us what individual scientific theories tell us about the world, then the true value of those theories is – to a certain extent – irrelevant. Metaphysical theses are conditionals of the form 'if theory x about phenomena $y, z, \ldots$ is true, then reality is as such.' Metaphysics is responsible for establishing these conditionals, yet the truth of the antecedent concerns science itself. The role of the metaphysicians is not that of judging which scientific theory is true, but to understand what is its ontological import *if true*. Furthermore, when a new theory replaces an old one, the former often retains a crucial aspect of the latter. When so happens, it might well be that the metaphysical import of the inherited features of the old theory will be a tile in the more complex metaphysical description of the new one. Thus by doing the metaphysics of 'false' theories, we might as well gain insights on the metaphysical aspects of the theories which will replace them (Monton, 2008, 2013). If we add to the picture the heuristic value that metaphysics plays in the scientific understanding of the world, we can conclude that metaphysics is worth to be practised even in the absence of a theory of everything: if we sit and wait for too long, it is highly likely that when we have a theory of everything, we will lack the conceptual machinery to understand what it actually tells us about the world.[48]

Progress is not inherited. In a recent paper, McKenzie (2020) proposes an interesting argument against the idea that naturalised metaphysics is epistemically more

[47] See, e.g., (Barrett and Halvorson, 2016). For arguments in favour of the idea that theories with different formulations are never metaphysically equivalent, see (Sider, 2020).

[48] And if one thinks about how troublesome is to put forward a coherent ontology of non-relativistic quantum mechanics, let alone its relativistic counterpart, one can feel that, perhaps, metaphysics waited already too much.

grounded than 'neo-scholastic' metaphysics. The gist of the argument is the following. As I argued above, the idea that reality can be investigated by a priori means is fragile from an epistemic point of view. On the contrary, the a posteriori methodology of empirical sciences proved itself epistemically reliable. The hope of naturalised metaphysicians is exactly that of inheriting a solid epistemic ground from empirical sciences by taking as a starting point of metaphysical theorising our best scientific theories. However, McKenzie (2020) argues that this hope is forlorn. That currently held scientific theories will be replaced in the future is part and parcel of scientific progress. Indeed, scientific progress is explained by a better approximation to the truth. A scientific theory is replaced when there is a new theory that is able to describe more features of the target phenomena, do more accurate and new predictions, put forward more explanation, etc., and still retain all the explanatory power of the older theory. The idea is that the new theory is more true than the older one, because it gives a more detailed description of reality. However, McKenzie (2020) argues, if the notion of approximate truth makes sense for empirical sciences, it does not work for metaphysical theories. For instance, it does not make sense that 'Humeanism' is approximately true: either laws of nature are explained in terms of regularities or not. There is no progress if there is no approximation to truth in the succession of metaphysical theories. And if there is no progress, and we know already that the metaphysical theses we derive from our best scientific theories are false, then the epistemic ground naturalised metaphysicians hoped to inherit from sciences is lost. If this is so, then naturalised metaphysics is not epistemically better than the neo-scholastic one, or so the argument goes.

As McKenzie (2020) notices, there are many parts of the argument that can be resisted. In particular, two of the premises of McKenzie (2020)'s argument – metaphysics aims to investigate the fundamental structure of reality, and metaphysical views are general and not bound to single scientific theories – are explicitly rejected in my account of naturalised metaphysics. Finally, I remarked above that doing metaphysics out of scientific theories is a valuable enterprise even if the scientific theories in question are false or less true than those that replaced them.

Hence, McKenzie (2020)'s argument is not troublesome for my understanding of naturalised metaphysics. However, the argument is incredibly interesting from my point of view because the analysis of scientific progress implicitly assumes a strong form of scientific realism (Psillos, 1999).[49] Indeed, the thesis that scientific progress is achieved only through a better approximation to reality can be defended only if it is assumed beforehand that what science does – or aims at – is exactly that of giving a true description of the world.[50] However, the connections between scientific realism and naturalised metaphysics are less obvious than how they seem. This brings us to the last objection to naturalised metaphysics that I discuss in the chapter.

[49] The next chapter of the thesis is dedicated to scientific realism and antirealism. Therefore, I do not belabour on the details of these views here. For the present chapter, the following gloss is more than enough: scientific realism is the view that science gives an (approximately) true description of the world, both in its observable and unobservable characters; Scientific antirealism can be defined as the negation of realism.

[50] For a primer on philosophical accounts of scientific progress, see (Niiniluoto, 2019).

Scientific Realism. As hinted at the end of the last paragraph, naturalised metaphysics seems to need as a background assumption a strong form of scientific realism. If one does not believe that scientific theories are a description of reality, then doing metaphysics out of them would seem a hopeless endeavour from the very beginning. On this basis, naturalised metaphysics has been criticised (Jaksland, 2016). Scientific realism is a highly controversial thesis itself among philosophers of science. It is quite safe to assume that an approach built upon a highly contentious thesis is as shaky as possible. What is worse is that scientific realism, at least in its more strong variants, relies on a certain meta-metaphysics. Or so I argue below.

A standard formulation of scientific realism (Psillos, 2005a,b) describes it as a threefold thesis that consists of a metaphysical, a semantic and an epistemic component. The first two are:

- *The metaphysical thesis:* The world has a definite and mind-independent structure.
- *The semantic thesis:* Scientific theories should be taken at face value. They are truth-conditioned descriptions of their intended domain, both observable and unobservable. Hence, they are capable of being true or false. The theoretical terms featured in theories have putative factual reference. (Psillos, 2005a, p. 688)

The first thesis coincides with 'metaphysical realism', the view that the external world is independent of human perception, existence and conceptualisation. In contrast, the second thesis is usually understood as a form of a correspondence theory of truth: the truth value of the propositions we utter depends on whether the world is as the proposition describes it. The classic example is that the proposition "the snow is white" is true because the content it expresses corresponds to a mind-independent fact – i.e., that the snow is white. According to scientific realists, our best scientific theories are (at least partially) true in the same sense.

Now, both theses are metaphysical, and they are controversial themselves. But what is relevant here is that these metaphysical theses are not motivated by our best scientific theories. Rather, they are part and parcel of old good a priori metaphysics, so fiercely criticised by (Ladyman and Ross, 2007). If strong forms of naturalised metaphysics affirm that *every* metaphysical view has to be motivated by our best scientific theories, then the core theses of scientific realism cannot be seen as 'good' metaphysical theses. And if these theories must be discarded as, so to speak, bad metaphysics, then also scientific realism has to follow the same fate. And this is obviously problematic insofar as scientific realism was the pillar of the very strong forms of naturalised metaphysics.

On the one hand, the contradiction above is a prima facie incoherence that needs to be addressed by friends of naturalised metaphysics. On the other hand, this shows that scientific realism and one's meta-metaphysical view are necessarily intertwined. As a stance on the debate about scientific realism implies a particular meta-metaphysical stance, an account of naturalised metaphysics is incomplete if no explanation of what science aims to do is also explained.[51] This suggests that a way of addressing the problem is that of taking a clear stance on the debate on scientific realism. Moreover, a stance on this debate might also help naturalised metaphysics to address some of the concerns raised above. To me, there are two possible ways the friends of naturalised metaphysics could take. The first, and perhaps more natural one, is to bite the bullet endorsing a strong form of scientific realism. This would imply, however, a softening of the naturalised constraint which has to be put on the metaphysics by accepting that (at least some) metaphysical debates are meaningful even if they are not motivated by our best scientific theories. The second consists in the exact opposite move: relaxing, within scientific realism, the metaphysical and semantical theses, thus maintaining a strong naturalised attitude toward metaphysics.

The first is the most natural to defend; and possibly the most straightforward to articulate – as strong forms of scientific realism have been defended in detail already. However, the second is, to me, more appealing. Not only because it is less searched in the literature but also because it seems prima facie contradictory to soften the metaphysical thesis of scientific realism if we aim to defend an approach to metaphysics. Nonetheless, if we want to take the fallibilist character of metaphysics seriously, perhaps we should give up the idea that we can describe reality as it is. Rather, we should accept the idea that the whole scientific project, of which metaphysics is part, consists in putting forward models of reality with no certainty whatsoever of whether we succeed or not. The problem is how to defend such a view without trespassing into scientific antirealism. The next chapter of the thesis (§3) is dedicated to an articulation of an account that, within the bounds of scientific realism, gives justice to this idea.

2.3 Conclusions

The aim of the chapter has been to answer the central question concerning the legitimacy of metaphysics: whether metaphysics can grant us knowledge about the world and, if so, how. I started by presenting three different approaches to metaphysics that share the views that (i) metaphysical enquiry is conducted a priori, (ii) metaphysics is more fundamental than empirical sciences (either epistemically or ontologically) and, as a consequence, (iii) metaphysics is independent of other branches of knowledge. I discussed each view separately, and I outlined some problems. In particular, I highlighted that a priori methods have a weak epistemic ground and, as such, they cannot guarantee any form of knowledge about the actual concrete world.

Then, I turned to a fourth meta-metaphysical view which seems to be increasingly popular in the recent literature, which comes under the name of 'natural-

[51] That the two debates – scientific realism and whether metaphysics has to be naturalised – are strongly connected can be further appreciated if one thinks that structural realism – advocated, inter alia, by (Ladyman and Ross, 2007) – is both a particular stance on scientific realism and a metaphysical doctrine.

ized metaphysics.' I proposed my own understanding of naturalized metaphysics by discussing, first, the differences between metaphysics and empirical sciences. Second, what aspects of non-naturalized approaches must be saved, and some essential features of naturalized metaphysics' epistemology. Finally, I presented some problems that naturalized metaphysics has. In particular, I argued that one's meta-metaphysical view is closely linked to their stance on the debate of scientific realism and antirealism. Thus, an account of naturalized metaphysics is incomplete if no clear stance on how science gives us knowledge about the world is taken, and vice-versa. Finally, I stressed that there are two main ways of declining the relationship between naturalized metaphysics and scientific realism: either accepting a strong form of scientific realism – with the consequences highlighted above (§2.3) – or weakening the metaphysical component of scientific realism itself. The next chapter (§3) will be devoted to exploring the second way.

Chapter 3
Science and Metaphysics

3.1 Realisms

The debate about the existence of unobservable theoretical entities (unobservables, hereafter) posited by our best scientific theories plays a central role in the philosophy of science. Indeed, such a debate is directly or indirectly connected to most of the other big questions that philosophers of science have at heart, e.g., What is a scientific theory? What is progress? When is a theory confirmed? The debate divides into two main camps the philosophers of science involved. On the one hand, Scientific Realists argue that scientific theories are (approximately) true descriptions of the world. Hence, unobservables exist; moreover, their existence is independent of human beings' natural languages, conceptualization, epistemic status, and so on (Psillos, 1999; Chakravartty, 2007).[1] Furthermore, scientific theories' truth is understood as direct correspondence with the mind-independents structures of reality. The correspondence theory of truth (Koons and Pickavance, 2017, Ch. 2) is a metaphysical theory according to which sentences are either true or false *simpliciter*, as follows. The meaning of an (assertoric) sentence is a non-representational and linguistic entity called 'proposition'. A sentence is true if the world is as the proposition – expressed by the sentence – describes it, and false otherwise.[2] Our best scientific theories are true, according to scientific realists, in the same way: they are made true by mind-independent facts of the world. On the other hand, Scientific antirealists reject one of the claims above. Antirealists, for example, claim that the science aims at 'empirical adequacy' rather than to truth, or the epistemic ground to commit ourselves ontologically to the existence of unobservables is insufficient (van Fraassen, 1980; Laudan, 1981). As the general description above suggests, 'scien-

[1] In the rest of the chapter, I stick to 'mind-independent', to avoid unnecessary redundancies. The reader who holds an antirealist view according to which the relevant entities depend on something else, they could read into my 'mind-independent' their favourite kind of dependency.

[2] Here it is a classic example of how the correspondence theory works (Tarski, 1983; David, 2020): the sentence 'the snow is white' is true in virtue of the fact that its meaning corresponds to a mind-independent state of affairs, i.e., that the snow is white (Fine, 2017).

tific realism' and 'scientific antirealism' are umbrella terms rather than individual philosophical views. They are families of philosophical accounts that differ significantly from each other. However, the views belonging to the same camp are pooled by accepting a minimal set of theses. Hence, general discussions about scientific realism and antirealism are possible – by focusing on the theses shared throughout each camp – with the proviso that some conclusions might need substantial revision once individual members of the camp are considered.
From a general perspective, philosophical views that claim that some entities a_1, $a_2 \ldots a_n$ exist are called 'realist' position about a. Realism about a kind of entities a hinges, usually, on two claims (Miller, 2016):

(i) a_1, $a_2 \ldots a_n$ exist.
(ii) a_1, $a_2 \ldots a_n$ are mind-independent.

Realists about the existence of a not only hold the conjunction of (i) and (ii), but usually add an epistemic claim to them. Something along the line of:

(iii) Human beings can know that (i) and (ii) are true.

While (i) and (ii) seems to be necessary for realism,[3] weaker and stronger forms of it differentiate themselves on how (iii) is formulated. Antirealism, instead, can be defined as the negation of realism. Antirealists positions deny that a_1, $a_2 \ldots a_n$ exist or are mind-independent, or that, since (iii) is false, we have no rational reasons to believe in (i) and (ii). Now, one can hold a realist or antirealist position for every kind of entity. Mathematical entities, God, properties, non-individuals (entities that lack self-identity), moral values, are just a few of the many entities whose existence has been disputed by philosophers. Insofar as debates concerning realism range over really different entities, it is quite natural to think that realist and antirealist stances are largely independent of one another. That is, there is nothing contradictory in being, say, a realist about mathematical entities and an antirealist about moral values or non-individuals. For sure, some combinations are more 'natural' or more easily defensible. For instance, realists about God tend to be realists also about moral values, insofar as one can think that God's very existence ground the reality and mind-independence of objective moral values. Despite some connections, the debates remain logically independent from each other, and any combination of realism and antirealism about these entities is a defensible one.

[3] As presented by Schaffer (2009), one of the central notion of neo-Quinean metaphysics (§2.2.1) is that of grounding. According to this approach, whether something exists is trivial: almost any entity we can think of exist insofar as we quantify over them. However, what is philosophically relevant is what *ground* these entities; that is, whether these entities' existence depends on other entities or not. This metametaphysical approach shows that (i) is not enough to characterize realism. Indeed, everything exists according to this approach: God, numbers, unicorns, and (almost) whatever entity you can think of. Yet, Schaffer (2009) argues that, e.g., in his view, God exists but his existence is grounded in human beings' own existence: it is a product of human beings' imagination, believes and thoughts. Now, such a view does not seem like a form of theism. Friends of theism do not just want to claim that God exists. Rather, they would insist that they mean something different from what Schaffer has in mind with' existence.' For this reason, (i) seems too weak to capture a form of realism without any qualification of what it is meant by existence – qualification provided by (ii).

The debate concerning *scientific* realism is just one of the realists/antirealists debates sketched above. As such, one would presume that such a debate is independent of other disputes about the existence of other entities. However, such logical independence seems to have been lost in contemporary literature. Indeed, now-standard presentations of scientific realism define it as if *metaphysical realism* were one of its essential components (Psillos, 1999; Chakravartty, 2017; Wright, 2018); where metaphysical realism is the view that the is a mind-independent world (Khlentzos, 2016). However, even if these debates are connected, they still are logically independent: there is no argument that shows that a stance on one debate entails a given position on the other. Any combination of scientific and metaphysical realism and antirealism is a possible view. Indeed, even the prima facie less appealing combination of the four, i.e. scientific realism and metaphysical antirealism, have been advocated in the past.[4]
As mentioned at the end of the previous chapter (§2.2.3;§2.3), the account of naturalised metaphysics I proposed is in tension with scientific realism, which, in turn, seems to be one of its main prerequisites. The tension consists in the fact that metaphysical realism – the view that there is a mind-independent external world – and the correspondence theory of truth[5] are non-naturalized metaphysical theses. These views, indeed, are part of what (Ladyman and Ross, 2007) call 'neo-scholastic' metaphysics and are not motivated by our best scientific theories. If naturalized metaphysics needs scientific realism, and, at the same time, it declares as meaningless two of the three core tenets of scientific realism, then one could conclude that naturalized metaphysics is self-defeating. As I suggested already §2.2.3, there are two possible ways out from this impasse. The first one might be weakening my account of naturalized metaphysics, acknowledging as meaningful some metaphysics non-motivated by our best scientific theories. This, in turn, would request a rethinking on the whole account of how metaphysics should be practised – redrawing the borders between, so to speak, good and bad metaphysics. The second way out consists in investigating whether one could weaken the metaphysical commitment of scientific realism to make it compatible with the form of naturalized metaphysics presented before. This chapter aims to explore this second route. My objective will be to present a form of scientific realism according to which sciences aim to provide the best description of reality possible at a given time, even though there is no rational way to prove that such a picture describes reality's mind-independent structures. Moreover, according to this view, metaphysics – as articulated in §2 – is part and parcel of the scientific enquiry.

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[4] Most notably by Putnam (1981). Some logical positivists – that famously rejected metaphysical realism as a meaningful view – held stances similar to scientific realism (Carnap, 1950; Feigl, 1943; Schlick, 1932).

[5] There are different accounts of the correspondence theory of truth. Accordingly, some of them are not as metaphysically loaded as I characterized them. However, the most common way of presenting the view – in the form of truthmaker semantics – is metaphysically loaded. One can appreciate such a fact more if one considers that it has been argued that truthmaker semantics either involves grounding (Rodriguez-Pereyra, 2006) or should be abandoned directly for an account of correspondence in terms of grounding (Fine, 2017). Where grounding itself is a highly controversial metaphysical thesis itself (Wilson, 2014).

The structure of the chapter is the following. First (§3.2), I argue that the minimal claim that separates scientific realists and antirealists is devoid of any metaphysical commitment. In other words, the disagreement between scientific realists and antirealists stands even if we bracket metaphysical realism. Furthermore, I briefly discuss some sceptical views about the whole debate concerning unobservables to characterize the minimal claim above better. Second (§3.3), I provide a characterisation of metaphysical realism to introduce a further view – quietism – which is neither realism nor antirealism. Finally (§3.4), I argue that by assuming metaphysical quietism, one can still make sense of the debate between scientific realists and antirealists. As such, it is possible to craft a view, which I dub 'Scientific yet Quiet Realism',[6] which drop metaphysical realism in favour of metaphysical quietism. The conclusions of the whole first part of the thesis end the chapter (§3.5).

3.2 Scientific and metaphysical realisms

If the debates over scientific and metaphysical realism are independent, it should be trivial to settle whether scientific realists are committed to metaphysical realism. It follows from the very fact that they are logically independent that the answer is 'no.' Yet, some of the most prominent scientific realists characterise their view with a robust metaphysical commitment. If such a component were an essential part of scientific realism, it would follow – by definition – that scientific realists are committed to the stronger form of metaphysical realism. Psillos (1999) is quite explicit on this point. In his now-standard definition of scientific realism, the view hinges on three theses. The first is metaphysical, whilst the other two are, respectively, semantic and epistemological (Psillos, 2005a). The metaphysical theses is a clear commitment to a strong form of metaphysical realism:

> *The metaphysical thesis*: The world has a definite and mind-independent structure. (Psillos, 2005b, p. 688).

From the considerations above, two conclusions are possible.[7] Either the standard characterisation of scientific realism is the right one, and so the scientific and the metaphysical realism are not logically independent; or they are logically independent, and the standard definition of scientific realism is the characterisation of a particular form of scientific realism – which happens to be one of the most shared – rather than of what scientific realists agree on. I argue now that for the second conclusion.

As remarked before, 'scientific realism' is an umbrella term that pools views that differ significantly from each other. To the point that saying 'I am a scientific realist'

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[6] An earlier presentation of this view has been published in (Corti, 2020).

[7] A third one might be that whether scientific realists are committed to metaphysical realism is not really substantial, as it depends only on how we define the views. The problem, in other words, is semantical and far from being philosophically relevant. As I argue below, I think that the problem concerns definitions, yet it is substantial on its own.

is almost uninformative, if one does not specify whether they are convergent realist (Putnam, 1982a; Doppelt, 2007), deployment realists (Alai, forthcoming), entity realists (Hacking, 1982; Cartwright, 1983), perspectival realists (Massimi and McCoy, 2020), semi-realists (Chakravartty, 1998), structural realists (and if so whether they are ontic or epistemic structural realists) (Worrall, 1989; Ladyman, 1998) – to quote just the most well-known variations on the theme of scientific realism. Since 'scientific realism' is a label under which all the views above gather, putting forward its definition is not a matter of tastes or conventional choices. Rather, defining scientific realism means dissecting these views to extract, so to speak, their least common denominator. The main claims upon which each philosopher build their view about science and its aims. I argue now that there are three reasons not to consider metaphysical realism one of the least common denominators of scientific realism. For the sake of clarity, my aim is not that of arguing that scientific realists should not posit metaphysical realism as the cornerstone of their views. Rather, that being scientific realists does not *automatically* mean being also metaphysical realists.[8]

Firstly, no point of disagreement between scientific realists and antirealists hinges on metaphysical realism. In fact, modern antirealists only challenge scientific realism's semantic and epistemological components. By itself, this could be a contingent issue, insufficient for arguing that metaphysical realism is inessential to scientific realism. However, a strong case could be made if one could make sense of the debate between scientific realists and antirealists without appealing to metaphysical realism. And I do think that one could make sense of the debate without relying on metaphysical realism, as the following thought experiment shows:

> Suppose a philosopher, let us call him Al, is a solipsist. That is, Al believes to be the only real entity. You, me, the external world which surrounds us is, according to Al, a product of his mental activity. Perhaps Al thinks to be a brain-in-a-vat, the character of the film Matrix or a Leibnizian monad – the details do not really matter here. What matters is that every entity Al perceive is, according to him, just one of his *ideas* – their existence is grounded in the existence of Al himself. When Al dies, or so he thinks, everything else will disappear with him. Despite reality being a product of Al's mind, Al notices that his reality is made of regularities. Different objects fall to the ground at the same speed (when air friction is neglectable), the sun always rises and sets from the same direction, and whenever he presses the 'a' key on his laptop, an 'a' appears on his Word document. Note that Al believes in the existence of those entities: "Of course my laptop, the sun, and whatever it takes you are there" – Al would say – "yet their existence is bound to mine. They exist, albeit all of them are less fundamental than me," he would hastily add. During his life, Al notices that some of his ideas – called 'scientists' by the many people-ideas surrounding him devote their existence to the study of the regularities above. Scientists claim that some of these regularities are due to unobservable entities. For example, scientists explain the functioning of Al's laptop in terms of small particles moving inside it, which are responsible for what they call 'electrical current.' Al believes these scientists, to the point that he believes these unobservable exist even though Al cannot perceive them directly. That is, unobservables are *as real as* Al's other ideas, such as his laptop, the sun and scientists themselves. Al and his friend Lucy strongly disagree on two things. First, even if Lucy is a solipsist as well, she believes to be the most fundamental entity. Second, Lucy believes that the theories put forward by scientists do not describe the external world she perceives. These theories allow

[8] And, as a consequence, arguments in favour of scientific realism do not translate into arguments for metaphysical realism.

> her to make accurate predictions about the phenomena she experiences. Yet, she thinks there are no good epistemic reasons to believe that the unobservables posited by these theories exist in the same sense in which the sun, Al's laptop and scientists themselves do. She believes only in the existence of what she can directly perceive and nothing else.

Why should Al think that unobservables exist in the same way as the objects he perceives in his everyday life do? Al's argumentation would be along the following lines. Al would start by noticing that scientists' predictions are really accurate. So accurate that scientists have been able to build sophisticated machinery such as laptops that work according to their predictions. Now, scientists explain the functioning of these machineries – and many other phenomena as well – in terms of unobservable entities – electrons, in the case of Al's laptop. If electrons do not exist, Al continues, then the fact that the laptop works as it does would be miraculous. Since Al is sceptical of miracles, he concludes that electrons existence must not be really different from the laptop's existence..[9] According to Al, though, neither electrons nor his laptop existence has to be understood in a metaphysically loaded way: they are not part of a reality independent from Al's mind. However, it is not clear that Al's metaphysical antirealism makes him a scientific realist. Surely, he would disagree with other scientific realists about unobservables' mind-independence. But also Lucy would disagree with her fellow scientific antirealists about the mind-independence of every day's entities. Yet, Al's disagreement with Lucy seems stronger: they profoundly disagree on whether we are rationally entitled to believe that the scientific practice discovers something about what surrounds us – be it mind-independent or not. Accordingly, Al would strongly disagree with instrumentalists – claiming that scientists' speaking of unobservables must not be taken literally – and empiricists – agreeing with Lucy that no commitment about unobservables has to be taken. Hence, it seems that Al would take the parts of scientific realists in the dispute with scientific antirealists. To me, it seems that Al and Lucy are indeed, respectively a scientific realist and an antirealist; and their disagreement has nothing to do with their take on metaphysical realism. To me, the thought experiment shows that the debate between scientific realists and antirealists makes sense even if metaphysical realism is bracketed. Hence, there is no reason to consider the latter an essential component of scientific realism.

Secondly, it is a fact that contemporary scientific realists *want* to commit themselves to metaphysical realism to strengthen their views. Some posit it at the very heart of scientific realism Devitt (1991); others go as far as to claim that it is the least that a scientific realist has to claim Massimi (2018). However, that they accept metaphysical realism because they want to strengthen their views is not enough to establish that such a commitment is necessary. To my knowledge, only one argument goes in this direction, and it is due to (Psillos, 1999, Ch. 3). The argument is supposed to show that "any meaningful defence of scientific realism" (Psillos, 1999, p. xviii) requires a commitment to metaphysical realism. To show this, Psillos argues at length that Carnap's view – which can be summarised as a form of metaphysical antirealism and scientific realism – is incoherent. Whether Psillos' attack on the Carnap-

[9] As the reader will have already noticed, the structure of Al's argument retraces that of the 'No-Miracle Argument' (Putnam, 1975, p. 73).

pian view is successful or not, it still fails to establish the ambitious conclusions he wants to argue for. First and foremost, because showing that Carnap's view fails is not enough to rule out any attempt of combining scientific realism with stances different from metaphysical realism. The incoherence of Carnap's stances does not imply that metaphysical realism is an essential part of scientific realism. Second, Psillos assumes that the only alternative to metaphysical realism is antirealism. This is not entirely correct, as we are going to see in what follows (§3.3). Third, Psillos main targets are, to me, views that attempt to stay neutral on the debate between scientific realism and antirealism. By his argumentation, it is clear that Psillos thinks that there is a clear cut boundary between the two camps. However, he does not put forward any argument – aside from the critique of Carnap's view – in favour of the thesis that the demarcation between the two is metaphysical realism.[10]

Thirdly, and finally, the now-standard presentations of scientific realism are historically contingent, in the following sense. In contemporary philosophical literature, metaphysical antirealisms are "beyond the pale of serious possibility" (Rosen, 1994, p. 277). Hence, metaphysical realism taken as an assumption seems to the vast majority fairly innocent, if not a matter of common sense. On the one hand, it is true that those that nowadays dispute metaphysical realism are few, and they are by far a minority among their peers (Khlentzos, 2016). On the other hand, though, metaphysical antirealism found some advocates also among scientists that advocated it allegedly on scientific – rather than purely philosophical – grounds (d'Espagnat, 1979; Wheeler, 1989). And as a matter of fact, some interpretations of quantum mechanics are at odds with realists interpretations to the light of some paradoxical consequences of the theory.[11] Moreover, views that combine metaphysical antirealism with scientific realism, such as those inspired by the work of Kant and logical positivists, are old-fashioned for today's standard. They have been harshly criticised in the past, and their commitment to metaphysical antirealism is of no help for their contemporary defence.[12] However, the tastes of our contemporary peers do not dictate what is and what is not logically independent. Analogously, the scarce defensibility of the combination of scientific realism and metaphysical antirealism does not amount to scientific realists' commitment to metaphysical realism. Exactly because the denial of metaphysical antirealism does not coincide with realism – as I argue for below (§3.3).

I take the three reasons above to provide reasonable evidence that the minimal claim one has to accept to consider themselves a scientific realist does not involve any metaphysical commitment. Let us see what such a minimal claim could be instead. Since childhood, human beings learn how to speak about what they experience.

[10] And it would be quite strange to do so, if one thinks about it. Indeed, notorious antirealists such as van Fraassen (1980) firmly defend metaphysical realism. Hence, the acceptance of metaphysical realism can be hardly seen as what divides scientific realists from antirealists.

[11] That quantum mechanics needs a revision of our commitment to metaphysical realism has been argued in (Fuchs, 2016). For a paradox of quantum mechanics that shows some tensions between the theory and strong forms of metaphysical realism, see (Frauchiger and Renner, 2018). See (Corti A., Forthcoming) for a philosophical review of the paradox.

[12] See, e.g., Field (1982)'s criticism of (Putnam, 1981).

Moreover, human beings tend to develop quickly a picture of what the world is like: we assign names to object, and we build beliefs about their behaviour.[13] When human beings grow up, their understanding of what surrounds them becomes increasingly complex and sophisticated. Yet, our physical world's beliefs are not – on their own – metaphysically loaded. That is, claiming that 'there is a laptop in front of me', say, is not by itself a philosophical statement but a commonsense discourse. We refer to what we perceive and attribute to other human beings' perceptions without any commitment to how things are independently from ourselves. For sure, we build expectations based upon past experiences – I expect to find my laptop where I left it yesterday – which are largely independent of our thought. Yet, are these very expectations that support the truth value of our everyday discourses,[14] rather than a philosophically elaborated view about metaphysical realism. To me, the thought experiment of Al and Lucy shows that the minimal claim to which scientific realists must commit is that unobservables' status is at least the same as that played by everyday objects in the – admittedly quite sketchy – picture above. That, say, 'electrons are responsible for the electrical current that makes my laptop work' is true in the same sense in which 'there is a laptop in front of me' is. To put it in a catchphrase, the scientific realists' minimal claim (**SRMC**, hereafter) should be something like:

> **(SRMC)**: Unobservables – as posited by our best scientific theories – are at least as real as the everyday entities perceived by human beings.

The idea behind **(SRMC)** is that human beings develop a naive narrative of the world; such a picture is then further enlarged and explained by scientific enquiries. The naive and the scientific representations of what surrounds us are part and parcel of the same picture, rather than being in opposition.[15] This does not mean that scientific discoveries cannot contradict our naive understanding of the world. The history of science actually reveals that the opposite is the case. In this sense, scientific discoveries are often revisionary. Yet what I mean when I said that scientific explanations are part and parcel of the same picture is: (i) they do not concern a different world from the one we observe empirically in everyday experience; and (ii) to be trustworthy, scientific theories must provide an explanation of why the empirical world is different from our perception.[16] To put it differently, **(SRMC)**

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[13] The process of creating believes about the physical world starts quite early. See (Baillargeon, 1996, 2004)for reviews on some aspects of infants' belief about the physical world.

[14] A small notice, to clarify further what I mean. Suppose scientists find proof that there is only one object in the world: the universe itself. That is, Horgan and Potrč (2000)'s existence monism is true. Even if it were the case, everyday discussions and agreements about what we perceive would be possible. We would still continue to understand what we mean by 'there is a laptop in front of me;' and if someone says that there is indeed a laptop in front of me because we both perceive one, we would be able to understand what they mean by saying that such a sentence is true even if – given existence monisms – it might be strictly speaking false.

[15] Note that this is controversial among scientific realists. See (Sankey, forthcoming) for a defence of this claim.

[16] The reader will remember the 'phenomenological' gloss I put on the neo-Quinean meta-ontological criterion in §2.2.2.

provides the idea that the meaning of '*x* exists' or '*x* is real' is the same independently from whether *x* is a every day's entity or an unobservable posited by our best science. And this is independent of any particular explanation of what 'existence' and 'being real' mean.[17] That is why the claim is, after all, *minimal*. It presupposes an understanding of reality just in empirical terms, with no commitment whatsoever to "the illusory, the fictitious, and the purely conceptual" (Feigl, 1943, p. 386) sense attached by metaphysical realists to the world.
Now that we distilled the minimal claim that separates scientific realists from antirealists, we have to characterise metaphysical realism and antirealism to analyse how they fit with scientific realism. Before turning to this task (§3.3), it is worth say something more about **(SRMC)**. In particular, it is instructive to discuss a view which – prima facie – endorses **(SRMC)** as its main tenet.
To be fair, one can find many claims that share the spirit of **(SRMC)** in the literature. The idea that everyday and scientific entities are at least metaphysically on a par, then, is not something entirely new. For example, Rescher (2010, p.86) argues that "science and common life [...] neither deal with different realms of being, nor yet is one of them reality-oriented and the other mere illusion. In ordinary life and science we emphatically do not address different realities or different modes of being". Similarly, McMullin harshly criticises the metaphysical component of scientific realism. To quote it fully:

> Recall that the original motivation for the doctrine of scientific realism was not a perverse philosopher's desire to inquire into the unknowable or to show that only the scientist's entities are "really real." It was a response to the challenges of fictionalism and instrumentalism, which over and over again in the history of science asserted that the entities of the scientists are fictional, that they do not exist in the everyday sense in which chairs and goldfish do. (McMullin, 1984, p.24)

The aversion to metaphysical realism led many to try to formulate philosophical views 'in between' – if not plainly neutral – about the debate between scientific realists and antirealists.[18] One of the most representative attempts is the one of Arthur Fine (1984a; 1984b; 1986). In several papers, Fine defends a view – which he calls 'the natural ontological attitude' or 'NOA' for short – which is supposed to be in between scientific realism and antirealism. After arguing that scientific realism and antirealism are not compelling, Fine proposes his view in terms of a stance that has to be taken toward science. This stance can be reassumed by letting science speak for itself, with no philosophical interpretation. In particular, the friends of NOA have to accept only one positive claim dubbed 'the homely line' by Fine:

> *The homely line*: We have to 'accept scientific results in the same way we accept the evidence of our senses.' (Fine, 1984b, p.96)

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[17] Friends of reductionism – the view that only fundamental entities exist – might be disappointed by my **(SRMC)** at first. Note, however, that **(SRMC)** is quite minimal. The 'at least' part is crucial. The view that only the fundamental unobservables exist (or that they are more fundamental than everyday entities) is not in tension with **(SRMC)**.

[18] Examples being Carnap (1950); Maddy et al. (2007). See (Wolff, 2019) for a discussion.

According to Fine, NOA is not a form of scientific realism or antirealism. It is, instead, the very refutation of understanding as meaningful the debate between the two parts. A suspension of the judgement, so to speak, insofar as no philosophical interpretation is needed for science itself. As such, NOA is presented as a stance one takes toward sciences rather than as a philosophical doctrine.
Admittedly, *The homely line:* and **(SRMC)** are strikingly similar. If 'accepting the scientific result' is understood as having metaphysical implications, then **(SRMC)** would be a consequence of the *The homely line* itself. And this is problematic: if **(SRMC)** is the minimal claim that separates scientific realists from antirealists, and such a claim is accepted by Fine – who explicitly rejects realism – then either (a) **(SRMC)** fails in its purpose, or (b) Fine's NOA is a form of realism, or (c) Fine would reject that **(SRMC)** follows from *The homely line*. I argue now that both (b) and (c) can be defended. Lucky me, most of the exegetic work has been already done in (Musgrave, 1989). Indeed, Musgrave (1989) presents a careful analysis of (Fine, 1984a,b, 1986), and he convincingly argues that there are only two coherent ways of interpreting NOA. It could be an (almost) standard form of scientific realism or a ejection to any philosophical commitment whatsoever – including metaphysical realism. If the former interpretation is the right one, then **(SRMC)** does his job since NOA would be, after all, a form of scientific realism. If the latter is the case, then it is quite easy to see that **(SRMC)** does not follow from *the homely line*. Indeed, if *the homely line* is devoid of any philosophical implication whatsoever, it cannot, *a fortiori*, contain any thesis concerning the metaphysical status of whatever entity. I have personal opinions concerning which of these interpretations is the right one. However, what I need here is just that (a) is false. Both interpretations deliver it, and there are no reasons to argue for one of them here – especially since I am not a historian of philosophy. However, what I would like to remark before concluding is that Fine's attempt to craft a view in between scientific realism and antirealism has a clear motivation: Fine explicit suspicion toward metaphysical realism brought him to an attempt of separating himself from those scientific realists which, instead, endorsed such a commitment. Under the idea that scientific realism entails metaphysical realism, Fine decided that NOA is not a realist view. Yet, Fine considers scientific antirealism as simply absurd. On the contrary, the only element of critique he moves toward realism is in its metaphysical component. His prose is clear on this point in several passages:

> What then of the realist, what does he add to his core acceptance of the results of science as really true? [. . .] what the realist adds on is a shout of "Really!" So, when the realist and antirealist agree, say, that there really are electrons [. . .] what the realist wants to add is the emphasis that all this is really so. "There really are electrons, really!". (Fine, 1984b)
> For realism, science is about something; something out there, 'external' and (largely) independent of us. The traditional conjunction of externality and independence leads to the realist picture of an objective, external world; what I shall call the World. According to realism, science is about that. (Fine, 1986, p. 150)

Be that as it may, it is not my intention to defend one interpretation of NOA. Whatever the right interpretation is, **(SRMC)** can be seen as the minimal claim which divides scientific realists and antirealists. It is now turn to make substantial progress toward our original aim, that is, to investigate whether scientific realism can be devoid of its strong metaphysical component to solve the tension between my naturalised meta-metaphysics and scientific realism. Naturalised metaphysicians, as characterized in §2.2, consider legitimate only those metaphysical questions that are motivated or informed by our best scientific theories. The tension between this meta-metaphysical view and metaphysical realism is that, on the one hand, metaphysical realism is not a metaphysical view motivated by our best scientific theories; on the other hand, metaphysical realism (allegedly) is a necessary component of scientific realism, which is, in turn, essential to defend a naturalised approach to metaphysics.

3.3 Characterizing metaphysical realism

In Fine's explicit intentions, NOA was a form of '*scientific* quietism', as he explicitly states: "NOA allies itself with what Blackburn [...] dismissively calls 'quietism'" (Fine, 1986, p.175). Where 'quietism' – as we are going to see in more detail later on (§3.3.3) – is a third view, neutral on what realists and antirealists debate. The present section's aim, instead, is to assess the defensibility of combining scientific realism with *metaphysical* quietism. The program is the following. I start by outlining the core intuitions of metaphysical realism and antirealism (§3.3.1); then, I proceed by presenting a more precise characterization of metaphysical realism, which employs Chalmers (2009)'s machinery of 'furnished worlds' (§3.3.2). Finally (§3.3.3), I present a form of metaphysical quietism inspired, admittedly, by logical positivists' attitude toward metaphysical realism. All of this will be instrumental in arguing that the debate between scientific realists and antirealists is meaningful even by assuming metaphysical quietism. Finally, I present a form of scientific realism which rejects metaphysical realism (§3.4). Conclusions of the thesis' first part follow suit (§3.5).

3.3.1 Metaphysical realism and antirealism

As their scientific counterparts are, also metaphysical realism and realism are families of views. Metaphysical realists share the belief that the reality which surrounds us exists and it is strongly independent of human beings' minds. Metaphysical realists believe in what Fine – mockingly – calls the 'World' in capital w. Metaphysical realism has, at its core, two main intuitions. First, that the World existed before, exists now and will exist after human beings' presence. Second, that the world has a mind-independent structure. To these core ideas, often metaphysical realists add an epistemic commitment: the existence of the World and/or its structure can, indeed, be known by human beings. As for any debate over realism – as sketched in §3.1 – metaphysical antirealism amounts to the negation of one of the realist's core tenets. To sum up, consider the following claims:

1. There exists a mind-independent external world (i.e. the World).
2. The World has a definite mind-independent structure.
3. We can know that 1.
4. We can know that 2.
5. We have access to (at least part of) the Worlds's mind-independent structure.

The first two claims are the minimal ones to which a metaphysical realist is committed. One can craft stronger and weaker forms of metaphysical realism by choosing which epistemic claim (among 3., 4. and 5.) has to be retained. On the contrary, metaphysical antirealists usually deny either 1., or 2, or both. Where the most radical form of antirealism, à la Berkeley, consists in rejecting 1. Moreover, antirealists usually deny also one of the epistemic claims among 3.-5.

3.3.2 Furnished worlds and the varieties of realism

To differentiate different metametaphysical stances, Chalmers (2009) uses a piece of logical machinery to define the different views. The core idea of Chalmers' machinery is that of proposing (Carnap, 1950))'s frameworks in an intuitive and theory-neutral way.[19] This machinery can be meaningfully applied to metaphysical realism, as to separate different possible views.
Let us start by summing up the relevant details of Chalmers (2009)'s machinery. We call a class of singular terms in an idealized language a 'domain' ($\mathfrak{D}$). For every language, the domain consists of the complete list of all the terms that refer to some entity. Furthermore, we call a map from worlds to domains a *furnishing function*. Furnishing functions have the role of specifying the ' class of entities that are taken to exist in that world' (Chalmers, 2009, p. 108). Finally, we call *furnished worlds* each ordered pair of a world and a domain ($\langle w_n, \mathfrak{D}_n \rangle$) which are connected by a furnishing function.
With all the definitions above in mind, we can postulate that:

> for any ordinary utterance, the context of utterance determines a furnishing function. Intuitively, this function corresponds to the ontological framework endorsed by the speaker in making the utterance. For example, ordinary discourse about tables and chairs may involve a context that determines a commonsense furnishing function. Typical mathematical discourse may involve a context that determines a furnishing function that admits all sorts of abstract objects, and so on. If we make the standard assumption that for every utterance there is a world of utterance, then the world of utterance combined with the furnishing function of the context of utterance will together determine a domain. It is this domain that will be used to assess the truth of the utterance. (Chalmers, 2009, p. 108)

Thus, existential claims, e.g., 'there are a electrons', are true if the entities involved in the sentences – electrons, in this case – are members of the domain of the furnished world picked by the furnished function which, in turns, is determined by the

[19] To avoid Quine (1951)'s criticisms. Moreover, Chalmers' machinery is theory-neutral in the sense that, for instance, does not presuppose a particular account of scientific theories; Carnap's one, instead, assumes a syntactic account.

sentence's context of utterance.[20] That is, the context in which we utter a sentence matters for picking its true value. The context, indeed, picks a furnished world – a named list of entities – such that an existential claim is true iff the entity mentioned is part of that very furnished world. Suppose we are, say, at a conference of mathematicians. The furnished world of such a context contains every abstract mathematical object one can think of: numbers, functions, operators, etc. If we asked around whether numbers, functions and operators exist, we would receive a bothered answer: "Of course numbers, functions and operators exist. That's what the whole conference is about!" On the contrary, if we were to ask the same question in a pub, say, we would be mistaken for drunks. The furnished world of the pub context contains all everyday entities we are familiar with – human beings, glasses, chairs, tables, and so on – but no abstract mathematical entity.

This machinery is enough to propose a clear characterization of different understandings of metaphysical realism. Furthermore, it will help us to make sense of the emphasis that realists want to add to some of their existential claims. As Fine puts it (§3.2), they want to add to some existential claims the "emphasis that all this is really so, 'There really are electrons, really"' (Fine, 1984b). Let us start with metaphysical realism. I do not intend to put forward an exhaustive taxonomy; rather, I limit myself to characterize three of the most popular views in the literature. Their characterizations are as follows:

> **(DMR) Diehard Metaphysical Realism** There is at least one context of utterance such that it directly picks – as if it were a furnished world – the World, i.e., the mind-independent external reality.
> **(MMR) Metaphysicians' Metaphysical Realism** The World is in a one-to-one correspondence with a single furnished world.
> **(QMR) Quotiening Metaphysical Realism** The World is in a one-to-one correspondence with a class of isomorphic furnished worlds.

The first kind of metaphysical realism implies that existential claims might be of two kinds: they can either quantify (i) over a single furnished world picked out by a context – as when we say that 'numbers exist' at a conference of mathematicians – or (ii) it can be unrestricted. The domain of quantification is – in some sense or another – the World itself. Accordingly, **(DMR)** posits that there is an 'unrestricted quantifier' that ranges over every entity that exists in the world. As such, unrestricted existential claims are metaphysically loaded since they are not restricted to a particular domain. Rather, their truth-value is established directly through a comparison with the World. The core idea of this approach is that we can – independently of the context – speak about the mind-independent world directly. Such a view corresponds, to my understanding, to the 'Nominalistic Method' discussed in Sider (2009, p. 415).[21]

By far, **(DMR)** is one of the strongest forms of metaphysical realism one could have in mind. However, also **(MMR)** is quite strong on its own. According to it, indeed,

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[20] Note that this definition of truth is remarkably similar to the notion of 'truth in a structure', as presented in Hodges (1985).

[21] See, for criticisms to the view, Wrigley (2018).

there is a single domain that is in a one-to-one correspondence to the World. The view accepts that existential claims are always restricted to a domain, yet only one domain is the 'real' one. The one that 'carves Nature at its joints' (Sider, 2011), to put it in a famous catchphrase. The idea is that truth values depend on the context in which we utter our existential claims, but only one context is successful in talking about how the World is. A view along these lines has been proposed by Sider (2009), who dubs 'Ontologese' the language spoken in 'the metaphysics room'. According to him, metaphysics has a privileged place compared to that of the other contexts exactly because it is the only one in which it is possible to refer directly to how the world is structured in itself. Metaphysicians, according to Sider (2009), speak – or so they try, at least – a language of their own which happens to be the right one to describe the mind-independent structures of reality. As such, **(DMR)** is very popular among analytic metaphysicians.[22].

(QMR) is, among the three views here discussed, the weakest. The view shares the idea that there is a furnished world that describes the World, yet such a furnished world is not unique. Rather, there is a class of equivalent furnished worlds, each representing a correct description of reality, even though they disagree with each other about what exists. The view that theories with different metaphysics might be equivalent – in some sense or another – is known as 'quotiening.'[23] Friends of **(QMR)** have to defend quotiening, in at least some cases.[24] This form of metaphysical realism is born as a challenge to Putnam (1982b)'s thesis that metaphysical realism necessarily entails the existence of a unique true theory able to describe reality – the 'One True Theory', in Putnam (1982b)'s own words.[25] To my knowledge, **(QMR)** is defended by those metaphysical realists – such as many philosophers of science – that have a somehow deflationary attitude toward many questions debated by non-naturalized metaphysicians. For instance, a friend of **(QMR)** might argue that, for instance, the debate between three-dimensionalists (Fine, 2008) and four dimensionalists (Sider, 2001) in the metaphysics of time are unsubstantial. And this is so because one can map all the claims that one view can put forward in claims made by the rival, and vice versa Miller (2004). Hence, these theories are – in a sense that has to be further specified – equivalent; as such, both theories are equally true despite their conceptual differences. In other words, they are metaphysically equivalent ways of cutting reality – in the same sense in which, say, a sentence in English and its Italian translation have the same meaning despite they are expressed in different languages.

We saw above that Fine's complain with scientific realism lies in its metaphysical component. In particular, he stresses a lot that scientific realists are not happy to say that unobservables exist; rather, they want to add that they REALLY exist – as if

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[22] Other than Sider (2009), a representative example is Fine (2009). Criticisms have been put forward by many philosophers, e.g., Hirsch (2008, 2009); Price (2009)

[23] See Sider (2020), for a discussion on quotiening in metaphysics of science.

[24] That is, whether two theories are equivalent depends on how theoretical equivalence is defined. Such a definition distinguishes rival theories from theories that can be both accepted by a quotiener as equivalent.

[25] See, e.g., Field (1982).

the adverb ‘REALLY’ would change the meaning of existing. Chalmers’ machinery is useful to capture what such a ‘REALLY existing’ might mean. The core idea is that even accepting that truth-values depend on the context of utterance, certain contexts are privileged. So we could trace a difference between metaphysically loaded and unloaded claims. Consider the example of a defender of existence monism (Cf. §3.2) again. They would accept that ‘there is a laptop in front of me’ is true in our everyday life. Yet, such a sentence is strictly speaking false – for the friends of existence monism – if we are speaking of what REALLY exist, that is, we are doing metaphysics. Every day’s objects are part of the domain of everyday life context, but they do not REALLY exist as the Universe does. Chalmers’ machinery allows one to define exactly what this ‘REAL existence’ (**RE**) means. As expected, it has to mean something different for every kind of metaphysical realism:

RE$_{(\textbf{DMR})}$ An entity REALLY exists iff it is part of the external world, and we quantify over it with the unrestricted quantifier.

RE$_{(\textbf{MMR})}$ An entity REALLY exists iff it is a member of the unique furnished world, which is in a one-to-one correspondence with World.

RE$_{(\textbf{QMR})}$ An entity REALLY exists iff it is a member of the collection of equivalent furnished worlds that are in a one-to-one correspondence with World.

The distinction here introduced will not play a significant role in what follows. Rather, characterizing metaphysical realism in further details was just instrumental in providing a better characterization of metaphysical quietism.[26] Before turning to this job, I just want to draw a small conclusion from the present subsection. The small taxonomy introduced here shows that different versions of metaphysical realism are substantially different from one another. As a consequence, claiming that ‘metaphysical realism’ is part of one’s scientific realism is not informative if no further specification of what one means with ‘metaphysical realism’ is added. Such a specification is almost inexistent in the scientific realist literature.[27] However, which metaphysical realism is assumed in one’s scientific realism is quite relevant, insofar as it has strong implications on other debates in the philosophy of science – the debate concerning theoretical equivalence being an obvious example (Coffey, 2014; Barrett and Halvorson, 2016; Teitel, 2021). Hence, metaphysical realism is not an innocent assumption. Rather, it is a nest of different views that can be challenged and have significant import on one’s view about science.

3.3.3 Metaphysical Quietism

We characterize at the very beginning what realism and antirealism – in general – amount to. However, an explicit rejection of realism does not amount to an accep-

[26] This partially explain why I do not provide a taxonomy of metaphysical antirealism as well. Such a taxonomy can also be easily derived by which claims, among those accepted by metaphysical realists, are rejected.

[27] There are, of course, notable exceptions, e.g., (Massimi, 2018).

tance of antirealism. As a matter of fact, indeed, a third view about the existence of any kind of entity is possible. Such a view is usually called ‘quietism’, and it is discussed through and fro the literature in different context. The core idea of quietism is that there are no convincing reasons to side neither with realists nor with antirealists. Depending on their specific arguments, quietists consider the debate between realists and antirealists meaningless, unsubstantial or in principle impossible to be settled. We saw already a quietist view on the debate concerning scientific realism: Fine’s Natural Ontological Attitude. It is now time to turn to form of quietism concerning metaphysical realism.
Recall our brief characterization of metaphysical realism (§3.3.1):

1. There exists a mind-independent external world (i.e. the World).
2. The World has a definite mind-independent structure.
3. We can know that 1.
4. We can know that 2.
5. We can know that 2., and we have access to (at least part of) the Worlds’s mind-independent structure.

A noteworthy form of metaphysical quietism consists in denying that metaphysical realism and antirealism are meaningful theses: claims 1. and 2. are neither true nor false. They are claims which hinge on our intuitions; yet, they have not a definite meaning whatsoever. Famously, such a view has been heartily advocated by logical positivists (Carnap, 1966; Schlick, 1932). A more contemporary form of quietism, instead, hinges on the idea that 1. and 2. have a meaning which is impossible – given human beings’ epistemic capabilities – to be determined. Among others, such a view has been defended by McDowell (1994) and (Rosen, 1994). In particular, the latter’s formulation is remarkably clear:

> We sense that there is a heady metaphysical thesis at stake in these debates over realism - a question on par with the issues Kant first raised about the status of nature. But after a point, when every attempt to say just what the issue is has come up empty, we have no real choice but to conclude that despite all the wonderful, suggestive imagery, there is ultimately nothing in the neighbourhood to discuss.[. . .]
> If it makes no good sense to deny the realist’s characteristic claims, then it makes no good sense to affirm them either. Quietism, as the view is sometimes called, is not a species of realism. It is rather a rejection of the question to which ‘realism’ was supposed to be the answer. (Rosen, 1994, p.279-280)

The aim of what follows is to articulate a different form of quietism,[28] which I dub ‘Epistemological metaphysical quietism’:

> **(EMQ)]** **Epistemological Metaphysical Quietism:** In principle, the debate between metaphysical realists and antirealists cannot be settled. The epistemological capabilities of human beings is such that there is no way to say whether 5. (and 4., as a consequence) is true. There might be a mind-independent external

[28] As we saw, quietism is a general view that can be hel about any kind of entity whose existence is endorsed by realists and denied by antirealists. In what follows, I use ‘quietism’ to refer simply to metaphysical realism – to avoid repetitions of ‘metaphysical quietism.’

world. However, whether our theories describe its mind-independent structures is impossible to know, insofar as we have not a direct access to them.

Let us see in more details to what **(EMQ)** amounts to. First, **(EMQ)** is not exactly agnostic toward 1. Rather, 1. is considered as a useful working hypothesis with no harm. The reason why 1. is not explicitly rejected is that such a rejection would amount to a surrender to radical scepticism, i.e. the view according that the content of human beings' whole experience is misleading.[29] That the whole of our experience is just illusory amounts to the idea that the external reality we perceive is a kind of dream which we mistake for reality: we could be 'brains-in-a-vat' manipulated by mad scientists (Putnam, 1981), or we could be unaware computer programs as in the movie 'The Matrix' (Khlentzos, 2016). On the one hand, the scenario imagined by radical sceptic are – by definition – impossible to rule out: by assumptions, no empirical evidence can be used to rule them out exactly because it is the very empirical experience that these scenario are supposed to undermine. Hence, it s true that there are no guarantees that we live in an external mind-independent world rather than in an hallucination casted by an evil daemon, as Descartes suggested. On the other hand though, these scenarios are hardly compelling. As they cannot be ruled out, they also lack serious reasons to be accepted as true. There is no evidence that we live in a simulation, nor that our experience is not grounded in a mind-independent external-world. As such, according to **(EMQ)**, 1. is an unproblematical working assumption.

What friends of **(EMQ)** are, instead, sceptical about 2. Or, more precisely, they are sceptical that there are rational ways of determining when one of our theories 'latches on' – correctly describe – the alleged reality's mind-independent structures. Surely enough, we have some criteria do discern good and bad scientific theories, and, as such, theories that have a higher or lower probability of describing these structures. Yet, empirical adequacy, novel predictions, simplicity, explanatory power, and so on, are not enough to prove with certainty that our theory describes how reality is mind-independently. On the occasion of his promotion at the University of Berlin, the well-known physicist Max Planck presented such a irretrievable scepticism as part and parcel of the scientific understanding of reality:

> Let us suppose that a Physical representation of the Universe had been found which fulfils all our demands, and therefore one that can completely and accurately represent all laws of Nature empirically known; still that that image even remotely resembles "real" Nature, can in no way be proven. [. . .] Even in Physics, the phrase holds good that "There is no Salvation without Faith," – at least a faith in a certain reality outside ourselves. (Planck, 1914, pp. 69-70)

Planck's words masterfully sum up the core tenet of **(EMQ)**: human beings can put forward different theories and criteria to chose among them which one is better. However, even in the extreme case in which we will have a theory of everything, whether such a theory 'carves Nature at its joints' (Sider, 2011) is beyond of what

[29] The most famous form of radical scepticism is Descartes' argument of the evil daemon (in the *Meditations on First Philosophy*). A more contemporary challenge to metaphysical realism in the form of a radical scepticism is Putnam (1981)'s 'brains-in-a-vat' argument.

human beings can assess. What we have now are different ways of describing reality without any guarantees whatsoever that one of them describe the World's mind-independent structures.

A more clear elaboration of the core tenet of **(EMQ)** can be put forward by analyzing what metaphysical statements boil down to. Consider a metaphysical statement such as 'this t-shirt instantiates the property of being black'. Metaphysical realists understand such a claim as a truthful way of talking about reality: the t-shirt is an entity clearly detached by other entities that belongs to the World, and it has a property – that of being black – mind-independently. The t-shirt would be black, even if no one would look at it. Metaphysical antirealists, instead, understand metaphysical statements as the one above in terms of mind-dependent entities. Human beings divide the t-shirt from the background and conceptualize it as an individual entity because it is cognitively salient. However, the way in which we separate our experiences in individual objects has to do more with our perception than to how reality is in its own. The properties the t-shirt instantiates are, as well, part of a mind-dependent conceptualization of experience. Friends of **(EMQ)**, instead, refuse both the understandings above.

On the one hand, they reckon that metaphysical statements are supposed to be our best way of describing human beings' experience of reality; yet, on the other hand, they are agnostic about whether reality is exactly as our most accurate metaphysical statements depict it. Since human beings cannot know reality's structures, all metaphysical debates concern, according **(EMQ)**, what is the best way of describe reality that is available to human beings at a certain time. As such, quietists[30] are happy to engage in metaphysical debates.

However, if they are pressed about whether reality really is as their favorite metaphysics, they will simply answer that it is the *best* metaphysical picture we have so far, but no guarantees about its truth are available. And this is so because – to quietists – philosophical and scientific debates concern what counts as the *best* description of reality as a whole, rather than what reality is on its own. Humanity has always tried to put forward a coherent description of the whole reality by investigating its own possible experience. Such a description – which I later call the 'empirical furnished world'[31] – is far from being complete.[32] Yet, to the quietists, what philosophers and scientists disagree about is what should we put in this coher-

[30] As we saw, quietism is a general view that can be held about any kind of entity whose existence is endorsed by realists and denied by antirealists. In what follows, I use 'quietism' and 'quietists' to refer simply to, respectively, Epistemological metaphysical realism **(EMQ)** and its defenders – to avoid lengthy repetitions.

[31] I will talk of the empirical furnished world as if it were unique. Note, however, that such a choice is made to keep the discussion simple, rather than for taking a stance toward quotiening. Indeed, quietism is clearly favorable to the idea that there might be many equivalent ways of describing reality such that there are no rational reasons to chose one of them among the other. Hence, friends of **(EMQ)** would be happy to concede that there might be many equivalent furnished worlds, rather than one.

[32] A list of entity cannot be enough for describing the whole of reality. Indeed, it seems inevitable that some structure – relations, properties, laws of natures, etc. – must be included in a detailed description of the World. Chalmers (2009) provides a sketch of how to add structures to the ma-

ent description of reality, with the proviso that whether such a description latches on the mind-independent structures of the World is something we cannot, in principle, discover.

By employing Chalmers' machinery introduced above (§3.3.2), we can make the idea above even more precise. According to quietists, the contexts in which statements are uttered determine – through a furnishing function – particular domains; existential claims are true if the statement involved is a part of the domain picked by the corresponding furnishing function, and they are false otherwise. Now, where antirealists insist that there is no correspondence between any furnished world and the World, quietists are agnostic. In other words, 'REAL existence' as defined above (§3.3.2) makes no sense whatsoever for the antirealists. For the quietists instead it does make sense but, given our epistemic access to reality, we cannot evaluate when an existential statement is or is not REALLY true. Truth-hood and false-hood make sense in different context, but we have no guarantee that some of these context actually describe the world as it is mind-independently. The positive claim that friends of **(EMQ)** hold is that there is, after all, a privileged furnished world which I dub the 'empirical furnished world'. This furnished world is composed to all the entities which are part of our best attempt to provide a coherent and exhaustive description of the World. The empirical furnished world is what human beings built, at first, through their experience, and that is later enriched by scientific and philosophical discoveries. As a matter of fact, such an empirical furnished world does not exist yet. Humanity has a whole still lack a comprehensive and coherent picture of reality. What we have, instead, are a number of theories which model phenomena at different scales. However, the quietist argues, to reach a unified description of reality is the final aim of the scientific enquiry. Every scientific revolution, discovery and successful experiment is a tile toward the construction of this unified puzzle. As such, debates concerning what exist are understood by quietists as debates concerning what should we put in this privileged furnished world – what should we kept in our comprehensive theory of everything. Nonetheless, even if we will have one day a coherent and exhaustive empirical furnished world, the central claim of **(EMQ)** is that we will never been able to know that the entities in its domain REALLY exist, or not. Whether the empirical furnished world is the best way possible for human beings to partitioning reality or it really reflects mind-independent structures is something human beings are bound to never know with certainty. Hence, scientific and philosophical disagreements are seen, in the quietist's eyes, as disagreement about what is our best – i.e., empirically adequate, coherent, simple, and so on – description of the World, rather than about how the World is mind-independently structured. For example, three-dimensionalists (Fine, 2008) will disagree with four-dimensionalists about whether temporal parts should be member of the empirical furnished world. Analogously, string theorists (Green et al., 1987, 2012) and friends of other approaches to quantum gravity, e.g. Loop Quantum Gravity (Rovelli and Vidotto, 2015), disagree on how space-time has to be quantized. String theorists will insist that strings must be part of our best description of the world – the em-

chinery above. For what it is discusses here, however, these details are not fundamental and I will not linger more on them in what follows.

pirical furnished world – insofar as they are responsible of space-time emergence, whether defenders of other approaches will negate that. Quietists happily join these metaphysical and scientific debates, with the awareness that whether their favourite view is REALLY true is bound to be unsettled. Yet, these debates are far from being futile or irrelevant for the quietists. Rather, they are our best shot for understanding how reality could be; even though, whether our best shot hits its target, is doomed to be unanswered.

3.4 Realism without 'Reality'

In the last section, we saw that apart from metaphysical realism and antirealism, there is a third view: quietism. Such a view consists in the rejection of the whole debate that separates realists and antirealists. In particular, I presented a form of metaphysical quietism, namely, **(EMQ)**, according to which one cannot know with certainty whether our theories truthfully describe the structures of reality. It is now time to turn back to our original aim. Namely, to investigate whether it is possible to be scientific realists without holding strong metaphysical commitments. In other words, if there is a way of making sense of the idea that unobservables exist, without a commitment to their 'REAL existence,' defined above (§3.3.2). If that were possible, then endorsing such a view might help mitigate the tension between naturalized metaphysics and scientific realism: the latter would still be a prerequisite to the former but not non-naturalized theses – such as metaphysical realism – would be counted as legitimate metaphysical theories. I argue now that it is possible to obtain a metaphysically lighter form of scientific realism by accepting **(EMQ)**. To do so, I have to show that the debate between scientific realists and antirealists makes sense even by assuming such a quietist stance toward the existence of a mind-independent external world. The present section is fully dedicated to this aim.
Let us start by putting together the work done so far. In section 3.2, I argued that the minimal claim that separates scientific realists from antirealists is the following:

> **(SRMC)**: Unobservables – as posited by our best scientific theories – are at least as real as the everyday entities perceived by human beings.

As such, any kind of scientific realism has to accept **(SRMC)**. How to make sense of it by using the definitions of metaphysical realism elaborated in §3.3.2? In that section, I introduced two predicates: existence and REAL existence. Where the former means being part of a furnished world and the latter of being part of a peculiar furnished world, that is, the one which can be mapped in the World. Hence, according to metaphysical realists, when we are talking in a metaphysically loaded way, we are concerned with REAL existence rather than existence simpliciter. For example, when two philosophers debate about whether, say, numbers exist, they would not stay content with the claim that they exist when we take part in a conference of mathematicians. Rather, they want to know whether numbers REALLY exist, if they are part of the furnished world that correctly describes reality as it is. It follows that, for metaphysical realists at least, two entities are metaphysically on a par if they both REALLY exist or if they both exist in the same domain.

According to quietists (§3.3.3), though, the notion of REAL existence is not available. Remember that, according to **(EMQ)**, it is in principle impossible to verify whether an entity REALLY exists or not. Therefore, 'metaphysically on a par' might mean for the quietists just 'being a member of the same furnished world.' However, this is problematic: furnished worlds are sets of entities. Insofar as given two entities, there is always a set that has both of them as a member, it follows that the notion of 'metaphysically on a par' relied on the quietists is trivial. Everything, by definition, is metaphysically on a par with everything else. To avoid the problem above, quietists have to rely on a privileged furnished world: the empirical furnished world discussed above that contains every entity that is indispensable to provide the most accurate and coherent description of reality. This empirical furnished world is 'our best theory' of how reality could be mind-independently. As such, quietists have to postulate that 'metaphysically on a par' means to be a member of the empirical furnished world.

Now that we have recollected the main points of the work done so far, we can now see that even by assuming **(EMQ)**, the debate between scientific realists and antirealists holds its meaningfulness. Scientific realists symphatethic to quietism will hold a view which I dub 'Scientific yet quiet realism.'[33] The starting point of this view is **(SRMC)**: the trust we put in the scientific enterprise cannot be different from the one we have in our everyday experience. Consequently, scientific yet quiet realists will accept that unobservables exist in the same way everyday objects do: they must be part of the best description of reality. As we believe that laptops, human beings and all everyday's objects exist, we must accept that electrons, black holes, dark matter and so on exist in the same sense. They are necessary for our best description of the phenomena that surround us. Accordingly, scientific yet quiet *antirealist* will deny that this is so. Rather, they will contend that unobservables posited by our best science exist in the same way in which, say, numbers do: they exist only in the furnished world that is picked only in particular contexts. As such, electrons, black holes, viruses, and all the fancy unobservables scientists posit to put forward some explanations exist only in those contexts in which the corresponding furnishing function picks a scientific furnished world. They exist in laboratories and in scientific courses and conferences at the university. Yet, they do not exist at the pub, say, or when we are relaxed at home watching tv.[34] And this is so because our best description of the World can be the best without any appeal to these entities. Perhaps, following van Fraassen (1980), scientific yet quiet realists can argue that there are no rational grounds to consider unobservables as on a par with everyday objects; and, as a consequence, our best – i.e. rationally justified – description of nature must

[33] 'Quiet' also because they avoid any desk-thumping, "foot-stamping shout of 'REALLY!'," as asked by (Fine, 1984b).

[34] Note that I do not mean here just that black holes are not physically in pubs or in my house. That is trivially true, independently of scientific realism. What I mean is that a sentence like 'there are black holes' turns out to be false when one utters it in my house, and true if one utters it during a conference in black holes physics.

be devoid of all these scientific entities which cannot be directly observed.
As I argue above (§3.2), the debate concerning the existence of unobservables would still make sense if the metaphysical component of scientific realism were bracketed. Here, we had another confirmation: even by assuming a rival of metaphysical realism, that is, quietism, the debate between scientific realists and antirealists makes sense. That is, we can imagine two philosophers that genuinely dispute whether we should believe in the existence of electrons or not, even if they agree on metaphysical quietism. To conclude, I would like just to define the scientific yet quiet realism I introduced here along the lines of Psillos (2005b)'s characterization of standard scientific realism. Scientific yet quiet realism is a threefold thesis with a metaphysical, an epistemic and a semantic component. The metaphysical component is **(EMQ)**: albeit a mind-independent external world exists, and our theories allow us to provide better and better descriptions of it, it is impossible to know whether it has a mind-independent structure and whether our best theories REALLY describe it. The semantic thesis is the same as standard scientific realism: scientific theories must be taken at face value – both in their observable and unobservable domain – and they can be either true or false.[35] Finally, the epistemic thesis consist of the claim that the scientific inquiry – of which metaphysics is a part of, as argued in §2 – is our best attempt of finding a description of the World. As such, unobservables posited by our best scientific theories *must* be included in our best description of reality.

3.5 Conclusions

In chapter §2 I presented four approaches to metaphysics intending to defend one of them, i.e., naturalized metaphysics. According to this view, metaphysics is part and parcel of the scientific enquiry of reality, and its aim is exactly that of specifying what our current best scientific theories tell us about the world. One of the assumptions of naturalized metaphysics is scientific realism, the thesis that our best scientific theories can describe the mind-independent structures of reality. On the one hand, scientific realism is a prerequisite to naturalized metaphysics, insofar as the latter's methodology is based on the thesis that the scientific enterprise is epistemically more reliable than a priori metaphysics. On the other hand, the form of naturalized metaphysics I proposed claims that metaphysics has to be motivated by our best scientific theories. However, the metaphysical components of scientific realism are not motivated by our best scientific theories. As such, there is a tension between the idea that metaphysics has to be motivated by our best scientific theories and the metaphysical assumptions needed to trust the ontological imports of scientific theories themselves.

[35] One could advance the objection that in the absence of metaphysical realism, no scientific theory could be true. There is no space to address such an objection, which would involve a discussion about syntactic and semantic views of scientific theories. However, note that it has been argued that one could make sense of the truth of a theory even without the assumption that the phenomena described by the theory have a mind-independent structure (Giunti, 2016).

In this chapter, I proposed a solution to the tension above by investigating whether one could weaken the metaphysical component of scientific realism. In particular, I argued that the debates between scientific realists and antirealists are meaningful even if we do not consider the metaphysical components of scientific realism. Then, I characterize a view called metaphysical quietism, which consists of a refusal of both metaphysical realism and antirealism. According to this view, whether our theories describe reality's mind-independent structures is something that cannot be known with certainty. As such, we have to accept the inevitable fallibilism of our theories and be aware that scientific and metaphysical debates concern our currently best ways of describing reality rather than reality itself. Finally, I argued that the debate between scientific realists and antirealists is meaningful even if metaphysical quietism is assumed. Hence, I presented a form of scientific quietism – dubbed Scientific yet Quiet Realism – which is a form of scientific realism with weak metaphysical commitments. Accepting this form of scientific realism might be a way to solve the tension between naturalized metaphysicians and scientific realism. Indeed, Scientific yet Quiet Realism, through its explicit rejection of metaphysical realism, allows one to (i) interpret metaphysics as part of the scientific enquiry, (ii) accept the fallibilists character of science and metaphysics, (iii) accept the idea that science and metaphysics's aims is that of providing the best description of reality which is possible at a given time; and yet, (iv) it avoids any commitment to non-naturalized metaphysical theses such as metaphysical realism.

To conclude, in my view, metaphysics is an important part of scientific realism. However, scientific realists often refuse to take metaphysical commitments or avoid talking about the metaphysics of scientific theories. In chapter 4, we are going to discuss an example of it – i.e., Saatsi (2020), who argues that scientific realists must not commit to a precise metaphysics of spin. However, if metaphysical realism is part of one's scientific realism, then a metaphysical commitment is requested. That is, one cannot simply argue that a given unobservable posited by our best science exist; rather, its metaphysics has to be investigated. Otherwise, the commitment is just that a given name – that of that very observable – refers to something, even if such a something lacks any metaphysical characterization. Naturalized metaphysics and scientific realism are so intertwined that a stance on one of them is requested to put forward an account of the other. As naturalized metaphysicians have to rethink the relationship between their discipline and scientific realism, scientific realists have to rethink their metaphysical commitments. To sum up the core of what has been argued in the first part in a catchphrase: science and metaphysics are parts of the same coin – the impossible quest of understanding the structures of reality.

Part II
The metaphysics of Spin

Chapter 4
Spin

The second part of the thesis investigates the metaphysics of spin in an ‘interpretation neutral’ approach to non-relativistic quantum mechanics. The most pressing question I investigate is under what conditions one could say that spin offers a genuine example of metaphysical indeterminacy – the idea that indeterminacy concerns things in themselves rather than our representation of them (§5, §6). In particular, I propose a characterisation of spin in terms of the determinable-determinate relation, and I present a novel metaphysical account of spin (§7.5). Final conclusions end the thesis.

In the present chapter, I present (§4.1) my approach to the metaphysics of quantum mechanics by elucidating some details of the ‘interpretation neutral’ approach, as called by Wallace (2019), as well some of its underlying motivations. Then (§4.2), I turn to the role of spin in non-relativistic quantum mechanics. After having motivated the thesis’ focus on spin, I offer a concise mathematical characterisation of spin (§4.3). Finally (§4.4), I discuss a recent argument whose conclusion is that scientific realists should avoid any commitment to spin. Conclusions follow suit (§4.5).

4.1 Non-relativistic quantum mechanics and its metaphysics.

At the very least, there are two ways in which scientific theories might be underdetermined (Ladyman, 2012; Stanford, 2021). First, more than one scientific theory might adequately explain a set of empirical data. Hence, it is impossible to choose among competing theories by appealing only to empirical means. In some cases, the underdetermination might be a fact of contingent technological limitations. Some experiments might determine which interpretation is correct, but we lack the technology to perform them. In other cases, underdetermination is more severe: experiments to discern which alternative is correct are impossible. The second form of underdetermination concerns the possibility that the formalism of a single theory is compatible with more than one ontology. Hence, it is impossible to precisely answer the question ‘How the world is like if that theory is true?’ by appealing only

to empirical data. More than one plausible answer can be put forward without any rational way of choosing the correct one among them.
Non-relativistic quantum mechanics (QM hereafter) is underdetermined in both ways. On the one hand, we have many theories – often called 'interpretations' – explaining the same set of empirical phenomena.[1] They are bona fide rival theories because they have different dynamical equations and ontologies. And in principle, some experiments could discern which interpretation is inadequate. Alas, these experiments are not, currently, feasible, e.g., (Deutsch, 1985). On the other hand, almost any interpretation is compatible with more than one ontology. Take, as an example, the three main realist interpretations of QM: Many Worlds, Bohmian Mechanics and Ghirardi, Rimini and Weber's spontaneous collapse model (GRW, hereafter). Each of them are compatible with different ontologies; examples are:

- Overlapping vs Diverging Many Worlds (Wilson, 2020);
- Bohmian mechanics with or without a primitive ontology[2] of particles – respectively, (Daumer et al., 1996) and (Albert, 2015, Ch. 6-7);
- GRW with an ontology of matter density distribution in space vs an ontology of flashes in space-time (Allori, 2015).

There is no good answer to how scientific realists should face this underdetermination. For once, the underdetermination in QM is one of the most serious in the history of science.[3]
Egg (2021) argues that there are two main ways out of the underdetermination for scientific realists.
The first one is to defend one of the realist interpretations. This horn of the dilemma can be defended, in turn, in three different ways. One might be willing to enter the entrenched camp of finding some criterion to distinguish which theory is better than the rival ones and arguing that one's favourite interpretation is better than the others according to the criterion chosen. Alternatively – as acknowledged by Egg (2021) – one might either argue that the aim is to explore the *possible* ontologies of QM rather than the correct one or deny that there is any underdetermination in the first place. The second alternative traces back to one's conception of metaphysics. Exploring the possible metaphysics of QM is a meaningful enterprise if one believes that metaphysics has to explore the space of metaphysical possibilities. Furthermore, this route makes the second layer of QM's underdetermination even more severe. The third alternative seems as much problematic. Albert (2015) and Bricmont (2016) both argued that the QM's metaphysics is not underdetermined. Still, by advocating two different interpretations, they end up – ironically, as Egg (2021) notices – to confirm what they wanted to deny. As such, friends of particular interpretations of QM should rather take the first alternative and work out some theory assessment criteria and then argue that their interpretation is better in those

[1] For a survey on the underdetermination of the three main realist interpretations of QM, see (Lewis, 2016, §3).

[2] For a review on primitive ontology in QM, see (Allori, 2015).

[3] Since many cases of underdetermination are either somewhat artificial or they held for a shorter period (Ladyman, 2012, Ch.6).

regards. Alas, this is no small feat by any means (Alai, 2019).
The second way out of the underdetermination dilemma consists of committing to these interpretations 'common core'. However, this move seems to bring one back to the somehow instrumentalist reading of quantum mechanics commonly associated with (the majority of) the founding fathers of the theory: QM is a mathematical theory that allow us to accurately predict many phenomena even though it cannot be a description of how the quantum world is. Indeed, sticking to the common core of these interpretations means accepting the 'standard' formulation of QM – the one taught to physicists in the standard textbooks[4], to be clear. However, it has been thought for ages that textbook QM cannot be interpreted *realistically*, i.e. it was impossible to associate to its formalism any *coherent* ontology. Indeed, almost every realist interpretation of QM modifies the formalism of textbook QM.[5] Looking back at textbook QM in search of some commitment might be seen as "the very opposite of ontological progress" (Egg, 2021, p. 6).
Given the reasons above, it seems prima facie complex for realists to take the second way out of the dilemma and still retain a realist stance toward QM. Nonetheless, one could take this route by arguing that textbook QM has some ontological import. In the first chapter of his recent book, Maudlin (2019) vigorously argues that 'textbook QM' is not a scientific theory but rather a mathematical 'recipe.' To be a scientific theory, according to Maudlin (2019), a mathematical apparatus must be supplemented with an ontology, i.e. a map between the formalism and some (allegedly existing) entities. However, nothing impedes one to take QM's formalism and seeing whether it can be understood ontologically. The fact that such a map between textbook QM and a coherent ontology has been deemed impossible for ages is no guarantee that such an ontology is impossible. And as a matter of fact, that textbook QM can be understood realistically has been recently advocated by several philosophers in the recent literature.(Cordero, 2001; Bokulich, 2014; Wolff, 2015; Calosi and Wilson, 2018; Egg, 2021; Schroeren, 2021a,b). This approach to QM's metaphysics follows from the idea that being realist or anti-realist toward a single scientific theory is a stance one assumes – following McMullin (1984).[6] Metaphorically speaking, the realist and anti-realist stances are two glasses through which we read the mathematics of a scientific theory. With the anti-realist glasses on, one reads the mathematical formalism as a valuable tool to make empirical predictions devoid of any ontological meaning. Instead, with the realist glasses on, one reads the theory's mathematical structures as a truthful description of some physical phenomena. Textbook QM is no different: nothing impedes one to 'wear' their realist

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[4] E.g., (Griffiths, 1960; Hughes, 1989; Sakurai, 1994). Wallace (2019, §1) offers a short and precise synthesis of the principal axioms of textbook QM.

[5] To stick to the examples above: Many Worlds interpretation rejects the collapse principle – and older formulations add to each term infinite sets of worlds and a measure given by the Born's rule (Deutsch, 1985), a pilot wave is added to describe the dynamics of particles in Bohmian mechanics, and the Schrödinger's equation is modified in GRW's models.

[6] It is essential to stress that here I am referring to the stance one takes toward *individual* scientific theories, a stance that is somewhat independent of one's position on the scientific realism debate. Indeed, even among the diehard scientific realists, anyone would accept that some scientific theories must be understood instrumentally.

glasses, say, and try to cash out the ontological commitment of this theory. Bokulich (2014), e.g., clearly addresses such a point:

> 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 (e.g., Ptolemy’s astronomy was once thought to be a realist theory, describing how the solar (or, rather, geo-) system really is, while others (such as Osiander and contemporary surveyors) take it instead to be only an instrumentalist theory, providing nothing but a means for calculation). 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”. (Bokulich, 2014, p.460, fn.13)

In the rest of the thesis, I follow this last way of looking at QM. I investigate some metaphysical consequences of QM by sticking to the textbook formalism, thus following an ‘interpretation-neutral’ approach (Wallace, 2019, p.3).[7] As such, the ontological conclusions I reach in the thesis are thus limited to the standard formalism only, and significant work has to be done to check whether they hold good in other interpretations as well. If one will, this can be seen as a natural consequence of my approach to naturalized metaphysics (Cf. §2.2.2) rather than a limitation of my analysis. I do not extensively defend here this methodological assumption to the metaphysics of QM. Instead, I limit myself to sketch three reasons why such an approach is worth investigating.

First and foremost, the account of naturalized metaphysics I proposed intimates one to do metaphysics from our current best scientific theories. This, in turn, asks for an explanation of what qualifies a theory as the ‘best’ among those we currently have. I mentioned in the previous chapters that we lack a theory of everything. Instead, the picture of reality depicted by empirical sciences is fragmented: we have different theories that successfully model the phenomena within a domain – where the domain is usually described in terms of physical parameters such as energy, scale, and velocities. What makes a theory the best theory is its ability to account for *all* the known phenomena – and possibly make new empirically confirmed predictions – within a domain. And the ”somewhat messy and recipe-like form in which it appears in textbooks” (Egg, 2021, p.6) is what makes QM one of our best scientific theory (Cordero, 2001; Saatsi, 2020; Egg, 2021). If QM really is one of our best scientific theories, then we must commit ourselves ontologically to QM’s description of the phenomena within its domain.[8] Let me qualify this claim with a small remark. Accepting that QM has some ontological import does not mean claiming that

[7] I avoid using ‘Copenhagen interpretation,’ although it is sometimes used in the literature, since it is arguably misleading (Howard, 2004). Similarly, ‘orthodox’ QM reminds many philosophers of an instrumentalist understanding of the theory. Hence, I use instead in what follows ‘textbook QM’, ‘standard QM’ or simply QM.

[8] What is precisely the domain of QM, is – once again – controversial (Holland, 1996). For sure, it concerns entities at the atomic and subatomic scales (Leighton and Sands, 1965). However, starting from the fact that everything is ultimately made of atoms and that some quantum effects are relevant for macroscopic entities, some deduce that quantum mechanics could explain in principle

its description is somehow fundamental. We know already that QM is not a fundamental theory; as such, its depiction of reality will be non-fundamental as well. Metaphysics aims to characterise reality if a given scientific theory is true. If the scientific theory in question is not fundamental, its metaphysics will also be made of non-fundamental structures. However, as I argued above (§2.2.2), this does not mean that one should avoid cashing out its metaphysical import. The metaphysics of non-fundamental theories has its value.[9]

Second, different interpretations of QM are characterised by how they modify the standard formalism. As such, extracting the ontological commitment of the standard formulation might be a reasonable methodological criterion. Perhaps, the metaphysical consequences of the standard formulation will be able to highlight some metaphysical aspects of other interpretations as well. This possibility can be further appreciated in the case of spin. In some interpretations of QM – most notably, some formulations of Bohmian mechanics (Daumer et al., 1996) – spin is not an intrinsic property of quantum entities. However, these interpretations must recover what QM empirically predicts by relying on the spin. As such, it might well be that some metaphysical description of spin must be present in these interpretations as well – albeit spin would turn out as a non-fundamental component of these interpretations' ontology Egg (2021).

Third, we lacked the metaphysical resources to treat the import of the two most distinct aspects of QM: superposition and entanglement.[10] The principal novelty of superposition and entanglement is that they imply a violation of what is known in the literature as *value definiteness* (Calosi and Wilson, 2018; Dorato, 2020; Michels et al., 2021): the metaphysical thesis that properties (physical 'observables', in the language of physicists) have precise values at all times. Where *value definiteness* makes sense in classical mechanics, it clearly must be dropped in the light of QM. I show how superposition and entanglement entail a violation of value definiteness in the following chapter (Cf. §5.3.2). What matter at this stage is the fact that the lesson one has to take home from this violation of *value definiteness* differs from author to author, and the consensus is nowhere to be found. Some argued that superpositions and entanglement must be understood as if quantum mechanics points out the limit of human epistemology, others that quantum mechanics is a theory

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any phenomena – the whole universe included. I must confess I feel uneasy with the latter view – for once, the wave function of a small every day's object cannot, in practice, be written down. For another, classical mechanics remains the best theory at everyday objects' scale (with velocities significantly less than c). Hence, I assume in what follows that the domain of QM is the atomic and subatomic scale when relativistic effects are negligible.

[9] This is argued in more details by other philosophers as well, e.g., Monton (2004); Williams (2019); Egg (2021).

[10] Lewis (2004) wrote that "quantum mechanics is, in a nutshell, a theory about superposition," thus contradicting Schrödinger (1935) who more famously wrote: "I would not call [entanglement] one but rather the characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." From a mathematical point of view, entanglement is nothing but superposition on a composite Hilbert space. However, superposition raises different metaphysical consequences in the case of simple and composite Hilbert spaces. Consequently, I consider both distinctive aspects of QM, and I treat them separately in what follows.

concerning ensembles rather than single systems; others that no meaningful question can be posed before measurements are performed, thus echoing an empiricist understanding of scientific theories. The list could continue forever. The only realist understanding of the violation of value definiteness seemed to accept that quantum properties are indefinite in themselves, without any appeal to limitations of either our knowledge or their mathematical representation. Unfortunately, the idea that quantum particles have indefinite properties were so incredible that it motivated many to adopt an anti-realist reading of the theory. However, the idea that indeterminacy might be a feature of reality has been increasingly explored in the recent metaphysical literature. Since we now have many detailed accounts of metaphysical indeterminacy, it might be worth attempting to understand the formalism of textbook QM realistically. As Darby (2010, p. 228) puts it: “With a metaphysical thesis crying out for an indisputably worldly example, and some physics in apparent need of metaphysical clarification, making a connection promises mutual benefits.” Since investigating whether QM offers a genuine example of metaphysical indeterminacy – and if so, under what assumptions – is a relevant part of the rest of the thesis, it makes sense in this context to focus on textbook QM.

4.2 Spin

The thesis does not aim to propose a complete ontology of QM. Instead, sticking to the methodology outlined in §2, I focus on a narrow aspect of QM, that is, the theory’s description of spin. Spin is considered an irreducible property of quantum particles, which can be imagined as an intrinsic and additional degree of freedom of a particle’s magnetic moment. It can be ‘imagined’ as such because the attempts of construing spin in literal mechanical terms failed (Cf. (Morrison, 2007)); no further reductive physical explanation of what is spin have been successfully proposed so far.[11] Thus, spin ended up being considered an intrinsic physical property of particles, on a par with mass and charge.

Even if we set aside methodological criteria, further reasons motivate my focus on spin only. For me, an important one has been that spin has no equivalent in classical theories. Contrary to energy, position and orbital angular momentum, no property corresponds to spin at the classical limit. Having spin is the mark of quantum entities, and, as such, it is a central aspect of the whole theory. Furthermore, spin is often invoked to introduce the ontological conundrum of quantum ontology, e.g., (Albert, 1992, Ch. 1). The reason why spin, rather than some other observables, is invoked in these cases is two-fold. First, it is mathematically simpler than other observables. As such, the hope is that metaphysical conclusions on the spin will apply to other observables if their differences amount just to a more complex mathematical characterisation. Second, since spin has no equivalents in classical mechanics, we have no pre-theoretical intuitions about it. This allows one to investigate the metaphysics

[11] But see the authors quoted in fn.3, p. 38, in (Saatsi, 2020).

of spin with few preconceptions about what is and what is not possible; conversely, pre-theoretical intuitions inevitably influence one's analysis when, say, the position of quantum entities is involved.

As a matter of fact, spin cannot be measured directly (Cf. Morrison (2007)); consequently, distinguishing between the purely mathematical and the physical content of QM's description of spin is not an easy task. Moreover, the fact that spin cannot be simultaneously measured along different axes is a significant conundrum of quantum theory. The problem is not merely that spin's spatial components cannot be measured; rather, that supposing that they have a definite value before measurements lead to conclusions contradicting QM's empirical predictions, as shown by some formal theorem (Gleason, 1957; Kochen and Specker, 1967). The fact that spin's spatial components lack simultaneous definite values leads some to doubt spin's physical reality. However, since the founding fathers of QM, the strong impression that spin is a *physical* property of quantum entities is widely shared (Morrison, 2007).

The impression above is strengthened when one realises how fundamental spin is for contemporary science. Indeed, spin is so essential that imagining removing its status as a physical property means rewriting a substantive part of our best current theories. And it seems impossible to imagine theories without spin as successful. Saatsi (2020) offers a long list of spin's application and relevance. Yet, as explicitly acknowledged, Saatsi (2020)'s examples are just the tip of the scientific relevance of spin. In a footnote (Saatsi, 2020, p.41, fn.11), he notices that the website arxiv.org – which is the most used preprint archive by physicists – contains almost 38,000 papers with 'spin' in the title. The number at the time of the present writing increased to 46,225. Here, a short list of examples should be more than enough to convince the reader of spin's fundamentality in contemporary science.

Spin has theoretical implications in many subfields of physics, such as condensed matter physics, optics, and high-energy elementary particle physics (Cf. Duplantier et al. (2009)). From a theoretical point of view, the mathematics of spin seems to be fundamental, e.g., in elementary particle physics to combine quantum mechanics and the theory of relativity. Moreover, spin's theoretical implications extend to empirical sciences other than physics, such as molecular biology and chemistry (Cf. Likhtenshtein (2016)). The contemporary biological understanding of photosynthesis and chemical explanation of molecular bonds, say, heavily rely on the spin. Furthermore, spin has incredibly vast technological applications that range from spectroscopic techniques – that, setting aside physics' laboratories, are crucial for modern organic chemistry and medicine – to quantum computing. Moreover, the applications of spin to electronics led to the founding of a new discipline, spintronic, which studies how to manipulate electric currents based on electrons' spin to develop new nanotechnologies. That the theoretical understandings and the technological achievements above are possible without spin is prima facie inconceivable. Furthermore, as highlighted by Saatsi (2020), the applications of spin to electronic currents make spin effectively manipulable. That is relevant if one agrees that manipulability is a decisive criterion in favour of an unobservable's existence (following Hacking (1983)).

The incredible empirical success of QM in general, and spin in particular, seem to point toward the necessity of some realist understanding of QM's description of spin. Before turning to metaphysical aspects of spin – to whom the next chapters are dedicated – I finish the present chapter with two relevant digressions. First (§4.3), I sum up how spin is mathematically described in QM. This section offers a concise description of the mathematics of spin and is also instrumental in setting the notation of what follows. Then (§4.4), I discuss some recent challenges to spin's existence. Determining whether a scientific realist must commit themselves to spin is crucial for what follows. Indeed, if spin were not real, its metaphysical characterisation would be pointless. Conclusions follow (§4.5).

4.3 Mathematical interlude

From a mathematical point of view, spin is introduced theoretically through the group $SU(2)$ – a group which corresponds to rotations.[12] Why should the rotation group characterise the spin? The answer would be along the following lines. The orbital angular momentum[13] L of a particle is the generator of the rotation group $SO(3)$. Insofar as $SO(3)$ is not simply-connected,[14] its universal covering[15] cannot be equal to $SO(3)$ itself. One can show that the universal covering of $SO(3)$ is the group $SU(2)$. Although the two groups ($SO(3)$ and $SU(2)$) are locally equivalent, i.e. they have the same algebra,[16] they differ globally, i.e. a rotation of 2π is equivalent to a null rotation only for $SO(3)$ but not for $SU(2)$ (where a null rota-

[12] To be more precise, SU(2) is the group of all $2x2$ unitary matrices with determinant 1, i.e. the rotations in the complex plane without reflections.

[13] The angular momentum of a point particle in classical physics is defined as $L = r \times p$, where r and p are respectively the position and the momentum. The same definitions are used in QM, but r and p are promoted to self-adjoint operator acting on the Hilbert space $L^2(\mathbb{R}^3, d^3x)$. Physicists add the word orbital to distinguish it from the spin angular momentum.

[14] A topological space is said to be simply connected if every path between two points a and b can be continuously transformed into any other path connecting a to b while preserving the two endpoints in question. It can be shown that the differentiable manifold structure of $SO(3)$, which is a ball B^3, is not simply-connected.

[15] The universal cover of a connected topological space X is a simply-connected space Y with a map $f : X \to Y$ which is called the covering map. Strictly speaking, given a topological space that is not simply connected, it is possible (under some assumptions) to build its simply-connected completion (universal covering) by means of a proper map (covering map). If a topological space is simply-connected, then it is its universal covering. A universal cover is always unique.

[16] This statement presupposes some acquaintance with group theory. In short, let g be an element of a group G that acts globally on a topological space (under further assumptions, this is true at least for Lie groups, like the ones mentioned in the text). Given a group, one can write down its algebra. The exponential map gives the relation between an element of the group and its algebra: $g = e^{i\theta\tilde{g}}$, where θ is a parameter that moves elements of the topological space and $\tilde{g}$ is an element of the algebra. Here, 'Locally' means that θ is sufficiently small. Therefore, to say that g_1 and g_2 are locally coincident, is the same as to say that $G_1 \ni \tilde{g}_1 = \tilde{g}_2 \in G_2$, i.e. that G_1 and G_2 have the same algebra.

tion is equivalent to a rotation of 4π). Since the elements of the two groups have different global properties, it follows[17] that there must be two operators of the angular momentum: the so-called *orbital angular momentum* L and the *total angular momentum* J. The two angular momenta must have different global properties and conditions on the spectrum. In particular, the spectrum of the operator $\mathbf{L}\cdot\mathbf{n}$ belongs to the set $\hbar\mathbb{Z}$, while the spectrum of operator $\mathbf{J}\cdot\mathbf{n}$ belongs to the set $\pm\hbar\frac{\mathbb{N}}{2}$. The orbital angular momentum acts on the squared integrable function of $\mathbb{R}^3$. Let us call the Hilbert space where J acts $\mathscr{H}_j = L^2(\mathbb{R}^3, d^3x)\otimes\mathscr{H}_s$, (where $L^2(\mathbb{R}^3, d^3x)$ is the space where L is usually defined and $\mathscr{H}_s$ is, for the time being, an unknown Hilbert space). Since $J \neq L$ and since the Hilbert space on which J should act is bigger than the Hilbert space on which L acts, we conclude that there must be a new angular momentum, S, such that $\mathbf{J} = \mathbf{L}+\mathbf{S}$. Not only S must act on the left-over part of the bigger Hilbert space ($\mathscr{H}_s$), but by construction $\mathbf{S}$ should be the generator of $SU(2)$ such that $\mathbf{J}$ act on the Hilbert space $\mathscr{H}_j$ as follows:

$$\mathbf{J} = \mathbf{L}\otimes\mathbb{I}+\mathbb{I}\otimes\mathbf{S}\,. \tag{4.1}$$

As the reader will have already understood, S is indeed the spin. The introduction of spin using the group $SU(2)$ is so explained by the necessity of $SO(3)$ of having a universal cover different from itself.[18]
As we have just seen, spin in QM is an infinitesimal generator of the rotation group $SU(2)$, described by a vector $\mathbf{S}$ (which in turn can be represented as a self-adjoint operator on $\mathscr{H}_s$). The fact that $\mathbf{S}$ is a generator of $SU(2)$ means that its components should obey the following commutation relations:

$$[\hat{S}_i,\hat{S}_j] = i\hbar\varepsilon_{ijk}\hat{S}_k \qquad , \qquad i = x,y,z\,, \tag{4.2}$$

where ε_{ijk} is the Levi-Civita symbol. The fact that the spin components do not commute, together with the fact that they all are self-adjoint operators acting on the same domain, also implies that there is an indeterminacy principle between them:

$$\Delta\hat{S}_i\Delta\hat{S}_j \geq \frac{\hbar}{2}\left|\varepsilon_{ijk}\left\langle\hat{S}_k\middle|\hat{S}_k\right\rangle\right|\,. \tag{4.3}$$

Operators corresponding to spin along different directions are not the only operators that can be built. Indeed, it is possible to build up the operator $\hat{S}^2 = \mathbf{S}\cdot\mathbf{S}$. Mathematically, $\hat{S}^2$ is called the Casimir operator of the $SU(2)$ group. An interesting feature of these kinds of operators is that they commute with any other group

[17] From the considerations above, it follows that a unitary representation of $SU(2)$ in $\mathscr{H}$ is described by a unitary operator $\tilde{U}(\phi,\mathbf{n})$ (Bargmann's theorem). Such an operator is given in the exponential map applied to an element of the algebra $su(2)$, that we call J (Stone's theorem). Since rotation of 2π is not the null rotation, the spectrum of J cannot be an integer, but it must consist in half-integers. That is to say that J is 'bigger' than L, since it also contains semi-integer eigenvalues (whether L contains only the integer ones). If J is bigger than L, there must be a S such that $J = L+S$.

[18] Note cursorily that, to obtain representations avoiding any ambiguity of phase, one must move to the universal cover (Bergmann's theorem).

operator. Indeed, starting from (4.2), one can show that $\hat{S}^2$ commutes with every component (i.e. $\hat{S}_i$, for every i). Moreover, $\hat{S}^2$ commutes also with $\hat{P}$ and $\hat{X}$.[19] In other words, that $\hat{S}^2$ commutes with all the elements of the irreducible set of observables $\{\hat{\mathbf{X}}, \hat{\mathbf{P}}, \hat{\mathbf{S}}\}$. Since, only identity commutes with all operators $\{\hat{\mathbf{X}}, \hat{\mathbf{P}}, \hat{\mathbf{S}}\}$, it follows that $\hat{S}^2$ should be a multiple of the identity. Being a multiple of the identity entails that an eigenvalue of $\hat{S}^2$ will characterize every particle. Finally, since $\hat{S}^2$ commutes with $\hat{S}_i$, it is possible to choose an arbitrarily component of spin to characterize the spin states of the particle as follows:[20]

$$\hat{S}^2\,|s,s_i\rangle = \hbar^2 s(s+1)\,|s,s_i\rangle \qquad , \qquad s \in \mathbb{N}/2\ ; \tag{4.4}$$
$$\hat{S}_i\,|s,s_i\rangle = \hbar\, s_i\,|s,s_i\rangle \qquad , \qquad s_i = \{s,\ s-1,\ \ldots, -s\}\ , \tag{4.5}$$

where i is here a generic axis upon which the spin can be measured. To each measurement of spin along a given spatial direction, for example the x-axis or the $z-$axis, there it will be associated an operator of spin that will behave as equation (4.5). In the example, the eigenvalue relations of the corresponding operators of having spin along the x-axis and the $z-$axis will be:

$$\hat{S}_x\,|s,s_x\rangle = \hbar\, s_x\,|s,s_x\rangle \qquad , \qquad s_x = \{s,\ s-1,\ \ldots, -s\}\ ; \tag{4.6}$$
$$\hat{S}_z\,|s,s_z\rangle = \hbar\, s_z\,|s,s_z\rangle \qquad , \qquad s_z = \{s,\ s-1,\ \ldots, -s\}\ . \tag{4.7}$$

After measurement of spin along one of these axes, the x-axis say, the state of the system will be $|s,s_x\rangle$, where the eigenvalue s_x will be the outcome of the measurement. Similar eigenvalue relations hold for every possible axis upon which one can measure the spin.

In the case of the electron, which is a spin-$\frac{1}{2}$ particle, every operator of spin along a particular spatial direction has two possible eigenstates: 'spin up' with eigenvalue $+\frac{1}{2}$ and 'spin down' with eigenvalue $-\frac{1}{2}$. In the philosophical literature, these eigenstates are often represented, respectively, as $|\uparrow\rangle_i$ and $|\downarrow\rangle_i$, where i stands for the axis upon which spin is aligned. That is, for every axis, the relationship between operators and its eigenvectors and eigenvalues are the following:

$$\hat{S}_i\,|\uparrow\rangle_i = +\frac{1}{2}\,|\uparrow\rangle_i\ ; \tag{4.8}$$
$$\hat{S}_i\,|\downarrow\rangle_i = -\frac{1}{2}\,|\downarrow\rangle_i\ . \tag{4.9}$$

I also employ this simplistic notation in the rest of the thesis when referring to electrons' spin.

The well-known empirical fact that these values of spin along different axes cannot be measured simultaneously has its mathematical explanation in (4.2). This equation signals us that an indeterminacy relation holds between the spin values along differ-

[19] As all S_i do, insofar as they act on different Hilbert spaces.

[20] Note that when s and s_i are inside the ket, they just label the eigenstate, whereas when they are outside the ket they mean the eigenvalues of the operators $\hat{S}^2$ and $\hat{S}_i$.

ent axes. Furthermore, some mathematical theorems show that spins along different directions cannot have any definite value before a measurement. A classic way of understanding these results is to regard these indeterminacy relations as something that does not concern our epistemic status. Rather, it pertains to quantum systems themselves. Theorems like Kochen and Specker (1967); Gleason (1957) and Pusey, Barrett, and Rudolph (2012) show indeed that if one assumes that spin values along different axes have a definite value, then one reaches a contradiction. These results show something deep about how the quantum domain is rather than what human beings can know of it. Yet what they tell us precisely is a matter of controversy. The next chapter (§5) aims to investigate under what conditions one can claim that these indeterminacy relations among spin values can be understood as a genuine case of metaphysical indeterminacy. Instead, we now turn to whether scientific realists could avoid any realist commitment to spin.

4.4 Is spin a physical property?

Asking whether scientific realists should commit themselves to the existence of spin should raise an incredulous stare. Even from the brief description above, that spin satisfies every condition that realists need should be evident. QM, in general, is one of the most empirically confirmed theories we have ever had, and spin is one of its central concepts. Furthermore, spin is manipulable in its technological applications, and it is fundamental to unify different disciplines (such as explaining quantum effects in chemical bonds). Moreover, it is central to explain uncountably many phenomena, and thus it has tremendous explanatory power. From photosynthesis to the manipulability of electronic current with spin valves, these phenomena would be miraculous if spin were not a physical property. Finally, spin is theoretically necessary for combining quantum mechanics and special relativity. Not to mention that spin is one of the properties, together with mass and charge, through which particles are differentiated in the standard model. To avoid any commitment to spin means that a significant part of contemporary well-established science must not be understood ontologically. This consequence is something that scientific realists should be reluctant to accept.

However, a small debate concerning spin realism rose in the recent literature (Vickers, 2020; Saatsi, 2020; Egg, 2021). In particular, Saatsi (2020) and Vickers (2020) propose the same argument against a commitment to spin, yet they draw significantly different conclusions from it.

I dub Saatsi (2020)'s and Vickers (2020)'s argument 'The spin underdetermination argument.' In a nutshell, it can be thus summarised. Spin is not necessary for the empirical successes of QM. The inessentiality of spin derives from the fact that there is at least one interpretation of QM, *viz.*, some form of Bohmian mechanics (Daumer et al., 1996), according to which spin is not a property. This interpretation explains alleged spin behaviour in terms of other properties and relations, such as the particle's determinate location. Insofar as at least one interpretation of QM is devoid of spin as a physical entity, caution is needed concerning our commitment to spin. Here, the conclusions of Saatsi (2020) and Vickers (2020) diverge. According to the former, scientific realists must not commit themselves to any *metaphysics* of spin whereas, according to the latter, no commitment whatsoever is due.

Egg (2021) already replied to Saatsi (2020)'s and Vickers (2020)'s argument by showing, instead, that some commitment to the existence of spin is warranted. Indeed, Egg (2021) successfully showed that their argument crucially relies on the idea that spin is a *fundamental* properties of particles. For example,[21] wave-function realists[22] claim that the only fundamental entity of QM is the wave function. Prima facie, one might think that wave function realists do not commit to spin.[23] However, this means either confounding ontology with *fundamental* ontology, or subscribing to the view that ontological commitment is warranted just for fundamental entities. Indeed, wave function realists have to show how particles and everyday objects – measurement apparatuses, in particular – emerge from the wave function to explain QM's empirical success. Even wave function realists, then, should commit to the idea that spin is an intrinsic property of particles even though spin is not a fundamental property – since the entities that instantiate it are not fundamental themselves. By insisting that one's commitment cannot be reduced to fundamental entities only, Egg (2021) ends up arguing that some commitment to spin is, after all, necessary. I am not much interested here to precisely assess whether Egg (2021)'s reply is successful. Rather, I think other lines of replies can be put forward to argue that scientific realists ought to commit themselves to spin. In the next two subsections – §4.4.1 and §4.4.2 – I sketch how scientific realists could resist, respectively, Saatsi (2020)'s and Vickers (2020)'s different conclusions.

4.4.1 Truth vs. progress realism

Saatsi (2020) calls 'truth-content realism' the standard form of scientific realism, and he argues that this forms of realism commit one to strong metaphysical commitments. Being a realist, say, on QM means accepting that QM is true because its mathematical apparatus is a true description of reality. When truth-content realists admit to being ontologically committed to an unobservable, they also commit to its metaphysical description. Then, Saatsi (2020) argues that the case of spin underde-

[21] The example that follows is mine. Egg (2021) instead makes a similar example applied to Bohmian mechanics. Honestly, I am not sure that Egg's strategy could be applied to Bohmian mechanics. Indeed, Bohmian mechanics explains spin in terms of the particle's position. Furthermore, the strong impression is that nothing emerges in this case: 'having spin up', say, is understood as the fact that a particle is literally in the upper part of the magnetic field of a Stern-Gerlach apparatus.

[22] The view has been firstly proposed in (Albert and Loewer, 1996). Ney and Albert (2013) is a classic collection discussing several details of wave function realism. Ney (2021) is an updated presentation of the view as well as a discussion of the main challenges raised against it.

[23] The total wave function of any system surely contains a spin part. However, here I mean spin *qua* property of particles.

termination shows that truth-content realism is somehow inadequate for quantum mechanics: since spin is not real in some interpretations, realists should avoid commitments to its metaphysics and, as a consequence, rethink their whole realist approach. In particular, Saatsi proposes to accept instead his form of scientific realism, which he dubs *Progress realism* (Saatsi, 2019, 2020). The core of progressive realism is that metaphysical hypotheses, which truth-content realists can hardly avoid, are idle in explaining a scientific theory success; furthermore, they exactly are what is discarded when a more fundamental theory replaces a current one. Consequently, progressive realists avoid commitments to the metaphysical aspects of science. On the one hand, progressive realists insist that "the empirical successes of theoretical science are by and large due to theories latching onto reality in ways that ground those successes" (Saatsi, 2020, p. 43). On the other hand, they claim that an appropriate representational relationship holds between scientific theories and the world, yet nothing can be said about that relationship. Progressive realists can claim that from our perspective, the empirical success of current scientific theories and the failure of past ones is due to progress in the representational success of our theories. Yet, this is supposed to not be "an assertion about the world, or about what we can claim to know" (Saatsi, 2020, p. 43).[24] The cases of spin underdetermination should show that realists must "avoid getting sucked into 'deep' metaphysics" (Saatsi, 2020, p. 50) and adopt a *Progress realism* instead toward spin.[25] I have two main worries with Saatsi (2020)'s conclusions.

First, the underdetermination argument relies on the fact that spin is not a physical property in some interpretations of QM. If we cannot rationally choose an interpretation over the others, then we should not commit ourselves to spin. Indeed, the fact that spin is not necessary to account for QM's empirical success should be a signal for realists that no commitment is necessary.[26] However, Saatsi argues that a different conclusion has to be drawn from the problem of spin underdetermination, namely, that:

> none of the candidate theories [i.e. the realist interpretations of QM] seems worthy of the realist's epistemic commitment, given their involvement of metaphysical assumptions that go beyond what realists should deem responsible for quantum theories' explanatory and predictive success. (Saatsi, 2020, p. 48)

It seems to me that there is a missing step in Saatsi's argument. The problem of spin underdetermination consists of us asking ourselves whether 'spin' refers to some physical entity or not. By it being absent in some interpretations of QM, we conclude that no commitment to it is due. And I meant any commitment whatsoever: not only we should avoid committing to a particular metaphysics of spin, but we should

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[24] On the notion of 'representation relation' see also (Saatsi, 2016).

[25] Note that Saatsi (2020) does not criticise here metaphysics of science as a field of research. Indeed he explicitly acknowledged that investigating 'deep' metaphysics is worth pursuing. Rather, he wants to argue here that scientific realists, as such, has not to be committed to deep metaphysics of any kind.

[26] Also to avoid offering a new example for the pessimistic meta-induction (Laudan, 1981), in the case in which one of those interpretations without spin will be confirmed in the future as the correct one.

also avoid answering the simple question of whether ‘spin’ refers to a physical entity or not. Perhaps, from spin underdetermination, it does follow that scientific realists must avoid commitment to realist interpretations of QM in general. However, this conclusion does not, by itself, entail that one has to be a realist – progressive or not – about spin. Nonetheless, Saatsi happily concedes that ‘spin’ has some factual reference – spin, as described by QM, is in some representative relation with reality, to use his terminology. As far as I can understand, the missing premise to Saatsi argument is that scientific realists have to ‘ignore’ realist interpretations of QM and instead opt for a realist yet interpretation-neutral approach. We have to be realists toward spin (and QM, more generally), given spin’s incredible relevance for contemporary science – as (Saatsi, 2020, §3.2) acknowledges – but only in its textbook description. The missing passage to derive Saatsi’s conclusion is that the underdetermination has to be solved by adopting a realist and interpretation neutral approach to QM. On the one hand, this is a substantial assumption that takes a clear stance toward the problem of QM’s underdetermination. Since I am sympathetic with this approach, I have not much more to say than what I already said above (Cf. §4.1). On the other hand, it shows that the argument favouring progressive realism is weak. If scientific realists must not take seriously realist interpretations of QM (and follow an interpretation-neutral approach instead), then the spin underdetermination argument itself should not be taken as a severe problem in the first place. However, that argument is taken by Saatsi as the reason to accept *Progress realism* toward spin. In this way, Saatsi seems to implicitly assume a view that undermines the main reason in favour of his *Progress realism*.

The second issue I have with Saatsi (2020)’s conclusions concerns his account of scientific realism, viz., progressive realism. The following criticism of Saatsi’s view has been already discussed thoroughly in (Egg, 2021). Hence, I limit myself just to a quick sketch of it. As it should be clear by presenting my account of scientific realism, I am sympathetic toward the idea that severe epistemic limitations prevent us from knowing how reality is. In one way or another, Saatsi’s and my view are pretty close: their core indeed is that scientific realists’ beliefs in scientific theories do not concern the world itself or our knowledge[27] of it. In my view, however, the scientific and the metaphysical descriptions have to go hand in hand. And the reason is that with no clear metaphysical picture, it is unclear what scientific theories tell us about the phenomena they describe. Inserting some epistemic humility in standard scientific realism is one thing. But to avoid saying anything whatsoever about the commitment is another. Accepting that ‘spin’ is in a representational relation with reality without accepting to propose possible metaphysical descriptions is tantamount to accepting the claim that the word ‘spin’ refers to something. Albeit, according to *Progressive realism* we never know what such a something might be. Nevertheless, the commitment to a putative factual reference of scientific terms is too weak to account for the incredible success of scientific theories. When scientific realists explain a Stern-Gerlach experiment by means of spin interacting with the inhomogeneous magnetic field, they do so by talking of spin as an intrinsic property of

[27] Where ‘knowledge’ here I mean the standard definition, i.e. justified true belief (Ichikawa and Steup, 2018).

particles. They not only explain these kinds of phenomena by saying that the word 'spin' refers to something going on in the Stern-Gerlach; 'something' which they cannot say anything about. They rather insist that particles deviate such and such *because* they have this property of having spin, which is truthfully described by QM's formalism. If one rejects any metaphysical commitment whatsoever, then one's realist stance toward a scientific realist ends up being just the weak belief that some words (or some mathematical structures) refer to something. The scientific and the metaphysical descriptions of physical phenomena have to go hands in hands if one believes that empirical sciences *explain* the phenomena they investigate. Accordingly, scientific realists must commit themselves to the deep metaphysical aspects of scientific theories.[28]

4.4.2 Against spin antirealism

Vickers (2020) presents the spin underdetermination argument to conclude that (selective) scientific realists must not be committed to spin. The very fact that spin is absent in at least one interpretation of QM is more than enough, according to Vickers, to set the realist free of any commitment to spin's existence. The conclusion drawn by Vickers is too strong, and it can thus be resisted in many ways.[29]
First of all, the phenomena accounted for by textbook QM are indeed explained by other interpretations as well. However, as we have seen above (§4.2), spin is fundamental in several scientific branches other than physics, and it is at the very core of the development of uncountably many technological applications. Hence, to conclude that no commitment to spin is necessary, one must not only show that QM is underdetermined in its domain. Indeed, one must also show how the interpretations without spin can explain the phenomena and the technologies whose functioning is explained in terms of spin. How do spintronics' devices and photosynthesis, say, work according to the Bohmian interpretation[30] that excludes spin from the range of physical properties? How should we reformulate the standard model if we cannot rely on the spin to distinguish different particles? To my knowledge, some work in this direction has been done recently.[31] Yet, much more remains to be done to show that the existence of spin, as a central concept in contemporary science, is

[28] Obviously, the metaphysics of science is a field too vast to pretend that every scientific realist has a clear commitment to every metaphysical aspect of all our best scientific theories. I mean that scientific realists, as a community, have to elaborate metaphysical commitments of our current best scientific theories.

[29] An obvious line of reply consists in arguing that there are several criteria to choose among rival theories, and not all the interpretations of QM are really on par concerning them. In particular, Vickers (2020) makes the big assumption that Bohmian mechanics can be framed in a relativist context, which is something disputed by many. Saatsi (2020) acknowledges directly that not all the interpretations are equal, and yet he seriously faces the consequences of spin underdetermination. I do not belabour more on this line.

[30] In at least some variants, such as those quoted by Vickers (2020).

[31] See, e.g., Benseny et al. (2014); Pladevall and Mompart (2019); Nikolić (2019).

underdetermined. Otherwise, one could argue that, given the importance of spin for empirical sciences different from physics, one has to prefer realist interpretations of QM that consider spin a physical property to those that deny it.

Second, that underdetermination alone is enough to set the realist free of any commitment seems too strong as a criterion. If we take this criterion seriously, we should conclude that QM deserves no commitment whatsoever. Given any quantum entity, I can think of at least, at least a (realist) interpretation that disputes its existence. There are no particles in GRW with an ontology of flashes, but there are, according to Bohmian mechanics (Esfeld and Gisin, 2014). GRW with flashes and Bohmian mechanics agree that the wave function has a quasi-nomological status (Esfeld and Gisin, 2014). Yet, they disagree with wave function realists that consider it a physical entity (Ney, 2021) and with those that consider it just a mathematical apparatus (Suárez, 2015; Rovelli, 2018b; Lombardi, 2019). The list could continue. The point is that if underdetermination is enough for avoiding realist commitments, then scientific realists should avoid any commitment whatsoever to QM. Perhaps, the reader is happy to accept such a conclusion. However, the incredible empirical success of QM seems a strong clue toward the necessity of at least *some* realist commitment. Without further arguments favouring the idea that QM, in general, deserves no commitment, the underdetermination of a quantum entity by different interpretations seems too strong as a criterion for determining the realist's commitment.

Finally, one of the stronger arguments in favour of scientific antirealism is the pessimistic meta-induction (Laudan, 1981). The argument assumes that we are in the same epistemic conditions as our predecessors. By noticing that many unobservables to which past scientists were committed turned out to not exist according to contemporary theories, it concludes that we should not commit ourselves to any unobservable. A common strategy among realists to address this argument consists in showing that part of the factual reference of past theories is conserved in new ones. Past theories were not wholly false, and their true part – recovered in contemporary theories – explains why they were considered empirically successful in the past. Showing that some entities/explanations are preserved during the theory change is vital for justifying contemporary realists' commitments. Similarly, it has been argued that one reason for doing metaphysics out of 'false' theories is that such work helps cash out the metaphysics of more fundamental theories (Cf. §2). Either if one is a realist or accept the meta-metaphysical view above, one should adopt the following criterion to choose among rival metaphysics: Given two scientific theories such that one is more fundamental[32] than the other, if an entity x is part of the ontology of the more fundamental theory, then all things being equal, one has to prefer a metaphysics of the less fundamental theory that includes x rather than one that excludes it. On the one hand, the realists should accept this criterion to show that something is preserved during the theory change. On the other hand, the metaphysicians should be happy to investigate phenomena in a stricter domain before investigating them in a more fundamental one.

[32] Here it is a rough definition of fundamentality. A theory is more fundamental than another one if the latter's domain is a subset of the former's domain, and all the phenomena investigated by the latter are explained in terms of entities of the former.

Let us turn back to the case of spin underdetermination. If one accepts the criterion above, one should conclude in favour of spin's existence. The reason is, roughly, the following. We know already that non-relativistic QM is a partially false theory. Quantum field theory (QFT, hereafter), both in its relativistic and non-relativistic fashions, is more fundamental than QM.[33] Similarly, the attempts of combining quantum mechanics and general relativity can be seen as an attempt of proposing a theory more fundamental than both QFT and general relativity. Now, the exact ontology of QFT is an entrenched camp, and nothing secure can be said about it (Kuhlmann et al., 2002). However, we know already that spin is an essential part of it. The fact that spin is necessary for making quantum theory relativistic can be considered as a strong clue toward the necessity of some realist commitment toward it. Let me quickly sketch below why spin is so important in QFT. The first reason is the so-called *Dirac argument*, which originates from Dirac's first attempt of turning quantum mechanics into a relativistic theory (Cf. Weinberg (1995, Ch. 1.1, 2.4)). Relativity theory describes nature at high speed, i.e. for $v \approx c$, and unifies time and space. A formal consequence of the unification of space and time into spacetime is that Lorentz transformations can mix time and space coordinates. A *Relativistic Quantum Theory* should have the same features; that is, space and time should be on the same footing. However, the Schröedinger equation has only one time derivative but two spatial derivatives. Thus, to extend the validity of the Schröedinger equation to the relativistic realm, one needs to modify it to treat space and time in the same way formally. Starting from Klein and Gordon's partially failed[34] attempt of making Schröedinger equation relativistic, Dirac looked for an extension of it with the following desiderata: the equation had to be (i) linear in time, to solve the probabilistic interpretation problem of the Klein-Gordon equation, (ii) linear in space, to have space and time on the same footing; and (iii) it must be possible to recover from it the Klein-Gordon's equation. Dirac showed that to satisfy these constraints, one has to introduce an extra discrete label on the wave function. This additional discrete label accounts for a particle's spin. As such, spin ends up being a necessary ingredient to extend the formalism of quantum mechanics to the relativistic realm.

A second reason why spin is so fundamental in relativistic extensions of QM can be appreciated once one realises that contemporary formulations of QFT mathematically characterise spin and mass in an analogous formal way. The main lesson of Special Relativity is that the physics should look the same in all *inertial frames*; and in a relativistic spacetime, if the coordinates x^μ represent an inertial frame, then also the coordinates $\tilde{x}^\mu$ defined as:

$$\tilde{x}^\mu = \Lambda^\mu{}_\nu \, x^\nu + a^\mu \ , \tag{4.10}$$

represent an inertial frame (where $\Lambda^\mu{}_\nu$ is a *Lorentz matrix* and a^μ is a vector of spacetime translation). In other words, inertial frames are defined as the equiva-

[33] That is, QM's domain should be a subset of QFT's domain. By putting some energy and velocity limits to QFT, one should be able to recover QM's predictions.

[34] The solution of Klein-Gordon's equation does not have a probabilistic interpretation, as it is the case for ordinary quantum mechanics.

lence class of the transformation (4.10). If we want a theory to be relativistic, we thus have to build an invariant theory under the transformations above. The set of all Lorentz transformations and spacetime translation forms the Poincaré group. Hence, the mathematical requirement that a theory should obey to be relativistic is that it should be invariant under the Poincaré group. However, we do not know yet how these group's elements act on the fields of QFT and whose fields have nice transformation properties under these transformations. To understand these features, one has to look at the representation theory of the Poincaré group, which tells us which are the fundamental building blocks of the theory. The Poincaré group has two Casimir operators[35]: $P^{\mu}P_{\mu}$ and $W^{\mu}W_{\mu}$. The former represent the value of the particle's mass, while the latter represent the value of the particle's spin. Thus, a relativistic quantum theory particle[36] is defined in terms of its mass and its spin. In other words, mass and spin are treated formally as two parameters to classify the fundamental building blocks of the theory. Now, as a matter of fact, no scientific realist disputes the existence of mass as an intrinsic property of particles. Since mass and spin have the same mathematical description in QFT, we should conclude that a commitment toward the existence of the property of having mass entails a similar commitment toward spin. That is if we want to avoid being antirealist toward mass as well.

4.5 Conclusions

In the chapter, I presented two philosophically relevant prerequisites of what follows: an interpretation-neutral approach toward QM and the relevance of spin as a quantum property. These are two substantial philosophical assumptions that, for a proper defence, would require a separate thesis each. Instead, the chapter aimed to offer a general description of them and sketch some reasons in their favour.

In particular, the fact that scientific theories might be underdetermined is a considerable problem that any scientific realist faces. Indeed, underdetermination menaces the scientific realist's claim that our best sciences offer a true description of the world: if different theories describe the same phenomena, how can we choose which description is the right one? Unfortunately, the case of QM's underdetermination is one of the most severe. The theory is underdetermined by more than 70 years, and it also presents (at least) two layers of underdetermination: (a) the phenomena within the quantum domain can be explained by different interpretations of QM, and (b) each interpretation is compatible with different ontologies. In the chapter, I showed – following Egg (2021) – some ways out of the underdetermination, and I presented the one assumed in the rest of the thesis: an interpretation-neutral approach to the metaphysics of QM. Such a choice has been motivated by several reasons. First, it

[35] A Casimir operator is an operator who commutes with all the algebra generators.

[36] I am not committing myself here to the view that QFT's ontology is an ontology of classical particles, i.e. tiny 'billiard balls'. It might very well be that the fundamental ontology is instead a field ontology. Be that as it may, as far as I know, nobody ever disputed that whatever the building blocks of QFT are, they have a mass.

best fits the debate addressed in the thesis, and it makes sense to focus on the standard formalism from a methodological point of view. Second, that textbook QM satisfies the criteria for being one of our best contemporary scientific theories, and, as such, it deserves some realist commitment.
Then, I turned to a presentation of spin in non-relativistic QM. I first explained the relevance of spin for the contemporary scientific understanding of the world. After a brief description of how spin is formally characterised in QM, I turned to a recent critique of the idea that scientific realists must be ontologically committed to spin. The argument against spin realism proposed independently by Saatsi (2020) and Vickers (2020) hinges on the fact that spin's very existence is underdetermined since some interpretations of QM do not consider it as a genuine physical property. I discussed, in turn, Saatsi (2020)'s and Vickers (2020)'s different conclusions by arguing instead that scientific realists should commit themselves to spin.

Chapter 5
Quantum metaphysical indeterminacy

5.1 An introduction to metaphysical indeterminacy

Metaphysical indeterminacy ('MI' hereafter) is the idea that the uprising of indeterminacy is imputable to the world itself, rather than either to the words we use (Fine, 1975) or to our knowledge (Williamson, 1994). MI has often been regarded as *a priori* incoherent. Such an attitude has been encouraged by three main facts. First, many thought that MI necessarily allows the possibility of indeterminate identities. Given the strength of Evans (1978)'s argument against such a position, the majority of philosophers have concluded that MI is impossible. Second, many's attitudes on MI were influenced by authoritative philosophers like Russell (1923) and Lewis (1986), who explicitly banned MI as conceptually contradictory or, if coherent, blatantly impossible. Third, there were no positive reasons for thinking that MI was conceivable in the first place. Indeed, precise and convincing accounts of MI were nowhere to be found, leading many to the conclusion that MI is inconceivable.

The attitude toward the possibility of MI now seems to be more relaxed than before, primarily because logically clear accounts of MI have been put forward in the literature. Moreover, many authors have argued that these accounts help make sense of different phenomena.[1]

As a matter of fact, many think that the most compelling reason for taking MI seriously is that some interpretations of non-relativistic quantum mechanics, plus some extra assumptions, seem to suggest a genuine case of it. Even if the first examples of quantum MI proposed[2] are no longer seen as genuine, that the properties of quantum objects are metaphysically indeterminate is still a live option (Bokulich, 2014; Wolff, 2015).

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[1] E.g., future (Barnes and Cameron, 2009) and fictional (Darby, Pickup, and Robson, 2017) states of affairs.

[2] Notably the one proposed by Lowe (1994) as a counterexample to (Evans, 1978); for a reply to Lowe (1994), see (French and Krause, 2003). The contemporary general agreement is that quantum mechanics does not conclusively support indeterminate identity. See Saunders (2006); Muller and Seevinck (2009); Saunders and Muller (2008); Muller (2015) for arguments that quantum particles are discernible, and French and Krause (2006) for an extensive analysis of identity in QM.

The structure of the chapter is the following. I start (§5.2) by elucidating what quantum indeterminacy is and which phenomena are responsible for its uprising. Then, I briefly survey the assumptions that are necessary to conclude that QM offers a genuine example of MI.[3] Then (§5.3), I present a map of the account of metaphysical indeterminacy available in the literature. Following Wilson (2013a), I divide them into 'meta-level' and 'object-level' accounts, explaining the common intuitions that lie underneath these approaches. To give a concrete example of what these approaches consist in, I present the most representative account of each family, namely, the meta-level account proposed by Barnes and Williams (2011) (§5.3.1) and the object-level one defended in Wilson (2013a) (§5.3.2). Finally (§5.4), I explain why Barnes and Williams's account fails in modelling quantum indeterminacy. The point is similar to that already put forward by Darby (2010) and Skow (2010), who showed that Barnes and Williams's account is in contrast with the theorem presented by Kochen-Spekker. I rely instead on a corollary of Gleason's theorem. The result is the same, but its explanation is by far simpler and more elegant. Short conclusions follow suit (§5.5).

5.2 Quantum Indeterminacy, and where to find it

A common way of talking about quantum mechanics is to claim that Hermitian operators represent observables. Where observables – such as position, momentum and spin – are understood as physical properties of quantum systems. In particular, the value of an observable is determined if and only if the system's state is mathematically represented by an eigenvector of the operator associated with the observable above. Consider, for example, the observable 'having spin along the $x-$axis.' A way to mathematically represent such an observable is by the operator $\hat{S}_x$. A quantum system will have a definite value of that operator if, and only if, its state is an eigenstate of $\hat{S}_x$.

Every possible state in which a particle could be is an eigenstate of some operator. However, that same state will not be an eigenstate of some other operators.[4] These cases might be subsumed in two classes of phenomena: superposition and entangle-

[3] For a different review on quantum indeterminacy, see (Calosi and Mariani, 2021).

[4] This follows from the indeterminacy relations (4.2) that we saw in §4.3. Operators, such as those of spin along different axes, are often called 'incompatible observables.'

ment.[5] Let us consider them in turn, by taking the example of spin$-\frac{1}{2}$ along different directions.

- *Superposition.* Given some properties of the mathematical vector space in which spin properties are represented, if a system is in a definite state of, say, spin up along the x-axis ($|\uparrow\rangle_x$), it is necessarily in a superposed state of spin along, say, the z-axis:

$$|\uparrow\rangle_x = \frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z) \ . \tag{5.1}$$

  Since $\frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z)$ is not an eigenvector of the operator $\hat{S}_z$, when the system is in $|\psi\rangle$, it does not have a definite value of spin along the z-axis.

- *Entanglement.* Let us take two electrons with spin as the only degree of freedom. Suppose that: (i) they are respectively represented mathematically by the Hilbert spaces $\mathscr{H}_1$ and $\mathscr{H}_2$, (ii) that $|\uparrow\rangle_{z_1}$ and $|\downarrow\rangle_{z_1}$ are possible state in $\mathscr{H}_1$, as $|\uparrow\rangle_{z_2}$ and $|\downarrow\rangle_{z_2}$ are states of $\mathscr{H}_2$, (iii) that $|\uparrow\rangle_{z_1}$, $|\downarrow\rangle_{z_1}$ are eigenvectors of $\hat{S}_{z_1}$ as well as $|\uparrow\rangle_{z_2}$ and $|\downarrow\rangle_{z_2}$ are eigenvectors of $\hat{S}_{z_2}$ and (iv) $\hat{S}_{z_1}$ and $\hat{S}_{z_2}$ are the operators that stand for the observables of 'having spin along the $z-$axis' of the two particles. Now, the composite system will live in the tensor product of the two Hilbert spaces:

$$\mathscr{H}_{12} = \mathscr{H}_1 \otimes \mathscr{H}_2 \ . \tag{5.2}$$

  A possible state (of $\mathscr{H}_{12}$) in which the composite system might be is the following:

$$|0\rangle = \frac{|\uparrow\rangle_{z_1} \otimes |\downarrow\rangle_{z_2} - |\downarrow\rangle_{z_1} \otimes |\uparrow\rangle_{z_2}}{\sqrt{2}} \ . \tag{5.3}$$

  A state like (5.3) is not an eigenvector of $\hat{S}_{z_1}$ and $\hat{S}_{z_2}$.[6] Hence, when the particles are in (5.3), their spin along the $z-$axis lacks a definite value.

Both states (5.1) or (5.3) are not eigenstates of the operator $\hat{S}_z$, which represents the observable of having spin along the z-axis. Friends of quantum indeterminacy claim that when the state of a system is not an eigenstate of a Hermitian operator, the system indeterminately instantiates the property that corresponds to that operator.

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[5] Calosi and Wilson (2018) claim that a third case is that of incompatible observables. Now, even if incompatible observables are conceptually different from superposition, from a physical point of view, the fact that some operators do not commute is exactly the cause of superposition. Hence, it is safe to treat them as a unique phenomenon. One may wonder why one has to distinguish between entanglement and superposition, insofar as an entangled state is – mathematically speaking – nothing but a superposed state on a composite system. Even if they are (almost) the same mathematical phenomena, they are really different from a physical point of view. Since one of the thesis aims is to argue that these phenomena need two other metaphysical accounts, it is best to keep them separated from the very beginning.

[6] Surely $\hat{S}_{z_1}$ and $\hat{S}_{z_2}$ cannot be applied to $\mathscr{H}_{12}$. Here, it is implicit that $\hat{S}_{z_1}$ ($\hat{S}_{z_2}$) stands for the tensor product of itself and the identify matrix of $\mathscr{H}_2$($\mathscr{H}_1$).

These kinds of states are (allegedly) responsible for the rise of quantum indeterminacy: when a quantum system is in one of these states, the instantiation of one of its properties is indeterminate. An electron in state (5.1), say, would have a metaphysically indeterminate property of spin along the z-axis.
That quantum mechanics offers a genuine example of indeterminate properties can easily be resisted. Indeed, such a thesis relies on five main assumptions, which are, by themselves, highly controversial. In this section, I briefly discuss them in turn.

First assumption. The first assumption friends of quantum indeterminacy usually assume is an interpretation-neutral approach to the metaphysics of QM. Sometimes, they speak loosely of the 'standard' or Copenhagen interpretation, e.g., (Bokulich, 2014). Since 'standard', 'orthodox' and 'Copenhagen' interpretations are usually associated with an anti-realist view of QM, it is more correct to claim they pursue an interpretation-neutral approach – as suggested in (Wallace, 2019, p.3). That is, they try to cash out the ontological commitment of the textbook formulation of QM, assuming that the theory is true. And the very motivation of it is that the deep indeterminacy presented by QM was thought to be impossible to account for in realist terms. Given the contemporary metaphysical models of MI, applying it to textbook QM has been considered a natural fit. The thesis shares this assumption, and I have already presented some motivations in its favour above (§4.1). Here, I limit myself to noticing that several authors have tried already to extend the models of quantum indeterminacy to other realist interpretations of QM. Such work has been done already for the Many World interpretation (Calosi and Wilson, Forth.; Wilson, 2021, Ch. 5), GRW (Mariani, 2022; Lewis, 2016, Ch. 4), Modal QM (Calosi, ms.), and Relational QM (Calosi and Mariani, 2020). Hence, there are already some certainties about the possibility of extending the account of quantum MI to other realist interpretations. Finally, it must be noted that a small debate rose about whether metaphysical indeterminacy can be non-fundamental.[7] In the metaphysical literature, Barnes (2014)'s argument that MI cannot be an emergent phenomenon has been disputed in (Mariani, 2020). In the literature on quantum indeterminacy, Glick (2017) argues that indeterminacy is emergent in the three main realist interpretations of QM and thus eliminable. For arguments against the idea that emergent phenomena are eliminable in this context, see (Calosi and Wilson, 2021; Mariani, 2021). Here, I assume that non-fundamental phenomena are worth investigating as fundamental ones. For once, if it were not true, then the whole idea of cashing out the metaphysics of QM would be a meaningless enterprise in the first place.

Second assumption. The second assumption needed by the supporters of quantum indeterminacy is connected to the first: realism and antirealism are stances through which one reads scientific theories. We already covered this assumption in the previous chapter, and there is not much more to add here (§4.1). To sum up, the idea is that even if textbook QM has been considered for ages an antirealist view, it does not mean that one cannot attempt to cash out its ontological commitment as if the

[7] See (Calosi and Mariani, 2021), for a detailed review.

theory were true.

Third Assumption. Hermitian operators are taken to represent mathematically the properties instantiated by quantum systems. In particular, the relation between the properties instantiated and their mathematical description is captured by the 'Eigenstate-Eigenvalue link' (EEL):[8]

> (EEL): A quantum system has a definite value v for an observable O *iff* it is in an eigenstate of $\hat{O}$ having eigenvalue v. In this case, the definite value is the associated eigenvalue v.

This principle bridges mathematical and metaphysical descriptions. Under the assumption that the metaphysical properties of a system (i.e. its observables) are modelled in quantum formalism by self-adjoint[9] Hermitian operators, (EEL) gives us the sufficient and necessary conditions under which we can claim that a quantum system determinately instantiates some properties: if a property O is represented by a Hermitian operator $\hat{O}$, then the system determinately instantiates that property if and only if its state is an eigenstate of $\hat{O}$. That observables stand for physical properties is a quite shared assumption.[10] In the case of spin, such an assumption is natural given the connection between the operators of spin along different directions and the experimental outcomes of spin measurements along those axes discussed in §4.3.

Fourth assumption. Quantum indeterminacy is metaphysical rather than semantic or epistemic. We have not seen yet how we have to understand the idea that indeterminacy is metaphysical. Whether it is possible to give a reductive definition of such a concept is a matter of controversy (see §5.3 below). A first way –which I like – of getting a grip to the notion is in terms of counterfactuals (Barnes and Williams (2011, fn.8), Barnes (2010)). The indeterminacy of a sentence p is metaphysical iff p is not epistemically indeterminate after we have precisified every piece of the representational content of p. In other words, a sentence without a clear truth-value expresses a metaphysically indeterminate state of affairs iff its lack of a truth value cannot be reduced to facts about knowledge and/or to the vagueness of the language in which it is expressed. Such a criterion is a litmus test to check when there is MI rather than a proper definition of it. Nonetheless, the criterion gives us sufficient and necessary conditions for the uprising of MI that are comprehensible and acceptable by those who are sceptical about MI or its coherency.

Given this rough characterisation of when we have metaphysical indeterminacy, it should be clearer why QMI is metaphysical. It can not be just semantic because

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[8] Or some of its variants, as it is discussed in Lewis (2016, Ch. 4).

[9] Self-adjointness and symmetry coincide in the case of finite-dimensional Hilbert spaces. For further discussions on the properties of operators acting on Hilbert spaces, see (Moretti, 2018, Ch. 5). Since the thesis' focus is on spin, that is represented by a finite-dimensional Hilbert space, I avoid a pedantic notation to favour the readability of the text. In what follows, I refer to them simply as 'operators.'

[10] See, e.g., the discussion in (Calosi, ms.).

equations are sharp and precise, and none of the imprecision connected with the ambiguities of natural languages is involved.[11] It is not epistemic because some mathematical theorems[12] show formally that if you try to cheat, so to speak, by assigning a definite value to all the properties, you contradict the empirically tested predictions of quantum mechanics. Moreover, the claim that such indeterminacy is epistemic amounts to accepting that there are non-local hidden variables that QM does not describe. It might well be that such a thesis has nothing wrong, per se. Nevertheless, to claim that there are hidden variables conflicts with the second assumption above. Surely, if the indeterminacy is epistemic, then QM is a wrong theory that has to be changed. But such a possibility lies outside the scope of the thesis.

textitFifth assumption. Note finally that (EEL) *per se* is not enough for claiming that the system indeterminately instantiates some properties. As has been argued by Glick (2017), one may take the (EEL) as saying that when the state of a system is not an eigenvector of an operator, it lacks the corresponding property altogether. Then, the fifth assumption taken by the participants of the debate is what I dub 'Property Nonetheless' (PN) (Corti, 2021):

> (PN): A quantum system always instantiates its observables (i.e. those properties that can be measured and represented mathematically through a Hermitian operator).[13]

(PN) guarantees that a quantum system instantiates its observables even when these observables lacks a precise value. For example, the idea is that a quantum system instantiates its property of 'having spin along the $x-$axis' also when it lacks a precise value for that property, i.e. it is not in state spin up or down along the $x-$axis. The intuition is that it is precisely in these cases that the properties of a quantum system are metaphysically indeterminate: we know that the system has a given observable, but it seems that it is neither true nor false that it instantiates its precise values. Indeed, such a principle is a heavy addition to the theory, not supported by the formalism itself. The (EEL) allows talking about instantiations of precise values of an observable. Still, it is silent of whether the observables are instantiated or not when they lack a precise value. A common way of reading (EEL) is by assuming that it signals the only case in which we are allowed to talk about the properties of quantum systems. Hence, no talking about observables without precise values

[11] Some authors, most notably Reichenbach, suggested that quantum mechanics is semantically indeterminate, in some sense or another. Even if this is true, crucially, it is not *only* semantic, i.e. a specification of the terms used do not wash away, so to speak, the indeterminacy. That (alleged) semantic indeterminacy in quantum mechanics reflects a more fundamental kind of indeterminacy, and one might be tempted to call it 'metaphysical', is acknowledged for instance by Reichenbach (1975, p. 94) himself: "The deficiencies [i.e. that some propositions have an indeterminate truth-value] must rather be regarded as the linguistic expression of the structure of the atomic world, which thus is recognised as intrinsically different from the macro-world, and likewise from the atomic world which classical physics had imagined."

[12] The most known formal results are those of: Gleason (1957); Kochen and Specker (1967).

[13] Note that the principle does not require that the properties of quantum systems are *definite* at all the times.

is permitted. Such an approach seems to entail that when a particle is not in (the neighbourhood) of a position eigenstate, then it lacks its position altogether. Such a view has been defended (Glick, 2017), but it is prima facie problematic. If we put an electron in a box, say, it will end up in a superposition of its position state, and all the possible position states will be inside the box. It seems true, then, that the particle *is* in the box even though it lacks any precise position. Hence, it appears that the particle has the position observable, even though it lacks any of its precise value.

Most of the assumptions above are controversial. I assume them in what follows, insofar as the thesis aims at investigating whether QM offers a genuine case of MI. Here, I limit myself to showing that these assumptions are not trivial by presenting a short list of views that resist the assumptions above. The first assumption is rejected by those that face the underdetermination of QM by following a different route among the ones presented in §5. For example, it is natural that defenders of a particular realist interpretation of QM will insist there is no hope in cashing out the metaphysics of textbook QM: that is why they would insist that their favourite interpretation exists in the first place. Similarly, the second assumption is resisted by those that believe that textbook QM necessarily lacks a coherent ontology. Perhaps they resist the idea of MI or that the textbook version of QM can put forward a sensible answer to the measurement problem. Furthermore, the (EEL) has been strongly criticized, in general (e.g. by Wallace (2019); for a reply, see Gilton (2016)) and in particular interpretations of quantum mechanics (e.g. by Albert and Loewer (1996); for a discussion on alternative links, see Lewis (2016, Ch. 4)). The third assumption is invalid in some interpretations of QM; moreover, there are three broad ways the ontology could be read off QM. Indeed, one could:

(1) take the quantum state non-ontologically, interpreting it as a "book-keeping" device that just informs us about the fundamental properties described in QM by self-adjoint operators,
(2) take the quantum state as the only ontological entity and consider self-adjoint operators as mathematical tools useful only for making experimental predictions
(3) or consider that states and operators have both an ontological weight.[14]

The third assumption is thus clearly rejected also by those that explicitly endorse (2) (Monton, 2004; Dorato and Esfeld, 2010; Belot, 2012). The fourth assumption might be resisted (either assuming an empiricist stance toward science or accepting some ψ-*epistemic* interpretation of quantum mechanics). Finally, the last assumption has been directly challenged by Glick (2017), who proposes an alternative ontology of QM.
Insofar as, in general, issues on quantum mechanics tend to be highly controversial, one could add further controversies to such a list. That being said, the assumptions above are not *ad hoc*, and have a great deal of plausibility. Insofar as my interest here is that of seeing if and under what conditions QM offers a genuine example of MI, it is not my interest to defend them here.

[14] See Glick (2017, p.206).

5.3 Accounts of metaphysical indeterminacy

As said at the beginning of the chapter, we have several accounts of MI nowadays. Following Wilson (2013a), one may divide the accounts of MI available in the literature into two broad categories:[15]

- **Meta-level accounts:** these approaches consider MI as worldly 'indecision' between different possible, mutually incompatible, precise states of affairs. In other terms, p is metaphysically indeterminate iff (i) there are at least two possible descriptions of a state of affairs involving p, i.e. p and $\neg p$, and (ii) these possible descriptions that do not contain in themselves any semantic nor epistemic indeterminacy fail in both determinately representing and misrepresenting the actual world.[16] The details vary slightly across models, but the general idea remains the same: a state of affairs is metaphysically indeterminate when the world itself is unsettled between (at least) two possible determinate states. Examples of meta-level accounts can be found in Akiba (2004), Barnes and Williams (2011) and Darby et al. (2017).
- **Object-level accounts:** this family of accounts claims that there are three possible states of affairs: the state that p, that $\neg p$ and the state that it is indeterminate whether p. MI is thus considered as a third state of affairs that cannot be reduced nor be understood in terms of the two definite states p and $\neg p$. Arguably, Tye (1990) is one of the progenitors of this approach to MI, albeit his account seems not so popular anymore for different reasons.[17] Other object-level accounts of MI can be found in Smith and Rosen (2004) and Wilson (2013a).

The contemporary research in MI seems to be oriented toward the former family of accounts rather than the latter. Such a fact may be surprising if one agrees with Wilson (2013a) that meta-level accounts somehow rely on the very same intuition that guided the critics of MI, i.e. that MI is not a particular state of affairs and that it does not make sense if not understood as unsettledness between *determinate* states of affairs.

We will briefly see the most popular accounts of the two families, namely, Barnes & Williams's supervaluationist and Jessica Wilson's determinables-determinates approaches.

[15] What follows is a rough and ready description of the main intuition that these approaches to MI try to capture rather than an exhaustive presentation of every view.

[16] Note that according to meta-level approaches, metaphysical indeterminacy does not correspond to accepting a third possible state of affairs, on top of p and $\neg p$. Rather, the core of their approach is exactly that indeterminacy is not a third way in which entities might be, but is rather reduced to the absence of a determinate description. This is thoroughly defended in (Barnes and Cameron, 2016), and it is contrasted to the object-level approaches that consider MI, instead, a third possible state of affairs (Wilson, 2017b).

[17] I.e. the open rejection of classical logic and the (alleged) difficulty of overcoming Evans' argument against indeterminate identity.

5.3.1 *The champion of Meta-Level approaches: Barnes & Williams (2011)*

According to Barnes and Williams (2011), the concept of MI has to be taken as primitive. On the one hand, they think no satisfactory reductive definition of the concept is available. On the other hand, they insist that the concept *is* intelligible, insofar as (i) it is possible to give necessary and sufficient conditions for it and (ii) it is possible to model it in a framework of modal logic. We are going to see (i) and (ii) in turns.

Necessary criterion. As far as (i) goes, Barnes and Williams propose different criteria. A clear one is the litmus test previously mentioned: we have MI when a sentence is still non-epistemically indeterminate after every term the sentence contains have been semantically precisified. Such a criterion simply tells us under which conditions there is MI. Take, for example, "John is bald". Such a sentence expresses a case of MI iff (i) its indeterminacy cannot be reduced to facts about knowledge of John and (ii) it has been precisified both to which entity "John" refers to and what "being bald" means, e.g. "John is *that* human being" and "being bald" = having less than ten hairs. It is easy to see that "being bald" never describes a metaphysically indeterminate state of affair: if we *choose* what "being bald" means and who is John, and we *know* how many hairs John has, then "John is bald" will always be either true or false. Yet the criterion above is enough to showcase the necessary conditions for suspecting that a phenomenon offers a genuine example of MI.

A supervaluationist treatment of MI. Let us turn now to (ii). They propose to regiment semantically the concept of MI in a framework of modal logic, on the lines of Akiba (2004). They start with classical supervaluationism as follows. They take the language L of first-order quantified modal logic. A model m of this language is:

$$m : \langle W_m, O_m, a_m, \|_m \rangle \ , \tag{5.4}$$

where:

W_m is a set of ersatz possible world.
O_m is a set of objects
a_m is the possible world that is considered the actual one in the model, i.e. the one that represents reality. It is imposed that $a_m \in W_m$.
$\|_m$ is an interpretation function, which maps names and predicates[18] of L into functions from W_m to O_m.

Two more definitions are needed. A *simple* $m-$*variable assignement* v is a map from variable of L to objects in O_m; and the *denotation* of a singular term of L at a world w

[18] They assume for simplicity that L contains only monadic predicates. They furnish in fn.30 how to extend it to polyadic predicates.

and variable assignment v is defined as either (i) the value of the intension assigned to the name by the interpretation function at w, or (ii) as the object assigned at the variable by v. As standard, they provides the following definition for truth at a world w, given variable assignement v in model m (w,v,m for short) (Barnes and Williams, 2011, p.15):

1. Fn is true at w,v,m iff the denotation of n at w,v,m is a member of $|F|_m(w)$.
2. $\neg\phi$ is true at w,v,m iff ϕ is not true at w,v,m.
3. $\phi \wedge \psi$ is true at w,v,m iff ϕ and ψ are each true at w,v,m.
4. $\exists x\phi$ is true at w,v,m iff ϕ is true at w,v^*,m, where v^* is an $m-$variable assignement differing from v if at all only in what it assigns to x.
5. $\diamond\phi$ is true at w,v,m iff ϕ is true at w^*,v,m, for some w^* in W_m.

So far, we have defined the notions of *truth-at-a-world*. But there are other important notions of truth in their framework. In particular, they define:

(TaM)] *Truth-at-a-model*: For $\phi \in L$, ϕ is true in m iff ϕ is true at a_m,v,m for all $m-$variable assignment v.
(TS) *Truth simpliciter*: ϕ is true simpliciter iff ϕ is true at m for m the intended model.

The 'intended model' quoted in the last definition is supposed to be the correct logical model of reality. This should be enough to get a first grip on how they intend to capture metaphysical indeterminacy. Their idea is that when there is MI, there is no fact of the matter about which model m is the intended model. In other terms, the notion of truth simpliciter does not apply. A rough and ready example should illuminate the idea better. Suppose that $U^x n$ stands for "n has spin up along the $x-$axis". If it is metaphysically indeterminate whether an electron e has spin up along the $x-$axis, then there will be two models – m_1 and m_2 – such that $U^x e$ is true in the possible worlds a_1 (the world considered actual in m_1) but false in a_2. If the impossibility of assessing which model is the intended one is not epistemic, then it is metaphysically indeterminate if $U^x e$.

For our scope, L is too impoverished for talking about indeterminacy. Indeed, so far, indeterminacy made its comparison in the meta-language only. But a language in which we can assert that something is determinate would be preferable. Hence, Barnes and Williams suggest enriching L with a determinacy operator D. A model m of this new language L' will be of the form of: $\langle W_m, A_m, O_m, a_m, ||_m \rangle$, where A_m is a subset of W_m which contains those worlds that are not *determinately unactualized*. They call the worlds that are not determinately unactualised 'ontic precisification'; a world is an ontic precisification if it does not *misrepresent reality*. Now, we can introduce the determinacy operator[19] as follows: $D\phi$ is true in m iff ϕ is true at w,v,m for all $w \in A_m$[20] and all $m-$variable assignments. The idea here is that something is determinate iff it is true in all the worlds that are candidates for representing reality. ϕ will be indeterminate instead iff it is not determinate neither that ϕ nor that $\neg\phi$.

[19] $D\phi$ is a well-formed formula if ϕ is, and no iteration of D are allowed.

[20] In the original text they say $\forall w \in P_m$, which is indubitably a typo as it is clear from the context and the fact that P_m is nowhere defined.

Clearly, if there is no indeterminacy, there will be only one world in A_m, i.e. a_m. When there is indeterminacy instead, we have several worlds in A_m; hence, we have as many models m_n that share the same set A_m but disagree about which world is the actual one. Again, such an indeterminacy will be metaphysical iff the impossibility of (non-arbitrarily) choosing a model (of the m_n) as intended cannot be reduced to facts about knowledge.[21] They further modify the framework to make necessary and possible operators interact with D and model higher-order indeterminacy. There is no need to review them here. The idea remains the same apart from the fact that the indeterminacy of the intended model is moved into a single model, as follows. They include in the model a set of sets of possible worlds, called 'halo of indeterminacy', and a set of 'selection function', which select which set of possible worlds is the set of ontic precisifications, and which world of that set is the actual one. Then, the indeterminacy inside the model consists of which selection function is the correct one to represent reality. What will be crucial in what follows is that every possible world is *complete* and fully *determinate*. In other terms, for every ϕ, ϕ must be either true or false in every world.

Now, they spend few words about what "misrepresenting reality" means, and they provide the following sketchy criterion:

- A world w is a possible ontic precisification iff $\forall p((w \text{ represents that } p) \rightarrow \neg D \neg p)$

I must confess I fail to appreciate such a definition inside the model. If we understand "represents that" as "p is true-at-w", then from "p is false at w" it follows that w can be a candidate for representing actuality only if it is determinate that not p. But clearly, in cases of MI, there will be a possible ontic precisification w that represents not p even though $D\neg p$ is false. To me, defining ontic precisifications in terms of D incurs a high risk of circularity. Since something is determinate iff it is true in every possible ontic precisifications (the worlds in A_m) of a model, we cannot define ontic precisifications in terms of what is determinate. A better route might be imposing some conditions in the meta-language, as follows. The intended model is the one that is supposed to represent reality. If we know that p, then we want that p must be true in the intended model. Otherwise, it would not be the *intended* one. Hence, we can propose the following informal definition: w is a possible ontic precisification iff $\forall p$ (p *should* be true $simpliciter$) $\rightarrow$ (p is true-at-w). This definition should give us a better grasp of what "misrepresenting reality" means. When choosing the set of possible intended models, we discard all the models that admit as ontic precisification worlds that blatantly fail to describe actuality. If there is no indeterminacy, we will end up with a unique intended model. If there is indeterminacy instead, we end up with a bunch of models that contain the same set of worlds A_m, and every

[21] Akiba (2015) argues that the Barnes and Williams (2011)'s account is not metaphysical, but semantic or epistemic at best. The same argument can be applied to other modifications of their account, e.g., (Darby and Pickup, 2019; Michels et al., 2021). I will not belabour this point in what follows.

world in that set is perfectly alike but for the indeterminate phenomena.[22] Moving such a requirement on the meta-level avoids the circularity above, without any loss of clarity.

The application to quantum cases. How to apply Barnes and Williams's model to quantum cases is, prima facie, straightforward. Let us see a simple example involving an electron prepared in the state spin up along the $x-$axis. Such an electron will be in a superposition of spin along the $z-$axis. An electron in such a state behaves in a way that cannot be explained by claiming that the system determinately instantiates either spin up$_z$ or spin down$_z$, but we simply do not know which. Nor it can be explained by claiming that the particle has both – since no experiment has ever revealed a particle with both directions of spin simultaneously – nor that it lacks both because that state is responsible for some peculiar phenomena empirically detectable.[23] Spin superposition seems a paradigmatic case of indeterminacy: both 'the electron has spin up$_z$' and 'the electron has spin down$_z$ seems neither determinately true nor false. Barnes and Williams's account straightforwardly models such an indeterminacy. We have two possible worlds that are candidates for actuality. These worlds, w_1 and w_2, are perfectly alike in all regards, but for a single proposition: according to w_1, that 'the electron has the property of having spin up$_z$' is true, while it is false according to w_2; in the latter indeed, it is true that 'the electron has the property of having spin down$_z$. Consequently, we have two possible models of how reality is. These models agree on which is the set of admissible precisifications (i.e. $A_m = \{w_1, w_2\}$), but they disagree on which of these worlds is the actualised one. According to the first one, w_1 is the actualised world. On the contrary, w_2 is considered the actualised one in the second model. Since there is no semantic vagueness, and there is no other piece of knowledge that might help us choose which of the two models is the intended one, it is metaphysically indeterminate if the electron has spin up or spin down along the $z-$axis.

As we are going to see, setting aside such a simplistic example, Barnes and Williams's account fails in accounting quantum indeterminacy. Indeed, their model of metaphysical indeterminacy poses too strong conditions violated by quantum phenomena.

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22 For example, suppose p and q are both indeterminate. We ends up with 4 possible worlds w_n that are candidate for representing actuality, i.e. $w_n \in A_m$, such that: (i) $\forall r$, iff $((r \neq p) \wedge (r \neq q)) \rightarrow (\forall w_n, r$ is-true-at-$w_n) \vee (\forall w_n, \neg r$ is-true-at-$w_n)$, ; (ii) p and q are true-at-w_1, p and $\neg q$ are true-at-w_2, $\neg p$ and q are-true-at$-w_3$, and $\neg p$ and $\neg q$ are true-at-w_4. Hence, we have four models candidate for being intended, which the only members of A_m are w_1, w_2, w_3 and w_4, but they disagree about which one of them is the actualized one, i.e. a_m.

23 An accessible review on this is the first chapter of Albert (1992).

5.3.2 The champion of Object-Level approaches: Wilson (2014)

Wilson (2013a) proposes a reductive account of metaphysical indeterminacy. In her view, metaphysical indeterminacy can be reduced to a particular way of instantiating properties. In particular, Wilson's account relies on the notion of determinable-determinate properties (Wilson, 2017a). The determinable-determinate relation ties more general one to more specific ones. The more general property (e.g. 'being coloured') is called a determinable, and its more specific ones (e.g.'being red') are called determinates. The relation of determination may have different levels so that a determinate of a given determinable might be, in turn, a determinable of a further determinate, e.g. 'being red', which is a determinate of 'being coloured', is a determinable of 'being Bordeaux.'[24] According to Wilson, metaphysical indeterminacy can be reduced to a particular way of instantiating determinable and determinate properties, as follows:

> Determinable-based Metaphysical Indeterminacy: What it is for a state of affair to be metaphysically indeterminate in a given respect R at a time t is for the state of affair 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. (confront with: Wilson, 2013a, p.366)

The idea is that there is metaphysical indeterminacy only when an entity instantiates a determinable property, but it does not instantiate only one of its determinates. Such a view relies on the assumption that talking about determinable instantiations without determinate instantiation is meaningful, and it imposes a revision of the standard axioms of the determinable-determinate relation itself. Assumptions convincingly defended in Wilson (2012) and Wilson (2013a). There are two ways in which a determinable might lack a unique determinate: it might have more than one or lack any altogether. These two cases are dubbed respectively 'glutty' and 'gappy' indeterminacy.

Gappy cases are the most straightforward case to be understood: an entity instantiates a determinable property O, but it fails to instantiate any of its determinate. An example Wilson (2013, p.379) proposes is that of open futures. If we know at time t that at time t' a certain determinable state of affair S will obtain, and it is not settled at t which determinate of S will get at t', then S is metaphysically indeterminate. For instance, 'there will be a sea battle tomorrow in a given place' is metaphysically indeterminate now if we know that tomorrow either there will be or not a sea battle in that place. Still, as of now it is impossible to say which determinate state of affairs will take place. In this case, the indeterminacy is gappy because the future state of affairs does not instantiate now any of its possible determinate.

Glutty cases, instead, might be understood in two ways. Either the instantiations of more determinate happens because the instantiation is relativised to something else (an entity, a state of affairs, etc...) – and there is no arbitrary way to choose a relativisation over the others –or because the instantiation of its determinates comes in degrees. The example of the first case – an iridescent feather – Wilson propose is

[24] I come back on determinable and determinate in chapter §7.2.1.

not illuminating. While the second is problematic for two reasons: that instantiation comes in degrees needs to be better specified, and it makes her account strikingly similar to that of Smith and Rosen (2004).

The application to quantum cases. Calosi and Wilson (2018) provides a detailed analysis of how the determinable-determinate account of MI might be applied to make sense of quantum phenomena. Again, I will sum up their analysis focusing on the simple case of spin superposition and by neglecting some details which are not strictly relevant for the time being.

Let us take the case of an electron in a superposition of spin$_z$ state again. If one accepts to understand 'having spin along the $z-$axis as a determinable with determinates 'having spin up along the $z-$axis' and 'having spin down along the $z-$axis, then it is metaphysically indeterminate whether the electron has spin$_z$. (Calosi and Wilson, 2018, §4.3) argue at length that such an indeterminacy cannot be understood as a case of gappy indeterminacy. The three main reasons are the following:

- Superpositions seems to involve a combination of determinate states of affairs rather than a lack of them.
- Superposition seems to involve an interaction of determinate states of affair, as in the case of the two-slit experiment.
- A glutty understanding of superposition would wash away the information stored by the coefficients of the superposed terms.

They propose to understand superposition in glutty terms. They discuss both the relativised and the instantiations in degrees strategies. On the former, they do not make many positive claims that would be useful to understand what they mean by relative instantiation in this context. Rather, they limit themselves to point out what such a relative instantiation *do not* mean. On the latter, they are somehow less vague. They propose associating the degree of instantiation to the squared modulo of the superposition coefficient. So, for instance, an electron in a state $|\psi\rangle = \frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z)$ will instantiate at a degree 0.5 both the properties of having spin up and spin down along the $z-$axis.

5.4 MI and quantum systems

At first glance, indeterminacy seems nothing but a failure of bivalence. We say that a sentence p is indeterminate when neither p nor $\neg p$ appears to be both wholly true nor false. For example, suppose you do not know how a particular dog is called. The sentence 'That dog is called Nala' is indeterminate precisely because you lack any good reason to say that such a sentence is true, but you do not have a good reason either to say that it is false. This is a straightforward and ubiquitous case of epistemic indeterminacy. This indeterminacy vanishes once we ask its owner the dog's name. Similarly, it goes for cases of semantic indeterminacy. It might be indeterminate whether a particular individual, John, say, is bald or not. So that 'John is bald', and its negation are not clearly true nor false. Again, the indeterminacy vanishes once we precify what we mean by 'being bald', e.g. we postulate it means 'having less than ten hairs.' Similarly, putative cases of metaphysical indeterminacy seem nothing but a failure of bivalence. Suppose that it is metaphysically indeterminate whether tomorrow there will be a sea battle or not. Both 'tomorrow there will be a sea battle', and its negation will lack a truth value. In these cases, though, the indeterminacy will not disappear by precifying the terms of our sentences or obtaining more knowledge. Cursorily, it has to be noted that in Wilson's account – especially in its degree instantiation variant – the indeterminacy vanishes. When an electron is in a superposed state, it is determined to instantiate the superposition terms up to a degree. Friends of metaphysical indeterminacy usually think that the failure of bivalence is the peculiar trait of indeterminacy, *tout court*. As we saw, Barnes and Williams (2011) put a great deal of effort into modelling indeterminacy as a failure of bivalence across different models, retaining its validity inside each possible world. At first, the indeterminacy presented by QM is similar. Take P: "having spin up along the $x-$axis". Px or not Px seems not to hold when x denotes a particle in a superposed state of spin$_x$.

However, what is peculiar of quantum indeterminacy is not the failure of bivalence. Instead, it is the failure of distributivity.[25] That is, in general, from $p \wedge (q \vee \neg q)$ it does not follows that $(p \wedge q) \vee (p \wedge \neg q)$. Indeed, there are cases in QM in which we know that both $(p \wedge q)$ and $(p \wedge \neg q)$ must be false, even thought $p \wedge (q \vee \neg q)$ might be true. A simple example is the following. Take spin along two mutually orthogonal directions, x and z. Take X as 'the electron has spin up along the $x-$axis' and Z as 'the electron has spin up along the $z-$axis;' consider, moreover, the negation of Z as 'the electron has spin *down* along the $z-$axis.' Suppose the electron is in a definite state of spin$_x$, and so it is in a superposition of spin$_z$ states. From $X \wedge (Z \vee \neg Z)$, it follows that $(X \wedge Z) \vee (X \wedge \neg Z)$. But we know that the latter is always false, exactly because both disjuncts must be false.

Following Hughes (1989, p.168-170), I give a brief explanation of why it is so. Spin-1 is a full set of observables, each with three eigenvalues representable on a Hilbert space $\mathscr{H}$; such a space will be three-dimensional, and the values n_1, n_2 and n_3 of the spin observables S_n will correspond to any orthogonal 3-tuple of rays. Where the observable S_n stands for all the possible observable of 'having spin-1 along the $n-$axis' one may find by substituting to n a concrete spatial direction. The rays associated with n_1 will represent the property of 'having spin up along n', n_2 the property of 'having spin down along n and n_3 that of 'having spin parallel to n.' To say that it might be possible that a particle has a definite state of spin along different directions simultaneously, we should be able to build a function that maps rays of this Hilbert space to $\{0,1\}$, and that it assigns one exactly to one ray of each set of mutually orthogonal rays which span $\mathscr{H}$. That is, it must assign 1 to one of n_1, n_2 and n_3, and zero to the other, for every possible spatial direction n. If the function assigns 1 to a ray, then it is true that the system has the property connected to that ray. If the function assigns 0 instead, then the system lacks that property. Let us assume

[25] I thanks Enrico Cinti for many discussions on this point.

now that there is such a function, which have the form of: $f : n_m \rightarrow \{0,1\}$, such that $f(n_1) = 1 \veebar f(n_2) = 1 \veebar f(n_3) = 1$ for every S_n.[26] A consequence of Gleason (1957)'s theorem is that there will be a density operator **D**, such that the function above will be:

$$f(n_n) = Tr(\mathbf{D}\mathbf{P}_{n_m}) \tag{5.5}$$

where $\mathbf{P}_{n_m}$ is the projection operator associated to the ray n_m, and the spectral decomposition of **D** is: $\mathbf{D} = \sum_{n_m} b_{n_m}\mathbf{P}_{n_m}$.[27] Now, let us take an observable like 'having spin along the $x-$axis', which is the 3-tuple of the rays x_1, x_2 and x_3. Suppose it is the case that the function assign to x_1 the value 1, i.e. we prepared the system in the state spin up along the $x-$axis. Then, the function above will take the following form:

$$f(x_1) = 1 = Tr(\mathbf{D})\mathbf{P}_{n_n} = Tr(\sum_{n_m} b_{n_m}\mathbf{P}_{n_m}\mathbf{P}_{x_1}) \tag{5.6}$$

Since $\mathbf{P}_{n_m}\mathbf{P}_{x_1} = 0$ if $n_m \neq x_1$, then

$$1 = Tr(b_{x_1}\mathbf{P}_{x_1}\mathbf{P}_{x_1}) = b_{x_1}Tr(\mathbf{P}_{x_1}) = b_{x_1} \tag{5.7}$$

– since we know in general that, given a projection operator $\mathbf{P}_j$, $\mathbf{P}_j^2 = \mathbf{P}_j$. Exactly because $\sum_{n_m} b_{n_m} = 1$ and $b_{n_m} \geq 0$, if follows that in this case **D** is nothing but the projection operator of x_1. Hence, if we suppose that the function $f(n_m)$ assigns value 1 to x_1, then its form will be:

$$f(n_m) = Tr(\mathbf{P}_{x_1}\mathbf{P}_{n_m}) \tag{5.8}$$

It should be clear now that such a function cannot associate the value 1 to another ray in the Hilbert space: the product of two projection operators is always 0 when the two operators are different. Hence, for every ray n_m different from x_1, the function will associate to it 0. This proves that if it is true that a system has a definite state of spin along an axis, then it cannot have a definite state of spin along any other axis. This explains why the disjuncts of the disjunction above – $(X \wedge Z) \vee (X \wedge \neg Z)$ – are both false.

The explanation above should immediately show why Barnes and Williams' account fails in describing quantum indeterminacy. Each possible world in their model is *determinate* and *complete*, as they explicitly acknowledge:

> Importantly, given our picture of indeterminacy, all the worlds in the space of precisifications are maximal and classical. For any p, each precisification will opt for one of p or $\neg p$. Thus, every precisification will represent as true the law of excluded middle, $p \vee \neg p$—and similarly for every classical tautology. (Barnes and Williams, 2011, p.10)

Hence, if we take the statement X and Z above, in each possible world, either X and Z are true, or X and $\neg Z$ are. If we accept understanding $\neg Z$ as 'the electron

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[26] Where $\veebar$ is the symbol of exclusive disjunction.

[27] Here, b_n are different coefficient for each projection operator **P**.

has spin down along z', then both these conjunctions must be false, as we showed above. Possible worlds that contains both X and Z or X and $\neg Z$ cannot be worlds in which quantum mechanics is true. If the actual world is quantum, then these possible worlds determinately misrepresent it. It follows that there is a quantum case that Barnes and Williams's account fails to model. Hence, their account is unsuitable to represent quantum indeterminacy.

5.5 Conclusions

The chapter proposed a critical review of the debate about metaphysical indeterminacy and its application to QM. After sketching some historical background, I presented the two main phenomena responsible for quantum MI's rise: superposition and entanglement. Then, I discussed the five main assumptions needed to argue that QM offers a genuine example of MI and some ways of resisting them. These assumptions, which are often implicitly assumed, are (1 & 2) a realist approach toward textbook QM, (3) the eigenstate-eigenvalue link, (4) that quantum indeterminacy is metaphysical and (5) that properties represented by operators are instantiated even if they lack a precise value. After discussing the assumptions above, I turned to the more metaphysical side of the literature on MI. In particular, following Wilson (2013a), I presented the account of MI as divided into two families: meta-level and object-level accounts. The main difference between the two is that MI is understood as a third possible state of affairs – on top of determinately p and determinately not p – only by object-level accounts. Meta-level accounts, instead, treat indeterminacy in terms of determinate states of affairs. I presented, in turn, the more discussed representant of each family, respectively, Barnes and Williams (2011)'s supervaluationist and Wilson (2013a)'s determinables-determinates views. Finally, I turned back to quantum indeterminacy, and I argued that Barnes and Williams (2011)'s approach fails in accounting for quantum indeterminacy. That Barnes and Williams's account fails in quantum context is not something new. Such a point has been raised in the literature independently by Darby (2010) and Skow (2010). They show the same point I made above using the much more complex theorem presented in Kochen and Specker (1967). The argument I presented admittedly has some limitations – restriction to Hilbert spaces with more than two dimensions, assumption that a complete set of observables exists for $\mathscr{H}$ – which the Kochen-Spekker theorem lacks. That being said, what is crucial is to find *a case* in which Barnes and Williams's account fail, rather than finding proofs as general as possible. The argument above furnishes a case – spin-1 – which is simpler but an equally effective way of stating the point of Darby (2010) and Skow (2010). In the next chapter (§6), I turn to recent accounts of quantum indeterminacy that attempted to save the intuition of Barnes and Williams (2011)'s account by avoiding Darby (2010)'s and Skow (2010)'s argument. In particular, I argue that quantum indeterminacy cannot be understood as a disjunction of determinate states of affairs. To do so, I focus on one of the most recent attempts: the situation semantic account proposed in (Darby et al., 2017).

Chapter 6
Against meta-level approaches to quantum indeterminacy

6.1 The quantum challenge to meta-level indeterminacy

The contribution of Darby (2010) and Skow (2010) to the debate about quantum indeterminacy has been taken as a challenge to develop meta-level approaches to MI able to deal with quantum cases. Indeed, many tried to put forward an account of MI that (i) retains the core intuition of Barnes and Williams (2011)'s model – that MI has to be understood in terms of determinate states of affairs – and (ii) addresses Darby (2010) and Skow (2010)'s objection. Examples of different ways of doing so can be found in Torza (2017, 2021), Simon (2018), Darby et al. (2017); Darby and Pickup (2019), Michels et al. (2021). From a general perspective, applications of meta-level accounts to quantum indeterminacy have been criticised sparsely in the recent literature. Calosi and Wilson (2018) offer some general arguments against the possibility of recovering the spirit of Barnes and Williams (2011)'s account. Similarly, arguments against Torza (2017), Simon (2018), and Darby and Pickup (2019), has been presented in Calosi and Wilson (2021), Fletcher and Taylor (2021) and Iaquinto and Calosi (2021).
The chapter argues that any MI account that reduces indeterminacy to determinate states of affairs fails to account for quantum cases. That is, I present a new[1] general argument that shows that representing quantum indeterminacy in the way above ends up contradicting one of the assumptions necessary for arguing that there is quantum MI in the first place. To do so, I focus on the account proposed in (Darby et al., 2017) and (Darby and Pickup, 2019). However, the results can be straightforwardly applied to any attempt to understand quantum MI in terms of determinate states of affairs.[2]

[1] An earlier version of the argument has been published in (Corti, 2021). The argument is closely related to the one independently put forward by Fletcher and Taylor (2021), published in the same volume of *Synthese*.

[2] As shown in (Fletcher and Taylor, 2021). A notable exception is Torza (2017, 2021)'s account according to which indeterminacy must be undestood in terms of an absence of determinate states of affairs. For arguments against Torza's account, see (Calosi and Wilson, 2018; Fletcher and Taylor, 2021; Calosi and Wilson, 2021).

The structure of the chapter is the following. I start (§6.2) by presenting the necessary details of Darby and Pickup (2019)'s account of MI. Then, I present my argument against the possibility of using their model to account for quantum indeterminacy (§6.3). After the presentation of my argument, I consider, in turn, possible objections to my argument (§6.4). In particular, one of these objections is instrumental for reformulating the argument in a more precise way (§6.5). Finally (§6.6), I discuss Darby's reply to my argument.[3] Conclusions (§6.7) end the chapter.

6.2 Darby and Pickup on quantum indeterminacy

Darby and Pickup (2019)'s account replaces the supervaluationist semantic of Barnes and Williams (2011)'s view with a situation semantics.[4] In a nutshell, situation semantics replace the possible worlds of supervaluationism with situations. Accordingly, the truth value of a proposition is assessed relative to a situation. Where situations are non-representational truth-makers, that is, they are parts of the actual concrete world.[5] In situation semantics, propositions are not true or false simpliciter, but their truth-value depends on the situations at which they are evaluated.

Furthermore, the core of situation semantics is that situations are intrinsically *incomplete*. They only concern some parts of a state of affairs, but they might be crucially silent on other parts or other states of affairs. Propositions expressing aspects of reality that are not present in a situation lack – according to Darby and Pickup (2019) – a truth-value when evaluated at that situation.

An example illustrates better how situation semantics is supposed to work. Consider a proposition like 'Alice is watching a football match.' The truth value of such a proposition depends on the situations at which it is evaluated: it is true if Alice is watching football in the relevant situation and false if she is doing something else in that situation. In those situations in which Alice is not present – such as the situation in which the Las Vegas Raiders are playing against the Pittsburgh Steelers – the proposition 'Alice is watching a football match' lacks a truth value. Similarly, if the relevant situation omits some details, propositions concerning those details lack a truth value. The situation in which Alice is watching a football match, say, does not make true the proposition 'Alice is watching a football match between the Raiders and the Steelers.' Finally, situations that contain contradictory states of af-

[3] Darby wrote a reply to my argument (Darby, Forth.). At the time of the writing, the paper is under review. However, Darby kindly shared the draft with me. I thank him for this and, more generally, for helpful comments on my work.

[4] Their account is much more articulated than the summary I give here. They discuss several details and variants of their account. To keep the discussion short, I stick here just to what is relevant to my purposes.

[5] Darby and Pickup (2019) claims that situations are parts of possible worlds. Insofar as Barnes and Williams (2011)'s possible worlds are ersatz, one might think that also Darby and Pickup (2019)'s situations are ersatz as well. However, this is in contrast with the usual understanding of situations. Hence, to follow the most charitable interpretation, I assume that also Darby and Pickup (2019)'s situations are concrete rather than ersatz.

fairs make the propositions involved in the contradiction truth valueless. Suppose that, for example, there is a situation in which Alice is, and at the same time she is not, watching a football match. The propositions ‘Alice is watching a football match’ and ‘Alice is not watching a football match’ both lack a truth value in the situation above.

How do we choose situations at which we evaluate our propositions? The core of situation semantics is exactly that truth values depend on the situations at which we evaluate them, and there is somewhat huge freedom in choosing what the relevant situations are. However, not all situations are equally good. Following Barnes and Williams (2011), Darby and Pickup (2019) introduce the notion of situations that are *candidates for representing actuality*. The idea is that only the situations that do not determinately misrepresent the actual world are candidates for representing actuality. This kind of situation is the only one that matters when we evaluate a proposition’s truth value. Consider the example of Alice watching a football match again. Any situation that determinately misrepresents the actual world, such as the situation in which Alice is not watching a football match, is not a candidate for representing actuality. On the contrary, any situation that makes true ‘Alice is watching a football match’ – and that it does not misrepresent reality in other regards – is a candidate for representing actuality. Since Darby and Pickup (2019) accept that a proposition can lack a truth value in a situation, also situations in which there is no Alice can be candidates for representing actuality. For example, the situation that makes true ‘a football match is being played’ is a candidate for representing actuality, even though that situation is silent on whether Alice is watching the match.

A couple of remarks are needed before we can proceed. I do not belabour these points in what follows. However, it is instructive for the reader to keep these relevant aspects in mind. Indeed, the argument I propose rests on my understanding of these points since they are not explicitly clarified in (Darby and Pickup, 2019). First, Darby and Pickup (2019) do not propose a logical model of their situation semantics. Hence, it is not entirely clear how to understand some relevant details. Take, for instance, the fact that some propositions may lack a truth value when evaluated at some situations. There are at least three ways of making sense of it. One could: (i) build a semantics with a partial evaluation function, (ii) introduce a third truth value or (iii) posit, following Levesque (1984)’s account, that it is not possible to assign a truth value to some propositions when they are evaluated at certain situations. Arguably, (iii) is closer to what I take to be Darby and Pickup’s intentions. However, such a semantic is baroque already at first-order, and there is no guarantee that the logic needed by Darby and Pickup’s account is feasible at all. Second, Darby and Pickup (2019) do not provide an exact definition of what ‘misrepresenting reality’ means. Moreover, we already saw that such a term was ambiguous also in Barnes and Williams (2011)’s account (Cf. §5.3.1). On the one hand, it is charitable to assume that Darby and Pickup (2019) use such terminology to follow Barnes and Williams (2011). I stick to such terminology in what follows as well. On the other hand, claiming that situations might misrepresent the actual world is misleading. Situations are supposed to be concrete parts of the actual world, so it does not make much sense to say that they might misrepresent (or represent!) it (Cf. fn.5 above).

That being said, my proposal of how to understand ‘determinately misrepresenting the actual world’ is the following. A situation determinately misrepresents the actual world if, given a proposition ϕ that should hold determinately true in the actual world, the situation makes true[6] a proposition that conjoined with ϕ would form a logical contradiction. For example, suppose Alice is watching a football match in the actual world. All the situations that make true a proposition that forms a logical contradiction if conjoined with ‘Alice is watching a football match’ – e.g., the situations that make true ‘Alice is *not* watching a football match’ – determinately misrepresent the actual world.

Given this rough presentation of situation semantics, we can now present Darby and Pickup (2019)’s account of MI. Their idea is to model indeterminacy in terms of *disagreement* between situations that are candidates for actuality. When the states of affairs are determinate, there is no genuine disagreement between the situations that are candidates for representing actuality. Suppose that, in the actual world, Alice is watching a football match while Bob is climbing. The situations that make true, respectively, ‘Alice is watching a football match’ and ‘Bob is climbing’, do not contradict each other: the first sentence is not false in the second situation, but it lacks a truth value – insofar as nothing is said about Alice in the situation in which Bob is climbing – and vice versa. As such, they do not disagree about reality. However, according to Darby and Pickup (2019)’s account, MI has to be understood in terms of disagreement between candidates[7] for representing actuality. They propose the following definitions of determinacy and indeterminacy:

> *Determinacy*: A proposition p is determinate if it is true in some situation which is a candidate for representing reality and false in no such situation.
>
> *Indeterminacy*: A proposition p is indeterminate iff it is true in some situation which is a candidate for representing reality and false in some other such situation. (Darby and Pickup, 2019, p. 9)

Darby and Pickup (2019) characterise determinacy and indeterminacy as meta-level properties. In particular, a proposition is determinately true(false) when, given all the situations candidates for representing actuality, the proposition (i) is never evaluated as false(true) at any of them and (ii) it is evaluated as true(false) in at least one. In contrast, a proposition is metaphysically indeterminate if, given all the situations candidates for representing actuality, the proposition (i) is evaluated as true at one of them and (ii) as false at another. To better see how the account is supposed to

[6] Note that the proposition has to be true when evaluated at that situation. The reason why this is crucial is that Darby and Pickup (2019) explicitly accept a situation as a candidate for representing actuality that contains contradictory states of affairs, e.g., a situation in which spin has more than one definite value of spin along an axis. However, in their machinery, the propositions expressing the fact that the system has those definite values would turn out to be neither true nor false when evaluated at that situation.

[7] As they explicitly mention, a situation is never contradictory alone: if a situation is composed of two contradictory states of affairs, the propositions expressing them lack a truth value when evaluated at that situation.

work, consider again the case of Alice is watching a football match. There will be three kinds of situations:

(A) Situations that do not say anything about Alice. In these situations, any proposition concerning Alice lacks a truth value.
(B) Situations that makes true the proposition 'Alice is watching a football match.'
(C) Situations that makes true the proposition 'Alice is not watching a football match,' or any other proposition that contradicts 'Alice is watching a football match.'

Whether it is determined or not that Alice is watching a football match, there always are situations of kind (A) that are candidates for representing actuality. Now, if it is determinate that Alice is watching a football match, then only situations of the kinds (A) or (B) are a candidate for representing actuality; in particular, at least one situation candidate for actuality must be of type (B). On the contrary, if it is metaphysically indeterminate that Alice is watching a football match, then there will be at least a situation of kind (B) and one of kind (C) among the candidates for actuality. Metaphysical indeterminacy is reduced to situations that are candidates for representing the actual world, and yet they disagree with each other about how reality is.
We can now turn on how Darby and Pickup (2019)'s view is supposed to account for quantum indeterminacy. They discuss, among others, the well-known example of Schrödinger's cat. A cat is trapped in a closed box that contains a device that kills the cat by releasing a poisonous gas if an atom of isotope uranium decays. The life of the cat hinges on whether the atom decays or not. Quantum mechanics tells us that, after a certain time, the uranium isotope will be in a superposed state of being decayed and being not. Since the uranium and the cat compose a bigger system, the upshot of Schrödinger's thought experiment is that also the cat will end up being in a superposition of being alive and dead. Darby and Pickup's account makes sense of the thought experiment as follows:

> Our model can deal with this case in a natural way. Consider the following three situations, each of which is a candidate for representing actuality: s_1, a situation in which the cat is alive, s_2, a situation in which the cat is dead, and s_3, the fusion of s_1 and s_2. In s_1, it is true that the cat is alive and false that it is dead. In s_2 it is true that the cat is dead and false that it is alive. So, what status do these propositions have? According to our definition, they are indeterminate, because there is a situation which is a candidate for representing actuality in which they are true and another situation which is a candidate for representing actuality in which they are false. What is the case in s_3? It seems s_3 is overdetermined with respect to the cat's mortality, as parts of s_3 disagree about whether the cat is alive. Specifically, s_2 precludes the proposition that the cat is alive and s_1 precludes the proposition that the cat is dead. So, following our argument above, the proposition that the cat is alive is neither true nor false in s_3 (as is the proposition that the cat is dead). The indeterminacy involved in superposition is therefore dealt with. (Darby and Pickup, 2019, p. 16)

From the quotation above, the reader should be able to see how Darby and Pickup (2019)'s account avoids Darby (2010)'s and Skow (2010)'s argument that troubled Barnes and Williams (2011)'s view. The crucial aspect is that any situation that contains contradictory states of affairs (let us call them, e.g., p and $\neg p$) is such that the

two propositions that express them lack a truth value when evaluated at that situation. Any proposition expressing either p or $\neg p$ is devoid of any truth value in the situation containing both p and $\neg p$. This feature is crucial for avoiding arguments based on the Kochen-Specker theorem. As we saw indeed in the previous chapter §5.4), the reason why Barnes and Williams (2011)'s view failed to account for quantum indeterminacy is exactly that it uses *maximal*[8] possible worlds. The necessity of all worlds being maximal entails that a quantum entity needs to have definite values for all its properties (in every possible world in which it is contained). However, the upshot of the Kochen-Specker theorem is that it is impossible to attribute to a quantum system definite values for all their properties at the same time. Darby and Pickup (2019)'s account, on the other hand, can easily avoid such tension. Insofar as situations are intrinsically partial, different properties are contained in different situations. Crucially, however, no situation will attribute a definite value to incompatible properties. Since situations can be silent on some aspect of the actual world, no situation has to assign definite values to all the properties of a quantum system – as, say, a situation that makes true 'Alice is watching a football match' is silent on what are the teams playing that match. By using disagreeing situations, Darby and Pickup (2019)'s view addresses cases such as the one presented in the Kochen-Specker theorem, thus accounting for quantum indeterminacy.
However, I argue now that there is an example of quantum indeterminacy that their account fails to model. After having presented my argument (§6.3), I discuss some possible ways of resisting it (§6.4, §6.5, §6.6). Conclusions follow (§6.7).

6.3 Against quantum situations

Let us start by assuming that in the actual world there is an electron with spin up along the x-axis. That is, the state of the electron is:

$$|\uparrow\rangle_x = \frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z) \tag{6.1}$$

According to the formalism of quantum mechanics, having a definite state of spin along the x-axis is equivalent to having a superposed state of spin along the z-axis. In other words, an eigenstate of the operator of spin along the x-axis is the same vector as the one expressed as a superposition of eigenstates of the operator of spin along the z-axis with coefficients $\frac{1}{\sqrt{2}}$.[9] The eigenstates of different spin operators are just other bases of the same space, through which one can represent any state of spin. Hence, not only spin up along the x-axis is mathematically equivalent to a superposition of spin up and down along the z-axis, but also vice versa. That is, spin

[8] Where 'maximal' means that "for any p, each precisification will opt for one of p or $\neg p$" (Barnes and Williams, 2011, p.10).

[9] This, of course, depends on how one labels the spatial axis and the vectors representing spin states.

up and down along the z-axis are mathematically equivalent to a superposition of spin up and down along the x-axis, as follows:

$$|\uparrow\rangle_z = \frac{1}{\sqrt{2}}(|\uparrow\rangle_x + |\downarrow\rangle_x) \tag{6.2}$$

$$|\downarrow\rangle_z = \frac{1}{\sqrt{2}}(|\uparrow\rangle_x - |\downarrow\rangle_x) \tag{6.3}$$

The reason why equations (6.2) and (6.2) hold is that the complex vector space in which spin states are represented is two dimensional and invariant under rotations. The invariance under rotations entails that one can take any couple of orthogonal vectors – like those corresponding to spin up and down on any axis – as the orthonormal basis of the space.[10] In equation (6.1), the orthonormal basis used was that of spin up and down along the z-axis; in that basis, one can describe state (6.1) as a linear combination of spin up and down along the z-axis. For the properties above of the complex vector space used for describing spin states, one can likewise describe the eigenvectors of $\hat{S}_z$ in the basis of spin up and down along the x-axis. As it is shown in (6.2) and (6.3), the result is that spin up and down along the z-axis are represented as superpositions of the eigenvectors of $\hat{S}_x$. To sum up, the state of a system represented as spin up (down) along the z-axis is mathematically equivalent to a superposed state of spin up plus (minus) spin down the x-axis. Where 'mathematically equivalent' means that they are the *same* vector written in different bases.[11] What is crucial for what follows is that the vector equivalent to a superposition of eigenvectors with different eigenvalues of the operator $\hat{S}_i$ – where i is any axis – is never an eigenstate of $\hat{S}_i$; rather, it is an eigenvector of some other operator $\hat{S}_j$, with $i \neq j$.

Let us turn back to quantum indeterminacy. In chapter 5, we saw under what assumptions one can understand a state like (6.1) as representing a case of MI. Darby and Pickup (2019)'s view accounts for such a case of quantum indeterminacy as follows. If an electron is in a superposed state of spin along an axis, then (at least) two situations are candidates for actuality: one in which the electron has spin up and another in which it has spin down along that axis. Darby and Pickup propose to capture the spin properties instantiated by the electron in these situations with two propositions. However, they do not make explicit what these propositions should be. For reasons that will be clear later on (§6.5), I propose the following ones:

(z^+): The state of the electron is an eigenvector of $\hat{S}_z$ with corresponding eigenvalue +1.
(z^-): The state of the electron is an eigenvector of $\hat{S}_z$ with corresponding eigenvalue -1.

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[10] The basis of a space is the minimal set of vectors whose linear combinations describe all the other vectors in the space. The dimensionality of the space – two, in the case of spin – specifies how many vectors compose a basis.

[11] Indeed, $|\uparrow\rangle_z$, can be imagined as a vector in the $|\uparrow\rangle_z$, $|\downarrow\rangle_z$ basis, as follows: $|\uparrow\rangle_z = 1\,|\uparrow\rangle_z + 0\,|\downarrow\rangle_z$.

These propositions should capture, respectively, that the system has spin up and spin down along the z-axis (given the (EEL), that connects the mathematical and the metaphysical descriptions of quantum properties; Cf. §5.2). Accordingly, if the electron in the actual world is in state (6.1), then two situations are candidates for actuality: s_1 and s_2 at which propositions (z^+) and (z^-) are, respectively, true. Importantly though, given some semantic assumptions shared by Darby and Pickup and some rules of linear algebra, the fact that (z^+) is true when evaluated at s_1 entails that (z^-) is false at that situation, and vice versa; i.e., (z^+) is false when evaluated at s_2. This is important because the very definition of MI in Darby and Pickup (2019)'s view is that something is indeterminate if it is true according to a situation candidate for actuality and false when evaluated at another situation candidate for actuality. Hence, the simple case of spin superposition is prima facie successfully modelled by Darby and Pickup as a case of MI. When the electron is in state (6.1), it is metaphysically indeterminate whether it has spin up or spin down along the z-axis; and this is so because the propositions representing these properties are true and false when evaluated at, respectively, two different situations that are candidates for representing actuality.

As we saw above, a situation is a 'candidate for representing actuality' if it does not determinately misrepresent any state of affairs of the actual world. Given this criterion, one can see that there are more relevant situations candidates for actuality than s_1 and s_2. The first one is the fusion of s_1 and s_2, let us call it s_3, that Darby and Pickup explicitly accept. In s_3, the electron has spin up and spin down along the z-axis simultaneously. However, since the state of affairs depicted by s_3 is contradictory, neither (z^+) nor (z^-) receive a truth value in s_3.

The second relevant situation is the one that represents the particle with the property of having spin up along the x-axis. Since in the actual world, the electron has spin up along the x-axis, the proposition expressing this state of affairs has to be determinately true. For Darby and Pickup's model, then, there must be at least a situation in which such a proposition is true (and it must not be false at any other situation candidate for actuality). We could capture such a proposition analogously to how we defined propositions expressing that the electron has the properties of having spin up and down along the z-axis. The result would be:

> (x^+): The state of the electron is an eigenvector of $\hat{S}_x$ with corresponding eigenvalue +1.

Consequently, we postulate that s_4 is the situation that verifies (x^+), i.e., the situation in which the electron has spin up along the x-axis. Let us sum up the four relevant situations candidates for actuality we have just discussed graphically. In what follows, I use 's_n: $\{p_1 \dots p_n\}$' as a shorthand for 'situation s_n is made of the states of affairs expressed in propositions $p_1 \ldots p_n$.[12] The situations above can thus be listed as:

s$_1$: $\{z^+\}$
s$_2$: $\{z^-\}$
s$_3$: $\{z^+, z^-\}$
s$_4$: $\{x^+\}$

So far, I have not added anything to what Darby and Pickup's account predicts. Situation s$_1$ to s$_4$ are explicitly accepted in their account.
It is now time to put forward my argument against their view. To do so, other than assuming that situations from s$_1$ to s$_4$ are candidates for actuality, I need to endorse the following principle. I dub it 'the principle of equivalent candidates for representing actuality' or (ECA*) for short:[13]

> (ECA*): Suppose that proposition ϕ_1 that contains a mathematical term o_1 is true when it is evaluated at a situation s$_*$ that is a candidate for representing actuality. Then, any situation s$_\dagger$, that verifies any proposition ϕ_2 that differs from ϕ_1 only in replacing o_1 with a mathematically equivalent[14] term o_2, is a candidate for representing actuality.

The intuition on which (ECA*) hinges is quite simple. I show it through an example. Suppose it is metaphysically indeterminate whether Tom Brady will end his career with seven or eight Super Bowl titles. According to Darby and Pickup's view, there are two possible future situations: a situation in which Tom Brady has won seven Super Bowl titles and a situation in which he has won eight of them. These situations verify, respectively, the propositions 'Tom Brady has won seven Super Bowl titles' and 'Tom Brady has won eight Super Bowl titles.' Now, (ECA*) simply guarantees that there must be a situation, among those that are candidates for representing actuality, at which also proposition 'Tom Brady has won three plus four Super Bowl titles' is true, a situation at which 'Tom Brady has won nine minus 1 Super Bowl titles' is true, and so forth. As (Darby, ms) rightfully notices, it is controversial whether 'Tom Brady has won eight Super Bowl titles' and 'Tom Brady has won nine minus 1 Super Bowl titles' are different propositions. Analogously, whether different situations verify them or not depends on the metaphysics of situations. If situations are identified by which propositions they make true, then it hinges on

[12] As (Darby, ms) notices, in the published version of the argument (Corti, 2021), I mistakenly asserted that propositions $p_1 \ldots p_n$ are *true* at the situation s$_n$. This is wrong in the case of s$_3$, as remarked above and explicitly stated in (Darby and Pickup, 2019). On the one hand, s$_3$ is the situation in which the electron has both spin up and spin down along the z-axis. On the other hand, both (z^+) and (z^-) lack a truth value when evaluated at s$_3$.

[13] An earlier formulation of the principle that I dubbed (ECA) has been presented in (Corti, 2021). However, the criticisms in (Darby, ms) made me realise that the earlier formulation could be sharpened. Here, I slightly modified its formulation to address the fair critiques above. This also explains the star in the acronym used here.

[14] That is, the terms are co-referential. They refer differently to the same vector, number, and so forth. Where 'differently' stands for, e.g., a vector written in different bases or a number written as the sum of other numbers, as shown in the examples below.

one's metaphysics of propositions. If instead situations are defined by the entities and properties they contain, then the very same situation would verify, e.g., both 'Tom Brady has won eight Super Bowl titles' and 'Tom Brady has won nine minus 1 Super Bowl titles.'

The definition of (ECA*) leaves the possibility that the situations and the propositions above are identical. That is, that ϕ_1 and s_* of the definition are identical to, respectively, ϕ_2 and $s_\dagger$. However, Darby and Pickup (2019, p. 11) claim that a situation that contains the electron with a definite spin state along the *x*-axis – like s_4 – is silent on the electron's spin along other axes. And this is crucial exactly because this is what allows their account to avoid the arguments based on the Kochen-Specker theorem (Cf. §6.2; Darby and Pickup, 2019, p.11). Given the mathematical description of spin states, one would conclude that the truth of 'the electron has spin up along the *z*-axis' entails the truth of 'the electron has not spin up along the *x*-axis'. Even if these propositions are not the very same proposition, they seem to share their truthmakers (in this case, the latter is true in the same situations in which the former is). As such, one might think that if the propositions above are the same proposition – or they are true in the same situation – then Darby and Pickup's account fails: (x^+) would be false at s_1 and, according to their definition of MI, it would turn out that it is metaphysically indeterminate whether the electron has spin up along the *x*-axis. This result would be, of course, problematic insofar as we started by assuming that the electron in the actual world has a definite state of spin along the *x*-axis. Assuming that they are different propositions made true by different situations is charitable toward Darby and Pickup's account.[15]

Even if (ECA*) has been introduced to grant as many favourable assumptions to Darby and Pickup's account as possible, it allows us to present a quantum example that their view fails to account for. We saw above that a definite spin state along the *z*-axis is mathematically equivalent to a superposition of spin along the *x*-axis. They are, indeed, the very same state represented in different bases. As a consequence, the proposition:

> (sup_x): The state of the electron is a superposed state of spin up plus spin down along the *x*-axis.

differs from (z^+) just by the fact that I replaced the mathematical terms of the latter – 'eigenvector of $\hat{S}_x$ with corresponding eigenvalue +1' – with mathematically equivalent ones, *viz.* 'a superposed state of spin up plus spin down along the *x*-axis.' Since there is a situation candidate for actuality that makes true (z^+), (ECA*) allows one to infer that there must be a situation candidate for actuality at which (sup_x) is true. Let us call this situation s_5.[16] Accordingly, the situations that are candidates for actuality when we have an electron in state spin up along the *x*-axis are:

[15] There are other strategies to avoid this simplistic argument against Darby and Pickup's account; some of which are discussed below (§6.4; §6.5) while others are presented in (Darby, ms).

[16] One may wonder why the fact that there is a situation in which the electron is superposed on the *x*-axis is not enough for showing that their account fails. There are two reasons for that. First, their account assumes that (z^+) does not misrepresent the actual world; hence, if (sup_x) is equivalent to (z^+), one cannot conclude that the former misrepresents the actual world without begging the question against their view. Second, meta-level approaches assume that a determinate

s_1: $\{z^+\}$
s_2: $\{z^-\}$
s_3: $\{z^+, z^-\}$
s_4: $\{x^+\}$
s_5: $\{\text{sup}_x\}$

Finally, my argument can be briefly stated. We assumed that the state of the electron in the actual world is (6.1). Accordingly, proposition (x^+) should be determinately true. However, given their definition of MI, it turns out as indeterminate. Indeed, it is true when evaluated at s_4, but it is false when evaluated at s_5. Since both s_4 and s_5 are candidates for actuality, it is metaphysically indeterminate whether the electron has spin up along the x-axis. However, the prediction of their account contradicts the very first assumption of the quantum case it has to model, that is, an electron with a *determinate* state of spin up along the x-axis. If Darby and Pickup's view fails to model such a simple case, it follows that it is not reliable for modelling quantum indeterminacy in general.

6.4 Objections to the argument

There are three elements that I added to Darby and Pickup's account in order to formulate the argument above:

(i) s_4 is a situation candidate for representing the actual world.
(ii) (x^+) is false when evaluated at s_5.
(iii) s_5 is a situation candidate for representing actuality.

As such, setting aside the assumptions explicitly shared by Darby and Pickup, there are three main ways of resisting my argument: resisting (i), (ii) or (iii). I now consider some possible ways of doing so. The rest of the chapter is thus structured. Subsections §6.4.1, §6.4.2 and §6.4.3 are dedicated, respectively, to investigate whether one could resist (i), (ii) and (iii). Section §6.5 discusses a particular way of resisting (iii). The reason why I keep such a discussion separated from §6.4.3 is that the objection discussed in §6.5 is instrumental for presenting a more sophisticated version of the argument. Finally, §6.6 discusses (Darby, ms)'s reply to my argument. Conclusions sum up the chapter's content (§6.7).

6.4.1 s_4 is not a candidate for representing actuality

The first move to block the argument above is to deny that s_4 is a candidate for representing actuality. The reasoning behind this would be the following. According to

state of affairs cannot misrepresent indeterminate ones. If one assumes that the logic of 'being determinate that' is similar to that of Barnes and Williams (2011), then also the converse is true. I come back on this point in §6.4.3.

situation semantics, to evaluate the truth value of a proposition, one has to consider the *relevant* situations for that proposition. On this basis, one could think that when there is MI, one must only consider the situations involved in the indeterminacy. Since s_4 is not relevant for the indeterminacy of spin along the z-axis, we should not consider it among the candidates for representing actuality.

Pushing this line of reply unnecessarily weakens Darby and Pickup's account. The state of affairs we are considering is that of an electron with a definite spin x state. The situation s_4 contains an essential part of the state of affairs we are interested in. The very fact that the electron has definite spin x explains why it is superposed on spin z. Claiming that s_4 is not relevant seems just *ad hoc*. Furthermore, their account's very spirit is distinguishing what is determinate from what is (metaphysically) indeterminate in the actual world. Therefore, if we assume that it is determinate that an electron has spin up along the x-axis, there must be at least one situation candidate for actuality that verifies the proposition expressing that the electron has a definite state of spin x. Insofar as s_4 does the job and does not misrepresent the actual world in other regards, there is no reason why one should not consider it a candidate for representing actuality.

Even granting that one could put *ad hoc* constraints to exclude s_4,[17] one has to accept some situation or another at which (x^+) is true. Any other situation that verifies (x^+) would be as good as s_4 to run the argument. Hence, the point is not that s_4 must be a candidate for actuality. Instead, there must be at least one situation that verifies (x^+), and such a situation would be equally suitable for restating the argument above. If no such situation is considered among the candidates for representing actuality, then Darby and Pickup's model would not account for an electron in a determinate spin x state in the actual world. Avoiding the conclusion of my argument above in this way leads one to accept an equally problematic consequence for Darby and Pickup's account.

6.4.2 (x^+) is false when evaluated at s_5

The second way of resisting my argument I consider is to deny that (x^+) is false when it is evaluated at s_5; rather – the objection goes – $(x^+$ must lack a truth value (when evaluated at s_5). The idea would be that s_5 contains the electron in a superposed state of spin x. When a system is in a spin superposition along any axis, the propositions expressing the properties that the electron has spin up or spin down along that axis are neither true nor false. After all, superposition is considered the paradigmatic case of quantum indeterminacy. Accordingly, if we evaluate (x^+) at s_5, it must lack any truth value. And, one could add, this reply is clearly on the right track insofar as it would make (x^+) determinately true, as it should be by assumption. Indeed, (x^+) would turn out to be true in s_4 and false in no other situations candidates for actuality.

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[17] E.g. one could posit that every situation must verify at least two propositions

This line of reply hinges on an ambiguity between the propositions concerning the mathematical description of quantum systems and those concerning the physical properties they instantiate. To see why this reply does not save Darby and Pickup's account from the argument above, one must distinguish them carefully. I do so below (§6.5), where I propose a more sophisticated version of the argument. Here, I limit myself to remarking that accepting s_5 as a candidate for representing actuality is worrisome, even granting that (x^+) lacks a truth value when evaluated at that situation. In particular, there are two main worries on top of my head.

Firstly, the state of affairs contained in s_5 is supposed to be metaphysically indeterminate since it contains an electron in superposition along the *x*-axis. To further analyse it in Darby and Pickup's model, one should allow some other situations as candidates for actuality: a situation in which the electron has spin up along the *x*-axis and one in which it has spin down. However, to do so is problematic. On the one hand, the situation in which the electron has spin down along the *x*-axis determinately misrepresent the actual world. On the other hand, (ECA*) would allow one to derive further situations in which the electron is in a superposition along every other axis, thus causing an infinite regression of precisification.[18] Secondly, if s_5 is a candidate for actuality, then also s_6 is one, where s_6 is the situation that verifies:

> (-sup$_x$): The state of the electron is a superposed state of spin up minus spin down along the *x*-axis.

(-sup$_x$) is just a proposition that contains a mathematical term equivalent to the one contained in (z^-) – and their equivalence is shown in equation (6.3). Even if one concedes to Darby and Pickup that (x^+) lacks a truth value when evaluated at s_5 and s_6, accepting these situations are candidates for actuality entails that a determinate state of affairs is understood as, say, worldly indecision between indeterminate states of affairs. An electron with a definite spin *x* value is understood as if the world is unsettled between different superposed spin *x* states. This is, of course, no knock-down argument. However, such a consequence is highly unappealing. Claiming that a superposed system has to be understood as worldly unsettledness between definite spin states makes intuitive sense. Yet the opposite – that a definite spin value has to be understood as unsettledness between indefinite spin states – makes little to no sense.

For the reasons above, friends of Darby and Pickup's account should deny that s_5 is a candidate for actuality rather than claiming that (x^+) lacks a truth value at s_5. We now turn on possible ways of arguing that s_5 is not a candidate for representing actuality.

[18] This consequence has also been noticed in Calosi and Wilson (2018, p. 2615) where Calosi and Wilson discuss how friends of meta-level approaches could save Barnes and Williams' account by endorsing non-maximal possible worlds

6.4.3 (iii) s_5 is not a candidate for representing actuality

There are three arguments to deny that s_5 can be a candidate for representing actuality. I shortly discuss two of them here, whereas §6.5 is wholly dedicated to the third one.
The first way consists of arguing that s_5 determinately misrepresents the actual world and, as such, cannot be a candidate for representing it. The reasoning behind this is that a superposed state of spin x determinately misrepresents a definite state of spin x. Intuitively speaking, I am sympathetic to this reasoning. However, it (a) is of no help to the friends of Darby and Pickup's account and (b) ends up begging the question against their view, as mentioned above (Cf. fn.16).
Such a move fails to save Darby and Pickup's account because if a superposed state determinately misrepresents the actual world, the converse should also hold. That is, a determinate state of spin must misrepresent a superposed state as well. The move above would indeed block my argument. Nevertheless, it would also entail that Darby and Pickup's account fails in modelling quantum indeterminacy by forcing one to accept that s_1 and s_2 misrepresent an electron in a superposed spin z state. As such, this is not an option for the supporters of their view.
Furthermore, arguing that a superposed state determinately misrepresents a definite spin state begs the question against meta-level approaches. The core assumption of these approaches is that a proposition like (z^+) does not determinately misrepresent an electron in a superposition in the actual world. If (z^+) and (sup_x) are equivalent, it is an *assumption* of their approach that (sup_x) does not misrepresent the actual world. This can be further appreciated when one considers what should be the logic underlying 'determinate' and 'indeterminate.' There is not much philosophical literature on this point, but the idea is the following.[19] Meta-level approaches introduce a determinacy operator Δ, which stands for 'it is determinate that.' A case of superposition is modelled in this logic as $\neg\Delta z^+ \wedge \neg\Delta\neg z^+$.[20] Crucially, such a characterisation of superposition does not misrepresent, and it is not misrepresented by, a determinate state of spin up (Δz^+) or spin down ($\Delta\neg z^+$) along the z-axis. Claiming that a superposed state determinately misrepresents a definite spin state amounts to rejecting in principle how meta-level approaches understand indeterminacy.
The second way to deny the admissibility of s_5 as candidates for representing actuality is that of rejecting (ECA*). I must confess I struggle to see any plausible justification for rejecting it. I do see why when we are dealing with epistemic context, the *sobstitutio salva veritate* might fail. Here is a simple example. Suppose that Bob knows that Chris roots for the Las Vegas Raiders, the American football team. Consider now the following propositions:

(B): Bob knows that Chris roots for the Las Vegas Raiders.
(B*): Bob knows that Alice's brother roots for the Las Vegas Raiders.

[19] See Fine (1975) for possible ways of defining 'it is determinate that' as a modal operator, and Darby (2010) for applications to quantum indeterminacy.

[20] As the reader certainly recalls, (z^+) is the proposition 'The state of the electron is an eigenvector of $\hat{S}_z$ with corresponding eigenvalue +1.'

There might be cases where (B) is true and (B*) is false, e.g., Bob does not know that Chris is Alice's brother. Accordingly, when we evaluate what an agent knows, the definite descriptions contained in the propositions matter for their truth value. In the case of (ECA*), instead, we are dealing with mathematically equivalent terms, focusing on the metaphysical referent of these propositions. In other words, we are dealing with a *de re* reading of them: we are talking about the referent of these terms, rather than the terms themselves. And in a *de re* reading, also (B) and (B*) are equivalent. (z^+) and (sup_x) have the same referent: an electron in the state expressed by equation (6.1). I do not see how one could deny the equivalence without denying the argument's starting point, viz., an electron in a definite spin x state is in a superposition of spin along the $z-$axis. Since this is a well-established experimental result, I view its rejection as at least suspicious.

6.5 Reformulating the argument

We now turn to the third way in which one could resist the claim that s_5 is a candidate for representing actuality. Investigating this possibility is instrumental for addressing the ambiguity between mathematical and metaphysical propositions mentioned above (§6.4.2) and putting forward a more sophisticated version of my argument.
One could point out that Darby and Pickup's situations must always be definite. They might be silent on some aspect of reality, but they do not contain any indeterminacy in themselves. Accordingly, s_5 cannot be a possible situation insofar as it contains an indeterminate state of affairs, i.e. an electron in a superposition of spin x.
The reasoning above against the possibility that s_5 is a candidate for actuality hinges on conflating propositions describing the metaphysics of quantum systems with propositions expressing their mathematical description. Consider the following propositions:

(z^+): The state of the electron is an eigenvector of $\hat{S}_z$ with corresponding eigenvalue +1.
(Up_z): The electron has the property of having spin up along the z-axis.

The (EEL) discussed in §5.2 should work as a bridging principle between (z^+) and (Up_z). In particular, (EEL) guarantees that (Up_z) is true if, and only if, (z^+) is. Indeed, (EEL)'s very core is that of connecting the mathematical description of a quantum system in terms of vector states to its metaphysical one in terms of properties instantiated. However, meta-level approaches that treat indeterminacy in terms of determinate states of affairs have to break the connection established by the (EEL).[21] The reason is that propositions concerning quantum systems' mathematical characterisation cannot be indeterminate. However, friends of meta-level approaches assume that propositions concerning quantum systems' metaphysical

[21] As remarked below, this is not problematic by itself. It is problematic for friends of MI, though, insofar as (EEL) is exactly what motivated quantum MI in textbook QM.

characterisation can be indeterminate. As such, these kinds of propositions turn out as inequivalent, thus showing that their account is in tension with (EEL). I now reformulate the argument above to highlight the point just mentioned. As I have done in the whole chapter, I stick here to Darby and Pickup (2019)'s view as an example. The central assumption is the same as above: an electron in the actual world in state (6.1), such that it is metaphysically indeterminate whether it has spin up or down along the z-axis. Note that, given such an assumption, proposition (z^+) is determinately false: the state of the electron, indeed, is determinately (6.1), which is not an eigenvector of $\hat{S}_z$. Now, according to Darby and Pickup (2019)'s view, the relevant situations that are candidates for actuality are the following:

s_4: $\{x^+\}$
s_7: $\{Up_z\}$
s_8: $\{Down_z\}$

where ($Down_z$) is:

($Down_z$): The electron has the property of having spin down along the z-axis.

Given the (EEL), (Up_z) and ($Down_z$) are equivalent to, respectively, (z^+) and (z^-). Hence, if s_7 and s_8 are candidates for actuality, then also s_1 and s_2 must be candidates as well.[22] Hence, the relevant situations are:

s_1: $\{z^+\}$
s_2: $\{z^-\}$
s_4: $\{x^+\}$
s_7: $\{Up_z\}$
s_8: $\{Down_z\}$

Finally, consider the following proposition:

(Sup_z): The state of the electron is a linear combination – with coefficients $\frac{1}{\sqrt{2}}$ – of the eigenvectors of $\hat{S}_z$.

By assumption, (Sup_z) should be determinately true. Indeed, proposition (Sup_z) just assert that the mathematical description of the electron's state is given by equation (6.1). If (Sup_z) is determinately true, then (z^+) and (z^-) should be determinately false. However, all of them turn out to be metaphysically indeterminate in Darby and Pickup's account. Indeed, (z^+) is true at s_1 and false at s_2. Vice versa, (z^-) is true at s_2 and false at s_1. Finally, (Sup_z) is true at s_4[23] but false at both s_1 and s_2. Thus, Darby and Pickup's view wrongly considers as metaphysically indeterminate some propositions that should be determinately true.

It might be that one could bite the bullet here and accept that propositions like (z^+),

[22] Whether (Up_z) and ($Down_z$) are different propositions from (z^+) and (z^-) depends on one's metaphysics of propositions, as remarked above (§6.3) and in (Darby, ms). Similarly, it might well be that s_1 and s_2 are identical to, respectively, s_7 and s_8. Here, I treat them as different just for simplicity.

[23] If (Sup_z) and (x^+) are different propositions, then (Sup_z) is not true at s_4. However, there must be, by (ECA*) and s_4, another situation that verifies it.

(z^-) and (Sup_z) have indeterminate truth values, thus restoring the connection between the metaphysical and the mathematical descriptions established by (EEL). However, this entails that the mathematical description of quantum states are unreliable for drawing metaphysical conclusions. In turn, this amounts to either the claim that superposition is nothing but indeterminacy of which eigenvector represents the state[24] or to a rejection of (EEL).

Unfortunately, both the possibilities above are worrisome for the defenders of quantum indeterminacy. As such, they cannot be endorsed by the friends of Darby and Pickup (2019)'s account. An epistemic reading is the only way of making sense of the claim that when a system is superposed, it is indeterminate which eigenvector represents the state. Superposition represents the fact that the electron has a definite spin state, but we ignore which it is before a measurement. However, such an understanding is dangerously close to an epistemic reading of QM's formalism. That mathematical descriptions do not tell us precisely what properties are instantiated by quantum systems has been considered the hallmark of epistemic interpretations of the quantum state. If the formalism of QM, established experiments or mathematical theorems forced me to accept such a position on the quantum state, I would be happy to accept it. However, if a view on MI forces me to it, I would instead move to another account of MI. And the very reason for that is that examples of quantum indeterminacy are possible precisely because the formalism is taken at face value. An account of MI that accepts an epistemic reading of the quantum state undermines – from the very beginning – the main reason for investigating quantum indeterminacy. Similarly goes for the rejection of (EEL). This principle justifies the genuineness of quantum MI in the first place. As such, it is not something friends of MI should lightheartedly reject.[25]

6.6 Darby's reply

In a forthcoming paper, Darby (Forth.) replies to the argument above. The gist of Darby's answer is that s_5 is not a candidate for representing actuality. In particular, Darby challenges my claim that one can derive from s_1 being a candidate for actuality and (ECA*) that s_5 must be a candidate for actuality. In particular, the reason why the derivation above does not follow is two-fold. On the one hand, Darby claims that "the properties of being indeterminate for *x*-spin and being *z*-spin-up are clearly distinct properties" (Darby, Forth., p.4); and this is so because

> the first [the electron having an indeterminate spin *x*] is a meta-level property, while the second [the electron having determinate spin up *z*] obtains (uninformative as this is) just when the system is *z*-spin-up in that situation. So, a (possible) situation in which the system is superposed in *x*-spin and the situation in which the system is *z*-spin-up look importantly different. (Darby, Forth., p.4)

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[24] That is, (z^+) and (z^-) have an indeterminate truth value.

[25] I say more on this point at the end of next section (§6.6).

On my side, considering them as different propositions was the main reason why I introduced (ECA) in the first place. On the other hand, Darby correctly points out that (ECA) cannot be applied to the situations that verify, respectively, 'The electron has the property of being z-spin-up' and 'The electron has the property of being indeterminate for x-spin.' The reason why (ECA) cannot apply to the propositions above is that "none of them describes, at least obviously, a situation that verifies a proposition that 'contains a mathematical object'." (Darby, Forth., p.6).
I concede that (ECA) cannot apply to the propositions above and that the phrasing of the principle was not as sharp as it could be. However, the reformulation of the argument that keeps distinct the mathematical and the metaphysical description of a quantum system hinges neither on s_5 nor on (ECA)/(ECA*).[26] Accordingly, Darby (Forth.)'s criticism seems to miss the target.
However, Darby sums up the core of the reply as follows: "So, the point is that the argument only gets going if couched in terms of properties rather than state vectors – but then the ECA, which revolves around 'mathematical objects' like state vectors, just doesn't apply" (Darby, Forth., p. 7). To my understanding, it seems that Darby here raises a second critique on top of the one above. On the one hand, Darby admits that the argument would be problematic if couched in terms of properties; however, in this formulation (ECA) cannot be used. So, my argument is unsound. On the other hand, Darby claims that the argument couched in terms of state vectors does not get going.
I agree that if the argument is formulated in terms of properties, then (ECA) does not apply. However, I am not sure why the argument couched in terms of state vectors fails. It is crucial whether the argument can or cannot be couched in terms of states since my reformulation of the argument above hinges on it. The reason why Darby claims that the argument cannot be stated in terms of state vectors is, to my understanding, the following:

> You can get mathematical objects into the picture like this (still using the example of a particle with determinate x-spin):
>
> (1') It is indeterminate whether the particle is in state $|+z\rangle$ or $|-z\rangle$.
> So (2') the situation of the particle being in state $|+z\rangle$ is a candidate for actuality
>
> And then argue that, since the particle is supposed to be in an x-spin eigenstate, and since $|+z\rangle$ is not an x-spin eigenstate, there is a candidate for actuality that determinately misrepresents it, which would be a bad result. But this time, as far as we can see, (1') is straightforwardly false. (Darby, Forth., pp. 6-7)

I am not sure whether this is enough to rule out situation (2') – that corresponds to s_1 above, since Darby's (2') corresponds to my (z^+)[27] – as a candidate for actuality. (Darby, ms) shows that (1') being a candidate for actuality is a *sufficient* condition

[26] We saw above one reformulation of the argument. The version of the argument presented in §6.5 is a refinement of a previous reformulation discussed in (Corti, 2021, pp. 5637–5640). Crucially, both reformulations – albeit, admittedly, I have been more explicit here – do not rely on s_5 and (ECA).

[27] As a reminder for the reader, (z^+) stands for 'The state of the electron is an eigenvector of $\hat{S}_z$ with corresponding eigenvalue +1.'

for (2') being a candidate as well. However, to rule out (2'), one has to show that (1') being a candidate for actuality is a *necessary* condition for (2')'s candidacy for representing actuality. So the question is whether there is another situation that is a candidate for actuality from which we can deduce that (2') is as well a candidate for actuality. As remarked above, (EEL) entails that such a situation is the one at which (Up_z) – i.e. 'The electron has the property of having spin un along the z-axis' – is true. The reason is that (EEL) connects a quantum system's mathematical and metaphysical descriptions. If (EEL) is true, then 'having a determinate spin property' is equivalent to 'being described mathematically by a determinate eigenvector of a certain operator.' Actually, one can read (EEL) in a stronger way: the fact that the system has that spin property makes true the fact that we describe its mathematical state in a certain way, rather than another.

For the sake of argument, I can accept that (2') and (Up_z) are different propositions, and even that they are verified at different situations, i.e., respectively, at s_1 and s_7. I am also happy to grant that (2') being verified at a candidate for representing actuality is not enough for showing that Darby and Pickup's account fails. As remarked above (Fn.16, and §6.4.3), if (2') and (Up_z) are equivalent, and their assumption is that (Up_z) does not misrepresent the actual world, one cannot simply claim that (2') misrepresents it. However, the fact that some situations verify (2') is still problematic exactly because they would make Darby's proposition (1') true;[28] the proposition which Darby explicitly considers "straightforwardly false."

In my view, the only way to block the derivation from s_7 being a candidate for actuality to s_1 being a candidate is to reject the (EEL) as formulated in §5.2. I formulated (EEL) as a biconditional, as it is quite standard in the literature on quantum indeterminacy (Darby, 2010; Calosi and Wilson, 2018):

> (EEL): A quantum system has a definite value v for an observable O *iff* it is in an eigenstate of $\hat{O}$ having eigenvalue v. In this case, the definite value is the associated eigenvalue v.

However, we can separate the two directions of the biconditional as follows:

> *If* (EEL): A quantum system has a definite value v for an observable O if it is in an eigenstate of $\hat{O}$ having eigenvalue v.
> *Only if* (EEL): A quantum system is in an eigenstate of $\hat{O}$ having eigenvalue v if it has a definite value v for an observable O.[29]

One needs only the (EEL)'s *If* direction to derive that s_1 is a candidate for actuality from the fact that s_7 is a candidate situation. Indeed, *If* (EEL) allows one to derive, from the truth of the proposition expressing that a system has a definite spin property, the truth of its mathematical description. Now, *If* (EEL) is also crucial to claim that textbook QM offers a genuine example of MI. Indeed, the very idea that there

[28] As shown in the reformulation of my argument in section 6.5.

[29] Sometimes the 'if' and the 'only if' parts of (EEL) are inverted, e.g., in (Myrvold, 2018). As such, it is claimed that the 'if' part is the non-controversial direction of the implication. It just depends on how one writes down the (EEL). In the definition I used in the thesis, it is the 'only if' direction that is non-controversial.

is quantum indeterminacy hinges on a particular way of reading the contraposition of *If* (EEL). We ask ourselves: What does the (EEL) tell us about an observable O when the system is not an eigenstate of $\hat{O}$? And the answer is that:

> We do not want to read the eigenstate-eigenvalue link as saying that it is simply illegitimate to ask about the z-spin of that particle – that takes us back to the worries about inapplicability instead of indeterminacy. Nor do we want to read it as saying that the particle (determinately) has neither property (neither $\uparrow_z$ nor $\downarrow_z$). We want to say instead that it definitely has one or the other, but it is indeterminate which. (Darby, 2010, p. 234)

The crucial detail for friends of quantum indeterminacy is the 'definite' in (EEL)'s definition. When the system's state is not an eigenstate of some operator $\hat{O}$, friends of quantum indeterminacy claim that the corresponding observable does not have a *definite* value. Where 'not having a definite value' does not mean that it lacks the property corresponding to $\hat{O}$ nor that it is illegitimate to investigate such a property. Instead, a system with an indefinite property is understood as it being metaphysically indeterminate whether the system instantiates that property.

To conclude, on the one hand, *If* (EEL) is at the very core of the idea that textbook QM offers a genuine example of MI. On the other, it is admittedly the most controversial direction of (EEL). Hence, friends of meta-level approaches that understand indeterminacy as worldly unsettledness between definite states of affairs perhaps can reject *If* (EEL) as well. Alternatively, they can abandon the interpretation-neutral approach to the metaphysics of QM – despite it being precisely the approach that motivated investigations of quantum indeterminacy in the first place – in favour of an interpretation of QM that lacks (EEL)'s *If* direction. It has been argued that MI is present also in these interpretations (Wilson, 2020; Calosi and Wilson, Forth.; Calosi, ms.). However, it is up to the friends of meta-level approaches to show either that *Only if* (EEL) is enough for presenting a genuine example of quantum indeterminacy, or that the kind of indeterminacy presented in QM's interpretations that reject *If* (EEL) can be explained in terms of unsettledness between determinate states of affairs.

6.7 Conclusions

Under the controversial assumptions discussed in §5.2, one can argue that non-relativistic quantum mechanics offers a genuine example of MI. In §5.1, we saw that there are two main families of approaches to MI: meta-level and object-level accounts. Insofar as QM is considered the best hunting ground for genuine examples of MI, several philosophers tested the compatibility of their account with quantum cases. Darby (2010) and Skow (2010) have shown that the champion of the meta-level approaches, Barnes and Williams (2011)'s model, fails to account for quantum indeterminacy. Their result has been taken as a challenge to develop meta-level accounts of quantum indeterminacy (Torza, 2017, 2021; Simon, 2018; Darby et al., 2017; Darby and Pickup, 2019; Michels et al., 2021).

The chapter proposed an argument against meta-level approaches that account for indeterminacy in terms of unsettledness between definite states of affairs. The chapter focused on one of the accounts that understand MI in such a way, namely, the situation semantics view proposed in (Darby et al., 2017; Darby and Pickup, 2019). After proposing the first version of the argument, I discussed some possible objections to it. This work has been instrumental in presenting a refined form of the argument that pinpoints the main reason quantum indeterminacy cannot be understood as unsettled between determinate states. The reason is that (EEL) tightly links mathematical and metaphysical descriptions of quantum systems. As such, meta-level approaches that treat MI as an indeterminacy of the truth values of the proposition concerning the metaphysical description of quantum systems must either admit that propositions concerning the mathematical description are indeterminate or that (EEL) has to be rejected. I argued that both the horns of the dilemma above are problematic. Indeed, taking the formalism at face value and accepting both the directions of the implications in (EEL) have been the main reasons that motivate many in investigating quantum indeterminacy. As such, they are not assumptions that friends of MI can lightheartedly reject.

The core of the present chapter is a negative result, viz. how one *should not* understand quantum indeterminacy. In the next chapter, I investigate the metaphysics of spin. In particular, the question I address is whether – given the assumptions discussed in §5 – the formalism of QM supports the idea that spin properties are metaphysically indeterminate.

Chapter 7
The metaphysics of Spin

7.1 The Metaphysics of Spin

The chapter aims at investigating the idea that spin is a determinable property by proposing several metaphysical models. One of the first works on the topic has been (Wolff, 2015). Wolff proposes a first understanding of spin number and spin components in terms of chains of determinable-determinate relations. The conclusion of Wolff (2015) is that spin components seem to offer a genuine case of MI. Most of the current literature focuses on whether spin components are metaphysically indeterminate and how to propose metaphysical models of it (Bokulich, 2014; Glick, 2017; Calosi and Wilson, 2021, 2018). Here, instead, I focus on the modelling of spin components in terms of the determinable-determinate relations adding substantial details to (Wolff, 2015)'s analysis. Such a work delivers, as a result, an exhaustive taxonomy of spin models. In turn, the taxonomy reveals a new account of spin that allows us to retain an always determinate metaphysics of spin. The view undermines the claim that spin superposition is a genuine example of MI. Given the methodological criteria adopted (§2), the analysis that follows is limited by narrow working assumptions. In particular, I work under the idealised hypothesis of free-single particle systems in pure states only. Furthermore, I focus on spin, ignoring other quantum properties' metaphysical status. However, insofar as spin superposition is often taken as the paradigmatic example of the paradoxical behaviour of quantum systems, the results of the analysis below are quite substantial on their own.

The structure of the chapter is the following. Section (§7.2) lays down the groundwork necessary for the rest of the chapter. In particular, I briefly introduce the determinable-determinate relation and the main axioms that regiment it (§7.2.1), before turning to how the (EEL) suggests a natural interpretation of spin properties in terms of determinables and determinates (§7.2.2). Then (§7.3), I present a taxonomy of metaphysical models of spin determinables and determinates that is supposed to be exhaustive. The result is the introduction of two families of views: *Spin Determinable Monism* and *Spin Determinable Pluralism*. After having discussed several relevant features of these models, I present two arguments against spin pluralistic accounts (§7.4). In the final section (§7.5), I introduce a novel view within the monistic camp (§7.5.1)[1] and I defend it from a straightforward objection (§7.5.2). Finally, I argue that the view presents a plausible account of spin superpositions when confronted with the main rivals available in the current literature, viz. Calosi and Wilson (2018)' determinable-determinate account of MI discussed in §5.3.2 and Glick (2017)'s sparse view (§7.5.3). The conclusions of the entire thesis follow suit (§8).

7.2 Spin as a determinable

From the metaphysical side, I assume that talking about properties is intelligible. What is the exact metaphysical definition of a property is a question tangential to the chapter's content; hence, no particular definition is assumed. Instead, I do individuate which mathematical structures of QM must be taken as representing physical properties. Furthermore, I assume that physical properties, such as mass, charge, and spin, can be understood in the determinable-determinate relation. The latter is a quite shared assumption (Dasgupta, 2013; Hawthorne, 2006); in the metaphysical literature, the standard account of, e.g., mass hinges on determinable-determinate relations (Hawthorne, 2006).

However, further reasons motivated my choice of investigating spin in terms of determinable-determinate relations. On the one hand, as we saw in §5.3.2, the main view of MI among the object-level approaches is the one proposed by Wilson (2013a), that accounts for MI in terms of the determinable-determinate relation. Furthermore, Calosi and Wilson (Forth., 2021, 2018) argued that the determinable-determinate account of MI models quantum cases of indeterminacy. Assuming that spin can be thus modelled allows me to compare, more simply, the view I propose with theirs.

On the other hand, assuming that the determinable-determinate relation models quantum properties makes the present investigation more wide-ranging. The relevance of the conclusions below does not necessarily hinge on the assumptions endorsed in the thesis. Let me just mention two examples of why this is so. First, the determinable-determinate relations is useful to model also relations. Therefore, even if I assume below that spin is a property, the accounts of spin discussed in the chapter would fit well also in a metaphysics of relations.[2] Second, it has been argued that the determinable-determinate relation is a useful way to characterise measurements (Swoyer, 1987): the physical quantity that is measured is understood as a determinable, whose determinates are the possible concrete results of the measurement. Accordingly, investigating spin in terms of the determinable-determinate

[1] I presented the view and a less structured taxonomy in (Corti and Sanchioni, 2021).

[2] Such as the metaphysics proposed by ontic structural realists (Worrall, 1989; Ladyman and Ross, 2007; Esfeld and Lam, 2011; Muller, 2015). For a primer on structural realism, see (Ladyman, 2020).

relation should be considered as meaningful even by those philosophers with little sympathy for metaphysical talk concerning QM. To them, the views discussed below concern what is the appropriate way of conceptualising a spin measurement, rather than how spin is metaphysically structured.
The present section lays down the necessary preliminaries for what follows, and it is thus structured. I start by giving a brief characterisation of the determinable-determinate relation (§7.2.1). Then (§7.3), I turn to how (EEL) allows us to understand spin properties in terms of the relation above.

7.2.1 The determinable-determinate relation

The determinable-determinate relation is one of the possible metaphysical characterisations of how more general properties are related to more specific ones (Wilson, 2017a). The more general properties are called *determinables*, while their corresponding more specific ones are called *determinates*. Furthermore, the determinable-determinate relation is relative to some level of determination. That is, a determinable of some determinates might be, in turn, a determinate of a more general determinable. The determinables at the highest level, call it L_0, are not determinates of any other determinable. These determinables that are at the top of a chain of determinable-determinate relations are called *maximally unspecific determinables*, and I label them as $D_1 \ldots D_n$. In contrast, the properties at the lowest level, viz. those that are not determinables of any further determinates, are called *maximally specific determinates*.
I label determinables/determinates at any level of specification different from L_0 as $d_1 \ldots d_n$, $dd_1 \ldots dd_n$, $ddd_1 \ldots ddd_n$, where the number of ds represents the property's level of specification. Every ds, dds, etc..., is a determinate of D at, respectively, level of specification L_1, L_2, and so forth. In turn, $dd_1 \ldots dd_n$ are determinates of (some) $d_1 \ldots d_n$ at level of specification L_2, $ddd_1 \ldots ddd_n$ are determinates of $dd_1 \ldots dd_n$ at level L_3, and so forth (see fig. 7.1).[3]
An example helps us to understand better how the determinable-determination relation works. Consider the property of 'being coloured.' Such a property is more general than the properties of, say, 'being blue' and 'being red.' At the same time, no property concerns colours, and it is more general than 'being coloured.' So 'being coloured' is a maximally unspecific determinable with determinates 'being blue', 'being red', and so forth. However, other colour properties are more specific than, say, 'being blue.' For example, 'being cyan', 'being navy' and 'being turquoise' are more specific ways in which something can be blue. The properties above are determinates of the determinable 'being blue' at a level of specification different from the level at which 'being blue' is a determinate of 'being coloured.' Insofar as there

[3] Figure 7.1 is a tentative visualizable guide of the determinable-determinate relation, rather than a detailed graphic representation of the determination structure. Below, I often use similar graphs for representing different spin determinable-determinate models. Again, they are supposed to be a just a visualizable guide.

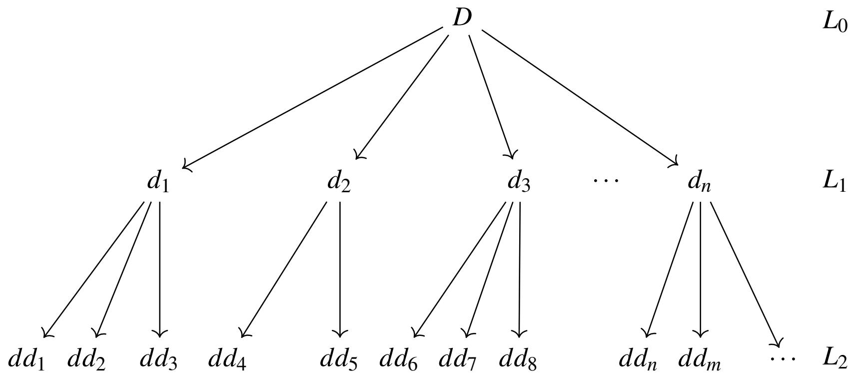

Fig. 7.1 Pictorial of a generic chain of determinable-determinate relations. D is the *maximally unspecific determinable* at level L_0. The determinates of D at level L_1 are $d_1, d_2, \dots d_n$. The determinates of $d_1, d_2, \dots d_n$ at level of determination L_2 are $dd_1, dd_2, \dots dd_m$. The arrows represent the determinable-determinate relations. The arrows always start from a determinable and point to one of its determinates. The dots in the graph stand for the missing determinables/determinates, thus signalling that the graph is incomplete.

are no more specific ways in which something can be cyan, navy or turquoise, they are maximally specific determinates (see fig. 7.2).
The determinable-determinate relation is characterized through some axioms. Following Wilson (2017a), I list and briefly discuss below the main ones:

- *(Irreflexivity)*: Property d is not a determinable or a determinate of d.
- *(Asymmetry)*: If dd is a determinate of d, then d is not a determinate of dd. Analogously, if d is a determinable of dd, then dd is not a determinable of d.
- *(Transitivity)*: If ddd is a determinate of dd, and dd is a determinate of d, then ddd is a determinate of d. Analogously, If d is a determinable of dd, and dd is a determinable of ddd, then d is a determinable of ddd.
- *(Increased specificity)*: Determinate properties are specific ways of having determinable properties.
- *(Relative, levelled determination)*: Properties are determinable or determinate only relative to other properties. Moreover, determinables and determinates can be determined at different levels of determination.
- *(Determinable inheritance)*: For every determinable d of a determinate dd, if an entity instantiates dd at a time t, then it instantiates d at t.
- *(Requisite determination)*: If an entity instantiates a determinable d, then for every level L_n of determination of d, the entity must instantiate some L-level determinate, i.e. some dd at L_1, ddd at L_2, and so forth.
- *(Determinate incompatibility)*: If an entity instantiates a determinate dd_1 of a determinable d at L_1, then the entity cannot instantiate any other determinate

$dd_2 \dots dd_n$, with $n \neq 1$, of determinable d at L_1. However, it can instantiate other determinates of d at a different level of specification, e.g., ddd_1 at L_2.

- *(Determinate similarities)*: Determinates of a same determinable admit a special kind of comparison; in particular, if dd_1 and dd_2 are determinates of d, then they are similar, and comparable, in respect of d.

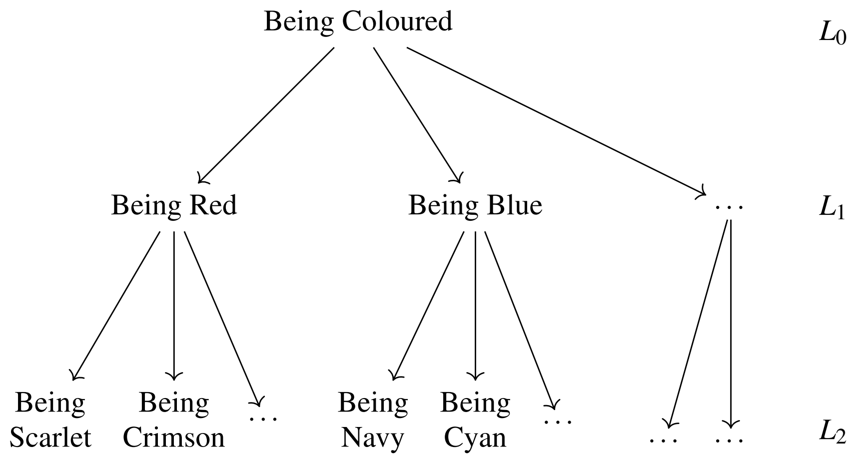

Fig. 7.2 Pictorial of the chain of determinable-determinate relations of colour properties. 'Being red' and 'being blue' are used just as examples of determinates of 'being coloured.'

The example of colours will illustrate how these axioms work. The first three are quite self-explanatory: 'being blue' is neither a determinate nor a determinable of 'being blue.' Since 'being cyan' is a determinate of 'being blue', 'being cyan' is not a determinable of 'being blue;' and, since 'being cyan' is a determinate of 'being blue', and the latter is a determinate of 'being coloured', 'being cyan' is a determinate of 'being blue' as well.

(Increased specificity) states that 'being cyan' is a specific way of 'being blue', and *(Relative, levelled determination)* clarifies that the determinable-determinate is relative to a level of specification. That is, 'being blue' is a determinable of 'being cyan' but a determinate in respect of 'being coloured.' According to *(Determinable inheritance)*, if an entity instantiates the property of 'being cyan', then it must also instantiate 'being blue' and 'being coloured.' Axiom *(Requisite determination)* guarantees that if an entity instantiates 'being coloured', then it also instantiates either 'being blue', or 'being red', or 'being green', and so on for every determinate of 'being coloured' at each level of specification. From the last two axioms, one can derive *(Determinate incompatibility)*: if an entity instantiates 'being cyan', then it cannot instantiate any other determinates of 'being blue', such as 'being navy' and 'being turquoise.' Finally, *(Determinate similarities)* follows from *(Increased specificity)*: since 'being cyan' and 'being navy' are different ways of 'being blue', they are sim-

ilar to each other, and they can be compared.
One could add further axioms to characterise the determinable-determinate relations. However, the list above suffices for the chapter's aim. On the one hand, the axioms above are the core of almost every presentation of the determinable-determinate relation. On the other hand, we are going to see that some possible accounts of spin as a determinable entails a violation of *(Requisite determination)*.

7.2.2 From (EEL) to spin determinables and determinates

To interpret spin properties in terms of the determinable-determinate relation, one needs to find in QM a principle that links the mathematical description of spin with its metaphysical characterisation. Fortunately, textbook QM already comes equipped with such a principle: the eigenstate-eigenvalue link (EEL) we already discussed in previous chapters (§5, §6). As I argued above (§5.2), (EEL) connects the mathematical descriptions of a quantum system in terms of states and operators to its metaphysical description in terms of *physical properties* instantiated. Furthermore, (EEL) refers to two different kinds of properties, one more specific than the other. Let us recall once again (EEL)'s definition:[4]

> (EEL): Given a physical quantity O represented mathematically by a self-adjoint operator $\hat{O}$:
>
> > (1.) A system in a state $|\psi\rangle$ possesses a definite value of O if and only if $|\psi\rangle$ is an eigenstate of $\hat{O}$, $\hat{O}|\psi\rangle = o_i|\psi\rangle$.
> > (2.) In this case, the definite value is the associated eigenvalue o_i.

The first line of (EEL) - "a physical quantity O (is) represented by a self-adjoint operator $\hat{O}$" - clearly states that the operator $\hat{O}$ represents a general property O. The reason why the property mentioned in the first line is a general one is that bullet (1.) specifies under what conditions a quantum system instantiates a *definite* value of $\hat{O}$. The properties of 'having $\hat{O}$' and 'having a definite value of $\hat{O}$' are two different properties; in particular, the latter seems a specific way in which an entity can instantiate the former. This brief characterisation is in line with standard accounts of physical properties in terms of determinables and determinates. In the case of, say, mass, 'having mass' is a determinable property with determinates having mass 3kg', 'having mass 4kg' and so on (Hawthorne, 2006). Consequently, one can read (EEL)'s claim that operators represent physical properties in two ways: (i) operators $\hat{O}$ represent determinable properties and (ii) their eigenvalues – as bullet (2.) suggests – represent the determinates of the determinable above.
It is interesting to notice that the determinable-determinate distinction I have drawn

[4] I use a definition of (EEL) different from the one used in previous chapters. In particular, the one used here is a slight rearrangement of the one presented in (Wallace, 2019, p. 2). I switched to this definition because it allows us to appreciate better the distinctions between the determinables and the determinates of physical properties. In previous chapters, this aspect was not relevant, so I opted for a more streamlined definition, the one standardly quoted in the literature on quantum MI.

above between operators and their definite values is implicitly assumed in Hughes (1989, Ch. 6), a now-classic textbook of QM. Inspired by Hughes (1989, Ch. 6), I propose the following notation for quantum determinables and determinates. Determinables are simply represented by operators[5] and their determinates by the tuple composed by the operator and *one* of its eigenvalues. As such, $\hat{O}$ represents the determinable O and $(\hat{O}, o_1)$, $(\hat{O}, o_2) \dots (\hat{O}, o_{\hat{i}})$[6] represent O's determinates.

Having specified how the determinable-determinate distinction applies to quantum properties, it is now time to turn to spin. My starting point is Wolff (2015), who already proposed a characterisation of spin in terms of determinables and determinates. In particular, Wolff (2015) argues that since spin is characterised as a vector, it is fully specified by two magnitudes: the spin number and the spin components.[7] We saw in §4.3 that spin is mathematically described by the following equations:

$$\hat{S}^2\,|s,s_i\rangle = \hbar^2 s(s+1)\,|s,s_i\rangle \qquad , \qquad s \in \mathbb{N}/2\,; \tag{7.1}$$

$$\hat{S}_i\,|s,s_i\rangle = \hbar\, s_i\,|s,s_i\rangle \qquad , \qquad s_i = \{s,\ s-1,\ \dots, -s\}\ , \tag{7.2}$$

Prima facie, equations (7.1) and (7.2) tell us that a particle's spin is indeed characterised by two different quantities. In particular, equation (7.1) describes the spin number while (7.2) characterises the component of spin along any axis i.

Wolff (2015) already showed that analysing the spin number as a determinable is almost straightforward if not natural. The determinable of the spin number is represented by $\hat{S}^2$, and it represents, so to speak, 'how much spin' a particle has. The determinates of $\hat{S}^2$ are the concrete spin numbers that could characterise a particle, e.g., $\frac{1}{2}$, 1, $\frac{3}{2}$, 2, and so on. Following the notation set above, I label the determinates of $\hat{S}^2$ as ($\hat{S}^2$, $\frac{1}{2}$), ($\hat{S}^2$, 1), ($\hat{S}^2$, $\frac{3}{2}$), and so forth. Furthermore, the spin number partitions the spin Hilbert space in superselection sectors, thus dividing particles into kinds: electrons are spin-$\frac{1}{2}$ particles, photons have spin-1, and so forth. As such, the spin number is a fundamental property of spin that allows us to distinguish different particles (see fig. 7.3).

In a similar way, one may read equation (7.2): given any direction i along which spin can be aligned, operator $\hat{S}_i$ represents the determinable property of 'having spin along the i-axis.' Its determinates are the concrete orientations of spin along that axis. In the case of spin-$\frac{1}{2}$ particle, every $\hat{S}_i$ has two eigenvalues: $+\frac{1}{2}$ and $-\frac{1}{2}$. Hence, $\hat{S}_i$ has two determinates, that I label as $(\hat{S}_i, \frac{1}{2})$ and $(\hat{S}_i, -\frac{1}{2})$, that stand for, respectively, the determinates 'having spin up along the i-axis' and 'having spin down

---

[5] A more accurate characterisation would be $(\hat{O}, o_i)$, viz. the ordered tuple of an operator and the set of its eigenvalues. To avoid a pedantic notation, I simply refer to determinables as being represented by operators.

[6] The hat on the i indicates that $o_{\hat{i}}$ is a specific eigenvalue of $\hat{O}$, so to distinguish it from o_i of fn. 5 that represents, instead, the set of every eigenvalue of $\hat{O}$.

[7] Actually, Wolff (2015) argues that there is a third one, namely, the sense, whose determinates are spin up and spin down. However, since spin is not actually a spatial vector, I am not sure whether it is correct to characterise it with a sense. Rather, it seems to me that spin up and spin down along a given axis are determinates of the determinable 'having spin along that axis.' And this suffices to characterise spin as a property.

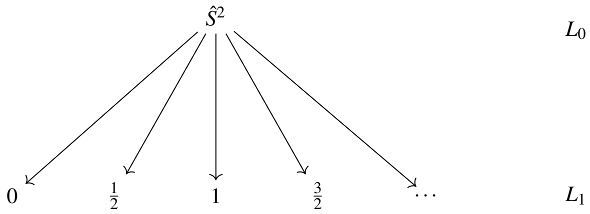

Fig. 7.3 Pictorial of spin number determinable-determinate relations.

along the *i*-axis.' For example, in the case of spin-$\frac{1}{2}$ systems, the determinable of having spin along the *x*-axis is represented by $\hat{S}_x$ and it has determinates $(\hat{S}_x, \frac{1}{2})$ and $(\hat{S}_x, -\frac{1}{2})$.

As Wolff (2015) notices, it is standard in determinables-determinates accounts of vectors to treat a vector's magnitude and direction in terms of separate chains of determinable-determinate relations. Spin states can indeed be represented as vectors in Hilbert spaces. However, spin's magnitude and directions are intrinsically connected. Even if it makes sense to keep the magnitude and the direction separate for general vectors, the case of spin is peculiar and deserves a different treatment. The reason is that the magnitude of spin states, i.e. the spin number, crucially determines the possible orientations of spin components. As shown in (7.2), the eigenstates of $\hat{S}_i$ are labelled with two terms: s and s_1. The latter represents a specific eigenvalue of $\hat{S}_i$, where the former is the eigenvalue of $\hat{S}^2$. Note that this is not just labelling. The number of eigenstates (and, as a consequence, of the eigenvalues) of $\hat{S}_i$ is *fixed* by the eigenvalues of $\hat{S}^2$. If we chose spin number $\frac{1}{2}$, then every $\hat{S}_i$ has two eigenstates – spin up $\left|\frac{1}{2}, +\frac{1}{2}\right\rangle$ and spin down $\left|\frac{1}{2}, -\frac{1}{2}\right\rangle$. Instead, if we chose spin number 1, then every $\hat{S}_i$ has three eigenstates – $|1,1\rangle$, $|1,0\rangle$ and $|1,-1\rangle$; and so on and so forth.

This means that when we chose to model the spin of a particle, we first chose which kind of particle it is by determining its spin number. This choice, in turn, determines the possible spin states for every spin operator along any other axis. As such, an operator of spin along any axis, $\hat{S}_i$, does not represent the property of 'having spin along the *i*-axis.' Rather it represents the property of 'having spin-*n* along the *i*-axis.' This can be further appreciated by the notation set above, that correctly distinguishes 'having spin-$\frac{1}{2}$ up along the *i*-axis' and 'having spin-1 up along the *i*-axis', labelled, respectively as $(\hat{S}_i, +\frac{1}{2})$ and $(\hat{S}_i, +1)$. In what follows, I focus on spin-$\frac{1}{2}$ systems only. As such, I avoid specifying the spin number every time I speak of spin properties along an axis. Analogously, I left the $\frac{1}{2}$ implicit when labelling spin determinates, e.g., I label the spin determinate 'having spin-$\frac{1}{2}$ along the *i*-axis' as $(\hat{S}_i, +)$, instead of $(\hat{S}_i, +\frac{1}{2})$. These simplifications are adopted just to improve the readability of the text but are philosophically innocuous. Indeed, the content of what follows is independent of a quantum system's spin number.

Before turning to possible metaphysical models of spin components, it is important

to discuss a relevant feature of QM's formalism briefly. A theorem often ignored in standard presentations of QM is relevant for our purposes. The reason why it is ignored is that it is actually trivial from a mathematical point of view. However, it has significant consequences on the metaphysics of spin.
This theorem has been called by Susskind and Friedman (2014, p.90) *Spin-polarisation Principle*, and it states that:

> (SPP) *Spin-polarisation principle*: Any state of a single spin is an eigenvector of some component of the spin. In other words, given a generic state $|\psi\rangle \in \mathbb{C}^2$, there exists some direction $\mathbf{n}$ such that:

$$\hat{\mathbf{S}} \cdot \mathbf{n} \ |\psi\rangle = \frac{\hbar}{2} |\psi\rangle \ . \tag{7.3}$$

Where $\hat{\mathbf{S}}$ is the spin vector:

$$\hat{\mathbf{S}} = \frac{\hbar}{2} (\sigma_x, \sigma_y, \sigma_z) \ , \tag{7.4}$$

in the Pauli matrices σ_x, σ_y and σ_z representation of the $SU(2)$ group. A quite technical, yet pedagogical, reconstruction of the polarisation vector, a proof of the uniqueness of $\mathbf{n}$ and a detailed discussion of $\hat{\mathbf{S}} \cdot \mathbf{n}$, can be found in (Sakurai, 1994, pp. 165-168).
Why is the *Spin-polarisation principle* so relevant for the metaphysics of spin? The reason is that equation (7.3) entails that for any spin state $|\psi\rangle$, there is always a unique vector $\mathbf{n}$ – called *polarisation vector* – along which the component of spin is predictably $+\frac{1}{2}$. Note that $\mathbf{n}$ is a vector in ordinary three-dimensional space. Equation (7.3) allows us, among other things, to connect spin states in the spin Hilbert space to concrete directions along which spin can be measured, e.g., with a Stern-Gerlach device.
This theorem is relevant from a metaphysical point of view because it guarantees that there is always a spatial axis along which one of the results of a spin measurement has probability 1. In other words, a free single-particle system (in a pure state) has a definite spin state along one axis prior to any measurement. The theorem guarantees that among the determinables represented by $\hat{S}_x$, $\hat{S}_z$, $\hat{S}_y$, and any other $\hat{S}_i$, there is one direction along which the particle's spin is determinately aligned. And we can say that the particle has a definite spin state along a particular direction before measurements are performed because (SPP) guarantees that if we were to measure the spin along that direction, its result would be certain. This entails that if we understand spin determinables as being represented by operators of spin along every possible axis, $\hat{S}_x$, $\hat{S}_z$, $\hat{S}_y$, and any other $\hat{S}_i$, then before any measurement the system determinately instantiates a determinate of one of them.
One may wonder whether we can *know* along which direction spin is aligned before any measurement. The answer is that we cannot determine it for a free-single particle system in a pure state. We can reconstruct the spin state that the system had before any measurement is performed, but we need to perform some measure-

ments to do that.[8] However, (SPP) guarantees that the system's spin is aligned along an axis with a determinate orientation. That is, the fact that we do not know along which axis the system's spin is aligned is a form of *epistemic* indeterminacy. Note that this is not to say that there is no metaphysical indeterminacy. If one, following (Calosi and Wilson, 2018), accepts that spin determinables are instantiated even if none of its determinates is, then (SPP) simply guarantees that *one* spin determinable is determined. However, the system's spin is *metaphysically* indeterminate in the directions different from the one of the polarisation vector. What (SPP) shows is that there is some epistemic indeterminacy involved in the case of a free-single system's spin. This is not in contrast with the claim that there is also some metaphysical indeterminacy.

Now that I have laid down the groundwork, it is time to enter the heart of the chapter. The aim of what follows is:

- to present an exhaustive taxonomy of broad families of views that understand spin in terms of the determinable-determinate relation. I dub them *Spin Determinable Monism* and *Pluralism* (§7.3);
- to argue that one has to prefer a broadly monistic view to forms of *spin determinable pluralism* (§7.4);
- to defend, within the monistic camp, a new account of spin (§7.5).

7.3 Spin determinables: monisn vs. pluralism

We saw above how (EEL) allows us to distinguish determinables and determinates properties of spin. It is now time to propose a taxonomy of the possible determinable-determinate spin models. I proceed by posing some relevant questions and by discussing their possible answers. Let us start with:

(Q_1): What are the maximally specific determinates of spin?

Question (Q_1) is simple because it admits a unique answer. The maximally specific determinates of spin are the concrete orientations that spin can have, such as 'having spin up along the x-axis', 'having spin down along the z-axis', and so forth. They are maximally specific determinates because no spin property is a more specific way of instantiating, say, 'having spin up along the x-axis.' I labelled these maximally specific determinates as the ordered tuple of their determinable and the eigenvalue connected to the relevant spin direction. For example, $(\hat{S}_x, +)$ and $(\hat{S}_z, -)$ stand for, respectively, 'having spin up along the x-axis' and 'having spin down along the z-axis.' Note that in this labelling, I took for granted that operators $\hat{S}_x$ and $\hat{S}_z$ stand for determinable properties – in line with the rough characterisation given above (§7.2.2). This leads us to the second question:

(Q_2): For every axis i, does the operator of spin along the i-axis – $\hat{S}_i$ – represent the spin determinable 'having spin along the i-axis'?

[8] I come back on this later on, in §7.5.2.

This question has received a unanimous answer from the literature up to date. From the metaphysics of QM to textbooks for physicists, it is often claimed that $\hat{S}_x$, $\hat{S}_z$, etc..., represent some properties. Under the assumption that having a definite value of, say, $\hat{S}_x$ is a different property from the one represented by such an operator, one concludes that $\hat{S}_x$, $\hat{S}_z$, etc..., must be the determinables of the determinates above. The following examples, taken by the literature of physics, philosophy of physics and scientific metaphysics, should be more than enough to convince the reader that considering $\hat{S}_x$, $\hat{S}_z$, etc..., as properties is part and parcel of the mainstream.
Physicists seldom use the word 'property' explicitly. Rather, they often refer to it as 'physical quantity' and, in the case of QM, 'observable.' However, in a realist understanding of a physical theory, these terms arguably coincide with metaphysicians' 'property' (Calosi, ms.). For example, in the early days of QM, operators have been considered 'elements of reality' – in Einstein, Podolsky, and Rosen (1935, p. 778)'s terminology.[9] That is, real properties that characterise the quantum systems of interest that can be measured and separated by the system's other features. However, there are examples of physics textbooks that explicitly consider spin components as 'properties.' A notable one is the now-classic (Hughes, 1989):

> We could say, for instance, that in the state $\mathbf{z}^+$ the particle has the property $(S_z,+)$; the value of S_z is predictable with certainty, and so there is an element of reality corresponding to it. However, we could also say that, in this state, the particle has neither the property $(S_x,+)$ nor the property $(S_x,-)$, that neither of these properties constitutes an element of reality. (Hughes, 1989, p. 159)

Contemporary textbooks are no exception. For example, we can read in (Norsen, 2017, p. 217)[10] that 'if the same property (x-spin or $z-$spin) is measured, ..." A similar way of talking can be found in textbooks in the philosophy of physics. For instance, the very beginning of (Albert, 1992, p. 1)'s introduction to QM is:

> The story concerns two particular physical properties of electrons which it happens to be possible to measure (with currently available technology) with very great accuracy. [...] Let's call one of them the "softness" of the electron, and let's call the other one its "hardness."

As Albert explicitly acknowledges in the footnote, 'hardness' and 'softness' stand for the properties of having spin along different directions.
All the references above, implicitly or explicitly, betray the idea that spin operators represent different (determinables) spin properties. One could say that our focus - viz. the question 'Do operators of spin represent different spin properties?' - is intrinsically a metaphysical one. As such, we should be charitable and allow physicists, and less metaphysically cautious philosophers, to freely use the word 'property' without being committed to the view here under scrutiny. Nevertheless,

[9] In the original paper, Einstein, Podolsky, and Rosen (1935, p. 778) refer to position and momentum rather than spin components. However, from (Bohm, 1989, p. 611 *ff.*)'s analysis of the paradox presented in Einstein, Podolsky, and Rosen (1935, p. 778) we can infer that spin components were considered 'elements of reality' as well.

[10] The book is a recent undergraduate introduction to QM, that also delves into some philosophical problem of the theory even though it is intended for an audience of physicists.

the issue is that the same way of talking of spin can be easily found in analytic metaphysics. From textbooks on the metaphysics of QM - e.g., "A physical property like $z-$spin is represented by an operator on the space of vector" (Lewis, 2016, p. 10) - to more research-oriented papers. Note that such a consideration holds good from older debates on the metaphysics of QM[11] to those just recently emerging.[12] Finally, there are few authors that make an explicit reference to the determinable-determinate machinery when talking about spin. Darby (2010, p. 233), for instance, claims that "each basis corresponds to an observable (a determinable, for example, spin in the x-direction) and the elements of the basis correspond to the possible values of the observable, (determinates of the determinable, such as spin-up and spin-down)."

It is safe to assume then that the standard view across the board is that operators $\hat{S}_x$, $\hat{S}_z, \ldots$, represent different spin determinables. Note that since spin can be measured along any spatial axis, the standard view entails that there are infinite spin determinables, one for each axis.

The work done above allows us now to formulate a less trivial question concerning spin's metaphysics in precise terms:

> (Q_3): Are the spin determinables represented by operators $\hat{S}_x$, $\hat{S}_z, \ldots$, maximally unspecific?

Even though a question like (Q_3) is fundamental for understanding how spin is metaphysically characterised, to my knowledge, it has never been investigated before in the literature. As such, it is not easy to find in the literature authors that endorse one answer or the other. Some authors speak of spin as if it were a single property (Bigaj, 2012; Cetto et al., 2020). Yet nothing is said about whether this property is connected with those represented by $\hat{S}_x$, $\hat{S}_z$, and so on. The only notable exception can be found in (Calosi and Mariani, 2020, Fn. 38), where it is claimed that these determinables are not maximally unspecific; however, Calosi and Mariani (2020) do not provide any further indication on which is the maximally unspecific determinables. For what concerns the rest of the literature, only the claim that $\hat{S}_x$, $\hat{S}_z, \ldots$, represent some properties can be constantly found. Yet, clues on whether there is a more general property of spin are almost inexistent. Perhaps, a question like (Q_3) is natural only if spin is understood in terms of determinable-determinate relations. Be it as it may, we can now distinguish between two families of spin determinables-determinates models based on how they answer (Q_3). There are two possible answers to it:

- *Pluralistic answer*: A quantum system's spin consists of an infinite number of chains of determinable-determinate relations of spin, one for each axis i along which spin can be measured. The maximally unspecific determinables of each chain is the property 'having spin along the i-axis' represented by the operator $\hat{S}_i$.

[11] For instance, in the debate over the role of dispositional properties in QM. See for example: Dorato (2007).

[12] The debate about MI is a paradigmatic example (Bokulich, 2014; Calosi and Wilson, 2018; Darby and Pickup, 2019).

- *Monistic answer*: A quantum system's spin consists of a single chain of determinable-determinate relations. The maximally unspecific determinable of this chain is the property of 'having spin.'[13]

By combining the *pluralistic* and the *monistic answers* with the work done above, we can propose now two general models of the metaphysics of spin in terms of determinable-determinate relations:

> (SDP) *Spin Determinable Pluralism*: A particle's spin consists of an infinite number of chains of determinable-determinate relations. There is a chain of determinable-determinate relations for every axis i along which spin can be aligned. The properties of 'having spin along the i-axis' represented by $\hat{S}_i$ is the maximally unspecific determinable of each chain. The maximally specific determinates of each chain are the concrete orientations that spin can have along the i-axis, viz. spin up and spin down, that are represented by $(\hat{S}_i,+)$ and $(\hat{S}_i,-)$. There is no further level of determination between the maximally unspecific determinables of spin and its maximally specific determinates (see fig. 7.4).
>
> (SDM) *Spin Determinable Monism*: A particle's spin consists of a single chain of determinable-determinate relations. The maximally unspecific determinable is the property of 'having spin.' The maximally specific determinates are the concrete orientations that spin can have along the i-axis, viz. spin up and spin down, that are represented by $(\hat{S}_i,+)$ and $(\hat{S}_i,-)$.

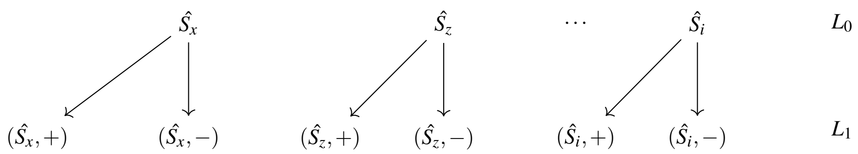

Fig. 7.4 Pictorial of *Spin Determinable Pluralism* for a spin-$\frac{1}{2}$ system. The maximally unspecific determinables are represented by $\hat{S}_x$, $\hat{S}_x$, $\ldots\hat{S}_x$. Operator $\hat{S}_i$ represts spin along a generic axis i, and the dots represent that there is an infinite number of them. For every axis i, every spin i determinable has only two determinates: 'spin up' and 'spin down along the i-axis' labelled, respectively, by $(\hat{S}_i,+)$ and $(\hat{S}_i,-)$.

(SDP) and (SDM), as defined above, are families of metaphysical models rather than single views. Indeed, different variants can be crafted by adding more metaphysical theses. For example, Glick (2017) argued that even accepting that operators of the form of $\hat{S}_i$ represent determinable properties, no determinable is instantiated when none of its determinates is – as axiom (Requisite determination) requests (§7.2.1). Glick calls its view the 'Sparse view.' According to Glick, if a system has, say, spin up along the x-axis, then no determinable of spin along different axes is instantiated.

[13] Remember that, given the considerations above (§7.2.2), the property should be 'having spin-n.' As such, the monistic answer is that there is a chain of determinable-determinate relations for each spin number. However, no system instantiates more than one at the same time.

In contrast, Calosi and Wilson (2018)'s view is that in that very case, the system's spin along the other axes is metaphysically indeterminate. In Calosi and Wilson (2018)'s account, this is understood in terms of the system instantiating the determinables of spin along every axis, but crucially lacking any spin determinates but for spin up along the x-axis.[14] Both the sparse view and Wilson's account of MI can be combined with both (SDP) and (SDM) to craft more specific variants of them.

Here, I am not interested in exploring the space of metaphysical possibilities. So rather than introducing several variants of (SDP) and (SDM), I limit myself here to present what I take to be the mainstream version of (SDM). I have shown above that considering operators $\hat{S}_x$, $\hat{S}_z$, etc..., as representing spin determinables, is part of the mainstream. However, there is no evidence of whether (SDP) or (SDM) is part of the mainstream understanding of spin. Since the literature seems to agree that operators $\hat{S}_x$, $\hat{S}_z$, etc..., represent spin determinables, the mainstream views must be either the (SDP) above or the following variant of (SDM):

> (SDM)$_s$ *Standard Spin Determinable Monism*: A particle's spin consists of a single chain of determinable-determinate relations. The maximally unspecific determinable is the property of 'having spin.' The maximally specific determinates are the concrete orientations that spin can have along the i-axis, viz. spin up and spin down, that are represented by $(\hat{S}_i,+)$ and $(\hat{S}_i,-)$. There is a level of determination between the maximally unspecific determinable of spin and its maximally specific determinates, composed by the determinables of 'having spin along the i-axis' for every axis i along which spin can be aligned. Every determinable of 'having spin along the i-axis' is a determinate of 'having spin,' and it has as determinates 'having spin up' and 'having spin down along the i-axis' (see fig. 7.5).

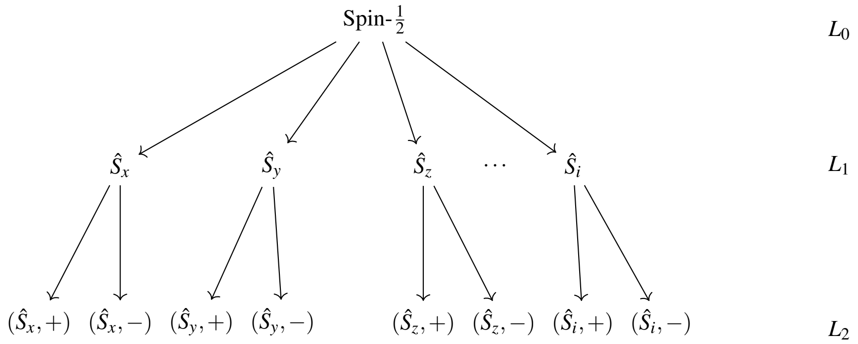

Fig. 7.5 Pictorial of *Standard Spin Determinable Monsim* for spin$-\frac{1}{2}$ systems. The maximally unspecific determinables is 'having spin-$\frac{1}{2}$' at level L_0. Its determinates at L_1 are the determinables of having spin along an i-axis, represented by operators of the form $\hat{S}_i$. The maximally specific determinates are the concrete orientations along which spin can be aligned.

[14] In the gappy variant. In the glutty one, all are instantiated but only one with degree 1. Cf. §5.3.2.

The determinable-determinate relation is not a piece of metaphysical sophistry useless for the philosophy of physics. Indeed, it helped us to model different metaphysical views of spin properties. So far, I have shown that (SDP) and (SDM) are two broad families of views, and I argued that the current literature is ambiguous on which of these families is the correct one. Furthermore, I outlined some possible ways of crafting different forms of (SDP)/(SDM). With these distinctions at hand, we can now delve deeper into the metaphysics of spin. The rest of the chapter aims at addressing the following questions:

(Q_4): Are there general arguments for preferring (SDP) over (SDM), or vice-versa?

And within the monistic camp:

(Q_5): Which mathematical object represents 'having spin,' the monists' unique maximally unspecific determinable of spin, if any?
(Q_6): Does the machinery of determinable-determinate relation allows us to introduce a new account, undiscussed in the literature, that deserves a serious investigation?

The next section (§7.4) is devoted to addressing (Q_4). In particular, I propose two arguments to prefer a broadly monistic view over a pluralistic one. As we will see, addressing (Q_4) incidentally reveals a plausible answer to (Q_5). Finally, I address (Q_6) in section §7.5 where I argue that there is a novel model of spin determinables-determinates that deserves further attention.

7.4 Against spin determinable pluralism

In this section, I put forward two arguments favouring (SDM). The first argument (§7.4.1) holds only under controversial assumptions. Even if some philosophers endorse these assumptions, the argument's conclusions are not as general as they could be. However, the first argument is also instrumental for the formulation of a more general argument that I present in §7.4.2.

7.4.1 Invariance

Throughout the second part of the thesis, I presented on several occasions some crucial properties of the operators of spin along different axes (§5.2; §4.3; §6.3). Let me list them below a final time briefly. First, spin along different axes cannot have at the same time a definite value. This is so because having a definite spin value along an axis entails being in a spin superposition along every other axis. Second, the space in which spin is represented is invariant under rotations. This means that: (i) any couple of orthonormal vectors that represent possible orientations of spin along an axis can be used as a basis[15] of the spin space and (ii) there is not a privileged basis that should be prefered. Third, a system having a definite spin value along a direction does not merely entail that it is in a superposition on the other. Instead, having a definite spin value on an axis and being in a superposition on another is the same state written in different bases.

The argument against (SDP) can be thus presented. The physics of the system does not change when we perform a change of basis. The physics described by, say, $|\uparrow\rangle_x$ is not different from the one described by $\frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z)$. That is, there is nothing physical that $\frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z)$ explains that it is not explained already by $|\uparrow\rangle_x$. Since there is not a basis privileged over the others, we should conclude that the *description* of the same state in different bases cannot have a strong metaphysical import. Since the system's physics is invariant under a change of basis, then a definite spin state on an axis and a superposition state on a different axis must correspond to the same metaphysical structure. Accordingly, the representations of a spin state in different bases are mathematically equivalent descriptions of a single vector. This runs against (SDP) because it shows that the representing a spin state in terms of the eigenstates of $\hat{S}_x$, $\hat{S}_z$, etc..., is (partially) arbitrarily: operators $\hat{S}_x$, $\hat{S}_z$, etc..., allows us just to describe the same spin space in different bases. But again, insofar as physics is left unchanged when we pass from one basis to another, no substantial metaphysical connotations have to be attached to these bases. The fact that states like $|\uparrow\rangle_x$ and $\frac{1}{\sqrt{2}}(|\uparrow\rangle_z + |\downarrow\rangle_z)$ are different characterisation of the same vector state – together we the considerations above – entails that we must give reality to this vector only. The uniqueness of the vector, in turn, shows that there is a unique general property of spin, contra (SDP).

To show the correctness of the argument above, one has to provide a better description of such a unique spin vector. The most straightforward way of doing so is by means of the well-known Bloch Sphere representation (see fig.7.6). In the Bloch Sphere representation, every point on the surface[16] of the sphere represents a spin state. Once one has chosen a basis for the space, the eigenvectors of $\hat{S}_z$, say, the coordinates of a generic spin state $|\uparrow\rangle_s$ are given by:

$$|\psi\rangle_s = \cos\left(\frac{\theta}{2}\right)|\downarrow\rangle_z + e^{i\phi}\sin\left(\frac{\theta}{2}\right)|\uparrow\rangle_z \ . \tag{7.5}$$

That the spin is invariant under rotations can be appreciated graphically: we can imagine rotating the sphere so that any pair of orthonormal vectors, say, spin x up and down, take the place of the basis chosen, viz. spin z up and down. When we perform this rotation, what changes is just the basis upon which we describe the state $|\uparrow\rangle_s$. There are only two structures that are invariant under this rotation: the

[15] That is, every spin state can be written as a linear combination of the vectors used as the basis.

[16] This is true for spin$-\frac{1}{2}$ particles in a pure state. For the states of composite systems, the point lies inside the sphere. Particles with a spin number larger than $\frac{1}{2}$ are treated, from a formal point of view, as composite system.

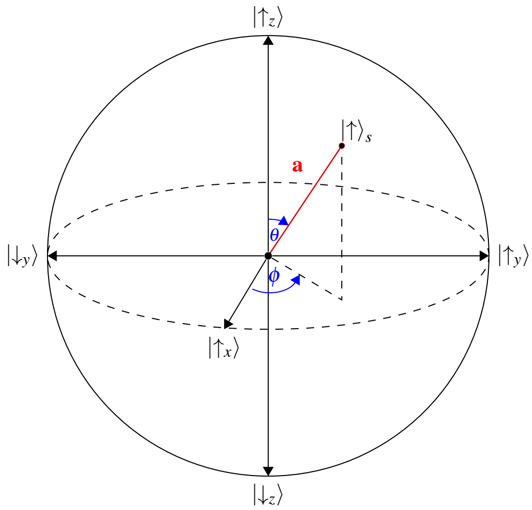

Fig. 7.6 The Bloch Sphere representation of a spin-$\frac{1}{2}$ particle.

state $|\psi\rangle_s$ and the *Bloch vector* **a**[17] which connects the centre of the sphere to the state $|\psi\rangle_s$. All the physically relevant information concerning spin are contained by the states at which **a** points, insofar as they are independent of the coordinates chosen to represent the Bloch sphere. Since the relevant physics is captured by the state $|\psi\rangle_s$ toward which **a** points, the former is the only mathematical structure to which one should seriously consider from a metaphysical point of view. The fact that such a spin state is unique, in turn, entails that there is a unique property of spin.
The argument above hinges on the fact that the spin *state* is unique. The uniqueness of the spin state entails the uniqueness of a general property of spin only if one reads off the spin properties from spin states. Now following Glick (2017, pp.205–206), one could separate three broad approaches to the metaphysics of QM:

(1) "one can view the quantum state non-ontologically," interpreting it as a "book-keeping" device only informs us about the fundamental properties described by self-adjoint operators; (ibid.)
(2) one can consider "the properties to be ontologically derivative and quantum states to be fundamental;" (ibid.)
(3) or "one can advocate a flat ontology for standard QM", and consider states and properties as on "equal foot ontologically". (ibid.)

The argument above requires that one's approach to the metaphysics of QM is either (2) or (3). That is, the argument only works if one is willing to assign the metaphysical status of 'properties' to quantum states (Monton, 2004; Dorato and Esfeld,

[17] $\mathbf{a} = a^i \hat{e}_i$ (where Einstein's rule is intended). The components of **a**, a_i, are contravariant. That is, **a** is independent of any coordinate $\hat{e}_i$ one chooses.

2010; Belot, 2012). Indeed, the argument shows that the spin state is invariant under a change of basis. Consequently, the uniqueness of the spin property follows if one gives a metaphysical weight to quantum states. However, as the characterisation of spin determinables and determinates provided in §7.2.2 suggests, the approach to the metaphysics of QM I implicitly assumed here is (1). That is, sticking to the spirit of (EEL), I assumed that the values of a quantum property are represented by the eigenvalues of an operator rather than by its eigenvectors. Hence, finding arguments that also friends of (1) would accept is not relevant just because (1) is assumed by some[18] but also because it is part and parcel of the working assumptions accepted here.

The argument presented in this section has been ineffective for my aims. However, its formulation allows me to present in the next section (§7.4.2) a stronger argument in favour of (SDM).

7.4.2 Uniqueness

The argument presented in the last section (§7.4.1) suggested that the descriptions of spin along a particular axis are, in some sense, arbitrary. On the one hand, the same vector can be written in different bases. On the other, none of these bases is privileged. Since the unique basis-independent spin vector is invariant under a change of basis, one should conclude that the physics of spin is described just by this very vector. However, we saw that the argument could only draw significant metaphysical conclusions if one accepts that state vectors represent quantum properties. A natural question that one who accepts, instead, that the eigenvalues of a given operator represent the value of a quantum property is: Is there a relationship between the unique spin vector and the eigenvalues of any operator? If such an operator exists, one should conclude that such an operator must have some metaphysical import. Following the work done in the sections above, one would conclude that this operator represents a determinable of spin, whose determinates are represented by the concrete orientations of the unique spin vector.

I argue now that such an operator exists, and it is the $\hat{\mathbf{S}} \cdot \mathbf{n}$ that appeared in equation (7.3) (Cf. §7.2.2). Since $\hat{\mathbf{S}} \cdot \mathbf{n}$ is the only operator that has as eigenstates every possible spin state, its existence has to be considered as an argument against (SDP).

The polarisation vector $\mathbf{n}$ can be parametrised through three angles (α, β and γ). To do so, one selects three spatial directions, say $\hat{\imath}, \hat{\jmath}$ and $\hat{k}$, and defines the following angles accordingly:

$$\cos\alpha = \frac{\mathbf{n}\cdot\hat{\imath}}{|\mathbf{n}|} \quad , \quad \cos\beta = \frac{\mathbf{n}\cdot\hat{\jmath}}{|\mathbf{n}|} \quad \cos\gamma = \frac{\mathbf{n}\cdot\hat{k}}{|\mathbf{n}|} \tag{7.6}$$

[18] E.g., it has been defended for Bohmian mechanics in (Suárez, 2015), Relational QM in (Rovelli, 2018b; Calosi and Mariani, 2020) and Modal QM in (Lombardi, 2019).

where $|\mathbf{n}|$ is the modulus of the polarisation vector. The polarisation vector itself is fully characterised by:

$$\mathbf{n} = |\mathbf{n}| \begin{pmatrix} \cos\alpha \\ \cos\beta \\ \cos\gamma \end{pmatrix} \tag{7.7}$$

By specifying the values of α, β and γ, one provides a full characterisation of the polarisation vector.

I can now show that every possible spin state is an eigenstate of $\hat{\mathbf{S}} \cdot \mathbf{n}$. Equation (7.3) shows that spin states are captured univocally by the polarisation vector $\mathbf{n}$. If we take a unit polarisation vector $\mathbf{n} = \{\cos(\alpha), \cos(\beta), \cos(\gamma)\}$,[19] the explicit form of this spin operator $\hat{\mathbf{S}} \cdot \mathbf{n}$ is given by:

$$\hat{\mathbf{S}} \cdot \mathbf{n} = \frac{\hbar}{2} \begin{pmatrix} \cos(\gamma) & \cos(\alpha) - i\cos(\beta) \\ \cos(\alpha) + i\cos(\beta) & -\cos(\gamma) \end{pmatrix} . \tag{7.8}$$

Once one fixes the values of α, β and γ, also the value of $\hat{\mathbf{S}} \cdot \mathbf{n}$ is fixed (if the system's spin number has been chosen). For example, if we chose $\alpha = 0$, $\beta = \frac{\pi}{2}$ and $\gamma = \frac{\pi}{2}$, the operator $\hat{\mathbf{S}} \cdot \mathbf{n}$ takes the form of $S \cdot \hat{x}$. Instead, if we take $\alpha = \pi$, $\beta = \frac{\pi}{2}$ and $\gamma = \frac{\pi}{2}$, $\hat{\mathbf{S}} \cdot \mathbf{n}$ takes the form of $\hat{\mathbf{S}} \cdot (-\hat{x})$. The corresponding eigenvectors of the forms of $\hat{\mathbf{S}} \cdot \mathbf{n}$ above are, respectively, $|\uparrow\rangle_x$ and $|\downarrow\rangle_x$ with eigenvalue 1. This notation may seems prima facie puzzling at first. The eigenvalue of every eigenvector of $\hat{\mathbf{S}} \cdot \mathbf{n}$ is $+1$. However, note that it in the case of $\hat{\mathbf{S}} \cdot \mathbf{n}$ it is the form of the operator that changes when we consider different spin states. This is due to equation (7.3), which requires that the eigenvalues of $\hat{\mathbf{S}} \cdot \mathbf{n}$ are always equal to $+1$. Now, since spatial axis can be parametrixed by means of three angles, $\mathbf{n}$ can point toward every possible direction. Given all the possible measures of α, β and γ, every possible spin state is an eigenstate of $\hat{\mathbf{S}} \cdot \mathbf{n}$.

Another argument that shows that every spin state is an eigenstate of $\hat{\mathbf{S}} \cdot \mathbf{n}$ is the following. There is a one-to-one map between the values of the polarisation vector $\mathbf{n}$ and the Bloch vector $\mathbf{a}$. A short proof of this mathematical fact is the following. Take an eigenvector $|\psi\rangle$ of $\hat{\mathbf{S}} \cdot \mathbf{n}$ aligned along the direction $\hat{s}$. Suppose we want to write it in a particular basis, let us say, the eigenvectors of $\hat{S}_z$? To do so, one 'rotates' the state $|\psi\rangle$ by two angles until $\hat{s}$ and $\hat{z}$ coincide. The mathematically correct way of doing so is given by equation (7.5). As we saw, (7.5) allows one to calculate the coordinates of every spin state on the Bloch sphere representation in the basis of the eigenvectors of $\hat{S}_z$. As a consequence, the eigenstates of $\hat{\mathbf{S}} \cdot \mathbf{n}$ and the states individuated by $\mathbf{a}$ are the same. Since every possible spin state is individuated by $\mathbf{a}$ in the Bloch sphere representation, every spin state is an eigenstate of $\hat{\mathbf{S}} \cdot \mathbf{n}$. Furthermore, $\hat{\mathbf{S}} \cdot \mathbf{n}$ is the operator of spin invariant under rotations. Indeed, a change of basis modifies both the Pauli matrices, i.e. $\hat{\mathbf{S}}$, and the components of $\mathbf{n}$, while,

[19] i.e. We take $|\mathbf{n}| = 1$. Recall that here I am assuming that the system's spin number is $\frac{1}{2}$ particles. Since spin number different from $\frac{1}{2}$ are treated as composite systems of multiple spin-$\frac{1}{2}$ systems, taking $|\mathbf{n}| = 1$ is a natural assumptions.

crucially, preserving the form of the operator $\hat{\mathbf{S}} \cdot \mathbf{n}$. To put it differently, the forms of $\hat{\mathbf{S}} \cdot \mathbf{n}$ captures the fact that the spin vector (and the state connected to it) does not change when we rotate the Bloch sphere.

To conclude, $\hat{\mathbf{S}} \cdot \mathbf{n}$ is independent of the choice of basis, and it has as eigenvectors every possible spin states with eigenvalue $+1$. Given the determinable-determinate characterisation provided above (§7.2.2), one should conclude that $\hat{\mathbf{S}} \cdot \mathbf{n}$ represents a spin determinable. Since there are no spin states that is not an eigenvector of $\hat{\mathbf{S}} \cdot \mathbf{n}$, one must conclude that such an operator represent the *maximally unspecific determinable* of 'having spin.' The uniqueness of $\hat{\mathbf{S}} \cdot \mathbf{n}$ entails, quite directly, that (SDM) is the correct family of spin determinables and determinates.

7.5 A new account of spin determinables and determinates

Section §7.3 ended up posing three questions:

(Q_4): Are there general arguments for preferring (SDP) over (SDM), or vice-versa?
(Q_5): Which mathematical object represents 'having spin,' the monists' unique maximally unspecific determinable of spin, if any?
(Q_6): Does the machinery of determinable-determinate relation allows us to introduce a new account, undiscussed in the literature, that deserves a serious investigation?

In the last section, I addressed (Q_4) by proposing two arguments to prefer (SDM) over (SDP). In particular, the second argument consisted in showing that every spin state is an eigenstate of $\hat{\mathbf{S}} \cdot \mathbf{n}$. Given the characterisation of spin determinables and determinates given above, the argument suggested that $\hat{\mathbf{S}} \cdot \mathbf{n}$ should be considered a maximally unspecific determinable of spin. The uniqueness of such an operator showed that there is a unique maximally unspecific determinable of spin, contra (SDP).

The present section aims to address (Q_6). The section is thus structured. In §7.5.1 I present a new account of spin in terms of determinable-determinate relations that I dub *Components Antirealism Spin Determinable Monism*. After having discussed the spirit of the account, I turn to a straightforward objection to the plausibility of the view (§7.5.2). Then (§7.5.3), I briefly evaluate *Components Antirealism Spin Determinable Monism* by comparing it to its main rivals, that is, Calosi and Wilson (2018)'s determinable-determinate account of MI and Glick (2017)'s sparse view, introduced in §5.

7.5.1 Spin components antirealism

Given that conclusions of §7.4 have been that (SDM) should be favoured over (SDP), one has to look within the monistic camp to find interesting accounts of spin. In §7.3, I showed that considering operators $\hat{S}_x$, $\hat{S}_z$, etc..., as determinables is part and parcel of the mainstream view. However, it has been argued that not every operator must represent a property (Daumer et al., 1996). When we interpret the theory, we posit that an operator represents a given physical property. Indeed, the (EEL) very beginning says that "*Given* a physical quantity O represented mathematically by a self-adjoint operator $\hat{O}$." That is, (EEL) – and the whole formalism of QM more generally – is silent on which operators represent physical properties. Accordingly, one could postulate that the operators of spin $\hat{S}_x$, $\hat{S}_z$, etc..., do not represent any property and accordingly propose a monistic model of spin in terms of determinables and determinates. The result that I dub here *Components Antirealism Spin Determinable Monism* $(\text{SDM})_{CA}$, would be the following account:

> $(\text{SDM})_{CA}$ *Components Antirealism Spin Determinable Monism*: A particle's spin consists of a single chain of determinable-determinate relations. The maximally unspecific determinable is the property of 'having spin' represented by the operator $\hat{\mathbf{S}} \cdot \mathbf{n}$. The maximally specific determinates are the concrete orientations that, for every axis i, spin can have along the i-axis, viz. spin up and spin down, that are represented by $(\hat{\mathbf{S}} \cdot \hat{\imath}, +)$ and $(\hat{\mathbf{S}} \cdot -\hat{\imath}, +)$. There is no further level of determination between the maximally unspecific determinable of spin and its maximally specific determinates (see fig. 7.7).

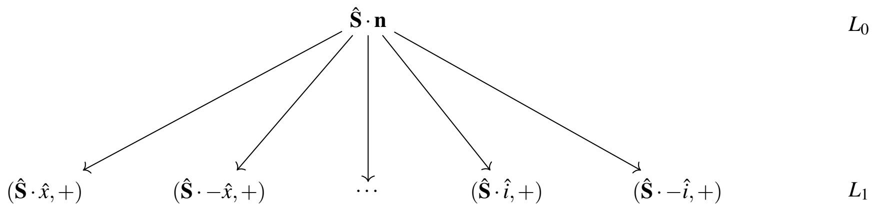

Fig. 7.7 Pictorial of *Component Antirealism Spin Determinable Monsim* for spin-$\frac{1}{2}$ systems. The maximally unspecific determinable is 'having spin-$\frac{1}{2}$ ' at level L_0 represented by operator $\hat{\mathbf{S}} \cdot \mathbf{n}$. Its determinates at L_1 are the particular directions along which the system's spin could be polarised that we obtain by substituting $\mathbf{n}$ with a precise spatial direction. Spin up and down along the x-axis, respectively, $(\hat{\mathbf{S}} \cdot \hat{x}, +)$ and $(\hat{\mathbf{S}} \cdot -\hat{x}, +)$, are portrayed in the pictorial as an example. The dots indicate that the number of determinates is infinite. The maximally specific determinates $(\hat{\mathbf{S}} \cdot \hat{\imath}, +)$ and $(\hat{\mathbf{S}} \cdot -\hat{\imath}, +)$, for a generic axis i, indicates that there are only two possible spin orientations for every axis. There is no further level of determination between the maximally unspecific determinable and its maximally specific determinates.

The reader might have noticed that the determinates of $(\text{SDM})_{CA}$ have a different notation from those of the other views discussed in the chapter. For example, having spin up and spin down along the $x-$axis is labeled by $(\text{SDM})_{CA}$ as $(\hat{\mathbf{S}} \cdot \hat{x}, +)$

and $(\hat{\mathbf{S}}\cdot -\hat{x},+)$, rather than the previous $(\hat{S}_x,+)$ and $(\hat{S}_x,+)$ (see also §7.4.2 and fig. 7.7). This might seem to be a tension with our previous definition of determinable-determinate relations in QM insofar as, previously, determinates of a given determinable were represented by the tuple made of the same operator with different eigenvalues. We have seen why we should take $\hat{\mathbf{S}}\cdot\mathbf{n}$ as a determinable and why the spin polarisation principle guarantees that the eigenvalue of this operator is always "+". Hence, spin up and spin down along any axis have the same eigenvalue. As such, what changes from one eigenstate to another is the direction of **n** and, as a consequence, the form of $\hat{\mathbf{S}}\cdot\mathbf{n}$. What is crucial about determinables and determinates is that a determinable is more general than its determinates, rather than the formal definition we provided. What matters is that a determinable has some generic features that are made more precise by its determinates. Hence, I do not take it as problematic to allow that the determinable of spin is the invariant form of the operator and that its determinates differ in the precise values that the operator may take, rather than for its eigenvalues (See figure 7.4).[20]

Now, according to $(\text{SDM})_{CA}$, operators $\hat{S}_x$, $\hat{S}_z$, etc..., do not correspond to any determinable of spin. The only spin determinable is the property of 'having spin' represented by $\hat{\mathbf{S}}\cdot\mathbf{n}$ and the maximally specific determinates of spin, that must be the same for every determinable-determinate model of spin as argued above (§7.3). This view is clearly radical, and it might even sound, prima facie, as completely implausible. Indeed, the core of the view is that of denying what I argued above is the standard view across different literature, viz. that operators $\hat{S}_x$, $\hat{S}_z$, etc., stand for physical properties. In the next subsection, I show that $(\text{SDM})_{CA}$ can present a reasonable account of spin measurement along different axes (§7.5.2) and that it has several advantages over its main rivals (§7.5.3). Before turning to that, I conclude the section with a description of the spirit of the view and a small clarification about the status of operators of spin along different axes.

I take the spirit of $(\text{SDM})_{CA}$ to be an orthodox but *realist* account of spin. The reason why it is a realist view is that the *spin-polarisation vector* guarantees that there is always a direction along which a system's spin is aligned. This entails that according to $(\text{SDM})_{CA}$, the determinable of a free-single particle system always instantiates one of its spin maximally specific determinates. As we saw (§7.2.2), the theorem above guarantees that this is true even before any measurement is performed. This helps us appreciate why $(\text{SDM})_{CA}$ can be considered an 'orthodox' understanding of spin. There are two broad reasons for that.

First, the account captures the physicists' intuition about the meaninglessness of asking the spin value along a direction when the system is not in one of the eigenstates of the operator of spin along that direction. Early fathers of QM and some contemporary physicists motivate on different grounds one cannot ask about spin values before measurement. Some of those reasons are outlined below. Be that as it may,

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[20] For once, the characterization above was conditioned by the mainstream idea that $\hat{S}_x$,$\hat{S}_z$, etc..., represent spin determinable. As such, it was natural there to characterize determinable in terms of operators and determinates in terms of eigenvalues. Since the formalism itself suggests that $\hat{\mathbf{S}}\cdot\mathbf{n}$ has to be taken as representing the property of 'having spin,' or so I argued, such a notational shift is not a serious threat to the view.

$(SDM)_{CA}$ has a straightforward account of why it is meaningless to pose such a question. The spin of a (free) quantum system is metaphysically determinate before any measurement but epistemically indeterminate. Moreover, every concrete orientation of spin is a determinate of the same determinable. As such, it is the determinable-determinate relation itself that explains why it does not make any sense to ask the value of spin x when referring to a spin x up system. Axiom (Determinate incompatibility) (§7.2.1) tells us that no entity instantiates more than a determinate for a given determinable at the same level of specification. Investigating whether a spin x particle instantiates any spin z properties is like asking 'Which shade of blue it is?' pointing to a wholly red object. The very fact that the object is red excludes that it has any determinate property of 'being blue.' An analogous answer is provided by friends of $(SDM)_{CA}$: it is the very fact that a system has, say, spin up along the x-axis that prevents the system from having any determinate value of spin z, spin y and so forth.

The second reason why $(SDM)_{CA}$ can be considered an 'orthodox' understanding of spin is that it provides answers to some of the paradoxes of QM in line with the ones given by orthodox QM. Take, e.g., Bohm (1989)'s retelling of the Einstein et al. (1935)'s paradox: there are two entangled electrons, and we simultaneously measure they spin along different axes. The conclusion of the paradox is a dilemma: either we accept that the description of QM is incomplete before measurements, or spin along different axes do not correspond to different 'elements of reality', to put it in Einstein et al. (1935)'s terminology. Mimicking the orthodox way of answering the paradox, supporters of $(SDM)_{CA}$ answer that the operators $\hat{S}_x$, $\hat{S}_z$, etc..., do not represent any property. However, as discussed in §4, the orthodox interpretation of QM leans toward antirealism. For example, Bohm concludes from the Einstein et al. (1935)'s paradox that the quantum world cannot "correctly be analyzed into elements of reality, each of which is a counterpart of a precisely defined mathematical quantity appearing in a complete theory" (Bohm, 1989, p. 619).[21] That is, Bohm denies that to *any* part of QM's formalism corresponds some element of reality independent from the other. Bohm is just an example among many. On the one hand, the paradoxes of QM have convinced many defenders of orthodox QM to settle for an anti-realist understanding of the theory or, following the empiricists' dogma, to judge as meaningless questions concerning the quantum world prior to any measurements; others have been convinced that QM signals an epistemic limitation intrinsic of human being (Dorato, 2020). And the list could continue. On the other hand, $(SDM)_{CA}$ allows one to accept that questions like those above are meaningless and still retain a realist metaphysics of spin that consists of a unique determinable of spin that is always determinate even before any measurements. All of this, at the price of *removing* some metaphysical structure of the theory, viz. by refusing to consider operators of spin along a particular axis as properties.[22]

Before proceeding into our analysis of $(SDM)_{CA}$, let me clarify a small but relevant

[21] Note that in the passage quoted, Bohm is talking about the impossibility of interpreting realistically orthodox QM.

[22] This result is peculiar if one thinks, instead, that the usual strategies for crafting realist interpretations of QM consisted in modifying the formalism of textbook QM, as discussed in §4.

aspect of the view. Claiming that $\hat{S}_x$, $\hat{S}_z$, etc..., do not represent physical properties does not mean that they have to be removed from QM's formalism. They are part and parcel of how the spin Hilbert space is built, and they are essential from a mathematical and pragmatical[23] points of view. The mathematics of QM has does not have to be rewritten from scratch. Being a realist on a theory does not mean claiming that every part of its formalism is in a correspondence relation with 'some element of reality.' Rather, it means trying to understand *which* part of the formalism is in such a relation. Accordingly, $(\text{SDM})_{CA}$'s claim that $\hat{S}_x$, $\hat{S}_z$, etc..., do not represent physical properties just amount to the view that there are insufficient reasons to consider them more than mathematical features of spin Hilbert space. For once, these properties do not have an explanatory power greater than $\hat{\mathbf{S}} \cdot \mathbf{n}$ and the concrete directions along which a system's spin is aligned, i.e. spin's maximally specific determinates. The next subsection aims at arguing in greater detail for this claim.

7.5.2 Components antirealism and experiments

I am aware that prima facie, $(\text{SDM})_{CA}$ might seem implausible, to say the least, especially because I argued that physicists' observables correspond to metaphysicians determinable properties that can be measured. On the one hand, such a correspondence between observables and physical properties was assumed since QM's early formulations.[24] On the other, denying that $\hat{S}_x$, $\hat{S}_z$, etc..., are properties seems to imply that they are not different observables. A claim that few would seriously consider. This fact, in turn, raises the question about why is it part and parcel of the mainstream to consider $\hat{S}_x$, $\hat{S}_z$, etc..., as representing physical properties. The reason why is connected to these operators' empirical value: measuring the spin along an axis i ends up changing the measured system's state into an eigenstate of $\hat{S}_i$. Given (EEL), it is natural to assume that we performed a measurement of spin i and such property is represented by $\hat{S}_i$. Accordingly, to defend $(\text{SDM})_{CA}$, one must give a plausible answer to the following question:

> (Q_7) If S_x, $\hat{S}_z$, etc... do not represent properties, how can we explain spin measurements along different axes?

Such a question is pressing insofar as the credibility of its answer determines the credibility of $(\text{SDM})_{CA}$. To start, note that accepting $(\mathbf{SM_{CA}})$ does not entail a rejection of the (EEL). On the one hand, as I remarked above, (EEL) is silent on *which* operators must be understood as representing properties. Since the correspondence between operators and properties must be argued for on independent grounds, denying that S_x, $\hat{S}_z$, etc..., represent any property does not contradict (EEL). On the other hand, eigenstates of every spin operator along an axis i are also eigenstates of $\hat{\mathbf{S}} \cdot \mathbf{n}$. Therefore, after a spin measurement, the system is always in an eigenstate of such

[23] That is, to calculate the possible outcomes of measurement easily. On this, see §7.5.2 below.

[24] Cf. the discussion above on (EPR) (§7.5.1). On this point, see also (Heisenberg, 1958).

an operator. As such, one can endorse $(\text{SDM})_{CA}$ and retain the (EEL) unproblematically.

I argue now that if one looks at concrete cases of experimental practices, then the impression that spin measurements along different axes measure different properties is weakened. Rather, the experimental practice suggests that they are measurements of the direction on the polarization vector only. Consequently, spin measurements along different axes can be taken as measuring the maximally specific determinable represented by $\hat{\mathbf{S}} \cdot \mathbf{n}$, rather than determinables represented by $\hat{S_x}$, $\hat{S_z}$, and so on. Let me show why by considering two brief arguments.

The first one consists in analysing a very simple problem of quantum tomography. Suppose that Alice prepares a beam of electrons in the same spin state. Now, Bob wants to know what such a spin state is. Besides asking Alice directly, is there an experimental procedure that allows Bob to know in which direction is aligned the electrons' spin in the beam? Quantum tomography teaches Bob that he has to perform three spin measurements along mutually orthogonal directions. For example, Bob could start by orienting the Stern-Gerlach magnet along the spatial $z-$axis. By letting the beam pass through the Stern-Gerlach, Bob collects the following results: a certain fraction $|\alpha|^2$ of particles deviates upward, and thus they have now spin up along the $z-$axis, and a certain fraction $|\beta|^2$ deviates downward (spin z down). If Bob has the patience of measuring a sufficiently large number of electrons, he can reconstruct from $|\alpha|^2$ and $|\beta|^2$, the state of the electrons in the polarised beam written in the $z-$basis. Intuitively, he does so by 'reversing', so to speak, Born's rule.[25] The state Bob obtains is a superposition of the following form:

$$|\psi\rangle = \alpha\,|\uparrow\rangle_z + \beta\,|\downarrow\rangle_z \;, \tag{7.9}$$

where $|\alpha|^2 + |\beta|^2 = 1$. Bob can now compute the spin state along the $z-$axis by writing down $\mathbf{n}$ in polar coordinates:

$$\begin{pmatrix} |\uparrow\rangle_n \\ |\downarrow\rangle_n \end{pmatrix} = \begin{pmatrix} e^{-\frac{i\phi}{2}} \cos\left(\frac{\theta}{2}\right) & e^{\frac{i\phi}{2}} \sin\left(\frac{\theta}{2}\right) \\ e^{-\frac{i\phi}{2}} \sin\left(\frac{\theta}{2}\right) & -e^{\frac{i\phi}{2}} \cos\left(\frac{\theta}{2}\right) \end{pmatrix} \begin{pmatrix} |\uparrow\rangle_z \\ |\downarrow\rangle_z \end{pmatrix} \;. \tag{7.10}$$

From α (or β) of (7.9), Bob can determine a first angle that appears in (7.10), i.e. θ or ϕ, along which the spin polarisation vector is pointing. Now, Bob has to repeat the process above on two other axes, say, the x and y axes, that are mutually orthogonal to z. By letting the beam prepared by Alice passes through the Stern-Gerlach oriented along the x and y axis, Bob can fully reconstruct the initial spin state of the electrons in the beam. Indeed, The measure on the $x-$axis fixes one of the angles not fixed by the first measurement, i.e. θ or ϕ of (7.10). Finally, the measurement result along the $y-$axis allows Bob to infer whether the electron in the beam had spin up or spin down along the axis upon which they were aligned – reconstructed

[25] The operation is not devoid of experimental errors. The accuracy of the reconstructed state, shown in (7.9), is proportional to the number of measured electrons. Given the theoretical focus of the thesis, I assume here that the number of electrons tends to be infinite, thus minimizing any experimental error.

through the first two measurements.
In the case above, measurements of spin along different axes does nothing but fix the *angle* between **n** and the spatial axes upon which the Stern-Gerlach apparatuses are oriented. It seems straightforward to interpret the measurements above in terms of the polarisation vector's direction rather than the measurement of different spin properties. Friends of $(SDM)_{CA}$ have to accept that the example above is a paradigmatic case of what happens, more in general, when we perform a spin measurement along a particular axis. That is, we do not measure different spin determinables, but we measure the angle between the currently instantiated maximally specific determinates of spin – the concrete orientation of spin – and the magnetic field's direction. Sure, three different measures are necessary to gather enough *information* to reconstruct the initial spin of the system. And in doing so, we need to use operators S_x, $\hat{S}_z$, etc..., and their eigenvectors as the basis for describing spin states. However, one must not be led astray by these pragmatic and mathematical necessities and be convinced that we measure different properties.
Let me just provide a second argument in support of the idea that every spin measurement along an axis i just measures the angle between the polarisation vector and the i-axis. The spin-polarisation theorem guarantees that a free-single particle system's spin is always aligned along a particular direction, even before performing any measurement. Above, we saw how we could, through three experiments, reconstruct what such a direction was before the measurements. Now I argue that the polarisation vector, which represents the spatial direction along which spin is aligned, pictorially illustrates the intrinsically indeterministic evolution of the system during a spin measurement.
Consider an electron with spin up along a generic axis $\hat{n}$. Suppose the electron passes through a Stern-Gerlach aligned along the $x-$axis. As illustrated in fig. 7.8, the polarization vector and the x-axis form an angle α. The electron can align itself along x in two ways: it can rotate either over angle α, ending in state spin x up, or over angle β ending in state spin x down, where $\beta = \pi - \alpha$. The system's evolution is indeterministic because there are no physical reasons why the polarization vector should prefer one direction or the over. Even though it is intrinsically indeterministic how the system's spin will be aligned, a sort of 'principle of least action', if one will, seems to hold: the chance to rotate through the smaller angle – α, in fig. 7.8 – is higher than the chance of it rotating through β, the larger angle. This can be appreciated from a formal point of view as follows. To predict the probability that the spin vector rotates in one sense or another, one should write its state in the x-basis[26]:

$$|\uparrow\rangle_n = \cos\left(\frac{\alpha}{2}\right)|\uparrow\rangle_x + \sin\left(\frac{\alpha}{2}\right)|\downarrow\rangle_x \ . \tag{7.11}$$

When $\cos^2\left(\frac{\alpha}{2}\right) > \sin^2\left(\frac{\alpha}{2}\right)$, the angle alpha is $< \pi/2$. In this way, the result $|\uparrow\rangle_x$ is more likely than $|\downarrow\rangle_x$. If instead, $\alpha > \pi/2$, the opposite happens, viz. $|\downarrow\rangle_x$ is more

[26] More generally, the i-basis, where i is the axis along which the Stern-Gerlach magnetic field is aligned.

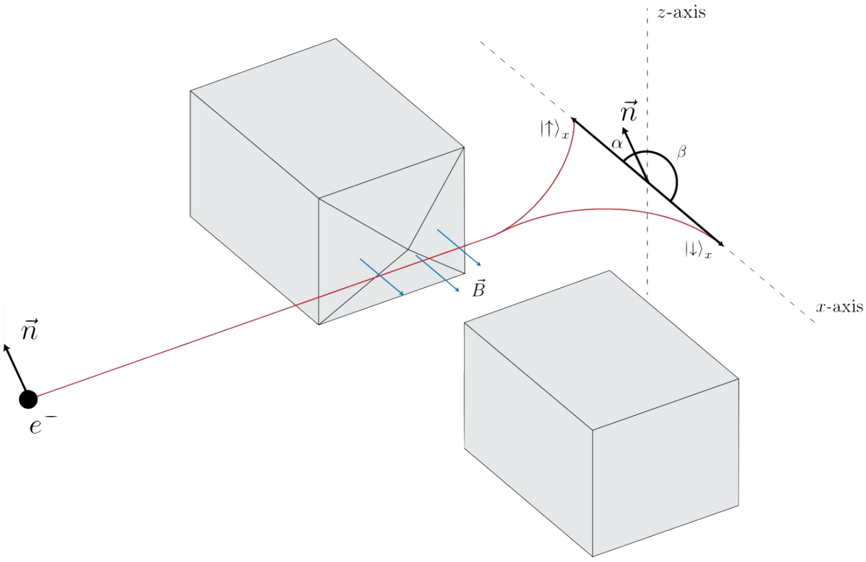

Fig. 7.8 Pictorial of a particle with polarization vector **n** passing through a Stern-Gerlach magnet aligned along the $x-$axis.

probable than $|\uparrow\rangle_x$. From the pictorial above (fig. 7.8), one can imagine that when the polarisation vector is more tilted towards the $+\hat{x}$ direction, then spin x up is more probable. Vice versa, when it is tilted toward $-\hat{x}$, it is more likely that the electron's spin will end up in state spin x down. When **n** is perpendicular to the magnetic field, both possible results are equally probable – as they should be.[27]

The equations presented in the arguments above are written on a specific spin basis. As argued above §7.5.2, operators S_x, $\hat{S}_z$, etc..., are essential mathematical elements of QM's formalism. However, the arguments above jointly taken shows that: when we measure spin along an axis, we are not measuring a property different from the spin property we measure on a different axis. This runs against the idea that rotating, say, a Stern-Gerlach apparatus, allows us to measure the different spin properties allegedly represented by S_x, $\hat{S}_z$, and so on. Rather, we measure the same spin property that changes its value according to how we rotate the Stern-Gerlach. The second argument shows that a simple Stern-Gerlach experiment can be explained in terms of the polarization vector only, in a one-to-one correspondence with maximally de-

[27] I assumed here that the evolution of the state of a system after an interaction with a Stern-Gerlach is instantaneous. This is in line with textbook QM's *collapse principle* (Wallace, 2019). As a matter of fact, we now have better explanations of quantum systems evolution, i.e. decoherence, and how spin interacts with magnetic fields, from quantum electrodynamics. I left these more complex theories out of the discussion since the focus of the thesis has been textbook QM. Note, however, that prima facie decoherence seems to strengthen the plausibility of the analysis above. Decoherence is a gradual process that could show the unitary evolution of the polarisation vector towards $\hat{x}$ or $-\hat{x}$.

terminates of spin. The concrete direction along which spin is aligned explains what happens when the system interacts with an inhomogeneous magnetic field, rather than a determinable property of 'having spin along the Stern-Gerlach axis.' That is, the Stern-Gerlach modifies the spin direction of the system by aligning it along an axis – the same axis along which the Stern-Gerlach magnetic field is aligned to – rather than performing, *strictu sensu*, a measurement of a property of having spin along that axis.

7.5.3 Evaluating the view

The fact that different directions of spin cannot have a simultaneous value is explained by $(SDM)_{CA}$ by renouncing to give a metaphysical import to operators S_x, $\hat{S}_z$, and so forth. However, we saw that the standard form of *Spin Determinable Monism* $(SDM)_S$, instead, consider them determinables of spin (§7.3; see fig.7.5). By itself, $(SDM)_{CA}$ does not say anything about what happens to other spin determinables when a system instantiates a determinate of one of them. Consider again the principle *Property Nonetheless* (PN) (Corti, 2021) discussed in §5.2:

> (PN): A quantum system always instantiates its observables (i.e. those properties that can be measured and represented mathematically through a Hermitian operator).

I argued that accepting this principle is crucial to argue that spin components are metaphysically indeterminate when they are superposed. Accordingly, if one accepts to understand quantum properties in terms of determinable and endorses (PN), then one ends up with Calosi and Wilson (2018)'s determinable-determinate account of MI. If instead, one rejects (PN), then one endorses Glick (2017)'s view according to which determinables are instantiated only if one of their determinates is.[28] I assume here that both Calosi and Wilson's understanding of quantum MI and Glick's sparse view accept the standard model of *Spin Determinable Monism*. On the one hand, they explicitly consider operators S_x, $\hat{S}_z$, etc..., representing determinables. On the other, since I argued above that (SDP) has to be abandoned in favour of (SDM), it would be uncharitable not to assume that they endorse the latter.
I argue now that $(SDM)_{CA}$ as a model of spin superposition is better than both the determinable-determinate account of MI and the sparse view. From a general point of view, it has a straightforward advantage over each of them. $(SDM)_{CA}$ retains the intuition that underlies the Sparse view without ending up with the inexplicable conclusion that determinables are miraculously created when measurements are performed. Such a conclusion, it has been argued, is a highly problematic feature of Glick (2017)'s view (Calosi and Wilson, 2021). Furthermore, the sparse view cannot account for the probabilities of spin measurement. Since the spin z property is

[28] A third view is the one proposed in (Funkhouser, 2006), according to which determinates are instantiated at a given probability. The view has been heavily criticised by Wolff (2015), and I do not discuss it in what follows.

not instantiated when the system has a definite state of, say, spin x, the fact that the outcomes of a spin z measurement have some probabilities – rather than others – is inexplicable from a metaphysical point of view. $(SDM)_{CA}$ and Calosi and Wilson (2018), instead, explain these probabilities. According to the former, it depends on the angles between the orientation of the spin polarisation vector and the axis along which spin is measured. According to the *glutty* variant of the latter, it is explained by the fact that the determinates of spin z are instantiated at a given degree different from one. The degrees of instantiation are crucially connected to the probabilities of aligning toward a direction rather than the other after a measurement. However, $(SDM)_{CA}$ allows a realist understanding of spin without relying on any form of metaphysical indeterminacy, which is, in itself, a highly controversial concept. That the resulting ontology of $(SDM)_{CA}$ is always determinate is a straightforward advantage that the view has over the determinable-determinate account of MI. All things being equal, there is no reason to prefer the radical metaphysics suggested by friends of MI over the perfectly classical and always determined portrayed by $(SDM)_{CA}$.[29] Setting aside these clear advantages, I think there are three main broad reasons why $(SDM)_{CA}$ has to be considered a serious competitor of Glick (2017)'s and Calosi and Wilson (2018)'s accounts of spin superpositions: it has explanatory power, it is – from a metaphysical point of view – ecumenic and it does not involve arbitrary properties.

$(SDM)_{CA}$ *is explanatory.* In evaluating the idea that spin components are metaphysically indeterminate, Wolff (2015)makes the following remark:

> What is more puzzling, and not fully explained by the model at hand [that spin components are metaphysically indeterminate], is the fact that different determinables should be so related as to make joint determination impossible. Why is it that a full determination of spin value in a particular direction precludes simultaneous determination in a different direction? Once again, it's not the fault of the model that this is so, but it shows that even if the model successfully captures certain features of spin, it does not provide a much better understanding of them. (Wolff, 2015, p. 385)

Wolff's raises in the quotation above an interesting point. Suppose we have an electron in a definite state of spin x up. According to Calosi and Wilson (2018)'s view, the spin z, say, values of the electron are metaphysically indeterminate. Their account explains the indeterminacy arising from superposition in terms of the electron instantiating the determinable of 'havin spin z' and, at the same time, either both its determinates (*glutty* MI) or none of them (*gappy* MI). Their account explains what is superposition from a metaphysical point of view. However, it is silent on *why* these properties are such that metaphysical indeterminacy arises. That is, they are silent on why spin components are incompatible observables where classical properties, mass and charge, say, are not. Perhaps friends of Calosi and Wilson (2018)'s account

[29] Yet, this might be taken to be the very disadvantage of $(SDM)_{CA}$. Such a view offers a straightforwardly classical metaphysics of spin, that is clearly incompatible with everything we thought to have learned from QM. However, remember that $(SDM)_{CA}$ is limited to free-single particle systems only. As such, it does not present an account of entangled spin states. As such, it might be that the metaphysics of entanglement is the very 'follies' of QM, rather than superposition.

can explain it in terms of metaphysical relations between these determinables. However, no such analysis has been presented yet. So there is not much I can add on top of it. Prima facie, they seem to consider it a brute fact: it just so happens that spin components cannot be jointly determinate.
Arguably, Glick (2017)'s sparse view cannot but take it as a brute fact as well. Insofar as the determinable of 'having spin z' is not instantiated when the system is in a definite spin x state, there cannot be any relation between these determinables. One of them, indeed, is not instantiated when the other is.
The only account that has an explanation of this fact is $(\text{SDM})_{CA}$. We already saw such an explanation: the very fact that they are the immediate determinates of the same determinable prevents them from being jointly determinate. It is part and parcel of $(\text{SDM})_{CA}$ that there is nothing mysterious in the fact that spin cannot be jointly determined along different directions because such a phenomenon is no different from the fact that something that has a mass of 3kg cannot have at the same time a mass of, say, 5kg.

$(\text{SDM})_{CA}$ *is ecumenical.* Where Calosi and Wilson (2018)'s and Glick (2017)'s accounts have to engage on controversial features of the determinable-determinate relation, $(\text{SDM})_{CA}$ is completely neutral. If one thinks that ecumenic views have to be preferred, all things equal, then one should conclude that $(\text{SDM})_{CA}$ is better – in this regard – to its rivals. Let me just briefly show three controversial aspects of the determinable-determinate relation on which friends of MI and the sparse view have to engage.
First, and quite obviously, they have to defend or criticise (PN). In the literature on determinables and determinates, there is no consensus on this issue. Some argue that determinables' existence does not depend on their determinates being instantiated (Wilson, 2012), while others argue that only determinates exist (Gillett and Rives, 2005). Furthermore, Calosi and Wilson (2018)'s account is based on a revision of some of the standards axioms through which the determinable-determinate relation is regimented.
Second, there is a huge debate whether determinables can be understood just as a disjunction of their determinates. Such a view is called 'disjunctivism,' and it is radically controversial both in metaphysics Wilson (2017a) and in the metaphysics of QM (Torza, 2021; Calosi and Wilson, 2021). Since it is part and parcel of Calosi and Wilson (2018)'s account that a determinable might be instantiated even if none of its determinates is, friends of this account must reject disjunctivism. The reason is that if a determinables D is nothing but a disjunction of the form 'instantiating either d_1, or d_2, $\ldots$ or d_n', then from the fact that an entity does not instantiate any d_n it trivially follows that such an entity does not instantiate D. On the other side of the debate, accepting a disjunctivist account of determinables and determinates gives the friends of the sparse view a straightforward motivation for rejecting (PN).
Now, in an influential and controversial paper, Rosen (2010) proposes the following criterion to distinguish disjunctive from non-disjunctive properties: one needs to know all the disjuncts of a disjunctive property for grasping the concept of such property; however, there is no need to know every determinate of a determinable for

understanding what it does mean instantiating the latter. The example he provides is the following: one knows what blue is – it can correctly distinguish, say, objects of different shades of blue from red and green objects – even though one has never seen a cerulean object. In contrast, take the disjunctive property 'being blue or red.' Someone that has never seen a red object in their life would not grasp such a property on the basis that they had seen blue objects. If we apply Rosen's criterion to spin, one clearly sees that the determinable of 'having spin' is understood even if one is not able to write down every possible axis along which spin can be aligned, insofar as they are infinite. One has the concept of spin even though, say, it has never measured it on the y-axis.

In contrast, suppose that a scientist – who knows nothing about QM – performs a spin measurement along the x-axis and, unlucky them, every particle they measure ends up in state spin up. It is really counterintuitive to say that he knows what 'having spin x' means if they only grasp the concept of 'having spin x *up*.' As such, the properties represented by $\hat{S}_x$, $\hat{S}_z$, ..., seem more disjunctive properties than determinables according to Rosen's criterion. Surely, this is an argument based on intuition and a controversial criterion. And it is part and parcel of Calosi and Wilson (2018)'s account that these properties are not disjunctives. However, it is up to them to propose an alternative criterion for distinguishing determinables from disjunctive properties and show that 'having spin x', 'having spin z', and so on, are determinables.

Finally, we saw that one of the axioms of the determinable-determinate relation is (Determinate similarities), according to which determinates of a given determinable must be similar. The maximally specific determinates of spin are similar and comparable because they are precise ways in which a particle can have spin. The views that accept that $\hat{S}_x$, $\hat{S}_z$, ..., represent some determinables have thus to explain in what sense, say, spin up x is more similar to spin x down than to spin z up. The straightforward answer is that the vectors representing them are aligned along the same axis, and they are the possible results of a single measurement. However, if one accepts that the maximally determinable of spin is represented by $\hat{\mathbf{S}} \cdot \mathbf{n}$, one cannot see such an explanation as arbitrary. The polarisation vector $\mathbf{n}$, which is a vector in ordinary three-dimensional space, is the very connection between this space and the spin Hilbert space. As argued in (§7.4.2), any maximally specific determinates of spin correspond to the concrete orientations of the polarisation vectors. From the point of view of the polarisation vector, pairing two aligned vectors is no better than pairing, say, vectors rotated by 90 degrees clockwise. As such, there is no privileged way of pairing these maximally specific determinates in the three-dimensional space. 'Having spin up along the $x-$axis' is thus not more similar to 'having spin down along the $x-$axis' than it is to, say, 'having spin up along the $z-$axis.' If one defends the view that $\hat{S}_x$, $\hat{S}_z$, ..., represent some determinable properties have to show that the contrary is the case.

The considerations above are not straightforward arguments for preferring $(\mathrm{SDM})_{CA}$ over Calosi and Wilson (2018)'s and Glick (2017)'s accounts. All the points above are admittedly weak if taken independently. However, jointly taken, they show that friends of $(\mathrm{SDM})_{CA}$ are exempt from engaging in defending controversial aspects of the determinable-determinate relation. On the other side of the debate, supporters of both MI and the sparse view have to take and defend a particular stance over them. From the perspective of a global evaluation of the views on the table, ecumenicity arguably plays an important role.

(SDM)$_{CA}$ *has no arbitrary, redundant an non-invariant properties.* I argue above that the way in which the (alleged) determinates of $\hat{S}_x$,$\hat{S}_z$, etc..., are paired is arbitrary from the perspective of the polarisation vector. Thus, considering them as determinables amounts to defending the idea that an arbitrary way of pairing them is privileged. However, these determinables are not only arbitrary, but they are also *redundant* and *non-invariant*. These features, arguably, suggest that they have not to be understood as physical properties. That is, if one agrees that adding redundant and arbitrary metaphysical structures – mathematically represented by non-invariant (mathematical) objects – to a theory should be avoided at all costs. In turn, this favours (SDM)$_{CA}$ over Calosi and Wilson (2018)'s and Glick (2017)'s accounts.
I argued above that these operators play a metaphysically redundant role. As §7.5.2 showed, they do not explain anything more than what could be explained in terms of the maximally unspecific determinable of spin and its maximally specific determinates. This is also because these operators are non-invariant descriptions of spin. As we saw indeed, $\hat{\mathbf{S}} \cdot \mathbf{n}$, $\mathbf{n}$ and the spin state connected to the latter are the only mathematical entities that are invariant under a change of basis. In the philosophy of physics, it is commonly assumed that the fundamental metaphysical content of a scientific theory must be read off from its mathematical invariant structures.[30] Without giving a full-fledged defence of it, I limit myself to putting forward a reason in favour of its plausibility. Consider the well-known example of a ship moving with constant velocity with an experimenter closed in its cargo so that no experiment allows the experimenter to understand whether the ship is moving or standing still. This has been customarily taken in the physical and philosophical literature as motivation for rejecting absolute velocities as fundamental properties in classical mechanics. Formally, this corresponds to accepting the Galilean group as the group of global symmetries of space-time. Assigning reality to the invariants of the Galilean group has been taken as a paradigmatic example so that this interpretation has been extended to all the global symmetries and, I contend, should also be extended to the group of spin representations ($SU(2)$).[31]
One of the tenets of naturalised metaphysics is avoiding adding unnecessary metaphysical structures to scientific theories. Following such an approach to metaphysics defended in §2, I take for granted that the criteria for property individuation must be drawn from scientific laws and practices. Hence, one should not accept in the ontology of a theory any property which does not play an explanatory role (not played yet by another entity). Adding metaphysical structures to some scientific theory is always possible; but it is reasonable only if it is motivated in the first place by the scientific theory under consideration. That only invariant mathematical parts of the

[30] See, e.g., van Fraassen (1989).

[31] That only invariants have a metaphysical weight is quite mainstream, e.g., by friends of ontic structural realism (Roberts, 2011; McKenzie, 2014). See (Morganti, 2018; **?**) for a discussion.

formalism have to be understood ontologically was an implicit premise of the arguments I presented in §7.4. For instance, the argument from the spin state invariance to *Spin Determinable Monism* (§7.4.1) crucially relies on the fact that if a change in the mathematical description does not entail a change in the physics of the system, then the mathematical descriptions above are not metaphysically significant. If one embraces such a meta-metaphysical principle coherently through the end, considering as metaphysically fundamental invariants only, the principle delivers the result that $\hat{S}_x$, $\hat{S}_z$ and the like must not be considered as representing any physical property. Surely, one could contend that invariant structures may be as real as non-invariant ones and that the only difference between the two is a question of fundamentality.[32] However, to do so, one should also provide compelling arguments and serious motivations. Even if I understand why in some theories one would want to give some derivative reality to some non-invariant structures, it seems problematic to argue that $\hat{S}_x$, $\hat{S}_z$, etc..., somehow "emerge" from a more fundamental property. The invariant character of **n** suggests that the maximally specific determinates of spin – individuated, indeed, by **n** – suggests that they must be more fundamental than the determinables represented by $\hat{S}_x$, $\hat{S}_z$, and so on. This is prima facie problematic for friends of Calosi and Wilson (2018)'s account. The straightforward explanation of the derivativeness of the properties represented by these operators would be in terms of the spin maximally specific determinates. However, it is part and parcel of their account that determinables are not metaphysically derivative over their determinates (Wilson, 2012). As such, the non-fundamental character of the determinables represented by the operators above is devoid of its most plausible answer.

I take the considerations above jointly taken to show that $(\text{SDM})_{CA}$ is a serious contender among the accounts of spin superposition. This account is, admittedly, quite radical. However, it also presents some relevant advantages over Calosi and Wilson (2018)'s and Glick (2017)'s accounts.

[32] Frame of references in relativity are a paradigmatic example; see: Lipman (2020). For arguments in favour of giving ontological weight preferably to invariants only in relativity, see: Gilmore et al. (2016).

Chapter 8
Conclusion

The first part of the thesis concerned the foundations of scientific metaphysics, one of the leading contemporary meta-metaphysical approaches to metaphysics. This part aimed at evaluating this approach by confronting it to other common conceptions of it. The first chapter discussed three of the foremost widespread approaches to contemporary metaphysics, highlighting their virtues and drawbacks. The acceptance of three theses unites these approaches: (i) metaphysical enquiry is conducted a priori, (ii) metaphysics is more fundamental than empirical sciences (either epistemically or ontologically), and, as a consequence, (iii) metaphysics is independent of other branches of knowledge. In contrast, scientific metaphysics' core is that metaphysical investigations have as starting point, and indirect testing ground, our current best scientific theories. One of the main motivations is that a priori investigations lack a secure epistemic ground that a posteriori enquiries – typical of empirical sciences – have. As such, if one aims at giving metaphysics some epistemic ground, then one has connect metaphysics to empirical sciences. The upshot is that if metaphysics has to provide us with *knowledge* about reality, our best scientific theories are the most reliable source of metaphysical information.

I outlined my own by discussing different accounts of scientific metaphysics presented in the literature. The core of my approach to scientific metaphysics is that none of (i)-(iii) has to be accepted. Rather, metaphysics has to be understood as part and parcel of the scientific effort of understanding reality. At the same time, my account of scientific metaphysics includes what I highlighted as virtues of the approach above: a reliable meta-ontological criterion, the value of a priori metaphysical models and the necessity of the dialogue with empirical sciences. Furthermore, I argued that metaphysicians must inherit important methodological traits from empirical sciences. In particular, metaphysicians have to accept their limited epistemic status, thus accepting that their models are as fallible as current scientific theories; metaphysical investigations must start from simple assumptions and increasingly generalise to more complex systems and, relatedly, metaphysical conclusions are limited to the case under study – rather than being fully general.

By proposing my account of scientific metaphysics, I showed that such an approach is problematic vis-à-vis one of its central assumptions. Scientific realism is the view that our best scientific theories are (approximately) true descriptions of the actual world. Such a view must be assumed to motivate why metaphysics' scope has to be limited to our best scientific theories. However, standard presentations of scientific realism include two metaphysical theses – metaphysical realism and the correspondence theory of truth – that are not motivated by our best scientific theories. Given scientific metaphysics' dictum that metaphysics is epistemically sound if it is motivated by our best science, the whole project of doing scientific metaphysics seems bound to fail before starting. In the thesis, I outlined two ways to resolve this issue, and I explored what I consider the most interesting one. In the second chapter, I presented a new account of scientific realism devoid of the problematic theses above. The core of the view I presented, dubbed *Scientific yet quiet realism*, is that metaphysics and empirical sciences aim at proposing the *best* model of reality possible at a given time. However, whether our models really 'carve Nature at its joints' is something behind human knowledge. As such, *Scientific yet quiet realism* is an appealing account of scientific realism for those naturalised metaphysicians that want to avoid the tension above. In the chapter, I limited myself by presenting the view and defending its coherence. However, further substantial work must be done to assess whether it is better than other forms of scientific realism and whether it can address the standard arguments in the antirealists' arsenal.

The second part of the thesis has been a concrete example of the methodology outlined in the first part. The third chapter outlined the thesis scope: the property of spin in non-relativistic QM. In particular, I motivated an interpretation-neutral approach to the ontology of QM and my choice of focussing on the spin. Spin is, indeed, one of the central concepts of contemporary science, and it has deep implications for many scientific branches other than physics and uncountable technological applications. Furthermore, a recent debate rose on whether scientific realists should be committed to the existence of spin. I directly engaged in the debate by arguing that the argument against spin realism is unsound. As such, contemporary scientific realists ought to commit to spin. Spin is so central in modern science that most of the physics we know should be rebuilt from scratches if it were not real. Furthermore, I argue that a scientific realist's commitment to some entity's existence cannot amount just to the claim that such an entity exists. Since metaphysics and science are part and parcel of the same project of understanding reality, committing to the existence of spin entails proposing metaphysical models of it, informed by our best science.

The rest of the thesis has aimed to investigate possible metaphysical models of spin. My starting point is the recent controversy on metaphysical indeterminacy. The idea that indeterminacy is *metaphysical*, that is, that it concerns how the world is rather than our knowledge or representations of it, has been controversial for ages. However, in the recent literature, many philosophers proposed precise metaphysical models of such a phenomenon. Furthermore, they argued that QM could offer a genuine example of metaphysical indeterminacy, individuating a quantum system in a spin superposition as a paradigmatic example. The first aim of chapter four has been investigating the necessary assumptions that must be endorsed to derive such a claim. These assumptions are often implicit in the debate over quantum MI. Making them explicit is, by itself, a positive contribution to the literature.

After having surveyed the necessary assumptions for arguing that there is quantum indeterminacy in the first place, I turned to the accounts of metaphysical indeterminacy currently available in the literature. The models of metaphysical indeterminacy available in the literature can be divided into two families: meta-level and object-level accounts. According to the former family, metaphysical indeterminacy must be understood in terms of the world being unsettled between *determinate* states of affairs. For example, in the case of spin superposition, the indeterminacy of spin is understood in terms of spin up and spin down being possible descriptions of the actual world. However, even if these descriptions do not determinately misrepresent the actual world, they are both neither true nor false descriptions of it. In other terms, metaphysical indeterminacy is understood in terms of a multiplicity of possible definite descriptions of the world such that none of them is true nor false. Object-level accounts, instead, understand metaphysical indeterminacy as being a state of affairs irreducible to determinate ones. One of the leading accounts of this family of approaches is the determinable-determinate account proposed by Jessica Wilson. According to her account, an entity is metaphysically indeterminate if it instantiates a determinable property, 'having spin x', say, but more than one (glutty MI) or none (gappy MI) of its determinates, i.e. 'having spin x up' and 'having spin x down.'

It has been argued that the champion of the meta-level approaches, i.e. Barnes and Williams (2011)'s view, fails in accounting for quantum indeterminacy. In chapter four, I proposed a simpler reformulation of the argument by means of the Gleason theorem. Since Barnes and Williams' possible worlds through which indeterminacy is accounted for are maximal, they require that every property of a quantum system must have a definite value at every possible world. However, the Gleason theorem shows at least one case in which that is not possible: the values of spin components of a spin-1 particle.

The failure of Barnes and Williams' account has been taken by many as a challenge to develop meta-level accounts able to deal with quantum cases. The aim of chapter 5 has been that of arguing that meta-level accounts are bound to fail if they understand quantum indeterminacy in terms of definite states of affairs. To show this, I took Darby and Pickup's situation semantics account of metaphysical indeterminacy as an example. In the chapter, I proposed an argument against their view. After having considered several ways in which friends of meta-level approaches might resist the argument, I put forward a more sophisticated version of the argument above. The upshot of this version is that the eigenstate-eigenvalue link connects the metaphysical descriptions of quantum systems, in terms of properties instantiated, to the mathematical ones in terms of operators and eigenvalues. The mathematical description of a quantum system is *never* indeterminate: even the states that are (allegedly) responsible for the rise of quantum indeterminacy are definite states in the Hilbert space. However, when there is metaphysical indeterminacy, friends of meta-level accounts claim that the metaphysical description of the quantum system is indeterminate. Since, according to the eigenstate-eigenvalue link, these descriptions are equivalent, they are either committed to the (implausible) view that mathematics is indeterminate or to rejecting the eigenstate-eigenvalue link. However, the latter is necessary for claiming that textbook quantum mechanics offers a genuine example of metaphysical indeterminacy. Thus, meta-level approaches that analyse indeterminacy as above fails to model quantum phenomena.

Even if I focused on Darby and Pickup's account in the chapter, the argument above could be trivially extended to any meta-level approaches that understand indeterminacy in terms of 'worldly indecision' between determinate states of affairs. However, one meta-level account of indeterminacy proposes an alternative understanding: the one presented by Torza. In his view, indeterminacy has to be understood as an absence of determinate descriptions of reality rather than a multiplicity as above. Whether my argument can be extended to Torza's view, and if, more generally, there are further arguments against this family of accounts, has yet to be investigated.

After having reached a negative result, that is, what indeterminacy is not, I turned in the last chapter to a positive characterisation of spin. The aim has been to continue Wolff's investigations of spin in terms of determinables and determinates property. In particular, I argued that the eigenstate-eigenvalue link could be naturally interpreted as suggesting that operators represent determinables where their eigenvalues represent their properties. Such an analysis of the eigenstate-eigenvalue link in terms of determinable-determinate relations allowed me to present an exhaustive taxonomy of metaphysical models of spin as a property. In particular, I argue that there are two families of models, I dubbed respectively, *Spin Determinable Monism* and *Spin Determinable Pluralism*. These families share the view that the maximally specific determinates of spin are the concrete orientations of spin along different axes. However, they crucially disagree on the maximally unspecific determinables – i.e. properties that are not determinates of some determinables. According to *Spin Determinable Monism*, there is a unique spin determinable that is the property of 'having spin.' On the other side of the taxonomy, models of *Spin Determinable Pluralism* claim that the properties of having spin along different axes are the maximally unspecific spin determinable. In other words, according to the former family, spin has to be modelled as a single chain of determinable-determinate relations whereas, according to the latter, as an infinite number of these chains. Since no detailed investigation on spin as a determinable has been proposed in the literature, I argued that there is no evidence on whether one of these families must be considered mainstream. However, by proposing a small review of the literature in physics, philosophy of QM and metaphysics, I argued that considering the operators of spin along different axes is the mainstream view. Accordingly, I proposed what I take to be the standard models within the two families.

The next step of the investigations has been to argue that *Spin Determinable Monism* has to be preferred over *Spin Determinable Pluralism*. In particular, I presented two arguments that show that the formalism of QM strongly suggests the former family. Albeit one of the arguments relies on some controversial assumptions explicitly rejected in the thesis, the other is fully general. Finally, the taxonomy presented allowed me to introduce a new model within the monistic camp that I dubbed *Components Antirealism Spin Determinable Monism*. The core of this view is that operators representing spin along different axes do not correspond to any determinable property, contra the received view. Rather, spin has a unique maximally unspecific determinable – 'having spin' – and infinite maximally specific determinates – the concrete spatial spin orientation, but no in-between determinable like 'having spin *x*', 'having spin *z*, and so on.

The view is admittedly radical exactly because it denies what is unanimously accepted in the literature. However, I argued that the view has many advantages and it is worth considering, especially when compared with its main rivals. The core of this view is that spin of a free-single particle system in a pure state is always determinate even before any measuring is performed, denying that superposition presents a genuine case of indeterminacy. The impossibility of understanding realistically phenomena such as spin superpositions have been considered for ages as the litmus test of the impossibility of understanding quantum mechanics in realist terms. As a matter of fact, the standard strategy for crafting realist interpretations of the theory has been modifying the formalism to 'wash away', so to speak, the indeterminacy above. In contrast, *Components Antirealism Spin Determinable Monism* restores a determinate realist ontology by refusing to give a metaphysical weight to some operator. Such an account is interesting in and on itself. First, it suggests a different interpretation of spin incompatible observables: the impossibility of jointly determining the values of these observables' values is not due to obscure relations between them; rather, it is due to them not being determinable properties. Second, it suggests a general strategy of interpreting incompatible observables different from spin, such as position and momentum in quantum mechanics, coordinates in non-commutative geometries and observables in quantum field theory. Third, the ontology of spin suggested by the account is fully classical and, prima facie seems to be adequate to explain simple spin measurement.

The relevance of the account is further appreciated when it is evaluated in contrast to its main rivals. In the literature up to date, several accounts of superpositions have been presented. In the chapter, I compared *Components Antirealism Spin Determinable Monism* to Wilson's determinable-determinate account of metaphysical indeterminacy and Glick's sparse view. I argued that there are metaphysical and meta-metaphysical reasons to prefer the former to the latter. As such, not only the view I presented is interesting in itself as a possible metaphysical model of spin, but it is also a serious contender in the debate on whether spin superposition offers a genuine example of metaphysical indeterminacy.

The conclusion above is, admittedly, limited to the assumptions endorsed in the whole thesis. In particular, following the methodological criterion settled in the first chapter, I focused on a narrow aspect of the theory, viz., the spin, and took many simplistic assumptions. For instance, I assumed that spin is a property without assuming any other particulars on the rest of the ontology, e.g. whether individual particles instantiate it or if properties are the fundamentalia of quantum ontology. However, appreciating these limitations shows, in turn, the possible future investigations opened by the work done in the thesis. First, I focused on spin only, whereas Wilson's and Glick's accounts are supposed to be about *every* quantum observable. As such, one might argue that if, say, position and momentum have to be accounted for in terms of one of their view, then it is less costly to prefer their account of spin to my own. Consequently, investigating whether the strategy of interpreting incompatible observables suggested in my work can be applied to other observables as well is crucial from a general philosophical evaluation of the views on the table. Relatedly, my account is limited to single free particles only. As such, it is silent on what is the metaphysics of spin when quantum systems are entangled. Whether entanglement present a genuine case of metaphysical indeterminacy depends, in turn, on what is the rest of the ontology. For example, if one assumes that quantum mechanics ontology consists of individual particles, then arguing that their entanglement entails some form of metaphysical indeterminacy is straightforward. If one considers two entangled electrons, their states are not eigenstates of any spin operators along a particular direction. However, it is a physically significant fact that this composite system has, say, total spin-1. Now, the very fact that it has spin-1 is explained by the fact that its subsystems have both spin-$\frac{1}{2}$. As I argued, the spin number and the spin components must not be understood as separate determinables. As such, from the fact that the two entangled electrons have a definite spin number but no concrete orientation of spin, one derives that spin's orientation is, in this case, metaphysically indeterminate. On the other side of the debate, if one accepts a structuralist or holist ontology, then the composite system is not made of *particles*. As such, there is no resulting indeterminacy.

To sum up, the simplistic assumptions I took in the thesis supported the investigation up of single-particle systems. To assess whether there is indeterminacy in more complex cases, one has to present further pieces of the ontology. Whether and how *Components Antirealism Spin Determinable Monism* can be combined to general metaphysics of quantum mechanics is, then, crucial to argue that it has to be prefered to its rivals as an account of spin superposition.

Finally, I focus only on the non-relativistic formulation of quantum mechanics. As mentioned, such a theory is less fundamental than its field relativistic variant. A natural continuation of the investigation of the thesis is to check whether the account can be framed in the context of quantum field theory. This is so natural, especially because I argued in the first chapter that doing metaphysics of 'false' theories has the value of providing us clues on the metaphysics of more fundamental ones. As such, framing *Components Antirealism Spin Determinable Monism* is really an obvious continuation of the work conducted here.

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