Realism and Intertheory Relationships: Interstructuralism, Closed Theories and the Quantum-Classical Limit

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Abstract

Today there is no agreement on which, if any, of the several known realist interpretations of quantum mechanics is the correct one, and disagreement on this matter is not merely verbal, but substantial. However, I will show that the interpretations share a common aim: to recover the classical world given that it is quantum. This aim responds to implicit and well-entrenched philosophical intuitions that can be phrased in terms of intertheory relations, traditionally involving theory reduction, in philosophy, and the quantum-classical limit, in the foundations of physics. However, not one of those notions is free from controversies, and many contest that there is a smooth transition from the quantum to the classical. Hence, the account of the relationship between quantum mechanics and classical mechanics is an unresolved problem, and the philosophical character of the underlying framework can be contested. This thesis will offer a critical analysis of current well-known realist interpretations and will also put forward an alternative framework. I will critically examine traditional views on intertheory relations and the recent view of interstructuralism. My main claim will be that the role of intertheory relations is overrated, because a more basic question has to be answered first: 'what is a quantum system?'. That will motivate my novel view. I will critically evaluate and defend an alternative view based on the philosophy of Werner Heisenberg. The view proposes that physical theories should be regarded as "closed" systems. This has immediate implications for how we should understand intertheory relations in general, as well as scientific realism in particular. My view will appear radical in comparison to traditional views of quantum mechanics. Yet, I will examine fruitful comparison with forms of realism such as perspectivism and metaphysical pluralism. I shall conclude with indications for future work.

Contents

Abbreviations

BM	Bohmian mechanics
CM	Classical mechanics
DN	Deductive nomological
EBK	Einstein Brillouin Keller
GCP	General correspondence principle
GRW	Ghirardi-Rimini-Weber interpretation
MIs	Modal interpretations
MWI	Many worlds interpretation
PR	Perspectival realism
QM	Quantum mechanics
SP	Scientific perspectivism
SR	Structural realism
STR	Special theory of relativity
WKB	Wentzel Kramers Brillouin

Chapter 1

Introduction

This chapter sets out the nature, purpose and motivations of this investigation, and describes and discusses the content and contributions of the subsequent chapters.

This dissertation pertains to the philosophy of physics. More specifically, it focuses on the realist interpretation of quantum mechanics (QM). QM was discovered in the twentieth century and it is empirically accurate. Yet, as of today, crucial philosophical questions about it remain unresolved. In particular, a realist interpretation of QM ought to answer at least the two following questions:

1. *What is a quantum system?*
2. *How does QM relate to classical mechanics (CM)?*

The purpose of this thesis is to analyse and critically engage with the philosophical approaches to these two questions. My principal argument will be that the nature of the relationship between QM and CM and the role it is expected to play in the realist interpretation of QM have been mistaken. As a positive proposal, I will argue that Werner Heisenberg's Closed Theories view may offer us a new perspective on a number of issues, including what kind of realist stance we should adopt towards QM. The Closed Theories view will appear radical by contrast to more traditional ones, and my aim is to convince the reader that my framework might open a novel way to engage the realist interpretation of QM and the relationship of QM with CM.

Today there is no agreement on which, if any, of the several known realist interpretations of QM is the correct one, and disagreement on this matter is not merely verbal, but substantial. If taken seriously, these views each represent mutually incompatible worldviews, such as the existence of many worlds in a quantum superposition, particles with trajectories in a configuration space and spontaneous collapses, which conflict.[1] Furthermore, I will argue that the known realist interpretations carry with them an in-built view on intertheory relations that shapes their stance; and, further, that there is enough common ground across the interpretations to recognise an underlying general framework. By identifying that common framework, I motivate a novel view based on Heisenberg's philosophy.

Therefore, I will examine relevant interpretations in order to explore their underlying interpretative strategy: how do they approach, at a 'meta' level, the interpretation of QM? Known realist interpretations do not ignore that the central question is question 1. mentioned above. However, I will argue that in practice the interpretations focus on a question of intertheory relations – question 2 – in order to respond question 1. That is, I will intend to show that their intention is to answer question 1 by recovering the classical world given that it is quantum. The interpretations include a rough and preconceived idea of how QM and CM relate, and from that they attempt to flesh out the real content of QM. I will argue that this strategy is common to many interpretations and deserves the name 'Received View of the Realist Interpretation of QM'. I will challenge this strategy and I will propose to reverse it. I will defend that, in essence, realism suggests the more reasonable order: first tackle question 1 and then tackle question 2. This follows from what I will call Core Realism. Otherwise, how could we explain the relationship between QM and CM without having a clear account of the real content of QM? I will articulate a view of physical theories that follows from Core Realism to interpret QM. In addition, I will supplement it with a view on physical theories based on the Closed Theories view initiated by Heisenberg.

Hence, in order to assess my working hypothesis of the existence of a Received View, I will critically examine the main interpretations of QM. After that, I will critically engage with views on intertheory relations in order to explore how the specific and complex problem of the relationship between QM and CM is considered by the literature. The QM-CM relation occupies a crucial part in the fields of both philosophy and physics. From both sides the traditional view is that QM is more fundamental and universal than CM. Therefore, the former is expected to give an account of the latter, *somehow*. On the philosophy side, the variety of views differ in how they cash out that *somehow*. Predominantly this involves the notion of theory reduction. Indeed, theory reduction is an established field in philosophy. The debate around reduction is in the assessment of its epistemic and ontological implications. I will examine traditional views relevant for my purposes.

[1] Although some may advocate that different interpretations can be seen as part of one project, the various interpretations are distinct, traditionally.

I will examine the traditional view of theory reduction developed by Ernst Nagel (1979), the view that Thomas Nickles (1973) specifically designed to apply to physics and the heuristic view proposed by Heinz Post (1971). Finally, I will also analyse the notion of emergence as a view of intertheory relations. Emergence has received a lot of attention recently, provoking various sub-fields of research. However, I will provide reasons for not engaging critically with emergence.

In turn, the problem of the QM-CM relation appears in physics as the quantum-classical limit (QM-CM limit). The QM-CM limit itself is a technical term that requires clarification and it is not free of controversies. I will discuss how relevant scholars interpret the QM-CM limit. Furthermore, I will critically engage with the main theoretical devices in which physics articulates the account of the QM-CM limit. They are the mathematical limits, Ehrenfest theorem, Moyal brackets and, more predominantly, the theory of decoherence. However, I will conclude that the QM-CM relation remains an unresolved problem.

Relevantly, that the account of the relationship QM-CM is problematic is not merely an academic assessment. Indeed, recent and relevant developments in physics cannot be accommodated by the traditional view. In particular, the notion that CM can be explained by QM fails to account for phenomena in the field of quantum chaos or modern semiclassical mechanics. This difficulty has motivated Alisa Bokulich (2008a) to develop a novel philosophical account of the relation QM-CM: interstructuralism. The consequences of a philosophical reflection on the field of semiclassical physics has a knock-on effect on wider questions about intertheory relations. This is such that Bokulich breaks with the traditional conception of theory reduction mentioned above.[2] One of the aims of this thesis is to engage with Bokulich's interstructuralism by examining this view, critically considering the role of modern semiclassical physics in the account of the relationship QM-CM and the associated consequences on our realist understanding and interpretation of QM.

Therefore, I will first explore the theory of semiclassical mechanics and its application to paradigmatic physical problems. Essentially, semiclassical mechanics aims to obtain an approximation of the wavefunction of a quantum system from CM. Martin Gutzwiller (1990) crucially developed his 'trace formula', which approximates solutions to the Schrödinger equation considering fictional classical trajectories. The most relevant physical case in this theory is the Rydberg atom in a magnetic field. In addition, there are: the phenomenon known as wavefunction scarring and the helium atom. These

[2] Bokulich is not the first philosopher to look at semiclassical mechanics. For instance, Sklar (2013) reviews the history and modern developments in chaos theory. Indeed, semiclassical mechanics is closely related to the study of classical and quantum chaos. One novelty in Bokulich's works is that she uses the developments of semiclassical mechanics in order to articulate the QM-CM relation.

three physical cases are relevant in current research in physics and I will discuss them in detail.

More specifically, I will engage with interstructuralism by examining its account of the physical problems mentioned above, the QM-CM relation in general, and its associated form of scientific explanation. Furthermore, I will critically assess interstructuralism from the point of view of realism, which is a central challenge for this view. Such a challenge arises in the following: modern semiclassical mechanics uses classical orbits to explain quantum phenomena. However, it is known that quantum objects do not follow trajectories, which entails that the model involves a fictional aspect. Moreover, those fictions cannot be eliminated by adding more information to the model. This prompts interstructuralism to provide a philosophical account of the form of explanation involved in the success of such a semiclassical model. However, the standard realism infers realist commitment from the relevant elements in the explanation (known as 'explanationism'). Hence, interstructuralism puts the standard realist between a rock and a hard place. I will explore interpretations of the role of the fictional orbits in semiclassical phenomena and their epistemic/ontological status. I will also discuss Samuel Schindler's (2014) criticism of the form of scientific explanation proposed by interstructuralism. As a result of that discussion I will devise an alternative account of the semiclassical phenomena, managing to recover a causal structure embedded in the well-known Woodwardian counterfactual account of explanation. My interpretation of the relevant elements of semiclassical mechanics will be based on Spencer Hey's (2016) notion of 'simplifying heuristics'. This is useful because it allows the realist to deny any realist commitment to a feature in the model despite its role in the explanation.

Let me summarise. With the conception of the Received View of the realist interpretation of QM I will argue that current interpretations mistakenly focus on intertheory relations, i.e. how CM arises from QM. Moreover, I will conclude that the traditional accounts of the relationship QM-CM are unsuccessful. Interstructuralism appears as a novel view of the relationship QM-CM that resolves the difficulties of the traditional view. However, interstructuralism faces a challenge: it has to justify its realism given that the form of explanation necessarily involves fictions. Now, even if one grants that this issue could be dealt with, there is something missing: the account of the interpretation of QM. Naturally, it would be unfair to criticise interstructuralism for not providing an interpretation of QM, as it never aimed to do that. Nevertheless, given the controversial form of explanation and the relevance of the relation QM-CM in the known realist interpretations, the realist still has to take a step back and try to understand what is really important here. Again, how could we have an account of the relationship QM-CM without being able to specify the nature of the quantum systems in realist terms, beyond claiming that they are 'quantum systems'?

Therefore, I will argue that the realist should reject that an interpretation of QM has to start from a preconceived view on intertheory relations and agree that the focus ought to be on the question: ‘what is a quantum system?’. I will provide an account that follows the methodology of Core Realism and it is supplemented by an appropriation of Heisenberg’s Closed Theories view. I will call it the Core Realism+Closed Theories view. I will defend the claim that this realist and pluralist view of physical theories is suitable to interpret QM in a novel manner. I will specify my view by drawing on novel distinctions made in the debate over realism. Firstly, I will look at Juha Saatsi’s (2015) novel proposal that distinguishes ‘recipe’ from ‘exemplar’ forms of realism, and I will argue that my Core Realism+Closed Theories embedded in the essence of realism provides an exemplar approach to QM. Secondly, I will look at P. D. Magnus’ (2012) distinction between those realisms with a ‘deep’ metaphysical commitment from those realisms that are metaphysically ‘shallow’, concluding that my view could be either shallow or deep in the first stage. Furthermore, I will contrast my view with recent and relevant forms of realism that also involve some element of pluralism, namely, Michela Massimi’s (2016) perspectival realism, Ronald Giere’s (2006) scientific perspectivism and Nancy Cartwright’s (1983) metaphysical pluralism.

By the end of this thesis I hope to have convinced the reader that a realist approach to QM has to have an open mind to ask the right question: ‘what is a quantum system?’. In particular, she should not have preconceived expectations about intertheory relations. That is, the realist should not demand that QM has to give a description of classical systems. Whilst it is reasonable to expect that there is a relationship between the realist content of different theories, it is not reasonable to expect in principle that any novel theory has to recover the description of previous theories in the language of the new one. Hence, the realist should not force QM to describe the classical properties of classical systems in quantum terms. However, it is plausible that QM described quantum properties of systems that can also exhibit classical properties. Finally, my overall strategy that divorces the question of intertheory relations from the crucial interpretation question will motivate interest in a realist interpretation that has hitherto fallen out of favour, namely, the modal interpretations.

In the following I shall provide a short description of each chapter:

- **Chapter 2:** I will articulate the Received View of the Realist Interpretation of QM. This will be my analytic tool to critically engage with the main realist interpretations: Many Worlds Interpretations (MWI), Bohmian Mechanics (BM), Ghirardi-Rimini-Weber (GRW), Wavefunction Realism and Modal Interpretations (MIs). The main challenge for the interpretations is the famous measurement problem, which I will discuss too. Alternatively, I will propose a realist framework called Core Realism. Yet, Core Realism is not rich enough to be a form of realism. I will supplement it in Chapter 6 with Heisenberg's Closed Theories. The central message in this chapter is that current realist interpretations overestimate the importance of the QM-CM relation.

- **Chapter 3:** I will engage with the traditional account of the QM-CM relation in philosophy and in physics. I will use analytic tools in order to critically engage with the philosophy side, which mainly focuses on theory reduction. Specifically, I will examine Nagelian reduction, Nickles' reduction and Post's heuristic principle. However, I will not examine emergence in detail and I will give reasons for that. In regards to the physics side, I will critically engage with the QM-CM limit. Specifically, I will look at the mathematical limits, Ehrenfest theorem, Moyal brackets, and, in more detail, decoherence. Decoherence plays a crucial role in recent developments in the foundations of QM. I will note disagreement in the philosophical literature on what the QM-CM limit amounts to and what decoherence can achieve with regards to its account. I will defend the claim that decoherence does not provide an account of the smooth appearance of the classical from the quantum. I will conclude by outlining possible strategies that the realist could undertake in order to overcome the challenges discussed in this chapter.

- **Chapter 4:** This chapter and Chapter 5 go in tandem. In the first part of this chapter I will introduce interstructuralism's underlying philosophical framework. Bokulich's novel account of the QM-CM relation is based on the philosophy of Paul A. M. Dirac, who considered physical theories as 'open'. The Open Theories view allows dynamical structures from one theory to play significant roles in another theory. Plus, it entails a gradual model of scientific progress and a model of how theories can develop in a piece-meal fashion. In the second part of this chapter I will introduce modern semiclassical mechanics. In particular, I will examine the Rydberg atom in a magnetic field, the helium atom and wavefunction scarring. These are crucial because each one of them is captured by Bokulich's view. Indeed, she aims to account for the QM-CM relation involved in these phenomena. The traditional views on the QM-CM relation (discussed in Chapter 3) fail to accommodate semiclassical phenomena. Theory reduction and the physical devices such as decoherence establish that CM can be recovered by QM. However, in semiclassical mechanics classical orbits play a significant role in explaining quantum phenomena, which conflicts with the presuppositions of the traditional views.

- **Chapter 5:** I will draw on Chapter 4 in order to engage with philosophical issues in Bokulich's account of the QM-CM relation, particularly around the notion of scientific explanation. One challenge is that, according to a standard realist view, i.e. explanationism, the realist infers ontological commitments from the working-posits of the explanation. Now, classical orbits (from CM) play a role in explaining quantum phenomena described by semiclassical mechanics. Hence, the realist would have to believe in the existence of the classical orbits, which are knowingly unreal in the quantum domain. I will make the following contributions: I will critically engage with Schindler's criticism of Bokulich's form of explanation; I will re-interpret the role of the classical orbits in the explanation of semiclassical phenomena; I will re-interpret their fictional status; I will introduce a causal counterfactual relation between the classical orbits and the phenomena; and I will resolve the tension between the fictional explanation and realism by offering an instrumental interpretation of the orbits. This will be done at the expense of acknowledging that further interpretational work is required in regards to the quantum nature of semiclassical phenomena. The conclusion of Chapter 4 and 5 is that even if interstructuralism is accepted as a successor of the traditional relation QM-CM, the question of the interpretation of QM remains unresolved. And following the Core Realist methodology discussed in Chapter 2, the realist should first have an account of quantum systems before exploring how QM relates to CM. Hence, a novel view on QM is motivated.

- **Chapter 6:** Drawing on Chapter 2 I will supplement my Core Realist view by introducing Heisenberg's Closed Theories view. I will examine and specify my Core Realism+Closed Theories view by drawing on recent developments in the debate on realism. Juha Saatsi and P. D. Magnus articulate two useful analytic tools. Saatsi's distinction assesses two broad realist strategies: the realist can follow a general recipe on the basis of individual successful cases, or she can always work on the basis of particular cases and keep an open mind as to her realist commitments. These are the 'recipe' and 'exemplar' strategies, respectively. Saatsi favours the latter strategy. I will further articulate this distinction, and I will also argue that my Core Realism+Closed Theories view is consistent with the exemplar realism. In turn, Magnus distinguishes between realisms that commit with a 'deep' metaphysics or with a 'shallow' one. The former looks for the very nature of the world, whilst the latter contents itself with an interpretation of the scientists' work and can focus on explaining phenomena. At this stage of development, my view can be used in both deep or shallow approaches.

- **Chapter 7:** I will articulate further Core Realism+Closed Theories view. Although my view will seem radical, I will show that known and serious realists views have similar characteristics to mine. I will contrast my view with other views that are somehow pluralists, namely, Ronald Giere's scientific perspectivism, Michela Massimi's perspectival realism and Nancy Cartwright's metaphysical pluralism. Giere's view includes the scientist within the representation. By contrast, my view is closer to traditional realism, whereby theories provide us with knowledge of the objectively existing world. Massimi's view faces similar challenges to the Closed Theories view, particularly specifying the notions of perspective/closure of theories, individuation and limits of perspectives/closed theories. I will note that neither Giere's nor Massimi's views have been applied to QM. Finally, Cartwright's view is different to mine. She assumes that the empirical world is completely given and known, and theories apply to that world in a patchy manner forming the dappled world. In my view, there is no recipe that suggests what the world is.

- **Chapter 8:** I will summarise the conclusions of the thesis. In addition, I will indicate two arguments that could be developed in future work. Firstly, given that my view does not assume that we know what a quantum system is, I cannot take realist commitments to views on intertheory relations as that goes against Core Realism. However, decoherence is empirically successful and underpinned by notions of intertheory relations. I will offer an instrumentalist interpretation of the success of decoherence. This is the same move that I will have made in Chapter 5 to accommodate the success of the models of the Rydberg atom and it is based on Spencer Hey's 'simplifying heuristics'. This allows the realist to avoid making wrong ontological commitments. The second argument intends to revive interest in the modal interpretations. In Chapter 2 I will have showed that MIs were abandoned because of the disagreement with decoherence. However, that argument is underpinned by a specific view on intertheory relations, which I reject. Therefore, in my view the realist can still consider the modal interpretations as a valid approach to QM.

Chapter 2

The Received View of the Realist Interpretation of QM

2.1 Introduction

This chapter articulates a novel line of analysis in order to engage with current widely discussed realist interpretations of QM. As it is well-known, realist interpretations of QM take the measurement problem of QM – as I will discuss in more detail below, the problem of explaining the determinate measurement outcomes, essentially – as a crucial issue. I will argue that such assessment of the relevance of the measurement problem is the result of adopting a particular philosophical framework. In addition, such adoption is not explicit in the literature, making its identification a difficult task. I call this framework a 'Received View of the Realist Interpretation of QM', although at times I will use Received View, for short.[1] Whilst I grant the possibility that a realist might be concerned with resolving the measurement problem without holding such a view, my point is that, by and large, the underlying motivation to tackle the measurement problem in the literature is captured by the Received View. I will argue that taking on this perspective of analysis will shed light on to the still unresolved realist account of QM.[2]

[1]The strategy of identifying a 'Received View' in order to engage with a topic is not uncommon. Other cases in philosophy of science can be seen, for example, in the Received View articulated by Suppe (1977) in the context of the history of analytic philosophy and the movement of the logical positivism, by contrast with the well-known and today widely held semantic approach. Or the Received View that French and Krause (2006) articulate and criticise in the context of how, historically, quantum entities were considered as non-individuals. I do not intend to engage in those debates, but I indicate that I am adopting a similar strategy. By contrast with those debates, the component of originality in my analysis lies in attempting such a labelling within the approach, evaluation and criticisms to received opinion in regards to the interpretation of QM from the point of view of scientific realism.

[2]As mentioned earlier, my contention is that there is no clear answer to the basic questions about QM: what is a quantum system really? What do wavefunctions refer to?

The debate on scientific realism is a central topic in philosophy of science. As such, it can be approached from a number of angles for various purposes. In order to specify my discussion, I will consider a distinction in this debate based on two possible types of question:

1. *What is realism?* This relates to the debate over the sort of commitments that realism entails, the worldview it holds, what it takes the aims of science to be, etc.
2. *How can realism be justified?* This debate aims at convincing the philosopher to prefer realism over anti-realism. Here the discussion is framed in terms of argument vs. counter-argument, criticism vs. reply, and so on.

These two issues are not completely independent, and the debate of realism/anti-realism (within question 2) has certainly provoked further development and nuanced versions of realism (within 1). However, for my purposes it will be worthwhile to focus mainly on discussion 1, on what realism is and what it can be. By taking this approach I am trying to convey that I will avoid entering into the debate on the problems that are presented as challenges for realism, such as the issues of the pessimistic metainduction and the underdetermination of theory by evidence. That is, I do not intend to provide arguments *for* realism by contrast with other views. Instead, I just assume from the outset a broadly realist attitude, and my intention is to discuss what that means in relation to QM.

My articulation of the Received View as a form of realism with regards to QM intends to exhibit commitments that are noncore, but additions to the core of realism. Hence, I will first outline the characteristics of the core of a realist interpretation of QM, that I call Core Scientific Realism, or Core Realism, for short. This will help me to exhibit the commitments that I identify in the Received View as *extra*. Core Realism picks out common and basic features from any generally conceived form of realism. Possibly, plainly denying any of the intuitions alluded to in the following statements will entail commitment to an anti-realist view.

Core Scientific Realism:

i) The world exists independently of us. The world is objectively independent of the subject, and so our consciousness and perceptive apparatus have no effect on how the world is.

ii) Our theories capture truths of, or are partially true of, or are approximately true of, or latch onto, the world. The different variants can specify a more concrete realist view, but broadly they all share the idea that there is a relationship between theoretical knowledge and the world.

iii) The realist is concerned with the interpretative question: how is the world according to the theory?

Indeed, i) and ii) are similar to standard accounts of realism. Papineau (1996, 2) considers the 'independence' and 'knowledge' theses in realism, whist Psillos (1999, xvii) includes the 'metaphysical' and 'epistemic' realist theses.[3] The distinction made before between questions 1 and 2 frees me from having to thoroughly justify the compatibility of (i) and (ii).[4] Also, the philosophical work required to spell out how theories capture true features of the world, or what notion of truth is involved, would take us into a debate that I do not intend to engage in here, the reason being that it would take the discussion too far away from the realist interpretation of QM.

Hence, Core Realism essentially claims that the world is independent of us, and that we know about the world through scientific theories. Yet, we do not say how the world is prior to interpreting the theory, it is the theory which tells us something about the world. This is the result of the *interpretation* of the theory, which is why I add (iii).[5] My (iii) is how Van Fraassen defines an interpretation of a theory. He is also concerned with my question (iii), although he is not a realist. He adds that: "However we may answer these questions [iii)], believing in the theory being true or false is something of a different level" (Van Fraassen 1991, 242). Of course, this follows his constructive empiricism, according to which "science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate" not that it is true, (Van Fraassen 1998, 1069). However, his conception of what an interpretation is amounts to the same as the realist's one.

I want to emphasise the status of commitments to intertheory relations in Core Realism. Whilst the above three statements define the basis for a form of realism, there are neither implications of commitment to a specific type of relation between different epistemic entities (scientific disciplines, or theories within one discipline), nor whether there should be a fixed and particular type of intertheory relation at all.[6] Thus, in order to withhold a particular type of relationship, further commitments – external to Core Realism – are required. And this will be crucial to conceiving the Received View.

[3] My purpose here is to articulate the Received View of QM, not to present a general debate about scientific realism.

[4] Discussing this would be beyond the limits of my analysis.

[5] In philosophy of science, standard realism infers realist commitments from the notion of scientific explanation involved. This is called explanationism. The question of how to do so remains a matter of debate. Following the distinction I made in page 10, to infer commitments from the notion of explanation is a realist strategy that belongs to question 2. I am concerned with analysing the realist interpretations of QM, and following the tradition in the interpretation of QM the realist content is the result of question (iii). However, I acknowledge that there is a gap between realism in general and the realist approach to QM.

[6] I will return to intertheory relations in the context of realism and QM in Chapter 3.

Therefore, I claim that Core Realism does not entail commitment to any specific view on intertheory relations. However, that does not mean that Core Realism precludes intertheory relations, nor that Core Realism supports a form of realism that is silent with regards to intertheory relations. By the contrary, I would advocate that undertaking Core Realism allows the realist to conceive a view on intertheory relations. A view on intertheory relations within Core Realism ought to be decided/addressed/discussed only once the basic aims of Core Realism are achieved, once the interpretation of the theory is appropriately developed. In short, Core Realism does not deny the possibility of intertheory relations, but only indicates a methodology: given a pair of physical theories, question (iii) should be addressed for each one of them, and then, and only then, the realist can decide whether the best relation between them is captured by a pluralist, emergentist, reductionist, or any other intertheory relation.

Indeed, there is a relevant comparison to make in order to help illustrate my argument. The separation I make between realism and intertheory relations reflects a similar distinction made by Psillos (2005).[7] There are two observations to make: firstly, both Psillos and I understand realism as a view that pertains to scientific theories. Secondly, there is similarity in the argumentative strategy between my argument and the one he develops in that paper. I claimed above that questions of intertheory relations are a separate issue from the interpretation of a theory. Psillos argues with a similar strategy, divorcing factualism from fundamentalism in realism. Factual realism commits to the existence of facts. That is, regardless of whether there are more and less fundamental facts, the factualist can believe that they are all real anyway. In turn, fundamentalism takes only an 'elite' class of facts as real, those that are irreducible, basic or fundamental, (Psillos 2005, 388, 390). I do not engage with his specific debate, but I note the strategy in order to clarify my assessment of the realist interpretations of QM.

Let us discuss the first topic. Psillos (2005, 386) distinguishes two forms of conceiving realism depending on whether it is taken to be a conception that pertains to scientific theories, or a conception about the world. He argues that although realism involves a metaphysical dimension, it should be committed to a factualist view of reality, based on the theories. The view that derives from Core Realism coincides with (Psillos 2005, 396). The realist starts from the theories, and the world is the way the theories say. There is a commitment to the reality of the entities posited by the theories (whatever they might be, and not in relation to what *we* think the world is).[8] Hence, to be a realist about physics means that we take the physical theories and interpret them in order to spell out their factual content.

[7] I am grateful to Matthias Egg for suggesting this similarity.
[8] I will come back to discuss this in Chapter 6.

Let us see the second aspect. I articulate Core Realism in order to divorce the interpretative question about a theory from the conception of intertheory relations. I argue that it is reasonable to spell out the interpretation of two theories first and then attempt to address the question of the relationship between them, rather than the converse. That is, 'what is the world according to theory T?' and 'what is the relationship between T and T'?' are two different questions. A similar argumentative strategy is articulated by Psillos (2005) when he divorces factualism from fundamentalism with regards to realism. He is engaging in a different debate in philosophy that the one I am engaging in here, but I indicate a similarity in the strategy (a meta-form of 'divide et empera'). He distinguishes that to have a realist attitude does not commit one to other attitudes, such as fundamentalism. Hence, one can be realist about biology without having to advocate that biological facts reduce to physical facts, for that is (a) a separate issue and (b) suspending judgement on that does not entail that there are no biological facts or entities, (Psillos 2005, 396). Similarly, I argued that a realist can work on question (iii) in regards to a theory without having to invoke intertheory relations because these are separate issues.

Now I will specify the Received View of the Realist Interpretation of QM. Unsurprisingly, the Received View is a realist one and it agrees with the first two statements of Core Realism. However, at the interpretative level, it modifies the scope of question (iii). The Received View can be characterised as a realist view which in addition involves crucially two considerations of intertheory relations, plus one interpretative question. The application this framework will become clear when I discuss well-known realist interpretations of QM in Section 2.2:

Received View of the Realist Interpretation of QM:

A) At the *metaphysical or ontological level*, the Received View holds the hypothesis that a certain type of metaphysical relation connects the everyday (classical, macroscopic) objects like tables and chairs, with more fundamental quantum objects like molecules, atoms and elementary particles. This metaphysical relation is hierarchical, imposing an order across quantum and classical entities. Some variants of these relations are: metaphysical reduction, emergence, composition, dependence and grounding.

B) At the *epistemic level*, the Received View advocates a certain relation between QM and CM, which is imported from broader conceptions of intertheory relations within realism. Some of these are: the cumulationist or retentionist hypothesis, epistemic reduction, general correspondence principle, emergence, among others.[9]

[9]See (Laudan 1981), (Nickles 1973), (Post 1971) and (Batterman 2002).

They are all different but share the consideration that the more fundamental theory – QM – is successor of the less fundamental predecessor – CM –, whereby the former has to include some of the terms or structures of the latter.

C) The Received View is concerned with the interpretative question: what is the theory telling us about the world constrained to accommodating A) and B)?

Thus, I argue that the Received View, as a realist one, is more specific than Core Realism, because the former includes the commitments of the latter, and augments it or modifies it by adding A) and B). A) and B) are statements of intertheory relations. Furthermore, the Received View includes the interpretative question C), which can be seen as iii) restricted to satisfying A) and B).

The Received View embraces the belief that a realist view of QM ought to give an account of the 'every-day' macroscopic objects arising from the quantum (via some suitable relation). This includes the belief that an explanation for the existence of tables and chairs, as stably localised and continuous objects, should be given. This will be appreciated below, in the face of current realist interpretations of QM, by analysing what their proposal is and more precisely, focusing on what they actually take the underlying challenges to be.

The purpose of presenting this original notion of a Received View in the field of the interpretation of QM is to provide an analytic tool for critically engaging with known interpretations. I argue that recognising the Received View as an underlying philosophical framework is a useful tool. Moreover, that considering that the Received View is, by nature, a philosophical framework, includes the merit of presenting targets for fruitful discussion. That is, once the Received View is identified, there is the possibility to conceive a different view to QM by opposition with the former.

Hence, I hope to have presented two frameworks within realism: Core Realism and the Received View of the Realist Interpretation of QM. These are not precisely contrary to each other. I discussed that the Received View includes the commitments of Core Realism. Also, neither of them are actual forms of scientific realism, they cannot be simply identified with the various currently advocated forms of realism such as structural realism, explanationism, semi-realism, among others.[10] Indeed, I have not attempted to engage with any specific version of realism. The reason is that, actually, the association between realist-minded approaches to QM and forms of scientific realism is a complex

[10]I will return to this crucial issue in Chapter 6.

matter of debate that remains so far unclear.[11] And my discussion is concerned with how realism, broadly construed, approaches the interpretation of QM.

Therefore, the existence of a Received View considered as a realist approach to QM underpinned by claims of intertheory relations is my working hypothesis. This entails that the relationship between QM and CM plays a crucial role in the current realist interpretations of QM. If this is at least plausible, then I will be able to conclude that relevance of the question about the QM-CM relation should be re-evaluated. Indeed, the methodology of Core Realism does not suggest that 'recipe'.[12] In Section 2.2 I will examine the most well-known realist interpretations of QM. I assess whether they follow the recipe of the Received View by highlighting their essential characteristics. I will finish by drawing the conclusions of the chapter in Section 2.3. Later on in Chapter 3 I will offer a more detailed account of the relationship of QM and CM from the point of view of both general philosophy of science and the foundations of physics.

2.2 Realist Interpretations of QM and the Received View

The interpretations of QM that I am interested in discussing are framed within scientific realism. However, it is recognised that what their realism amounts to should be spelled out more clearly,[13] and addressing such question is not my main purpose here. I claim that what I presented as the Received View in the previous Section maps the current realist oriented approaches to QM. The aim of this section is to briefly recall the main points of the different realist interpretations, picking out their explicit or implicit conceptions that highlight the extent to which their research programmes fit within the Received View. Needless to say, for each interpretation there is a whole literature and the topics branch out becoming complex fields of research. It is not my intention to discuss in detail each one of these literatures, but merely to mention their core aspects.

Whether realist interpretations of QM can be captured by the Received View will be discussed subsequent sections. Yet, it is worth discussing the measurement problem of QM first. That is, the problem of explaining the classical appearances from QM. This problem receives most of the attention in the philosophy of QM. I will argue that an interpretation that considers this problem as a central issue probably belongs in

[11]Indeed, it is becoming more recognised that the association between realism and each interpretation is not as detailed as one would desire. The AHRC 'Scientific Realism and the Quantum' project at the University of Leeds is dedicated to detailing that relation. The existence of such a project evidences my observation.

[12]The association of a realist methodology with a 'recipe' is taken from (Saatsi 2015) and I will return to this in Chapter 6.

[13]See footnote 11.

the Received View, because the Received View captures the idea that underpins the formulation of the measurement problem.

2.2.1 The Measurement Problem and the Appearance of Classicalities

If one looks at the literature on the realist interpretation of QM, the measurement problem is probably the centre of gravity. There is debate over the scope and meaning of this problem, as I will show later on in Section 3.3.4 of Chapter 3. For now it will suffice to take a broad understanding of this issue: the measurement problem from today is essentially the same problem that the founding fathers discussed.[14]

Let us discuss the measurement problem: At the outset, the object-system S and measuring apparatus M are assumed to be uncorrelated and in pure states. A measurement is seen as an interaction between the system and the measuring apparatus, both described by QM. The initial states for the apparatus is a 'ready-to-measure' state $|M_0\rangle$, whilst the initial state of the system is an unknown pure state $|\phi\rangle = \sum c_n |\alpha_n\rangle$, expanded in terms of an orthonormal basis of eigenvectors $\{|\alpha_i\rangle\}$ of the observable A (with non-degenerate eigenvalues, for simplicity). The measurement interaction is represented by a unitarian time evolution of the system+apparatus given by the Schrödinger equation, which obtains:

$$\sum_n c_n |\alpha_n\rangle \otimes |M_0\rangle \longrightarrow \sum_n c_n(t) |\alpha_n\rangle \otimes |\beta_n\rangle . \tag{2.1}$$

Here, $c_n = \langle \alpha_n | \phi \rangle$ and it is assumed that the measuring apparatus contains a digital indicator with values $\{b_1, b_2, \ldots\}$, which are the eigenvalues of the observable B pertaining to the apparatus with eigenvectors $\{|\beta_j\rangle\}$.

This is discussed in many sources. Following Joos (2000, 2), the problem essentially consists that the final state – the right hand side of eq. (2.1) – is a quantum superposition, from which a probability distribution for each possible outcome is obtained (via Born rule). However, the experiments indicate that the final state of the apparatus is a determinate one, for the apparatus indicator has a definite value at the end of the measurement. Therefore, the unitary evolution that takes the system system+apparatus from the initial state to the final state, cannot be easily interpreted to mean that the indicator of the apparatus actually has a definite value.

Hence, on the one hand the final state cannot be easily interpreted to mean that the apparatus, considered as a subsystem, has a definite value. On the other hand, we know that after an actual measurement has taken place, the apparatus does have a

[14]Namely, Born's widely known paper from 1926, (Born 1926); Von Neumann's famous book of 1932, Von Neumann (1996); and the famous description of the measurement by Heisenberg in (Heisenberg 1958). See (Landsman forthcoming) for an up to date discussion of this issue.

definite value. Given that QM tells us that the final state is a superposition, and QM is more fundamental than CM, how do we find a unique outcome to measurements? How do we explain the appearance of a particular result given that QM tells us there is a superposition? Superpositions of apparatus pointers are not observed, what is observed is the apparatus indicator in a certain position. Hence the final state must be wrong or re-intertepreted in a way that agrees with the classical appearance of a unique measurement outcome.

Put it another way, the problem is that the final state is a superposition, and quantum superpositions are not easily interpreted. They do not represent a large amount of systems each of which is in one of the elements being superposed (a classical statistical ensemble). Quantum superpositions cannot be interpreted to mean that that the system is in a certain state but it is unknown which state. Such an interpretation of the quantum superposition is not only conceptually wrong, but results in wrong empirical predictions, see (Cohen-Tannoudji et al. 1977, 252-255), (Schlosshauer 2005, 1270) and my Appendix A. Therefore, the question that the measurement problem presents is: how is the final state accountable for the fact that we observe that the apparatus indicator has a value?

The orthodox solution to the measurement problem is given by the collapse of the wave function. The collapse is a different time evolution to the one given by the Schrödinger equation, and it entails that the final state is that which is associated with the observed result in the apparatus. The original justification for the collapse was given by Von Neumann (1996), which involved the consciousness of the observer. For reasons I do not need to discuss, that justification has been mostly abandoned, although the collapse approach is still today taught in standard physics undergraduate courses, and mentioned in standard textbooks, e.g. (Cohen-Tannoudji et al. 1977; Sakurai and Napolitano 2011).[15]

As I mentioned before, the measurement problem is crucial in the currently considered realist approaches to QM. Indeed, a traditional criterion to map out the different interpretations is in terms of how they react to the measurement problem and the controversial notion of collapse. For example, a widely cited articulation of the interpretations based on the measurement problem is given by three alternatives in (Maudlin 1995). Another way to classify the interpretations is by dividing between collapse or non-collapse solutions to the measurement problem, see (Putnam 2005). Now, I am not concerned with discussing in detail their underlying arguments, nor comparing merits and demerits between Maudlin or Putnam.[16] I am merely noting the relevance to the measurement problem in the realist interpretations.

[15] The relevance of the collapse in the practise of physicists has been recently challenged, see (Cordero 2001; Wallace 2016a). However, this discussion does not concern us now.

[16] Nota bene: by citing Putnam's article I am not indicating that he is the most authoritative voice in the analysis of the interpretations of QM, but I mention it because that article nicely expresses a well-accepted classification.

The interpretations that provoke most research and are most widely adhered to in the literature are the non-collapse interpretations called Everettian mechanics (or Many Worlds interpretations, MWI hereafter) and Bohmian mechanics. Next there is the spontaneous collapse view initiated by Ghirardi, Rimini and Weber (GRW), and the non-collapse wavefunction realism. They were all developed as alternatives to the collapse approach by Von Neumann. Nevertheless, in addition to those options, there are other attempts, of which I will only discuss the modal interpretations (MIs). The reason MIs are worth considering for this dissertation will be explained when I discuss them in more detail.

Now, closely related to the measurement problem and a crucial aspect that a realist interpretation has to resolve, is the problem of the appearance of classicalities from the quantum.[17] A clear expression of this is given by Cordero, who begins his article with the following:

> In order to be a realist about quantum theory one needs to have a properly physical interpretation of the quantum state, along with an explanatory account of quantum systems capable of accomplishing three tasks, at least: one must (a) answer the question about the theory's scope non-dogmatically, (b) account for state-reduction phenomena in a physically respectable way, and (c) account for the 'classical' world of ordinary experience in appropriately scientific terms.
>
> (Cordero 2001, S301)

My Core Realism agrees with Cordero's first claim plus (a) and (b). The disagreement between my Core Realism and Cordero's view is in (c). My contention is that this claim ought to be justified further, because it is not obviously true, but relative to extra suppositions. Suppositions of intertheory relations that accord with the Received View: that theories relate to each other and given that QM is more fundamental than CM, the former has to account for the latter. Now, I am not denying Cordero's premises in order to undermine his argument, that would be a case of question begging. What I am saying is that his premises are not simply true in themselves. Hence, I do not think I am begging the question. My aim is to note how from the outset (literally the first paragraph of his article), there is so much expected from the realist interpretation of QM beyond giving a relationship between the theory and the world.

[17] Although there is disagreement over how to actually separate the matters (the measurement problem and the appearance of classicalities from the quantum, it is agreed that these are interwoven issues. This question will be discussed in more detail in Chapter 3.

The above claims made by Cordero are not his individual opinion only. Landsman (2007) also begins his comprehensive article on the quantum-classical limit with a similar statement:

> Most modern physicists and philosophers would agree that a decent interpretation of quantum mechanics should fullfil at least two criteria. Firstly, it has to elucidate the physical meaning of its mathematical formalism and thereby secure the empirical content of the theory. ... Secondly, ... it has to explain at least the appearance of the classical world.
>
> (Landsman 2007, 417)

Again, his first criterion is in agreement with my Core Realism and with Cordero's first claims plus (a) and (b). The second criterion is equivalent to Cordero's (c) and in tension with Core Realism. Now, of course, my articulation of the Received View above challenge the belief that this statement is obviously true, or to defend the claim that further justification is required.

Such requirement from any realist interpretation of QM is not strictly necessary *in order that* the interpretation is a realist one – e.g. an interpretation that agrees with my Core Realism to QM would be realist whilst it would not necessarily fulfil Cordero's (c) condition or Landsman's second criterion. It just happens that, as Landsman says and Cordero assumes, most philosophers would agree with that requirement. Indeed, I will argue below that actually the realist interpretations of QM do attempt to fulfil such criteria, although it is not really necessary, but results from adopting a philosophical framework. This does not mean that I claim that realists should abandon attempting to relate QM to the classical world. What I argue is that, methodologically speaking, they are two different questions. Indeed, within Core Realism, addressing Cordero's first set of claims and Landsman's first criterion, that is, the realist question of 'what a quantum system really is' or 'how the world could possibly be the way QM says', is a priority whilst the second set of claims are secondary.

By contrast with the Received View, Core Realism entails that the realist is primarily concerned with giving an account of the 'quantum' aspect of the world in appropriately realist terms – as representing something of a real character (Landsman's first criterion and Cordero's first claim plus claims (a) and (b)). Hence, the task of the realist in Core Realism is, above all, to be able to give an account of what a quantum state represents in the world. Conceiving the measurement problem in the way it is discussed in the literature, entails, I argue, attaching unnecessary conditions to the scope of a realist interpretation of QM. Core Realism does not limit the scope of the theory to explaining classical appearances. Because it is not trivially true that QM's domain includes the classical properties of classical systems.[18] Core Realism is open to letting the theoretical and experimental aspects of physics guide us into knowing the world better. It accepts that the final state of the quantum interaction is a superposition, and the interpretation of the quantum superposition is the crucial challenge. This conflicts with the attempts to find a way to get rid of the 'awkward superpositions', as Landsman (2007, 417) puts it. However, in this chapter my central concern is to propose a systematic analysis of the current realist interpretations, not to design a new one.

This discussion can be illustrated in Table 2.1, which compares the problem of QM and the implied methodology both in Core Realism and the Received View of the Realist Interpretation of QM.

	Core Realism	Received View
General Methodology	1. Provide a realist interpretation of the theory, question (iii) in page 10. 2. Address intertheory relation between QM and CM in agreement with 1.	1. Assume metaphysical and epistemic relations between QM and CM. 2. Provide a realist interpretation of QM in agreement 1.
The crucial challenge of QM is	to provide a realist interpretation of Ψ.	to provide explanation of the appearance of the classicalities from QM.

TABLE 2.1: Summary of the comparison of methodologies of Core Realism and the Received View.

Two more comments are appropriate before analysing each interpretation. Firstly, that what I present as the Received View and Core Realism, provides a contribution to the literature in that this scheme gives a novel way of classifying the different interpretations of QM. It represents an alternative to the traditional collapse/non-collapse or Maudlin's trilemma in Maudlin (1995). That is, now one can classify the realist interpretations by asking what type of intertheory relations they adopt. Questioning this, as I will argue, highlights the dominance of the broadly construed framework of the Received View. I claim that this results in philosophical constraints for the realist within their project of elaborating a metaphysics for QM.

Secondly, based on my discussion of the measurement problem, it seems that I attempt to dissolve it instead of solving it. Indeed, such an attempt is not entirely new and there are some similarities with current lines of research. The 'therapeutic approach' adopted

[18]That depends on what interpretation or version of QM one considers. It is well-known that, initially, Schrödinger had considered that the wavefunction represented waves of matter. His view was mistaken, as later developments showed.

by Friederich (2014) has a similar strategy. It entails the idea that the basic problems encountered in the literature (for a certain problem/topic) can be seen as based on misconceived assumptions which, if changed, dissolve, rather than solve the problems of the interpretation of QM. Friederich attempts to challenge basic conceptions of the very understanding of physical theories and their relationship the world, along the lines of the pragmatic approach of Healey (2012, 1989). I do not engage with their specific form of anti-realism as my work engages with realist views. However, I point out that there is a similar strategy in considering the conditions that allow the measurement problem to arise as a starting point.[19] I will argue that predominant realist interpretations of QM are captured by the Received View, where the measurement problem and the associated explanation of the appearance of the classical world is crucial. Hence, rather than focusing on devising a novel solution to the measurement problem, the problem itself will be dissolved or reconceptualised, according to Core Realism.[20]

Let us now then go over some of the most discussed realist interpretations of QM, with the aim of emphasising the extent to which they match the framework of the Received View.

2.2.2 Everettian Interpretations

Everettian interpretations (known better as 'many world interpretation', MWI) were initiated with the 'relative state interpretation' put forward by Everett III in his 1957 PhD thesis. MWI rejects the postulate of the collapse of the wavefunction and is presented as a straightforward reading of the standard quantum formalism. There are many versions of this approach, see Wallace (2003) and references therein. The most popular one is articulated by the Oxford School, namely, David Deutsch, David Wallace, Simon Saunders, Hillary Greaves, amongst others. In the words of Wallace, all there is to MWI is the following:

> [MWI] consists of two very different parts: a contingent physical postulate, that the state of the Universe is faithfully represented by a quantum state evolving in unitarian time evolution; and an a priori claim about that quantum state, that if it is interpreted realistically it must be understood as describing a multiplicity of approximately classical, approximately non-interacting regions that look very much like the 'classical world'.
>
> (Wallace 2013, 465)

[19]Another type of approach could simply accept the measurement problem as the problem that one has to solve.

[20]Another similar approach that I also do not discuss here is developed by de Ronde (2016).

In MWI, the object-system, the apparatus and the observer are regarded as one closed system described by QM. In the process of measurement (and in general, regardless of an actual experiment being made), all possible outcomes are realised in approximately separate but real (actual) worlds. The interaction between the different branches is negligible for practical purposes. Hence, the worlds are effectively unaware of each other. For an occurrence of an experimental result, all the other branches are necessary, even though one is realised in the branch where 'you are at'. The splitting of the world into many worlds as a consequence of the measurement interaction avoids postulating the non-causal evolution of the collapse. Now, whilst this interpretation is logically consistent, it has the radical implication of the literal existence of an immense – a denumerably infinite – plurality of worlds, approximately independently of each other.[21]

The typical criticism of MWI arises from the problem of the preferred basis, and the problem of the probabilities or the derivation of the Born rule. The former stems from the basic properties of a Hilbert space-based formalism: a vector in a Hilbert space can be expressed as a linear combination of infinitely many different orthogonal bases. Hence, the branches that compose the original state depend on the selection of a 'preferred' basis. Yet, the interpretation does not say which basis should be preferred. Decoherence is used in order to help MWI to resolve this problem: the observables (super)selected by decoherence are to be preferred.[22] Indeed, the revival of the interest in the MWI carried out by Saunders and Wallace in the 1990s, was closely associated with incorporating decoherence as a main element in the MWI, see (Wallace 2010) and (Saunders 2010, 5). This had been initiated by Zeh (1970). Briefly, decoherence can be taken to allow for the splitting of the worlds into approximately independent and approximately classical worlds.

It is generally thought that the probability determines which outcome will be realised in some way. Hence, the problem of probabilities relates to articulating the way that QM is meant to be a probabilistic theory, given that MWI proposes that all possible outcomes actually occur in different worlds. Deutsche, Wallace and Greaves defend that incorporating decision-theory into the interpretation resolves this, see (Wallace 2003). However, critics claim that the framework requires decoherence which in turn utilises Born rule in the first place, undermining the strategy, see (Baker 2007).

Now, the central concern in MWI is to 'explain the appearance of classicalities'. Indeed, the advocate of current versions of MWI is not looking for a metaphysical account of, for example, the quantum state during the time when the coherence is retained.

[21] The 'approximately independent' character of the branching is related to the 'approximately classical' states obtained from decoherence.

[22] At this point I assume the reader is familiar with basic notions of decoherence. I will discuss the preferred basis problem and decoherence in the broader perspective of the problem of the QM-CM limit in Chapter 3.

Although before the time that the coherences are delocalised the quantum system cannot be described in classical terms, no crucial worries seem to arise in MWI. In fact, MWI is a view that operates at the level of the macro-world, Wallace (2016b). What does MWI say about quantum systems for the short length of time when there is coherence? At that point, MWI presents a mathematical description of the quantum system. Of course, a mathematical description could be metaphysically construed in realist terms, for example by adopting a structural realist approach, although whether that is a satisfactory account neither is an incontestable issue, nor is a central issue for MWI.

In other words, in order to recover the classicalities – that is, in order to obtain the appearance of the macro-world – MWI suggests to consider an infinite number of worlds interacting with each other, which strictly speaking are in a superposition. However, 'for-all-practical-purposes' (FAPP) the worlds are independent from each other. This seems to be a high philosophical 'price' to pay. (Ladyman and Ross 2007, 182) warn that "multiple branches might be an artefact of incomplete descriptions and of the use of QM to represent the states of macroscopic objects", and MWI assessments are dependent on more fundamental physics which is yet to be developed. A view that is complacent to obtain a description FAPP, but not deeper than that, resembles a form of instrumentalism that is content with empirical predictive power. By claiming an account of what is only decidable FAPP, it is not clear that this view is concerned with providing an articulated connection between the theory and the world that is independent of humans.

However, this thesis does not aim to critically engage with the vast literature around MWI. Regardless of my assessment, I conclude that it is fair to conceive it within the Received View. This is because the idea that underpins MWI is that QM is more fundamental than CM and thus it has to explain why we observe the classical world. It expects QM to explain the classical world. Why do we observe the stable macro-world if the world is quantum? A question of this type captures they aim of current versions of MWI. And this question matches the framework of the Received View.

2.2.3 Bohmian Mechanics

Bohmian mechanics (BM) started as a non-collapse hidden variables research programme, and it has many ramifications. The original proposal by Bohm in 1952, modifies the standard formalism in order to be able to account for particles with definite positions. The currently widely known version of BM is the proposal initiated by Bell and followed by Dürr, Goldstein and Zanghi (DGZ). Dürr et al. (1996, 21-22) present that the complete description – i.e. without the need for the collapse postulate or further axioms – of an N-particle system is provided by its wavefunction $\psi(q,t)$ where $q = (\vec{q}_1, \vec{q}_2, ..., \vec{q}_N) \in \mathbb{R}^{3N}$, and also its configuration $Q = (\vec{Q}_1, \vec{Q}_2, ..., \vec{Q}_N) \in \mathbb{R}^{3N}$, where the $\vec{Q}_k$ are the positions of the particles (assuming neither electromagnetic force nor spinorial part are present). Now, the wavefunction evolves always according to Schrödinger's equation – no collapse hypothesis –,

$$i\hbar\frac{\partial\psi}{\partial t} = H\psi. \tag{2.2}$$

Furthermore, the actual motion of the point-mass particles of masses $m_1, m_2, ..., m_N$, evolve according to the guidance equation

$$\frac{d\vec{Q}_k}{dt} = \frac{\hbar}{m_k}\frac{Im(\psi^*\nabla_k\psi)}{\psi^*\psi}(\vec{Q}_1, \vec{Q}_2, ..., \vec{Q}_N), \tag{2.3}$$

where $\nabla_k = \partial/\partial q_k$.

Dürr et al. (2013) view it that the wavefunction is not as 'real' an element as the particles are. They consider that there is a primitive ontology constituted by particles with position in the ordinary (classical) space (for non-relativistic QM). The wavefunction is not part of the primitive ontology, and instead is a 'nomological entity' which governs the motion of particles, (Goldstein and Zanghì 2013, 92). The nomological role of the wavefunction is analogous to the role of the Hamiltonian in CM: to implement the law of motion for the primitive ontology, (Allori 2015, 111). See Dorato and Laudisa (2015) for criticism.

As noted by Barrett (2003), BM is grounded on a number of (metaphysical) assumptions of what QM – as a physical theory – is expected to represent or describe. BM shapes the standard quantum formalism and interprets quantum theory in order to be able to have the local beables or the primitive ontology, point-like masses. These particles do follow trajectories, although their nature is different to that of classical particles. One qualitative difference between CM and BM is the non-local character of the latter. In classical physics the velocity and positions of the particles are related by being conjugate variables, but are essentially independent from one another, whilst in BM all particles are related by the guiding equation $\vec{v}_k = d\vec{Q}_k/dt$.

Now, let us indicate the match between the Received View and BM. Firstly, it is worthwhile noting that within the BM research programme, a central challenge is to account for the problematic relationship between BM and relativity theory. DGZ hold that it is an urgent challenge to reconcile the non-local character of the theory and the fact that the equations (2.2) and (2.3) are not Lorentz-invariant. That is, to account for a smooth intertheory relationship between BM and relativity theory, see Dürr et al. (2013). To assess this as an urgent concern fits within the Received View.

Nevertheless, there is a seemingly unorthodox character to BM in considering the wavefunction as a scientific law. How exactly to spell this out is still a source of disagreement, for what does it mean that Ψ is a law? One might consider that this feature puts BM away from a Received View. However, I argue otherwise.

Defending a Bohmian interpretation, Allori (2015) argues that BM cannot consider Ψ to be real. The reason is that the wavefunction is not suitable to represent matter. Hence, I claim that this suggests that BM belongs in the Received View. Indeed, I argue that to require that the realist content equates representing matter matches well with the Received View. Underpinned by the view that CM captures true features of the world, anything else that the realist considers real also has to refer to what CM refers to, i.e. matter. Therefore, BM's criterion for what is real is justified by a view on intertheory relations. The Received View takes the 'everyday' to be really real. It asserts that the classical metaphysical elements – massive objects – are a guide to what is real. Methodologically, this is similar to saying that whatever QM tells us about the world, it must justify the appearance of classicalities.

Therefore, I argue that this discussion justifies considering BM within the Received View.

2.2.4 The Ghirardi-Rimini-Weber Interpretation

Originally initiated with Ghirardi et al. (1986), the interpretation known as Ghirardi-Rimini-Weber (GRW), is a collapse interpretation which, starting from explicit ontological presuppositions, modifies the Schrödinger equation by adding a non-linear term. In a way, the strategy is similar to BM: it modifies the formalism with a metaphysical agenda. However, BM is a non-collapse interpretation, whilst GRW explicitly attempts to obtain a physically acceptable collapse.

As Ghirardi et al. (1986, 471) state, the intention is to obtain the "unified derivation of the behaviour of all objects [microscopical and macroscopic] from the basic dynamics of the microscopic world". Thus, with the aim of suppressing linear superposition of states corresponding to macroscopically localised objects in far away spacial regions, they add in the dynamical equation, a stochastic term which corresponds to a 'localisation process' (formally identical to an approximate position measurement). The stochastic term is a formal device which produces the same effect as the collapse. Hence this interpretation takes it that the collapse of the wavefunction is a real process that occurs in nature regardless of an experiment being observed or not. However, instead of postulating a discontinuous time evolution like the advocate of the collapse does, here the 'spontaneous collapse' is considered a natural consequence of the proper dynamical process.

The modification of the standard formulation of QM involves incorporating two fundamental constants: σ, which relates to the precision of the collapse events and λ, which determins the rate at which the collapse events occur. The value of these constants is a contested matter, as they determine sensible experimental consequences, see Sebens (2015). Originally GRW proposed the value of $10^{-16}s^{-1}$ for λ the rate of collapse. This means that when a collapse happens at t_1 and a particle gets its position localised, there is a probability that the next collapse happens later at t_2 given by $P(t_2 - t_1 < \Delta t) = 1 - exp(-N\lambda\Delta t)$, where N is the number of particles. When the particle is localised, the wavefunction is given by the pre-collapse wavefunction multiplied by a peaked Gaussian, centred in the localised position. Now, if λ is 0, then the collapse does not happen and GRW becomes GRW_0, which Sebens (2015) equates to MWI.[23] In turn, σ and it was originally proposed to have a value of $10^{-7}m$. This determines the peak of Gaussian: a small σ determines a sharper peak.

This allows for the "possibility of accounting for the dynamics of macroscopic systems in terms of trajectories" (Ghirardi et al. 1986, 476). They postulate that all microscopic systems are subjected to localisation processes with an appropriate frequency.

Now, one of the main criticisms of GRW is the tails problem: the spontaneous collapse obtains a post-collapse wavefunction that is the pre-collapse wavefunction multiplied by the Gaussian function. But this function is not a $\delta(x - a)$ function – a localised function as one would expect. Then, however G is sharply peaked (given σ's value) and localised around its centre, it is still non-zero in other positions. Therefore, the tail of the Gaussian means an infinite (however small) spreading of the wavefunction, challenging that GRW delivers the localisation it promised, see Lewis (1995).

Let us discuss the extent to which GRW is aligned with the Received View. From a methodological point of view, the very aim of GRW is motivated by attempting to obtain classicality – localised, determined properties – from QM, although QM initially did not suggest these characteristics. That is, this interpretation is motivated to obtain a physical account of the projection postulate that was meant to resolve the measurement problem. Furthermore, the crucial challenge for GRW, the tails problem, is a result of engaging with that methodological ambition The reason why the tails problem is a problem for GRW is that the tails make the localisation of the wavefunction problematic.

[23]This widely accepted view assumes that MWI is the only alternative to a collapse interpretation.

In conclusion, GRW matches with the Received View. It mirrors its research programme explicitly, in that the aim of GRW is to force the theory to recover classicalities. GRW aims at getting rid of the quantum superpositions. In addition, the very criticism to this interpretation relates to whether it fails to recover non-superposed states.

2.2.5 Wavefunction Realism

The adherents of wavefunction realism, seemingly contrary to BM, hold that the wavefunction is more than a mathematical element, representing a concrete physical field. Recently, Albert (2013, 53) has argued that we literally live in the configuration space, and that the wavefunction is a physical object – essentially the same claim made in (Albert 1996, 227). This interpretation is presented as neutral with respect to the solution to the measurement problem, and Albert defends that his view is compatible both with GRW and BM.

One of the central criticisms of this view is that it hardly accounts for the fact that ψ is not a wave in the classical $3-$dimensional space, but in the configuration space, the dimensions of which depend on the number of particles of the system. If the system is the whole universe, the configuration space where the wavefunction is defines a $3N$-dimensional space, where N is the number of particles in the whole universe. And taking the wavefunction as ontologically real, it is argued, entails that the space in which the wavefunction is formulated is real too – by analogy with a classical wave and classical space. Consequently, a tension appears between stating the reality of the configuration space in view of the 3-dimensional space and time as it appears to us. Hence, whilst QM is framed in a high-dimensional space, macroscopic objects appear to be in a 3-dimensional space, and a failure to address this relationship is one of the central criticisms of the wavefunction realism. Indeed, Monton (2006, 783) argues against any real content for the wavefunction as follows: "while it is mathematically viable to represent the theory as consisting of objects evolving in 3N-dimensional space, it is not physically viable, because 3N-dimensional space is not an accurate representation of the physical, three-dimensional [classical] world".

Wavefunction realism is capable of being appropriated by the likes of Dorato and Laudisa (2015), who claim it would be dogmatic to accept only a 3-dimensional object as real, hence leaving open the option for a realist view on the wavefunction. They recognise that QM suggests a profound revision of the 'manifest image'. However, they claim that wavefunction realism has still a lot to achieve. That is, it has to explain the common-sense reality:

> However, in the case of configuration space realism, it is the whole worldview of common sense that is regarded as 'misleading', and since science relies on observations and therefore on common sense, the consequence that all our observations are radically illusory cannot be accepted.
>
> (Dorato and Laudisa 2015, 6)

I claim that the hypothesis of wavefunction realism fits with the methodology derived from the Received View. In previous cases I claimed alliance with the Received View by those who restricted the problem of QM to that of justifying/explaining the appearance of the classical world. By contrast, wavefunction realism seems to disagree with that project, pulling towards the opposite end. Wavefunction realism denies the existence of classical reality based on the alleged reality of the wavefunction. However, I claim that both extremes are pertinent within the Received View, as follows.

Wavefunction realism denies realism of classical physics and the everyday ontology of tables and chairs, in favour of a fundamentally real quantum wavefunction. This is methodologically analogous to claiming that a more fundamental theory has to justify the existence of tables and chairs. In the case of wavefunction realism there is only one reality: the wavefunction of the whole universe is the only real element. Hence, everything else is less real or illusionary, including the familiar macroscopic objects such as tables and chairs. As Dorato and Laudisa (2015) claim (see quotation above), there is a concerning suggestion: if all there is is a wavefunction, then the illusionary/less-real aspect of classical appearances ought to be accounted for.

Dorato and Laudisa (2015) argue that if the wavefunction realist is not concerned with explaining the evident difference between the classical world and the ψ-quantum world, and asserts an illusionary status to the former in the face of a reality of the latter, then they must at least clarify the scientific power of CM. Because CM is still widely utilised by scientists, in quite a successful manner! Therefore, the advocate of wavefunction realism should have to account for how QM will recover the quantum description of the models of CM, without appealing to CM at all. They should account for their selective attitude: instrumentalism attitude to CM and realism commitment to QM. According to the Received View, it is one theory or the other, but the possibility of both quantum and classical aspects being equally and simultaneously real, is not conceivable within any of the well-known interpretations. The Received View considers a hierarchy of fundamentality that orders the theories and their reality. In the case of previously considered interpretations, the hypothesis is used to claim that QM has to explain CM. In the case of wavefunction realism, the methodology of the Received View is used to claim that classicalities are less real, favouring the quantum one. In both cases, there is a strong commitment describable in terms of intertheory relations: the statements are justified in terms of how QM and CM and their associated real content relate with each other.

However, I point out an aspect of wavefunction realism that does agree with the methodology derived from Core Realism. Wavefunction realism asserts the reality of the wavefunction independently, and not relative to, the reality of classical appearances. Nevertheless, to claim that the wavefunction is what is real according to QM, does not deliver an answer to the realist/interpretational question 'what is the world like according to the theory?'. That claim as an answer to the interpretative question immediately raises a further question: how are we to understand that the wavefunction is real? So far wavefunction realism has not managed to tackle this realist question. Wavefunction realism constrains itself to justifying the claim that the apparent familiar 3D-world is an illusion, and this problem is captured by the type of issues appearing in the Received View.

2.2.6 Modal Interpretations

During the 1970s, Van Fraassen (1973) put forward his 'Copenhagen variant' of a modal interpretation (MI), which evolved into the final version in (Van Fraassen 1991). His view motivated other authors to develop other MIs, such as, (Dieks 1988), (Kochen 1985), (Healey 1989), (Bacciagaluppi 1995), and (Bub 1992). A strategy to classify them in terms of metaphysical assumptions has been put forward by de Ronde (2011), distinguishing those variants that start from metaphysical assumptions from those that start from the orthodox formalism.[24] The differences between the variants is manifested in two aspects: the underlying realist or anti-realist attitude towards science – as both camps have had advocates of MI –, and in the notion of modality, which has no agreed-upon meaning.

However, there are significant elements in common to all MIs. Vermaas (1999, 23) generally characterises MIs by the following properties: MIs stay close to the standard formalism of QM. They maintain that the quantum mechanical description of a system A is defined on a Hilbert space $\mathcal{H}^{\mathcal{A}}$. And, magnitudes of A are represented by self-adjoint operators $\hat{O}_A$ and the state of A is given by a density operator ρ_A. Secondly, the standard projection postulate is rejected in all variants, whilst the Schrödinger equation is maintained at all times, even when measurements are performed. Thirdly, MIs do not take QM as the theory of the microscopic realm, but to nature as a whole. Fourthly, MIs give rules to ascribe properties to systems at all times. The meaning of the states is

[24]Orthodox formalism means the Hilbert space formulation of QM without the collapse postulate, which is considered ad-hoc.

given in terms of the physical properties possessed by systems and not merely in terms of outcomes of measurements. Fifth, the property ascription rules do not simply ascribe one set of properties (in contrast with the eigenvalue-eigenstate link), but a number of sets of properties are ascribed together with the associated probabilities. Each set prescribes properties possibly possessed by the system and the probabilities that these properties are actually possessed by the system. A final common characteristic is that the probabilities that MIs use to ascribe properties to a system, are taken as representing ignorance about the actual properties of the system only, not about the state of system.

MIs were heavily criticised in the early 2000s and today are considered to be an obsolete research programme.[25] The main criticism is related to a mismatch between the 'definite properties' specified by MIs and the quantities selected by decoherence. This point was mainly developed by Donald (1998) and Bacciagaluppi (2000).[26]

However, from the point of view of Core Realism, one could dissolve the criticism by denying that there has to be a match, or even further, suspend judgement on the intertheory relations involved in the 'recovery of classical states'. Remarkably, a main component in the decoherence programme was the attempt to obtain classicalities from the quantum. In a way, the motivation for the theory of decoherence is to get rid of the unacceptable quantum superpositions and 'quantum weirdness'.[27] And this is captured by the framework of the Received View. Hence, the strength of the argument that MIs should be abandoned, because MI obtain results which conflict with decoherence, depends on whether one agrees with the Received View or not.

From the point of view of the Received View, an interpretation of QM should explain the appearance of classicalities. Therefore, conflicts with decoherence are not to be tolerated, for decoherence is the accepted explanation for the delocalisation of the quantum interference and the appearance of classical states. But if the Received View is abandoned, the argument against MI will not bite so hard. A shift to a different philosophical framework like Core Realism, whereby questions of the appearance of the classical from the quantum are not of utmost importance, could accommodate the research project of MI. Indeed, in Core Realism this is not a priority, being the central question to account for the quantum states in realistic terms. The attempt to interpret the theory without preconceptions on what a physical theory must satisfy is not part of the Received View, and then MIs are not obsolete: to associate a modal non-classical aspect with physics seems to be an interesting proposal, which can be followed outside the 'mandate' of restoring classicalities. Such an attempt is being explored independently by the likes

[25]For instance, in a general review on interpretations of QM, Putnam (2005) does not even mention MIs.

[26]I am grateful to Bacciagaluppi (2014) for clarifying this to me.

[27]I will argue for this in sufficient detail in Chapter 3.

of de Ronde (2016) and Kastner (2012), who associate a real content of QM with a non-classical notion of modality.[28]

2.3 Conclusions

Today, there is no agreement on which, if any, of the several known realist interpretations of quantum mechanics is the correct one and the various interpretations are not mere intellectual opinions. If taken seriously, they literally put forward incompatible world-views, such as the existence of many worlds in a quantum superposition, particles with trajectories in a configuration space and spontaneous collapses, which conflict.

In this chapter I have critically engaged with the realist interpretations by questioning whether, despite their evident differences, there is something relevantly common to them all. I established an analytic tool by conceiving a Received View of the Realist Interpretation of QM. From this vantage point, I analysed and discussed the well-known many world interpretations, Bohmian mechanics, GRW, wavefunction realism and modal interpretations, in order to show that their common features can be captured by the Received View.

This conception of the Received View is novel and it emphasises the role of intertheory relations within the main realist interpretations of QM at the metaphysical and epistemic levels. In the Received View there is the idea that a metaphysical relation holds between the objects of QM and the everyday objects of CM: 'tables and chairs reduce to, are grounded on, are composed by, supervene on, elementary particles'. At the epistemic level, the Received View considers that a theory reduction relation exists between QM and CM, because the former is more fundamental than the latter. With this framework one can see that the Received View takes it that 'any realist interpretation of QM has to explain the classical appearance of the world'. This is, essentially, a recipe to interpret QM: it states intertheory relations prior to the interpretation of QM and it places the ontology of QM subsidiary to account for macroscopic objects. Furthermore, this recipe says that any interpretation of QM ought to focus on responding to the complex relationship QM-CM. Indeed, I have argued that the well-known measurement problem, which is at the centre of most realist interpretations, can be seen as a problem of intertheory relations. That partly explains why the QM-CM limit is so relevant in the literature. This leads to Chapter 3, where I will critically engage with the account of the intertheory relation QM-CM both in the philosophical literature and in the foundations of physics.

[28]I do not engage with these with these projects here.

By contrast with the Received View, I presented a weaker realist framework. I put together the most basic elements of a form of scientific realism that looks at QM, which I called Core Realism. I have outlined the differences between these two frameworks. I claimed that, actually, the core focus of realism in regards to QM is the interpretation of the theory. This is expressed by questions such as ‘how could the world be the way QM says it is?’, or, ‘what is a quantum state?’. In addition, I noted that, in itself, Core Realism includes no recipes for conceptualising intertheory relations. By contrast with the Received View, in Core Realism the questions of intertheory relations QM-CM can be addressed once the interpretation question is resolved, but not before. However, Core Realism is not sufficient to be considered a form of realism. Core Realism ought to be supplemented further. I will return to this in Chapters 6 and 7.

Chapter 3

Intertheory Relations: Theory Reduction and the QM-CM Limit

3.1 Intertheory Relations: Useful Distinctions

In Chapter 2 I outlined the basis for a form of scientific realism called Core Realism, built upon very basic realist intuitions and aimed at approaching physical theories like QM. I characterised its methodology as a view that does not entail commitment to any specific approach to intertheory relations, and where the main priority when faced with a physical theory is to provide an interpretation of it, prior to attempting to address a relationship with other known theories. In addition, I also discussed that, to some extent, broadly accepted forms of realism can be seen as aligned with the view that I called the Received View of the Realist Interpretation of QM. The Received View is characterised by two statements regarding intertheory relations: one statement at the metaphysical level, and one statement at the epistemic one. Furthermore, I assessed the most well-known and advocated realist interpretations of QM as operating within the Received View. This is because, in a nutshell, their central concern is with providing a realist account of QM as an explanation of the appearance of the classicalities.

In this chapter I will discuss accounts of intertheory relations in the philosophy of science that are relevant for the Received View and the account of QM-CM relation, such as Thomas Nickles' reduction$_2$ and Heinz Post's General Correspondence Principle. Then, I will bring to the fore the devices articulated in physics, such as mathematical limits, Ehrenfest theorem, Wigner function and decoherence, which to a large extent were specifically designed to account for the appearance of the classical from the quantum. That is, they were designed to account for the QM-CM relation. I will critically engage with them and present objections to their soundness, raising worries for the Received

View. In addition, I will recall the criticisms raised by Bokulich as an argument for the need of a novel account of the QM-CM limit and I will contribute to that debate by expanding the range of criticisms. The take-home message is that, in view of those criticisms that I will recall and articulate, it shall be concluded that such devices within the relationship QM-CM do not stand on their own feet but are relevant relative to the Received View. Thus, this chapter will motivate not only a novel view of intertheory relations that addresses the relationship QM-CM, such as Bokulich's interstructuralism, but a deeper reflection on what a realist view on QM should account for.

In Chapter 2 I characterised the most well-known interpretations of QM – many worlds, Bohmian mechanics, wavefunction realism, GRW and modal interpretations – in terms of the framework that I called the Received View of the Realist Interpretation of QM. I described such a view by a statement of metaphysical character, which is the hypothesis that a certain type of (metaphysical) relationship accounts for the relation between the objects at the quantum level and the objects at the classical level, and by a statement of epistemic character, broadly similar with theory reduction.[1] However, I limit the breadth of my approach to analysing in more detail the epistemic intertheory relations, leaving the discussion of the metaphysical alternatives for future work.

I emphasise that my background research interest is with the realist account of QM: the question of what world is according to QM, what that amounts to and, ultimately, what a quantum system is meant to be. Consequently, it shall be noted that a necessary working hypothesis that I adopt is the conclusion of the previous chapter. That is, that we do not fully know what a quantum system is. This is evidenced by the disagreement in the literature on the meaning of QM and by the lack of an agreed-upon metaphysical view which articulates a quantum ontology. If these questions had clear answers, there would be little to discuss.

A central issue in philosophy of science is concerned with accounting for the way scientific disciplines and their theories relate to each other. Indeed, relevant research has been and still is being undertaken with this type of question at the centre, e.g. (Bokulich 2008a; Fletcher 2014; Rosaler 2013). As Butterfield indicates, there are two plausible broadly construed intuitions of reduction or pluralism,[2] both equally capable of being developed in epistemic (explanation) or metaphysical terms (identity of entities or properties):

[1]As it will become evident, upon the failure of reduction other views have been put forward, such as 'emergence', and others. I do not intend to address them all in detail. Broadly they are similar in the effect that one theory is less fundamental and relative in some way to the more fundamental one. Hence one could defend that CM is a simple case of QM, or that CM emerges from QM, or that CM supervenes on QM, or a mixture of them. These relationships are not all the same, but overall they are different to a view whereby a priori no relationship has be had, but one could be had as found a fortiori.

[2]In this chapter I will consider the main forms of theory reduction and I leave the analysis of relevant forms of pluralism for Chapter 7. Bokulich (2008a) views it that the mainstream view on intertheory relations falls within pluralism, but I am not convinced by her arguments. Instead I hold that in the physics community different theories are typically considered to be related by a form of reduction.

> One of the tasks of philosophy of science is to assess how well integrated our theories are. Indeed: are they integrated enough in terms of notions like explanation and the identity of theoretical entities or properties, that taken together they merit the metaphor 'raising edifice', rather than 'shambolic patchwork'?!
>
> (Butterfield 2011a, 930)

A widely accepted view tells us that psychology "reduces to" or "emerges from" neurosciences, that the latter is similarly related to biology, which then can be related to chemistry, and so on until one recognises that all sciences can be recovered from physics.[3] As I attempted to show with the conception of the Received View, a similar intuition is widely taken in physics. Particular cases in intertheory relations consider, for instance, the reduction between special relativity and Newtonian mechanics (CM).[4] This is essentially based on the mathematical fact that the Galilean transformations tend to Lorentz transformations when the ratio $v^2/c^2 \to 0$. Another example – yet not uncontroversially accepted across the board – is the relationship between classical statistical mechanics and thermodynamics: from considerations of the relevant case-dependent type of ensemble, one can recover laws of thermodynamics in terms of average values of statistical functions. However, Butterfield's question recalled above is very general and unapproachable without first introducing useful distinctions. The following distinctions will help me to advance an analysis and consider the topic of intertheory relations, by yielding a strategy that can question relevant forms of intertheory relations in the literature.[5] These distinctions will be the key to frame the type of philosophical reflection I pursue.

Consider that intertheory relations can be held to account for relationships between theories and between scientific disciplines. That is, a specific view of intertheory relations can be specified as to whether its target is to relate theories within a particular science or whether it looks at different sciences: a distinction by subject matter. Furthermore, an intertheory relation can be found between either theories or sciences that belong to the *same* historical period at the current time or in the past, or between theories or sciences from *different* historical periods. The selection can be made depending on whether one takes a historical perspective or whether the physicists of the relevant time still consider such theory. For example, one could attempt to relate heliocentric astronomy

[3] I understand this type of comment might trigger questions from other disciplines in philosophy, such as the question whether mental states are physical or not, and the like. I do not intend to engage in those discussions here, as will become clear soon.

[4] For my purposes I will overlook the nuances of Newtonian mechanics in relation to the currently accepted versions of CM. I will consider the different variants of CM all the same.

[5] These distinctions are suggested in Bokulich (2008a, Ch.1) and I am using them in a systematic manner.

and geocentric astronomy, regardless of the fact that today heliocentric astronomy is the dominant view. Or, one could look at current astronomy (involving considerations of cosmology within quantum or relativistic theories), in relation to historically previous astronomies, by considering the practice of astronomers. Thus, this distinction is in terms of considerations of time and development.

Therefore, we have the following:

1. **Subject matter:** Considerations of intertheory relations can be specified in terms of the epistemic object of interest. And there are two main subject matters: scientific disciplines and scientific theories. Each carries associated research questions:

    - *Scientific disciplines.* Science can be seen as comprising different disciplines, such as physics, biology, psychology, and many more. How are the different scientific disciplines related? Does psychology reduce to physics? Is biology more fundamental than chemistry? Can we currently make a clear distinction between physics and other 'physical disciplines' such as chemistry? These types of question involve a level of analysis which is different to the next level, as discussed above.
    - *Scientific theories.*[6] Theories are conceived within a particular science. There are questions on intertheory relations that focus on theories rather than on scientific disciplines: QM and CM are two physical theories, how do they relate to each other? Or, how are evolutionism and molecular biology related?

2. **Development of science with time:** the considerations here concern with developing relations between the scientific disciplines or theories focusing on two different temporal scales.

    - *Diachronic or horizontal relations.* This type of considerations might be invoked when the target is of a historical character. A question framed in this category could be: How plausible is it to conceive (theory T_1, science S_1) as a successor of (theory T_2, science S_2)? Is (T_1, S_1) the predecessor to successor (T_2, S_2)? Analogously, there is the following other type of approach.
    - *Synchronic or vertical relations.* Here we are approaching an analysis of how different theories or sciences relate with each other at one particular time-slice and describing processes at different levels. Do current psychological theories reduce to current neuroscience? Is the theory of medicine during

[6] I understand there is a debate over how to individuate theories. Indeed, are they axiomatic systems following the syntactic approach, or are they classes of models, following the semantic approach? I think that both camps can be identified with the distinction between theory and discipline at some discursive level. Hence, I shall not clarify this vagueness nor specify yet what I take a theory to consist in. I will discuss further on 'theories' in Chapters 6 and 7.

the Enlightenment reducible to, or emergent from, the biology at that same historical time?

Needless to say, the different types of considerations are not completely independent of each other. Typically a view will include a predominant element from the first group, mixed with a predominant one from the second one. Yet, a view on intertheory relations will typically emphasise one aspect of the analysis over the others. This can be illustrated, for example, by Kuhn's controversial proposal involving paradigm shifts and the subsequent incommensurability between paradigms. At one time slice the scientist working within the pre-crisis and revolution paradigm cannot – according to a possible reading of Kunh's view – communicate with the scientist who changed to the new post-revolution paradigm. That suggests a synchronic type of analysis, for the considerations involve, broadly speaking, the same period of time. And, as per the subject matter, it involves considerations predominantly at the level of scientific disciplines, and perhaps less predominantly at the level of scientific theories. Another example is the well-known work of Fodor (1974), who defends the autonomy of the special sciences from physics. Fodor's discussion also considers addressing the relationship between different sciences at one period of time, as it ignores the relationship between different theories within one discipline and neither does his discussion focus on the historical development of sciences. However, one could well take an approach that emphasises development of the subject matter over time, in order to present, for instance, QM as a successor of CM. That could be motivated by the historical consideration that the development of the latter was initially suggested by the failures of the former (think of the very beginning of QM with Planck's formula for the radiation of a black-body).

In short, to summarise the distinctions made above, it seems useful to conceive four types of possible approaches to intertheory relations that the philosopher can pick and choose from. This is obtained by combining the ingredients shown in Table 3.1. The emphasis or predominance of one of the combinations will, of course, determine the scope and limits of the discussion. In future, I will use these distinctions to engage with views on intertheory relations and I will use these distinctions to articulate my view too.

	Diachronic-horizontal	Synchronic-vertical
Special sciences and physics		
Theories within a science		

Table 3.1: Pick 'N' Mix table for developing a view on intertheory relations.

Furthermore, there is another distinction that should be appreciated. This was hinted at with the characterisation of the Received View of the Realist Interpretation of QM. Given a form of intertheory relationships, one can question whether it concerns with metaphysics, with the way objects of different subject matter relate, or whether it articulates an epistemic relationship about the theories that give us knowledge of the world.

In regards to the metaphysical dimension, we are considering the relationship between the ontic content that the theories latch onto. For instance, one could claim that tables and chairs (classical objects) are nothing over and above compositions of elementary particles (quantum objects); or that properties at the classical level are ontologically reducible to properties of the quantum system which is made of. And, this is degenerated by the fact that such distinction can be made to apply to the distinctions made above between subject matter, and development and time. For example, one could ask whether biological entities result from more fundamental quantum objects. Or whether there is a mind that is more than what is accounted for physics. These questions are broad philosophical questions and I just intend to note that they entail a view on intertheory relations.

By contrast, one could articulate a view within an epistemic dimension. That is, a view on intertheory relations could remain neutral in relation to metaphysical statements and propose an analysis in terms of heuristics and justification. For instance, one could question whether novel theories developed from the failures of previous theories. One could assess whether there is a recipe for the conception of theories by arranging pairs into a successor-predecessor relation. Or, one could question whether there is a unique form of explanation throughout physical theories or whether there is a plurality of forms explaining phenomena. Additionally, it could be questioned whether one phenomenon is explainable by a plurality of explanations or just by one.[7] These are also questions that pertain to intertheory relations.

With this distinction between metaphysical or epistemic issues, Table 3.1 can be extended to consider possible combinations. I show these in Tables 3.2 and 3.3.

Now I can specify the central questions of this chapter and their boundaries in greater detail. My interest is in the relationship QM-CM at the current period of time. And, my analysis will focus on critically engaging with the question of the relationship between two theories that are, to some extent, successful at different levels of description, without

[7] Scientific explanation is a vast topic in the philosophy of science and I do not engage with it here. I will do so in relation to specific quantum phenomena in Chapter 5.

Metaphysical range	Diachronic-horizontal	Synchronic-vertical
Special sciences and physics		
Theories within a science		

Table 3.2: Pick 'N' Mix table for developing a view on intertheory relations within a metaphysical dimension.

Epistemic range	Diachronic-horizontal	Synchronic-vertical
Special sciences and physics		
Theories within a science		

Table 3.3: Pick 'N' Mix table for developing a view on intertheory relations in epistemic terms.

one being completely replaced by the other,[8]. Thus, I consider neither the relationship between the special sciences and physics, nor adopt a historical perspective. This helps to clarify that, for instance, I will not weight heavily the historically motivated reasons to think that QM is a successor of CM.

Furthermore, although my ambitions lie ultimately with metaphysical questions about QM, most of my discussion will engage with relevant epistemic issues. This serves to limit the scope of this discussion and does not reflect what I consider to be interesting areas of inquiry.

Now, recall that in Chapter 2 I argued that the defining characteristic of the standard take on forms of realism that engage with QM is the assumption of an epistemic judgement on intertheory relations – one of the elements in the Received View of the Realist Interpretation of QM. This translates to claiming that QM is more fundamental than CM, and relatedly that QM is a successor of CM. There are metaphysical considerations too. This is reflected, for instance, by Landsman (2007, 513-514), who adheres to the "modern idea that quantum theory is universally valid and the classical world has no absolute existence". As discussed in Chapter 2, such a view has it as a central motivation that QM ought to account for CM. Hence the relevance of the QM-CM limit. But that has to be contextualised within the philosophy of science.

[8]For, regardless of the view of intertheory relations one could deploy, it is undeniable that classical physics is still both in development and being used by scientists. And evidence for this will be discussed in detail in Chapters 4 and 5 in the context of modern semiclassical mechanics.

Therefore, this chapter continues in Section 3.2 by discussing relevant philosophical frameworks that are most appealed to by the advocate of the Received View on the Realist Interpretation of QM, that is, theory reduction. In Section 3.3 I will turn to critically engage with what the physicists (and philosophers of science) refer to as the QM-CM limit. In Section 3.4 I will reflect on the outcomes of the analysis in previous Sections, and I will note three types of reactions in the field. Finally, the conclusions in Section 3.5 will summarise what this chapter has dealt with, and how to continue forward.

3.2 Theory Reduction

In this section I will review the main forms of theory reduction that are pertinent to the case at hand, that is, the relation between QM and CM. Let us recall the distinctions made on page 36, Section 3.1, where I established the scope and limits of my analysis of intertheory relations as focused on the relationship between two theories (in contrast to the relationship between different scientific disciplines), which are considered concurrently today (by contrast with a consideration of historical relevance in terms of the progress of science).

Within the topic of theory reduction, I will discuss Nagelian reduction, Nickles reduction$_2$ and Post's heuristic General Correspondence Principle. The motivation for each will be articulated within each subsection, yet I will anticipate them now: Nagelian reduction is arguably the first and most well-known model of theory reduction, and to a great extent further forms of reduction appeared as a reaction to it. In turn, the literature takes Nickles reduction$_2$ to express the best philosophical account of the relationship QM-CM, see Bokulich (2008a). Finally, I will discuss Post's work on intertheory relations presents rarely considered similarities with Nickles' view.

However, before going into each of these views, it is worthwhile commenting on a known conflict about the jargon associated with the term 'reduction'. As pointed out by Nickles (1973, 181), by Butterfield (2011a) and others, contrary senses are assigned to reduction in philosophy and in physics, see Figure 3.1: Given a less fundamental 'high-level' theory T_t (theory at the top) and a more fundamental 'low-level' theory T_b (theory at the bottom):[9] Physicists view more fundamental theories as successors to less fundamental predecessors, and they typically view T_b (more fundamental) being reduced to T_t (less fundamental). For example, special relativity reduces to classical mechanics when the speed of the system (v) is negligible compared to the speed of light c. Instead, for

[9]Actually, the seminal work on reduction by Nickles (1973) was motivated by distinguishing these two senses of reduction.

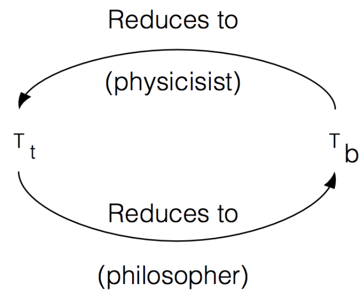

FIGURE 3.1: Difference in the jargons in philosophy and in physics.

the philosophers it is not that accurate to include predecessor and successor labels, as reductions here can be across different domains (that being clarified, I will keep the labels successor and predecessor): "T_1 reduces to, or is reducible to, another T_2 if, roughly speaking, T_1 can be shown to be part of T_2" (Butterfield 2011a, 927). That is, in the philosophical tradition, the less fundamental theory T_t reduces to the more fundamental theory T_b. This can be visualised in Table 3.4.

	Theory top (T_t)	Theory bottom (T_b)
Historical development	Predecessor	Successor
Fundamentality	Less fundamental	More fundamental
Hierarchy of phenomena	Higher level	Lower level
Example	Newtonian mechanics	Special Relativity

TABLE 3.4: Summary of the naming of theories in the traditional reductionist fashion.

3.2.1 Nagelian Reduction

The arguably best-known and most traditional form of reduction is the Nagelian model. Presented in (Nagel 1979), it has provoked significant debate in the literature. Typically, Nagelian reduction involves a notion of derivability. That is, a reduction occurs when a coarser theory is derivable from a finer theory, i.e. the former can be deduced from the latter. In the terms of the distinctions pertaining to intertheory analysis on page 36 in Section 3.1, Nagelian reduction can be used to account for relationships in both subject matter cases: across different scientific disciplines (say, psychology and physics), and across different theories within one discipline (say, thermodynamics and statistical mechanics). Furthermore, it serves purposes for both epistemic and ontic accounts of the reduction in the Received View, see page 13. This form of theory reduction is an ambitious framework that attempts to cover as much philosophical terrain as possible.

In Nagelian reduction, theory T_1 is reduced to theory T_2, if T_2 can provide a proof of all the theorems involved in T_1. In this case, the reduction represents a deductive explanation of the reduced theory, (Nickles 1973, 184).

Reviewing the topic of theory reduction, Walter and Eronen (2011) recall the two necessary conditions that must be satisfied in the Nagelian model:

a) Condition of connectability: The terms of T_1 are connectable with the terms of T_2 by means of a set D of suitable bridge-laws. That is, by empirical hypotheses which express material rather than logical connections. Hence, T_1 becomes a sub-theory of the augmented $T_2 \cup D$.

b) Condition of derivability: Given these connecting principles, all laws of T_1 can be derived from laws of T_2. Hence, theory T_1 is reduced to T_2, or in other words, T_1 becomes a definitional extension of T_2, when a set D of bridge laws is added to T_2 such that T_1 becomes a sub-theory of the augmented $T_2 \cup D$. T_1 is said to be derived from T_2.

Now, there are two variants of reduction: homogeneous and heterogeneous. If the bridge laws are not necessary for the reduction to obtain, then Nagel termed such reduction as "homogeneous", whereby the descriptive vocabulary of T_2 is straightforwardly included in the descriptive vocabulary of T_1. Otherwise, when the descriptive vocabulary of T_2 is not straightforwardly included in the vocabulary of T_1 and there are new terms, the bridge-laws are required and Nagel termed this case as "heterogenous reduction". For example, Galileo's law for freely falling bodies is homogeneously reduced to Newtonian mechanics and gravitational theory, because the specific subject matter in the former is present in the premises of the latter. Two typical examples of heterogenous cases are: the reduction of thermal laws to the kinetic theory of matter and the reduction of some laws of chemistry to QM.

In turn, heterogenous reduction is more complex than the homogeneous case and there are three typical ways to approach it: the instrumentalist proposal, the correspondence proposal and the replacement proposal. The instrumentalist proposal is characterised by a rejection of epistemic realism, whereby theories or scientific laws are neither true nor false, but merely rules for inferring observation statements. Hence, for instance, the kinetic theory of gases is considered just a set of rules for predicting the pressure of the gas, given changes in other variables, but it is not identified with an account of the gas. Within the correspondence proposal, in contrast, terms are taken to have meanings that are independent of the theories in which they are formulated. Then, bridge laws allow for the connection between terms that are distinctive of one of these theories.

In the final alternative, the replacement proposal, the function of bridge laws in the reduction is rejected and as a result, the whole ontology is replaced. Here, a change in the theory entails a change in the meanings of all its terms. This last approach to heterogeneous reduction was defended by Feyerabend but Nagel (2008) rejected it. Nevertheless, further analysis of this topic does not concern us here.

Overall, Nagelian reduction has fallen prey to strong criticisms and it is widely rejected.[10] From its derivational aspect, assuming the reduction of T_1 to T_2 is successful, Nagelian reduction implies that T_1 is derivative of T_2. This would also mean that T_1 is regarded as false. Consequently, we have a logic implication where the antecedent is equally false, as deduction is truth-preserving.

Nagelian reduction and the associated criticisms can be analysed by using the distinctions I made in Section 3.1. Firstly, I distinguished between views on intertheory relation by their focus on subject-matter: inter-theories within disciplines or inter-disciplines. Secondly, I distinguished the considerations in terms of time, by synchronic or diachronic types of intertheory relations. Finally, I considered whether the focus is epistemic or ontic.

Indeed, the strongest criticism of Nagelian reduction is the argument of multiple realisability. There are arguments of multiple realisability that attack the Nagelian reduction in both types of subject-matter. In regards to the relationship between scientific disciplines, there is the famous argument initially by Putnam (1967) and Fodor (1974). This argument concludes that the alleged relationship between psychology and neuroscience cannot be addressed within this scheme by observing, for instance, that one psychological kind, like pain, can be realised in a variety of significantly different physical kinds, like in the brain of a human, an animal, or in an artificially intelligent subject's electric circuits. Fodor (1974) then denied the possibility of type-type reduction – that is, reduction at the level of the properties – which is needed by Nagelian reduction, although Fodor does accept token-token reduction – that is, a reduction at the level of the events, that every natural event is a physical event.

Multiple realisability can also be used to criticise Nagelian model as a relationship between theories. Batterman (2014) identifies multiple realisability in the case of physics, where he considers the phase transitions (liquid-gas, solid-liquid, and so on) coexistence curve, obtained by plotting temperature vs. density of a fluid. The multiple realisation is in that the curve obtained for materials with different micro structures is the same for each fluid at its critical value for density and temperature.

[10]For a survey on criticisms to Nagelian reduction, see (Sklar 1993), (Bokulich 2008a, fn. 6, 7, 8; 141) and (Dizadji-Bahmani et al. 2010, 400-ff).

There is an intense debate over the role and importance of multiple realisability as an argument against Nagelian reduction. Sober (1999) attempts to put forward that it does not undermine a Nagelian-type reduction (essentially characterised as one that involves *explanation*), and this has been recently also advocated by Butterfield (2011a), and Dizadji-Bahmani et al. (2010). However, Batterman (2014) argues that Nagelian supporters do not succeed in overcoming this issue.

Nevertheless, there is a further criticism to the Nagelian model as a relationship between theories within one scientific discipline. It is the warning that the derivation relation between thermodynamics and statistical mechanics in physics would be extremely complicated to obtain and perhaps only valid under idealised conditions. Walter and Eronen (2011, 141) review several arguments here. They recall that, for example, thermodynamical concepts such as 'entropy' are not even associated with unique concept of statistical mechanics, but with a variety of concepts that do not exactly correspond to thermodynamic entropy.

Finally, following my distinctions to analyse a view on intertheory relations, we could question whether Nagelian reduction articulates an intertheory relation in either or both epistemic or metaphysical aspects. Nickles (1973, 183) classifies Nagelian reduction as involving an ontological relation, because it combines the ontic domain of theories, and the reduction becomes associated with 'elimination', 'trimming down', 'consolidation', of a specific domain.[11] However, as Butterfield (2011a) articulates, Nagelian reduction also works at the level of explanation, indicating that it is an epistemic dimension. For these reasons I think that Nagelian reduction can be used either at an epistemic or metaphysical level, or both. I summarise this analysis in Table 3.5.

Before going into what Nickles labelled as reduction$_2$, it is worth recalling that the topic of reduction is very broad in philosophy, broader than debates within the philosophy of science, and thus broader than the discussion of the QM-CM relation. As I presented it, Nagelian reduction appears in the literature as a general framework, to which novel forms of reduction are related or built upon. Therefore, there are debates that I leave out of my analysis. For instance, there is the so-called 'New wave' reduction, see (Walter and Eronen 2011) and references therein. This form of reduction involves logical derivations between theories and it is meant to be a general model of reduction that avoids reference to bridge laws. On the one hand, it has been argued that this is not different enough from Nagelian reduction and, on the other hand, New wave reduction is best associated with topics within the philosophy of mind, with the likes of Kim (2000) and others. However, my analysis here concerns with the case of the pair QM-CM. Now let us turn to the

[11]Nagelian reduction is included in what Nickles (1973) calls reduction$_1$ (the subscript will soon become meaningful).

	Multiple realisability against Nagelian Reduction
Across scientific disciplines	Putnam (1967) and Fodor (1974): pain, one psychological kind, can be realised in many physical kinds (brain of a human, an animal, or in an artificially intelligent subject's electric circuits.
Across theories within one science	Batterman (2014): phase transitions (liquid-gas, solid-liquid, etc.) shows coexistence curve for micro-structurally different materials is the same for each fluid at its critical value for density and temperature.

Table 3.5: Summary of the criticisms against Nagelian reduction in terms of distinctions (columns) articulated in Section 3.1.

second form of theory reduction, Nickles 'reduction$_2$', which is particularly relevant for the purpose of analysing the case of the QM-CM limit.

3.2.2 Nickles Reduction$_2$

It is typically agreed that the view that allegedly best approaches the QM-CM relationship is associated with a version of what Nickles (1973) dubbed 'reduction$_2$'. The work of Nickles not only develops a 'physicist' or 'scientist' reduction, but it is articulated in contrast with Nagelian model. Nickles considers Nagelian reduction within his 'reduction$_1$'. Yet, as the subscripts 1 and 2 suggest, reduction$_2$ is not conceived as the converse or opposite of reduction$_1$ (opposition would have been indicated by something like 'reduction$_p$' and 'reduction$_{\neg p}$'). Reduction$_1$ and reduction$_2$ are two different ways of providing an intertheory relationship. By presenting these two types of reduction, Nickles aimed at shifting conceptually away from a form of reduction that focused on logical derivation, to a one that would capture better the goings on in physical sciences.

In reduction$_2$, the successor theory T_2 is said to reduce to its predecessor T_1,[12] under "mathematical limiting operations and other appropriate transformations" (Nickles 1973, 181). Nickles characterises this reduction as 'domain-preserving', in the sense that "reduction$_2$ shows the successor theory to account adequately for the structured domain of phenomena inherited from its successful predecessor" (Nickles 1973, 185). Reduction$_2$ involves a non-deductive 'derivation' of one theory from the other, absorbing it, but without discarding the predecessor theory as incorrect. Yet, whilst there is a

[12]Recall the jargon diagram in Figure 3.1. In this physicist sense, the successor reduces to the predecessor.

logical consistency between the reduced and reducing theories, they can be concurrent theories and justified independently, so Nickles articulates it.

In my reading of (Nickles 1973), he conceived the central characteristic of reduction$_2$ as involving a number of different, noncompeting ways, whereby different limits (and other mathematical operations) are taken. That is, the signature of reduction$_2$ is not just the mathematical limit in itself, but the inclusion of a variety of methods that work in a noncompeting manner (which yet can be expressed in mathematical limits).

Nickles compares his proposal against Nagelian reduction. Whilst Nagelian model required a derivation of all the theorems in one theory proved in the other, reduction$_2$ does not necessarily obtain 'completely': "only very rarely will all the equations of T$_2$ reduce$_2$ to equations of T$_1$ under the [mathematical] operations" (Nickles 1973, 197).

Furthermore, he discusses:

> "[Reduction$_1$] is the achievement of postulational and ontological economy and is obtained chiefly by derivational reduction as described by Nagel... [Whereas reduction$_2$ is] *a varied collection of intertheoretic relations rather than a single distinctive logical or mathematical relation.*"
>
> (Nickles 1973, 181, my italics)

In particular, the theoretical devices in physics traditionally developed to account for the QM-CM relation can be captured by reduction$_2$. Here we find the mathematical limits (for instance, $h \to 0$), Ehrenfest Theorem, Moyal brackets and, perhaps more importantly, decoherence. These devices are not taken as evidence of a reduction relationship between QM and CM, they *are* the reduction relation. I will discuss them in sufficient detail below.

Now, in terms of my distinctions to approach intertheory relations in page 36 in Section 3.1, Nickles conceives his reduction$_2$ as pertaining to the relationship between different (physical) theories within a specific discipline, and not as pertaining to inter-disciplines. In turn, in terms of the consideration of time and progress, Nickles' analysis pertains to concurrent theories at the same period of time, therefore falling into the synchronic type. Finally, there is the distinction between an ontic or epistemic dimensions. Which type does reduction$_2$ articulate? That is, could the realist derive metaphysical claims from a case of reduction$_2$? I claim that she could not, because the function of Nickles' reduction is epistemic. In his words,

> Rather than to effect ontological and conceptual consolidation, the main functions of reduction$_2$ are justificatory and heuristic. The development of

> new theoretical ideas is heuristically guided by the requirement that these ideas yield certain established results as a special case (e.g., in the limit), and they are often quickly justified to a degree by showing that they bear a certain relation to a predecessor theory.
>
> (Nickles 1973, 185)

That is, a central component in this form of reduction is that the development of the successor is upon, or relative to, presenting a relation to the predecessor, and the justification of the former is dependent on the establishment of a relation with the latter. That is the heuristic conception of the reduction. By contrast with the above discussed reduction$_1$ (the philosophical reduction, or Nagelian reduction), reduction$_2$ does not involve any commitments with ontological implications. Therefore, the realist ought to obtain her claims of metaphysical character from elsewhere. Table 3.6 summarises the contrast between these two forms of reduction in their epistemic and ontic aspects.

	Epistemic intertheory relation	Ontic intertheory relation
Nagelian reduction$_1$	Derivation of one theory by the other: all theorems in predecessor can be proved in successor.	Explanation infers ontological consequences: domain-combining.
Nickles reduction$_2$	Heuristic and justification: predecessor sets a guide of development of successor upon the latter establishing a relationship to the former mathematically expressed in various non-competing ways.	Domain preserving, no replacement of one domain by the other. No consequences.

Table 3.6: Summary of the contrasts between Nagelian and Nickles' reductions in terms of the dimensions of the Received View from Chapter 2.

Nickles' reduction has received criticisms. Of course, Bokulich represents one of the critics and I will devote a full chapter to engaging with her view. However, it is worth briefly discussing another novel framework. Rosaler (2013, 13, 18) argues that the mathematical limits-oriented reduction articulated by Nickles does not suffice to account for the reduction QM-CM, and develops his own view. He does this partly through criticising Nickles, but, evidently, building upon Nickles' view. Rosaler's framework has two central characteristics: Firstly, it is 'local' in that the reduction is context-specific, without requiring one 'global' derivation across the entire domain. Secondly, it entails an 'empirical', rather than 'formal', relationship between theories/models: "While it is often supposed that reduction in physics is solely a feature of the mathematical or logical relationship between two theories or models, the question of whether one representation succeeds at describing the world in all cases where the other does often has a strong empirical component" (Rosaler 2016, 58). Now, his formulation is based on asserting that the reduction between two theories, reduction$_T$ is relative to a reduction between models, reduction$_M$. The reduction at the theory level occurs if and only if:[13] "for every system S in the domain of T_h – that is, for every physical system S whose behavior is accurately represented by some model M_h of T_h – there exists a model M_l of T_l also representing S such that M_h reduces$_M$ to M_l" (Rosaler 2016, 58). Hence, his reduction$_M$ is a three-place relation, where there is one theory-independent physical system (such as an electron), described or represented by two different levels models M_h and M_l. Rosaler applies his reduction model to decoherence, showing that the merits of the latter is captured in his framework and thus it works as a template for more general types of reductions. I do not intend to discuss Rosaler's view in more detail, but I will come back to engage in more detail with decoherence below. The aim of this commentary is to show how Nickles' reduction model still stimulates relevant research.

Now, the general characteristics of reduction$_2$ resemble the work of Heinz Post, with his General Correspondence Principle. I think that their relationship has not been explored with sufficient detail in the literature and I intend to do that in the next section.

3.2.3 Post's Heuristic Correspondence Principle

In this section I review Post's work on heuristics, including his General Correspondence Principle, as a view on intertheory relations and I intend to articulate a similarity with Nickles' reduction$_2$ – which is traditionally seen as the standard philosophical framework to account for the relation QM-CM. The relationship between these two writers with regards to intertheory relations has not so far received sufficient attention in the literature.

The underlying topic of Post's approach is that of heuristics. Post (1971, 215) challenges both that it is impossible to define a standard procedure for obtaining new theories, and that theories are designed through a trial-and-error process. Thus, Post (1971, 218) develops a procedure that is inductive in two ways: it leads from a weaker predecessor to a stronger successor and the successor retains the old theory in a "certain sense" to be discussed.

[13]Subscript h means higher-level, whilst l means lower level. Recall the jargons in Table 3.4. And mind that Rosaler articulates his view within the 'philosopher's' jargon, whereby the higher-level, less fundamental, theory (predecessor), reduces to the lower-level, more fundamental, theory (successor).

Post argues that the successor theory arises out of internal problems in the flawed predecessor and addressing these flaws sets the way towards developing the successor. The flaws are the 'footprints' of the successor within its predecessor, see (Kamminga et al. 1993, xviii). Among the non-exhaustive list of eight heuristic guidelines, he claims the most important one to be his General Correspondence Principle (GCP):

> any acceptable new theory L should account for the success of its predecessor S by 'degenerating' into that theory under those conditions under which S has been well confirmed by tests.
>
> (Post 1971, 228)

Meeting this requirement will ensure that the successor theory conserves the successful empirical consequences of its predecessor, the conservation of those features of the predecessor which conferred its explanatory power and that are conceptually fruitful. That means that although the successor can deal with 'higher' theoretical levels than its predecessor, the lower-level structure of the predecessor will be retained within its confirmed range of validity for the successor theory to be acceptable, see (Kamminga et al. 1993, xix).

In more technical language, the GCP means the following: Let us take S* as the well-confirmed part of theory S – which is never defined exactly – and, being pinned down by facts, it endures forever.[14] A shared domain of phenomena between theory S and successor theory L is assumed (or, in logical terms, that at least the intersection of the domains of S and L is not nil). As such, L explains its well confirmed part L*, and also S*, and the manner in which S* is wholesale taken over by L is for preserving coherence within S*.[15]

The correspondence between theories L and S is mediated by a translation key $\mathcal{T}$ and granted by conditions Q, such that, applying Q to L and translating via $\mathcal{T}$, S* is obtained ($\mathcal{T}$(L|Q)=S*).[16] This secures the absence of "Kuhn-losses" in theory change, insofar as L* explains the whole of S* in the manner just described, (Post 1971, 230).[17] L theory

[14]The fact that Post did not define the well confirmed part of the theory does not mean that this is an impossible task. I do not address this issue.

[15]The notion of explanation here is broad and not ontologically committing.

[16]It would clarify to recall that Post belongs well into the syntactic approach to theories, whereby a theory is identified with "the collection of all theorems provable in it" (Post 1971, fn 40), so that (L|Q) is actually taken to be a sub-theory of L.

[17]If the successor explains all the well-confirmed part of the predecessor, Kuhn-losses are thus avoided. In Kuhn's scheme, successful explanations in the normal science period pre-revolution could be lost in the post-revolution paradigm. These losses are known as Kuhn-losses. I do not intend to engage further with Kuhnian views here.

will in fact "embody a good deal of the (lower) theoretical structure of S-theory", (Post 1971, 229).[18]

Now, the progress from S to L including the retention of S's good parts can be pictured in the following way: "we have sliced off some very high levels [of S] and substituted new ones [from L], but the low (particularly classificatory) levels remain undisturbed within their confirmed ranges of validity" (Post 1971, 229). In the case of the pair QM-CM this sounds appealing, particularly if we are considering the following two notions. Firstly, that QM is more fundamental than CM, whereby QM successfully describes higher-level phenomena, and CM describes the lower-level and is inappropriate to describe anything 'higher' than ordinary experience of the 'everyday'. Secondly, considering the well-known Bronstein cube, which maps the relations between the domains of physical theories, CM is taken as valid in the region where $h \approx 0$ (ignoring the c axis from CM to relativity).

Post claims that his principle is normative and it enables the theorist to eliminate a candidate theory to succeed S if, for instance, it fails to explain S*. One typical case where the principle seems to work nicely is that of the relationship between CM and the special theory of relativity.[19]

Post exemplifies how his principle holds using examples from twentieth century science, and with some success. However, one of the biggest obstacles to the GCP is, the case of QM-CM limit:

> Paradoxically, the only counterexample we have been able to find to the [GCP] is the paradigm example of the relation of [QM] to [CM]. Contrary to the impression that may be given in some textbooks, it is not possible to reduce [QM] to [CM] except 'locally', i.e., with respect to certain sub-theories such as some of those involving angular momentum. Ehrenfest's theorem establishes a correspondence between the motion of the centre of a [quantum mechanical] distribution and the motion of the corresponding classical mass point. No correspondence with respect to higher momenta of the distribution has been found.
>
> (Post 1971, 233)

Despite this, Post defended his view, and considered that the failure to establish a general correspondence between QM and CM "should be regarded as a shortcoming of

[18]It is perhaps useful to emphasise that Post is challenging the Kuhnians, by claiming that there is some theoretical retention even through a scientific revolution. And of course, to some extent these Kuhnian losses are too what the movement of structural realism came to challenge. See (Worrall 1989) and (French 2014) for a recent structuralist development.

[19]I assume the reader to be roughly familiar with the example of the relationship CM-relativity.

[QM] in its claim to the status of L-theory, rather than as a breakdown of the [GCP]" (Post 1971, 234). Yet, one should be charitable and understand that not only could he have considered the conflict with GCP to be a flaw with QM, but also that at that time there were many unresolved issues with QM that would persuade anyone to question its validity.

However, less extreme advocates to the continuity of theories disagree here and emphasise certain correspondences between QM and CM.[20] Moreover, the widely discussed structural realism defended by the likes of Worrall, French, Ladyman, Saunders and others was developed partly on recognising and emphasising the structural continuity across theory change.[21] In the case of QM and CM, French and Saunders indicate the central role played by the plasticity of mathematics. This "heuristic plasticity" – term due to Saunders (1993) – of the theory or theoretical devices factors in the understanding of the path from the reducing theory to the reduced one. Hence, it seems to them and others – who stand for either epistemic or ontic variants of structural realism, as distinguished by Ladyman (1998) – that an important role in the theoretical continuity comes from the correspondence type of relationship between mathematical structures across the theories. A typical example here is the case of Moyal brackets in QM, which will be explored in detailed in Section 3.3, amongst other devices.

Post's proposal has received criticisms too. Radder (1991) has noted that not all equations of special theory of relativity "degenerate into" equations of CM in the way Post means: As it is well known, the equation $E = m_0c^2$ in relativity theory quantifies the energy associated with a particle in a reference frame where the particle is at rest. However, in CM a free, massive particle at rest has no energy. Hence Radder argues that the equation $E = m_0c^2$ does not degenerate into CM as Post would have expected. In turn, Saunders (1993) claims, against Post, that Kuhn losses are not entirely avoided. However, in defence of Post's view, Saunders also articulates a distinction between 'significant' and 'insignificant' types of losses and argues that only insignificant ones can be allowed. Discussing this further is not necessary for my purposes.

Now, having reviewed the main elements of Post's heuristic approach to intertheory relations, let us discuss it in relation with Nickles' one. This analysis can be carried out with the analytic tools discussed in Section 3.1. The first is the dual distinction that

[20]Namely da Costa and French (2003, 105), who consider the heuristic guidelines delineated by Post as one of the main arguments for the blurring of the limit between discovery and justification, and as providing grounds for their approach to the heuristic fertility of inconsistency. However I do not explore this issue.

[21]Actually, French (2006, 2016) has put an end to the 'alliance' between the two main motivations for adopting structural realism: the response to the pessimistic metainduction and the problem of theory change, and the challenges to the realist brought by modern physics, see Ladyman (1998). French considers that these two motivations can be divorced and whilst Worrall's initial proposal focuses more on the former motivation, his own ontic version emphasises the latter.

considers whether the intertheory relation focuses on a relationship between theories or disciplines, and whether the intertheory relation pertains to the history of the subject matter or whether it articulates a view at a specific period of time. Furthermore, there is the question of whether the view on intertheory relation makes claims of metaphysical or epistemic character.

In terms of subject matter, both Nickles' and Post's views aim at a relationship between scientific theories and do not specifically aim at establishing a relationship between scientific disciplines. This is clear from the discussion above. In turn, although Post succinctly builds up his view from a wealth of illustrative examples, drawn primarily from the history of physics, both Post and Nickles are concerned with the current theories (CM and special relativity or QM and CM).

Furthermore, we can discuss their approach to intertheory relations in terms of metaphysical or epistemic terms: are they relating the metaphysical content of the different theories or is the question relating notions of heuristics? It is clear from the discussions in Section 3.2.2 and this section, that both views are explicitly focusing on heuristics, on the problem of how new theories develop in relation to their predecessor. In this sense, they are both equally different to Nagelian reduction, or characterised in terms of reduction$_1$ by Nickles. In contrast with Nagelian account, Post's and Nickles' views suggest a different relation between the theories than 'logical derivation'. Although Post's view is more logic orientated – perhaps originated in his adopting of the syntactic view on theories – the intertheory relation focused in his GCP, is of a successor which 'degenerates' so that it contains the well-confirmed part of the predecessor. Nickles' view is similarly flexible, by laying down a number of different, noncompeting methods, expressed in mathematical terms which show the predecessor obtained from the successor.

A clear difference between their views is the following: Nickles takes the predecessor to guide the development of the successor via the latter establishing a relationship with the former (mathematical, and typically, but not exclusively, in terms of mathematical limits). By contrast, in Post's approach, it is the flaws in the predecessor which play a significant role in stimulating the development of the successor. This is what Post means when he claims that the difficulties in the predecessor are the 'footprint' of progress for the successor, see (Post 1971, 221). Of course, the successes of the predecessor (included in S*) are retained, as I discussed above, in the successor too. This discussion can be summarised in Table 3.7.

***

	Nickles' reduction$_2$	Post's GCP
Conception	Successor T_2 reduces$_2$ to Predecessor T_1.	Successor L degenerates into S* (predecessor's well-confirmed part).
Non-replacement	Non-competing ways expressed in mathematical language.	A translation key applied on L subject to conditions Q: $\mathcal{T}$ (L|Q)=S*.
Epistemic approach	Non-ontological consequences. Heuristic and justificatory driven.	Explicitly focused on heuristics.
vs Nagel's reduction	Logical consequence relation is not the reduction relation.	Logical consequence relation is not the reduction relation. But a logical style is maintained.
Difference	Development of successor upon establishing relationship with predecessor.	Flaws of predecessor as footprints for the development of successor.

Table 3.7: Summary of the similarities and differences between Nickles reduction$_2$ and Post's GCP.

The preceding offers a discussion of the main variants of theory reduction that are particularly relevant to the philosophy of physics. I have adopted a systematic approach to analyse the main traditional views by using the analytic tools introduced earlier. Now, theory reduction is not the only way to conceive intertheory relations. However, I take it that any framework that conceives of the very characterisation of 'predecessor' and 'successor' is reduction-friendly. For example Post (1971, 220) observes that "the very notion of a 'predecessor' theory S implies that we have two theories S and L which refer (in their statements) to at least some events or facts which are identifiably the same". Hence, once we label theories in terms of predecessor and successor, we indirectly force a relationship between them, possibly, in the manner of theory reduction or otherwise, but at least a specific one. More broadly, this sympathises with what I called the Received View of the realist interpretation of QM: the interpretations I briefly discussed in Chapter 2 closely resemble a reduction conception of intertheory relations.

The next section will review the main issues of a different but related view on intertheory relations. That is, the notion of emergence. After this, I will critically engage with the main ways that the Received View of the realist interpretation of QM accounts for the QM-CM relation. By and large, the framework underlying these ways, which are all well known, can be conceptualised with Nickles' reduction$_2$, and also Post's General Correspondence Principle.

3.2.4 Emergence

The Received View can articulate other forms of intertheory relation than theory reduction in order to account for the QM-CM relation. I have focused on theory reduction because I think that it is the dominant view implied in physics. Emergence has recently become relevant, particularly in philosophy. In this section I discuss the essential elements in the notion of emergence by drawing on recent literature. I will conclude by offering reasons why I will not consider it further.

Emergence involves various notions and there is debate on how to define its meaning and scope. It is also debated in a wide range of sub-fields of philosophy. Overall, emergence can be seen as a form of intertheory relation, articulating a relationship between phenomena/properties/theories that operate at significantly different levels of fundamentality. Different scholars defend different ideas on how to conceive emergence. For example, some conciliate emergence and reduction as two types of intertheory relations that can coexist, such as Butterfield (2011b); others, such as Bangu (2015), take emergence and reduction to be opposite notions. Emergence has a wide range of applications. From the point of view of the realist interpretation of QM, a useful idea to characterise emergence at first order is that it inter-relates the macroscopic realm and the microscopic realm. Hence, for my purposes this places the advocate of emergence within the Received View that I articulated in Chapter 2. In a way, the Received View collapses two crucially different questions: "what is a quantum state representing in the world?" and "how do quantum objects relate to macroscopic objects?". Or, in other words, the Received View considers that an answer to the first question has to be obtained from an answer to the second. However, as I argued in connection with my Core Realism view in Chapter 2, a realist interpretation of QM ought to consider the former question first and foremost. Instead, the second question, which can be seen as one of intertheory relations, can be approached only once the first question has been addressed. Otherwise, if we do not have a robust account of the nature of quantum objects, how could their relationship with classical objects be established?

Intuitively, emergent phenomena are emergent because they cannot be put in terms of properties and behaviour of more fundamental "building blocks". In a way, 'the whole is more than the sum of its parts'. This intuition allows philosophers to approach a number of issues, which result in various forms of emergence. One popular example is phase transitions in physics. We observe phase transitions even in daily life, such as when boiling water or freezing water to make ice. Thermodynamics describes phase transitions by saying that the system crosses a coexistence line in a plot of pressure vs. temperature. Now, from the point of view of thermodynamics, the high-level theory, the phase transition occurs where a thermodynamical potential, for instance the

Gibbs free energy potential, has a singularity. However, at the lower-level, quantum statistical mechanics assumes that water is composed of quantum molecules. Quantum molecules are more fundamental than classical systems, and their macroscopic behaviour is explained through considerations at quantum level. However, at the level of quantum statistical mechanics the description of that thermodynamical potential cannot have any singularity. Therefore, some argue that the phase transition is an emergent phenomena.

Part of the debate consists in classifying types of emergence. For his own purposes, Pexton (2016) provides a taxonomy of emergence. There are a wide range of notions involved. He presents the field of emergence as including two notions (epistemic and ontological) that each include two varieties (weak and strong). Hence, he identifies four forms of emergence.

Epistemic-weak emergence is the result of our activity. Hence, it involves a subjective element, since weak emergent phenomena do not make reference to objective features of the world. Rather, the conception of emergence here relates to a 'pragmatic' convenience. Hence, "there is nothing over and above the fundamental level entities/properties in combination and no absolute objectivity to emergent levels" (Pexton 2016, 92).

In turn, the strong variant of epistemic emergence is meant to be objective. This considers that the existence of non-fundamental phenomena is dependent on our epistemic representative choices. Nevertheless, strong-epistemic emergence takes that the representation of that phenomena as emergent is objective and robust. "The world looks a certain way for any observer (even Laplace's demon), but an observer-free world does not contain these emergent phenomena" (Pexton 2016, 93).

Then, there are two variants of ontological emergence. Ontological-strong emergence considers that non-fundamental entities exist and have novel powers or properties that are independent of the fundamental entities, and could even act causally. Finally, the ontological-weak emergence also considers that there are non-fundamental entities. However, they do not have properties or powers at the high-level, (Pexton 2016).

Pexton's taxonomy is useful to distinguish forms of emergence. In particular, both Bedau (2008) and Wilson (2010) label their forms of emergence as weak. Bedau (2008) aims to include objectivity in weak emergence. His claim is that "weak emergence is not just in the mind. Rather, it is a distinctive kind of complex, macro-pattern in the mind-independent objective micro-causal structure that exists in nature" (Bedau 2008, 444). However, in Prexton's taxonomy, Bedau's form of emergence is relabelled 'strong-epistemic', instead of weak. This clearly distinguishes Bedau's emergence from Wilson's weak emergence, which actually makes stronger ontological claims than Bedau's.

Of course, Pexton's taxonomy is not the only one. Other authors in the philosophy of emergence consider different classifications. Humphreys (2015) also divorces the labels 'strong' and 'ontological' to characterise forms of emergence. He defines strong emergence as a type of ontological emergence whereby there is downward causation – that is, where higher level properties can have a causal effect on lower level properties. An example of this would be mental properties causing physical properties. Furthermore, Humphreys (2015, 139) considers 'inferential emergence' instead of 'epistemic emergence', noting that limitations of what can be predicted can be distinct from limitations of what can be known. An example of this would be cases where unpredictable values of a physical quantity can be known and measured.

There are three reasons why I do not critically engage with emergence in this investigation. First, because emergent phenomena are supposed to – in some way – "emerge" from some antecedently known and understood more basic phenomena, and then the study of emergence concerns the account of that relation between both the emergent and the underlying phenomena. This seeks not only to do philosophical work in regards to specific scientific knowledge and the world, but also to understand the concept of emergence. I am sympathetic with the aim of clarifying that. For instance, one could want to compare Humphreys' and Pexton's classifications mentioned above. However, that task is outside the scope of this thesis. My specific concern is with the realist interpretation of QM. Moreover, I do not assume that QM is sufficiently or successfully known and understood, which is a necessary condition for exploring whether classical phenomena emerge from quantum phenomena. Hence, is difficult to assess whether emergence is a sound account of the relationship QM-CM.

Secondly, the debate around emergence not only assesses which phenomena is emergent, but also there are questions as to what emergence really is. Whilst the topic of emergence has received increased attention in recent years, there is still significant disagreement over the meaning of the terms. Humphreys (2015) notes that the term 'emergence' is used in an informal manner and at the moment it is difficult to clearly understand what different authors are considering emergence to be; whether they disagree with one another, or whether they are simply invoking different concepts. Despite forthcoming improvements in the clarification of the basic terms of this field, this debate will remain unclear, he assesses. Of course, there is lack of consensus in the debate of theory reduction too. That disagreement is of a different nature, however. In theory reduction the debate seems to be to decide which specific account best applies, and not what reduction is.

Thirdly, although many physicists do not engage in serious philosophical reflection, I think that the traditional view that best captures the practice of physics and the accepted relationship between QM and CM is that of reduction.

For these reasons I will focus on contrasting the philosophical issue of theory reduction appropriate to the specific case of the physics related to the QM-CM relationship.

3.3 The Received Account of the QM-CM Limit and its Problems

In the previous sections I drew on a range of ways in which philosophy of science has traditionally accounted for intertheory relations. I emphasised the role and relevance of theory reduction. My claim is that theory reduction broadly captures most of the practice of physicists and their intuitions with regards to the pair QM-CM (of course, with the exception of those who work in semiclassical mechanics, as I will discuss in Chapter 4).[22] However, that Nickles' reduction captures the practice of a large part of the physics community does not automatically mean that that is the correct way to think about it, in the face of the actual content of the two theories, nor the only way. Indeed, the progress and increasing relevance of semiclassical mechanics (particularly what is known as quantum chaos) will, sooner or later, influence the philosophical judgement of the QM-CM relation, which will have to be reassessed – and Bokulich's work is a seminal contribution to this issue.[23]

There are two metaphors that I think are useful to illustrate the novelty of semiclassical mechanics by contrast with more traditional approaches to the foundations of QM. According to the tradition, which I put in terms of the Received View of the Realist Interpretation of QM, and according to much of the instrumentalist use of physics, QM is more fundamental than CM and universally valid. On that basis, quantum phenomena is expected to occur at the macro level. However, for us, classically minded humans, this is unexpected and it is surprising when quantum effects are actually exhibited at our familiar classical scale, such as quantum entanglement in large systems, or other well-known effects. We expect QM in all scales, but it is still weird, surprising, or unexpected. Therefore, I put traditional issues pertaining to the foundations of QM (including teleportation or computing, amongst others) as dealing with "expected unexpected" phenomena. Here, the surprising progress and development is with the achievement and control of quantum effects appearing at the macro-scale, see for instance (Brezger et al. 2002; Ourjoumtsev et al. 2007) and (Landsman 2007, references made in p. 418).

[22]By contrast, Bokulich (2008a) assesses that, if looked at broadly, the dominant view on intertheory relations in physics is pluralism. I disagree with that assessment and in this section I show a range of theoretical devices that have been designed to account for the reduction QM-CM.

[23]This is so relevant that I will dedicate two chapters to analyse it in detail. At this stage I merely mention this.

The second metaphor pertains to semiclassical mechanics. This is less recognised in the philosophical literature. In semiclassical mechanics, as I will discuss in detail in Chapter 4, the underlying phenomena is quantum. Relative to our familiar macroscopic scale, that the world is quantum is unexpected. However, on the basis that QM is more fundamental and universally valid, the appearance of classical dynamical structures, such as classical trajectories, in the account of quantum phenomena is also unexpected. This is unexpected – for us classically minded humans – in the unexpected presumably quantum world. Therefore, I put semiclassical mechanics as dealing with a doubly unexpected phenomena – "unexpected unexpected" – where, although the phenomena are described by QM, their explanation crucially requires the use of classical dynamical structures. The development of semiclassical physics continues to progress, showing a surprising appearance of the classical within the 'purely' quantum.

Hence, semiclassical mechanics and foundations of QM seem to pull in opposite directions. Semiclassical mechanics is so relevant that I will dedicate Chapters 4 and 5 to its discussion, whilst here I will focus on the latter, more traditional approaches.

In this section I will engage with relevant and specific theoretical devices in physics that belong to the account of the intertheory relation between QM and CM, namely, the mathematical limits in Section 3.3.1, Ehrenfest theorem in Section 3.3.2, Moyal brackets in Section 3.3.3 and, in Section 3.3.4, decoherence. This group is not homogeneous. I take the first three devices to be representatives of the physical counterpart to the philosophical side of the relation QM-CM discussed in Section 3.2. In line with the traditionally accepted view, I take these as philosophically captured by the reduction$_2$ form articulated by Nickles (1973), discussed in Section 3.2.2 (and Post's GCP in Section 3.2.3). They play a heuristic and justificatory role, in the sense that the development of the successor (QM) is relative to presenting a relationship with the predecessor (CM). And such a relationship is cashed out in mathematical devices, which operate in a non-competing way, as Nickles describes it. Hence, I take them to relate to what is philosophically conceived as the intertheory relation between two theories: CM and QM. By contrast with these three devices, decoherence is relevantly different.

In turn, decoherence is closely related to what is known in the literature on foundations of QM, by the technical term of the QM-CM limit. There are some useful clarifications to make before we go any further. Firstly, a sector of the physics community, which perhaps does not engage in much philosophical reflection, considers decoherence to be a *phenomenon* that occurs in nature whenever a system interacts with the environment. However, as I will justify in detail, I will consider decoherence as a theoretical device that attempts to account for the QM-CM limit. And I will motivate this assessment by looking at relevant physicists and philosophers of physics who work in foundations of

QM, such as Zeh, Zurek, Joos, Bacciagaluppi, Landsman, Schlosshauer. Secondly, there is currently a hot debate on how to conceive the QM-CM limit, what role decoherence plays within it, and how to assess its success. Now, I do not intend to review all the relevant views and critically engage with them. Instead, I just want to show that there is sufficient disagreement on the terms and conditions of the problem. For example, Bacciagaluppi (2013) considers the measurement problem as a separate problem to the QM-CM limit (in the sense of the classical regime within QM), and argues that the solution to one does not solve the other (in both ways). He argues that decoherence does not solve the measurement problem, and that decoherence recovers predictions of CM, but only instrumentally. That is, in both cases Bacciagaluppi considers that a strong realist interpretative programme is still needed. Therefore, he concludes, the problems remain open to further discussion.

By contrast with Bacciagaluppi, Schlosshauer (2007, 49) considers that the QM-CM limit is a broader problem that has the measurement problem as a component of it. Furthermore, that the latter is actually three problems: the problem of the preferred basis, the problem of explaining the non-observability of superpositions in the macroscopic scale, and the problem of the outcomes, see (Schlosshauer 2007, 50-ff). His assessment is that decoherence solves the first two, but does not provide an answer to why there is an outcome to the measurement process.

To show yet another view, Landsman (2007, 419) presents both the mathematical limits – e.g. $\hbar \to 0$ and $n \to \infty$, which involve taking limits of equations – and, for instance the Ehrenfest theorem, and decoherence, as the devices that intend to solve the problem of explaining the appearance of the classical world from quantum theory. His assessment is that all of them fail, strictly speaking. What is relevant is that he considers the theoretical devices as generally attempting an account of the appearance of the classical world, given that it is quantum.

One possible explanation of the disagreement on what the QM-CM limit is and how decoherence fares with it, is that there is also disagreement on how to consider QM from a realist point of view. As I mentioned previously, the philosophical literature still does not have a clear and satisfactory answer to the realist who wants to be able to say what a quantum system really is – the interpretative question is still open to further discussion.

In the face of this situation, let me clarify my line of analysis. I will consider the QM-CM limit in a dual way: firstly, I will consider the role of mathematical limits, Ehrenfest theorem and Moyal brackets in the QM-CM limit understood as the physical counterpart of Nickles' and Post's heuristic approaches. In the subsequent sections I will argue that these three devices are only successful within a rather limited scope of applicability, and therefore deficient as an account of the passage from the quantum to the classical. This claim is accepted across the board.

Secondly, I will consider the role of decoherence in relation to the QM-CM limit, which is framed within the literature on foundations of QM. I will discuss that the QM-CM limit includes, in the foundations of physics, two main components: the measurement problem and the preferred basis problem, see (Schlosshauer 2005). My assessment will agree with Landsman (2007) and Bacciagaluppi (2016, 2013), in that decoherence aims at solving these problems, but ultimately it does not deliver (contra Schlosshauer).

The contribution of presenting this discussion is to assess the strength of these theoretical devices in the context of the particular scientific realist account of QM that was characterised as the Received View of the Realist Interpretation of QM in Chapter 2. Yet, the basic intuitions of realism, the judgement and measure of how fit-for-purpose these devices are does not rely on their predictive power only. By contrast, in the interests of the realist, more philosophically relevant questions should be the priority. Say that you have obtained a smooth and uncontroversial bridge between QM and CM via some formal device, and that you infer from that a metaphysical connection between quantum objects and classical ones. Then, that still does not remove the need for the question of the very nature of the quantum objects. Would it not be methodologically unsound to attempt to address a relationship between QM and CM, given that the realist has, today, not managed to spell out the nature of quantum objects any further than claiming that they are what quantum states represent, that they are whatever the physicists use to make predictions?[24]

As I developed in Chapter 2, I argue that the two questions – the intertheory relation between QM and CM, and the interpretation of QM – ought to come at significantly different stages in the development of a philosophical view on physics. A sound methodology would first have a conceptual apparatus, in the terms of French and McKenzie (2012) the right "metaphysical tool", to interpret the formalism of QM and be able to realistically account for and explain the experimental results.[25] At a second stage, a sound methodology would explore the possible relationship between one theory and the other. Core Realism, can afford to follow the intuition I described, as I have argued in Chapter 2. However, the very essence of the Received View of the Realist Interpretation

[24]Hence, for example, as I discussed in Chapter 2, MWI is currently considered by Wallace (2016b) to be a theory of the decoherent macro-world. And, when asked what is in the micro-world, he would reply: there is just a coherent quantum state. This response seems to address a QM-CM limit, explaining the appearance of the macro-world, but falls short of spelling out the physical content of QM any deeper than merely instrumental terms. And only a realist interpretation of QM mapped onto the Received View can be satisfied with that, although Core Realism would need more.

[25]The amount of metaphysics that a realist interpretation of QM has to involve is questioned by some. At this stage I assume the traditional view on realism whereby realism involves claims about the way the world is. In Chapter 6 I will explore this more, including less metaphysical aspects of realism.

of QM is to invert this methodology, and instead consider that the central issue in the interpretation of QM is the problem of intertheory relation: the QM-CM relation.

After this clarificatory discussion, I will move on to critically engage with the three theoretical devices of mathematical limits, Ehrenfest theorem and Moyal brackets, and then decoherence. For the reasons discussed above, I engage with decoherence in more detail and depth than the previous devices.

3.3.1 Mathematical Limits

The use of mathematical limits to address a reduction is not specific to the QM-CM case. In fact, as I discussed in Section 3.2.2, mathematical limits represent a paradigmatic case for addressing an intertheory relation. A well-known case is the pair CM and special relativity. Here, mathematical limits show that Lorentz transformations smoothly approach Galilean transformations when the speed of the system v is negligible compared with the speed of light c. More precisely, taking the perturbation parameter $(v/c)^2$ allows the Taylor expansion

$$\frac{1}{\sqrt{1-(v/c)^2}} = 1 + \frac{1}{2}\left(\frac{v}{c}\right)^2 + \frac{3}{8}\left(\frac{v}{c}\right)^4 + \frac{15}{16}\left(\frac{v}{c}\right)^6 + \cdots . \tag{3.1}$$

This enables an analytic limit for the relativistic momentum to approach the classical one:

$$p = \frac{m_0 v}{\sqrt{1-(v/c)^2}} \xrightarrow[(v/c)^2 \to 0]{} m_0 v. \tag{3.2}$$

Therefore, the reduction is symbolised as

$$\lim_{(v/c)^2 \to 0} F(SR) = F(CM), \tag{3.3}$$

where $F(SR)$ is a formula in special relativity and $F(CM)$ is a formula in classical mechanics. This goes in line with the views on theory reduction proposed by Nickles and Post discussed above. The claim is that the behaviour of the finer theory (special relativity) smoothly approaches that of the coarser theory (classical mechanics). Both instances, the limiting case as $(v/c)^2 \to 0$ and the case at the limit where $(v/c)^2 = 0$, are qualitatively similar.

However, research indicates that there are problems when using this strategy for the pair QM-CM. Berry and Mount (1972, 316) recall that, in this case the limit involved is generally singular, which means that a smooth 'reduction', 'recovery', or whatever epistemic relationship one might try to extract, is not uncontroversial. In particular, and building upon Berry and Mount's work, Batterman (1995, 2002) focuses on this distinction to develop his account of 'asymptotic reasoning' – the general issues of which I discussed in Section 3.2.4. To see this, let us take a general case of a limit

$$\lim_{\epsilon \to 0} f(x) = g(x), \tag{3.4}$$

where $f(x)$ and $g(x)$ are two well-behaved functions $\mathbb{R} \to \mathbb{R}$. Here, regular limits are characterised by the fact that the function $f(x)$ smoothly approaches the limiting function $g(x)$ as $\epsilon \to 0$. This is the type of limit found in relating CM and SR, eq. (3.3). By contrast, for singular limits it cannot be said that the 'limiting behaviour' (that is, the behaviour of $f(x)|_{\epsilon \to 0}$) is qualitatively the same as the behaviour in the limit ($f(x)|_{\epsilon=0}$). Indeed, the behaviour of $f(x)$ in the limit is of a fundamentally different character to the nearby solutions as $\epsilon \to 0$, see (Batterman 2002, 18-19).[26]

Therefore, in the quantum-classical case the situation is more complicated than the special relativity-classical pair. At the beginning of QM, Planck obtained the energy density for blackbody radiation U. As a function of the frequency ν we have

$$U(\nu)_{QM} = \frac{8\pi V}{c^3} h\nu^3 \frac{1}{e^{\frac{h\nu}{kT}} - 1}, \tag{3.5}$$

where V is the volume of the cavity, c is the speed of light, k is Boltzmann's constant, and T is the temperature. This formula converges to the classical Rayleigh-Jeans formula for the limit of 'high temperature' or 'low frequency', $\frac{h\nu}{kT} \to 0$,

$$U(\nu)_{Classical} = \frac{8\pi V}{c^3} kT\nu^2. \tag{3.6}$$

This mathematical fact is a heuristic guideline for further development and a footprint for the relationship between QM and CM – resonating with Post's and Nickles' reductions discussed above. The limit aimed at recovering an explanation of the classical world from the quantum. Indeed, the physics literature claims that when $\hbar \to 0$ the laws of QM "must reduce to those of CM" (Messiah 1961, 214). Now, two remarks will make this clearer: first, the word 'reduce' in the context of physics literature does not bear much philosophical reflection and perhaps Messiah and other physicists do not mean a rigorous notion of reduction. Moreover, Landsman equally talks of the intertheory

[26] To ilustrate this, consider, as Batterman does, the problem of calculating the roots of the function $f(x) = x^2 + x - \epsilon 9$ and of $h(x) = \epsilon x^2 + x - 9$, when $\epsilon \to 0$. In the limiting case $\epsilon = 0$, $f(x) = 0$ for $x = \{-1, 0\}$. As $\epsilon \to 0$ $f(x)$ always has two roots, which approach $x = \{-1, 0\}$. By contrast, in the limiting case $\epsilon = 0$ $h(x)$ is a linear function with one root at $x = 9$, although as $\epsilon \to 0$ with $\epsilon \neq 0$, $h(x)$ is a quadratic function with real or imaginary roots depending on the sign of ϵ. This difference is the difference between regular and singular limits.

Batterman (1995) motivates his notion of emergence grounded on these issues. He infers the existence of emergent phenomena which cannot be simply seen as reducible. Whilst I do not intend to assess Batterman's statements here, I point out that his argument against the claimed reduction relationship QM-CM takes the mathematical limits to provide that reduction.

relation in terms of "emergence", "approximate emergence" or "recovery". Although the practice of physicists resembles Nickles' reduction, it should not be understood that they actually pursue obtaining a reduction$_2$. On the contrary, Nickles' work attempted to account for this practice.

Second, obviously, $\hbar$ is a physical constant with units of action (energy times time). Therefore $\hbar \to 0$ is meaningless and normally one takes a dimensionless $\tilde{\hbar}$ instead. In the previous example, $\frac{h\nu}{kT} \to 0$. For example, take the time-independent Schrödinger equation for a mass m particle,

$$H\psi = \Big(-\frac{\hbar^2}{2m}\nabla^2 + V(x)\Big)\psi. \tag{3.7}$$

Here we could introduce an energy scale $\epsilon = \sup_x |V(x)|$, and a length $\lambda = \epsilon / \sup|\frac{\partial V}{\partial x}|$ (if these are finite). Then a dimensionless hamiltonian can be written by dividing both sides by ϵ and changing the variables:

$$\tilde{H}\psi = \Big(-\tilde{\hbar}^2\nabla^2_{\tilde{x}} + \tilde{V}(\lambda\tilde{x})\Big)\psi, \tag{3.8}$$

where now we have the dimensionless $\tilde{\hbar} = \frac{\hbar}{\lambda\sqrt{2m\epsilon}}$, and $\tilde{V}(\lambda\tilde{x}) = \frac{V(x)}{\epsilon}$. Then the limit is $\tilde{\hbar} \to 0$. Hence, every time a limit $\hbar \to 0$ is taken, a similar case-dependent-procedure is supposed.

Let us discuss the singular nature of limits in the QM-CM case. A standard explanation of this is given by (Berry and Mount 1972, Sec. 2.1). Firstly, consider eq. (3.8) when $\tilde{\hbar} = 0$, we obtain

$$0 = (V(x) - H)\psi. \tag{3.9}$$

Beyond the observation that this is not recovering the classical result, this would also mean that $\psi(x) = 0$ everywhere unless $V(x) = H$. The solutions to equation $V(x) = H$ are the 'classical turning points' and there the classical quantities typically diverge, (Berry and Mount 1972, 316). Hence, the limit $\hbar \to 0$ has to be studied more carefully.

A typical example considers an incident particle on a finite potential barrier of the form $V(x) = \frac{V_0}{1+\exp(-x/L)}$, where V_0 characterises how tall the step is, and L characterises how steep the step is, as shown in Figure 3.2. In the classical case when $E > V_0$, the total energy of the particle is greater than the potential and the particle travels over the barrier with no effect upon its dynamics, except for a change in its momentum, from $p_{-\infty} = \sqrt{E2m}$, to $p_{+\infty} = \sqrt{(E - V_0)2m}$. By contrast, in the quantum case even when the particle's energy is greater than V_0, both reflected and transmitted waves, in addition to the incident wave, are predicted. Now, if p_2 is the momentum of the particle over the step, the calculations show that in the limit where $p_2 L \gg \hbar$, the reflection

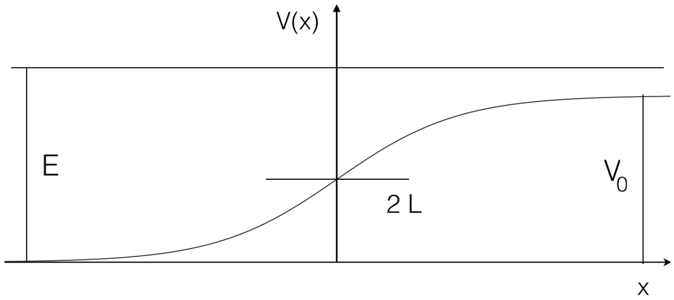

FIGURE 3.2: A particle with energy E is incident on a potential barrier of the form $V(x) = \frac{V_0}{1+\exp(-x/L)}$.

coefficient is $|R^2| = \exp(-4\pi p_2 L/\hbar)$ tends to zero. Considering $|R^2|$ as a function of $\hbar$, the singularity at $\hbar = 0$ is essential, and the limiting behaviour is dramatically different to the behaviour at the limit.

Hence, in the QM-CM case the equations cannot be written as a convergent sum of classical terms plus quantum effects, by contrast with the pair special relativity-CM where the limits are regular, (Berry and Mount 1972, 319). There is an immense literature on how to fix, extend and interpret this fact, see Berry and Mount (1972); Landsman (2007) for references. The relevant conclusion here is that taking the limit $\hbar \to 0$ does not simply obtain the classical equations and this theoretical device is therefore deficient to provide the reduction.

Further to $\hbar \to 0$, there are other quantities that can be taken to attempt a recovery of a classical result in a limit, such as $n \to \infty$, the limit of quantum number n that indicates the energy level, when energies are large $n \to \infty$.[27] In the paradigmatic case of the hydrogen atom, large quantum numbers were the hypothesis that connected Bohr's atom model with the already known value of the Rydberg constant. The spacing between the energy levels in the hydrogen atom becomes negligible as $n \gg 1$. Although this provided a heuristic/justificatory motivation for Bohr's project – resembling Posts/Nickles' epistemic account of theory reduction – the limit $n \to \infty$ is not uncontroversial.[28] Indeed, as Messiah (1961, 214) remarks, there are quantum effects that are independent of the discreteness of spectra and it is not straightforward that the limiting method achieves the recovery of the classical from the quantum.

Indeed, Hassoun and Kobe (1989) argue that more than one parameter is required in the limit concurrently, in order to obtain the 'value of correct classical observable'. For

[27] Landsman (2007, Sec. 6) considers the case $n \to \infty$ equivalent in some relevant way to the consideration of $\hbar \to 0$.

[28] Relevant discussion could focus on Bohr's philosophy, but I do not intend to enter into this debate, see (Bokulich 2008a, Ch. 4) and references therein.

example, in the case of the harmonic oscillator, the quantum case obtains the energy given by

$$E_n = (n + \frac{1}{2})\hbar\omega. \tag{3.10}$$

Now, none of the limits $\hbar \to 0$ or $n \to \infty$ tend to the classical case of continuum spectrum $E = \frac{1}{2}mA^2\omega^2$, (where A is the amplitude of the oscillator).[29] In the former limit, the energy tends to zero, in the latter, to ∞. In order to fix this, Hassoun and Kobe (1989, 658) take both limits concurrently with the constrain $n\hbar = J$ constant, which is the classical action for this problem $\pi m\omega A^2$. This obtains the approximation to the known classical result. But again, this seems to involve more complex procedures than merely taking a limit in order to obtain a classical result.

In conclusion, it is recognised in the literature that mathematical limits alone are incapable of providing a reduction relationship for QM-CM. Consequently, there are other attempts, namely, Ehrenfest theorem. My presentation of these as different devices is not unquestionable and Bokulich (2008a) and Landsman (2007) consider the latter as an application of the limit $\hbar \to 0$. Separating them is just a practical decision in order to arrange the discussion, but it does not make a difference to my assessment.

3.3.2 Ehrenfest's Theorem

The Ehrenfest's theorem can be accommodated within Nickles' reduction$_2$ as one of the non-competing ways to account for the reduction relationship. It is worth emphasising that the initial motivation for this theorem was explicitly to obtain states that would remain with 'small' spreads in position and momentum. Discovered by Schrödinger, Gaussian states for an harmonic oscillator conserve their shape and move in classical trajectories, among other properties, such as also having the minimum spread allowed by Heisenberg's relations.

First off, the quantum hamiltonian is built by quantising the classical one, replacing dynamic variables x, p by the unbounded operators $\hat{x}, \hat{p}$. To illustrate this, let us consider a particle under the influence of a potential $V(\mathbf{x})$, where $\mathbf{x}$ is the position operator (for convenience, hats are avoided).[30] The hamiltonian operator will be $H = \frac{p^2}{2m} + V(\mathbf{x})$.

[29] Consistency would have us stick with the hydrogen atom, but for our purposes it is simpler to consider the harmonic oscillator, details can be found in the Hassoun & Kobe's article.

[30] Consider this minor remark: if you want to use classical mechanical formulae to obtain quantum mechanical ones, the well known recipe of 'replace the classical x by the operator $\hat{x}$, and the classical p by $\hat{p}$' is only applicable in a limited number of cases. It does not give you the answer if your classical hamiltonian includes terms in the like of xp^2 because of the non-commutativity of QM. There, trial-error methods have to be applied.

We evaluate the time evolution of p, which is calculated as

$$\frac{d}{dt}p_i = \frac{1}{i\hbar}[p_i, H]. \tag{3.11}$$

Therefore, we have

$$\frac{d}{dt}p_i = \frac{1}{i\hbar}[p_i, V(\mathbf{x})] = -\frac{\partial}{\partial x_i}V(\mathbf{x}). \tag{3.12}$$

In turn, the time evolution of the operator x is $\frac{dx_i}{dt} = \frac{p_i}{m}$, because $[x_i, V(\mathbf{x})] = \mathbf{0}$. This, using Heisenberg equation of motion we obtain

$$\frac{d^2x_i}{dt^2} = \frac{1}{m}\frac{dp_i}{dt}. \tag{3.13}$$

Combining this with eq. (3.12), we have the vector equation

$$m\frac{d^2\mathbf{x}}{dt^2} = -\nabla V(\mathbf{x}). \tag{3.14}$$

Now, we are interested in calculating the expectation values of the operators in the Heisenberg picture where the state of the system ψ is fixed in time and known, and the operators evolve in time:

$$m\frac{d^2}{dt^2} < \mathbf{x} >= \frac{d}{dt} < \mathbf{p} >= - < \nabla V(\mathbf{x}) >=< \mathbf{F}(\mathbf{x}) >, \tag{3.15}$$

where we consider a force $\mathbf{F}(\mathbf{x}) = -\nabla V(\mathbf{x})$. Eq. (3.15) holds for any state. The 'Ehrenfest substitution' aims at obtaining classical trajectories for the mean values, replacing $< \mathbf{F}(\mathbf{x}) >$ by $\mathbf{F}(< \mathbf{x} >)$. Hence, eq. (3.15) will obtain a quantum version of Newton's second law. Sometimes this is taken as a characterisation of the QM-CM limit, because not only we obtained classical time evolution for the expectation values for the operators, but also $\hbar$ has disappeared and the centre of the wavepacket moves 'like' a classical particle under the potential $V(\mathbf{x})$. However, this conclusion has limited range of validity.

The substitution can be made only in a limited number of cases: when the force $\mathbf{F}$ depends linearly on the position coordinates $\mathbf{x}$, such as a free particle or an harmonic oscillator; and in the case where the wave function remains localised in a small enough region where the force is taking up an approximately constant value in the region spread in position, (Messiah 1961, 218). Although here Bacciagaluppi (2013, 433) points out that whilst we want a 'classical' state with small spread in both position and momentum, it is only the small size of the spread in position that determines whether the Ehrenfest substitution is viable.

Given these limitations, it is recognised that the Ehrenfest theorem cannot account for the relation QM-CM, see (Post 1971, 223) and (Bokulich 2008a, 21) amongst others. Plus, there are physically interesting cases, like a particle scattering off of a potential step, that do not satisfy the conditions for the Ehrenfest substitution. Finally, it is worth noting that the theorem works only for pure states and once interaction is introduced, the picture changes dramatically. This leads to the attempt to understand the appearance of CM from QM by the destruction of the coherence. The process of decoherence will be analysed in Section 3.3.4. However, there is one relevant case to discuss first.

3.3.3 Moyal Brackets

Moyal brackets appear in the context of the Wigner function. To describe where this comes from, recall that Dirac argued the following:

> The correspondence between the quantum and the classical theories lies not so much in the limiting agreement when $h \to 0$ as in the fact that the mathematical operations on the two theories obey in many cases the same laws.
>
> (Dirac 1925, 649)

With a slight difference in notation in that paper, Dirac is pointing at his own famous association

$$[q, p] = i\hbar\{q, p\}_{PB}, \tag{3.16}$$

where $[\ ,\ ]$ is the quantum commutator and $\{\ ,\ \}_{PB}$ the well-known Poisson bracket of CM. Another way in which this relation has been expressed is via Moyal brackets, in the context of the formulation given by the Wigner function.[31] There are various ways

[31]Within the form of structural realism advocated by the likes of French, Saunders and others, the Wigner function and Moyal bracket is the paradigmatic case of the plasticity of the theory. This similarity is, understandably, taken as an indication of a deeper relation between the theories (QM and CM), one that manages to reach beyond the scope of the heuristics. However, despite this strong formal analogy, as Dirac underlined, the qualitative differences remain valid, thus effectively downgrading the power of this formal – and not more than – indicative relationship. For example, CM is still a commutative formalism, whereas QM is non-commutative. The physical consequence of this mathematical fact cannot be overlooked by a formal analogy. Let us illustrate the physical difference with a simplified example: in CM the order in which measurement of two or more magnitudes is, by principle, irrelevant both to the empirical results, and to the state of the system. Consider this classical experiment: the system is a chair from the laboratory and you want to measure its colour, height and weight. Measuring first the height, then the colour, and, finally, the weight, will obtain the same results had the order been any other. Furthermore, no scientist would claim that the state of the chair would be relative to the order of the measurements. By contrast, in QM, the order of the measuring operations have a direct effect on the possible results (when the physical magnitudes of interest are described by mathematical operators that are non-commutative) and also, according to the standard view, on the state of the system. This relates to the problem of contextuality in QM. Therefore, this scheme might be useful in some circumstances, yet, the significant differences between the theories has to be considered.

to approach it, I follow Styer et al. (2002, 291), who express the Wigner phase-space distribution function for a single particle restricted to one dimension as

$$W(x,p,t) = \frac{1}{2\pi\hbar}\int_{-\infty}^{+\infty} \psi^*(x - 1/2y, t)\psi(x + 1/2y, t)e^{-ipy/\hbar}dy, \tag{3.17}$$

where x is position and p momentum. The evolution of $W(x,p)$ is given by Moyal bracket $dW/dt = \{H, W\}_{MB}$. When the potential V in the hamiltonian H is analytic, the bracket obtains as the classical Poisson bracket, plus some terms that give quantum corrections:

$$\frac{dW}{dt} = \{H, W\}_{MB} \tag{3.18}$$

$$= \{H, W\}_{PB} + \sum_n \frac{\hbar^{2n}(-1)^n}{2^{2n}(2n+1)!}\partial_x^{2n+1}V\partial_p^{2n+1}W. \tag{3.19}$$

The Poisson bracket generates the classical distribution for a function evolving in classical phase-space. Hence, if the quantum corrections are negligible, then classical dynamics is obtained within QM.

Despite its useful aspects enumerated in (Styer et al. 2002, 292), a problem with Wigner distribution function is it is not positive definite, thereby being dubbed a pseudo-probability distribution.[32]

A second problem appears when the classical system is chaotic.[33] In these cases, Wigner function does not deliver as an account of the relationship QM-CM. Instead, the quantum predictions diverge from the classical ones as the second term in eq. (3.19) is not

[32]Let us clarify: I refer to the non-classical aspect of quantum probability in a specific sense, which is best expressed by using the language of Von Neumann algebras, see (Redei and Summers 2007) and (Holik et al. 2013) for more discussion. "In noncommutative probability theory, a probability space is a triple $(\mathcal{M}, \mathcal{P}(\mathcal{M}), \phi)$ consisting of a Von Neumann algebra, its lattice of orthogonal projections and a normal state on the algebra" (Redei and Summers 2007, 399). When the Von Neumann algebra $\mathcal{M}$ is abelian (commutative), the classical starting point is obtained, Kolmogorovian probability. The departure from the classical originates in the noncommutativity of $\mathcal{M}$ (that is, $\mathcal{B}(\mathcal{H})$). In the case of Wigner distribution, the 'non-classicality' is of a different nature, since the 'probability' here is not positive definite. Whether it is worthwhile to consider a change in the definition of probability and whether this is a promising framework for QM, are questions that I do not engage with. At face value, the Wigner function does not give a non-classical probability in that strict sense, but it is something that is not a probability. Despite this technical comment, the Wigner function is widely used and relevant measurements have been reported in Nogues et al. (2000). Furthermore, this formalism of Wigner function underpins the explanation of the wavefunction scarring effect, which I will discus in Chapter 4, see (Gutzwiller 1990, 300) and the references therein.

[33] Chaotic systems are sensible to initial conditions. This means that two identical systems with very similar initial and dynamic conditions, present exponentially different evolution. A system is considered chaotic if its orbits in the phase space occupy the whole available space, instead of remaining within a confined region, and the orbits in the phase space seem disordered. The chaotic behaviour arises from nonlinearities in the differential equations from which the equations of motion are obtained.

In QM, systems cannot exhibit chaos. This is because the Schrödinger's equation is linear and the superposition principle holds always. Hence, there is no such sensitivity to initial conditions. Hence, the term 'quantum chaos' refers to cases where the classical analog to the quantum system is chaotic, but the case is in QM. See (Connerade 2005, Ch.10). This will be discusses again in Chapters 4 and 5.

negligible and for short times the classical predictions are the correct ones. This case was illustrated by Zurek and Paz (1999), who considered the classically chaotic system of Hyperion moon of Saturn. Through the evolution of Wigner function via Moyal brackets they predicted that, after 20 years the predictions of QM and CM diverge from each other. If QM was correct the moon would be found in a coherent superposition of classically distinguishable states. Obviously this is not the case, since the moon seems perfectly classical and it has been following a classically chaotic orbit for a lot longer than 20 years! In order to fix this problem and explain how QM correctly accounts for the classically chaotic orbit of Hyperion, they applied the decoherence programme.

Decoherence is seen as a phenomenon that occurs in nature when the system interacts with the environment and it plays a central role in many cutting-edge experiments and theoretical developments. The crucial relevance of decoherence motivates paying more detail than the rest of the topics discussed above. Equally, I will examine also the conceptual hypotheses involved in its development.

3.3.4 Decoherence and the QM-CM Limit

Previously I discussed three well known instances of the QM-CM limit, in the sense of obtaining theoretical features of CM as arising from the quantum. I argued that they are captured by Nickles reduction$_2$ or Post's GCP, whereby the successor theory (QM) progresses and develops by presenting a theoretical relationship with its predecessor (CM) – in the senses discussed earlier, e.g. end of Section 3.2.3. For each theoretical device I recalled criticisms known in the literature to the effect that they are all successful in accounting for the relationship QM-CM in a limited sense and, to a larger extent, deficient. Therefore they do not provide a sound and smooth account of the appearance of the classical from the quantum.

In this section I turn to what is known as decoherence in the context of foundations of QM. As I mentioned above in the introduction to Section 3.3, I consider the QM-CM limit, broadly understood as the problem of finding the classical arising from the quantum and more technically in the foundations of QM by the measurement problem and the preferred basis problem. Indeed, these two problems are the central issues for a realist interpretation of QM.[34]

[34]These two problems are central according to the Received View of the Realist Interpretation of QM: but, actually, how to explain the appearance of an empirical result, and the problem of the ambiguity of deciding which observable is being measured, seem to have an empiricist flavour. In Section 3.4 I will argue that realism should consider the problems of QM from the alternative Core Realist conception. A novel view on physical theories and intertheory relations would be more suitable than the Received View. This will be further discussed in Chapters 6 and 7.

To argue that decoherence provides the account of the appearance of the classical from the quantum is to argue that it resolves these two problems. However, as with the status of QM itself, there is disagreement over the role of decoherence. Therefore, I will offer a preliminary discussion before turning to the discussion of what decoherence is. Hence, firstly I will defend the following line of analysis: decoherence plays a role in the QM-CM limit understood in terms of the measurement problem and the preferred basis problem. I will then discuss these two technical problems. Secondly, I will discuss decoherence in as much detail as is necessary to philosophically engage with it: what are the physical and philosophical assumptions underlying the understanding of decoherence? How does the realist engage with decoherence? I am interested in in these questions. Thirdly, I will present arguments in favour of the claim that decoherence is not a solution to the interpretative problems of QM and given that the Received View takes those problems to be closely related to the problem of the QM-CM limit, the realist should consider interpreting decoherence in an instrumental manner and be motivated to consider a different realist framework than the Received View of the Realist Interpretation of QM.

Note that I dedicate significantly more attention to articulating an opinion on decoherence compared with the previous cases. This is because of its central relevance to current physics. There is a large amount of primary and secondary sources on decoherence and how it is accommodated within the philosophy of physics. Significant discussion is found in many places besides the original articles (Zeh 1970), (Zurek 1981, 1982, 2003), and others, e.g, (Giulini et al. 1996) (Bub 1997), (Schlosshauer 2005, 2007).

Preliminary Discussion: Decoherence and its Role in the QM-CM Limit

Decoherence has been developed through challenging the notion that quantum systems are closed and isolated from their environment, particularly since the work of Zeh (1970). Of course, it is known that all systems interact with their environment, but the qualitative step in the appreciation of decoherence is that the interaction between the system and its environment is such that the system cannot even approximately be considered as isolated. Hence, 'decoherence' is the name to the "disappearance" of the coherence in the superposition terms across macroscopically different properties in a really short timescale, due to the interaction with the environment, (Joos 1996, 2).

Evidenced by the immense written literature, technological progress and funding injected in this area, decoherence plays a central role in frontier projects. Zurek (2003) lists a large amount of applications and consequences of decoherence in physics, see also (Cirac and Zoller 1995). Environment-induced decoherence goes through much of the cutting-edge physics, both in theoretical and experimental areas. Nevertheless, much of the physics literature does not make concerted efforts to engage with philosophical reflection on physics, and thus the philosopher of science has valuable contributions to make.

I will argue that such a pragmatic role for decoherence does not override the fact that there are some considerations relevant to the philosopher – and, in particular, the realist – that should be taken with extreme caution, without at any point questioning the remarkable practical successes. Ultimately, my argument will be that although decoherence intends to account for the QM-CM limit and show the appearance of the classical from the quantum, it does not deliver. This claim is controversial. Indeed, although there is no disagreement on the instrumental value of decoherence, there is substantial disagreement on its status and its role in the realist account of QM.

Firstly, I will argue that there is significant vagueness in the conception of decoherence in the literature on foundations of QM. This is relevant because it is evidence for my claim that there is the disagreement on what decoherence is. Secondly, I will argue that decoherence does play a role in the conception of the QM-CM limit. And this is what I want to critically engage with.

First, let me point out some vagueness in talking of decoherence: Zeh (1996), one of the founding fathers of decoherence, speaks about the *programme* of decoherence, the *theory* of decoherence, the *phenomenon* of decoherence, and also in these terms: "Decoherence by 'continuous measurement' (as it was originally called) seems to represent the most fundamental irreversible *process* in Nature" (Zeh 1996, 12, my emphasis). In turn, Joos (1996, 2) talks about the *mechanisms* of decoherence. Finally, Schlosshauer (2007, viii) mentions decoherence as a *programme*. I do not argue that these terms entail a contradiction, but I do claim that this is not mere verbal disagreement. Whilst for the physicists all these terms might sound unproblematically similar, the philosopher is capable of recognising substantial differences.[35]

Now let me argue that decoherence does play a role in the account of the QM-CM limit, being something more than 'just a quantum phenomenon that occurs whenever the system interacts with its environment'. For instance, Zurek (2003, 717) expresses that decoherence provides the account of "how the environment distills the classical essence from quantum systems", an account of "why the quantum universe appears classical when it is seen 'from within' " (Zurek 2003, 718); Joos (1996, 1) opens the introduction to the collection of works about decoherence in the following way: "What distinguishes classical from quantum objects? What is the precise structure of the transition from quantum to classical? Is this transition smooth and harmless, or does it rather involve a sudden, abrupt change of concepts?"; in that same collection, Zeh (1996, 8-9) declares that "the theory of decoherence is to explain the difference in appearance between the

[35]I am grateful to Paul Knott for pressing on this point.

quantum and the classical under the assumption of a universally valid quantum theory"; indeed, that collection of works is titled "Decoherence and the Appearance of a Classical World in Quantum Theory"; finally, in his comprehensive review of the account of the QM-CM limit, Landsman (2007, 515-517) explains that, "originally, decoherence entered the scene as a proposed solution to the measurement problem and its goal is to explain the approximate appearance of the classical world from quantum mechanics seen as a universally valid theory". I hope that it will be clear to the reader that these comments support my claim that decoherence plays a relevant role in the account of the QM-CM limit.

Therefore, based on these two commentaries, I think it can be argued that decoherence is an account of the QM-CM limit, the explanation of the appearance of classicalities from the quantum, the justification for why the world appears classical, given that it is not.

After these clarificatory comments, I will defend the claim that as a response to the QM-CM limit, decoherence attempts to resolve the measurement problem and the preferred basis problem, which are considered to be central interpretative problems of QM according to the Received View. This is from the point of view of foundations of QM. Let us briefly return to the measurement problem, already mentioned in Section 2.2, and discuss the preferred basis problem. These two problems provide the basis for my discussion of decoherence.

QM-CM Limit as the Problem of Measurement and the Preferred Basis Problem

Now, as I discussed in the introduction to Section 3.3, there is some underdetermination in the understanding of the QM-CM limit. I will follow Schlosshauer (2005) in considering the QM-CM limit as the conjunction of the problems of the measurement and the preferred basis. Schlosshauer refers to the measurement problem as the problem of the outcomes, but this is mere nomenclature. Representing the standard view, he assesses that decoherence solves the problem of the preferred basis problem, but not the measurement problem.[36]

Let us revisit the measurement problem and see the problem of the preferred basis. In the measurement problem – discussed previously in Section 2.2.1 of Chapter 2 – the initial state of the system $\mathcal{S}$ and the measuring apparatus $\mathcal{A}$ in the Hilbert space

[36]Schlosshauer (2005) prefers to understand the measurement problem as including both the problem of the outcomes and the preferred basis, although I will keep them separated. The content of the discussion does not depend on these labels.

$\mathcal{H}^{\mathcal{S}} \otimes \mathcal{H}^{\mathcal{A}}$ evolves to a final state through Schrödinger evolution given by an interaction term between between them. That is,

$$|\psi\rangle \otimes |A_0\rangle \longrightarrow \sum_s c_s |s\rangle \otimes |A_s\rangle \,. \tag{3.20}$$

Consider, for instance, the double slit experiment. Even if the quantum particle is in a superposition of having passed the upper slit and having passed the lower slit, the Geiger-apparatus clicks once and at a specific point in the screen. Or, think of Schrödinger's cat paradox: the final state of the measurement is that the cat and the atomic system are in a superposition of dead– decayed and alive– non-decayed. However, once we open the box the cat is dead and the atom has decayed or the cat is alive and atom has not decayed.[37]

I discussed this earlier in Chapter 2. But let us indicate again the problem. Without a physical interpretation of the quantum superposition, or including an additional process, it is unclear how to consider that the right hand side of eq. (3.20) could be interpreted to mean that the pointer has a definite position/measurement outcome. The orthodox solution known as the "collapse of the wavefunction" was articulated by Von Neumann (1996) in 1932, whereby a non-unitary evolution forced the final state to be the one indicated by the apparatus. The eigenvalue-eigenstate link secures that the final state is the correct one. Hence, if the cat is alive when the box is opened, then the final state of the measurement interaction is $|\text{alive}\rangle \otimes |\text{non-decayed}\rangle$. Otherwise, if the cat is found dead, the final state is $|\text{dead}\rangle \otimes |\text{decayed}\rangle$. It is well known both that initially Von Neumann based his solution in the psycho-physical parallelism, which assigned a particular role to the consciousness of the observer. This view was subsequently abandoned due to criticisms particularly focused on the ad-hoc nature of the collapse. To a large extent, the realist interpretations of QM appeared as an attempt to provide an alternative solution to the measurement problem.

Now, besides the measurement problem, or the problem of the outcomes as Schlosshauer (2007, 53-55) and Bacciagaluppi (2013) call it, there is the less discussed problem of the preferred basis. This is explained in Zurek (1981, 1516) and it arises also from the final state in eq. (3.20), see (Schlosshauer 2005, 1272) and (Schlosshauer 2007, 55). Note that the final state can be expanded in different bases, which consequently leaves the observable that is being measured completely unspecified. Following Zurek (1981, 1516), because the apparatus is described quantum mechanically, one can change the basis from $\{|A_s\rangle\}$, to an alternative one $\{|A_r\rangle\}$: $|A_r\rangle = \sum_s \langle A_s|A_r\rangle |A_r\rangle$. Now, the final state of

[37] The double-slit experiment and Schrödinger's cat paradox are widely known thus I assume the reader to be broadly familiar with them.

eq. (3.20) is written as

$$\sum_s c_s |s\rangle \otimes |A_s\rangle = \sum_{s,r} c_s \langle A_s|A_r\rangle |s\rangle \otimes |A_r\rangle = \sum_r d_r |r\rangle \otimes |A_r\rangle . \tag{3.21}$$

Hence the ambiguity: what observable has been measured on the system, $\hat{S} = \sum_s e_s |s\rangle \langle s|$ or $\hat{R} = \sum_r e_r |r\rangle \langle r|$? For example, consider a 1/2-spin system with the pointer apparatus as the direction of the momentum operator, where the initial state of the system is

$$\frac{1}{\sqrt{2}} |\uparrow\rangle + \frac{1}{\sqrt{2}} |\downarrow\rangle . \tag{3.22}$$

There is an ambiguity here, because the measurement interaction will be the following:

$$\Big(\frac{1}{\sqrt{2}} |\uparrow\rangle + \frac{1}{\sqrt{2}} |\downarrow\rangle \Big) \otimes |0\rangle \longrightarrow \frac{1}{\sqrt{2}} |\uparrow\rangle \otimes |p_+\rangle + \frac{1}{\sqrt{2}} |\downarrow\rangle \otimes |p_-\rangle . \tag{3.23}$$

However, the final state at right hand side of eq. (3.23) could be expressed in a different basis:

$$\frac{1}{\sqrt{2}} |\uparrow\rangle \otimes |p_+\rangle + \frac{1}{\sqrt{2}} |\downarrow\rangle \otimes |p_-\rangle = \frac{1}{\sqrt{2}} |\rightarrow\rangle \otimes \left|p'_+\right\rangle + \frac{1}{\sqrt{2}} |\leftarrow\rangle \otimes \left|p'_-\right\rangle , \tag{3.24}$$

where $\{\left|p'_+\right\rangle , \left|p'_-\right\rangle\}$ is the momentum in the $\hat{x}$ direction, associated with the spin up or down in that same direction $\{|\rightarrow\rangle , |\leftarrow\rangle\}$.

Hence, the formalism applied to the isolated compound system $\mathcal{S}\otimes\mathcal{A}$ ambiguously allows different observables being measured. That is, the right hand side of eq. (3.20) does not define which observable is being measured. Following Zurek (1981, 1519), this is the well-known preferred basis problem. However, we know from experience that the apparatus measures the magnitude it was made to measure, and not a different one. The idea is that the measured magnitude is not arbitrary, even if the formalism says otherwise.

For any realist approach to QM, the measurement problem and the problem of the preferred basis are extremely troubling. Yet, one can do away with the issue if the collapse of the wavefunction and the associated eigenstate-eigenvalue link are simply accepted (whatever your justification/motivations might be). One could take the final state given by Schrödinger equation non-unitarily collapsed to the final state specified by what the observer finds as the outcome. In addition, that also determines the observable being measured. The orthodox approach solves both problems. However, the cost is precisely to rely on an ad-hoc principle and an empirically contentious non-unitary evolution. Indeed, physicists continue to rely widely on them despite the wide array of criticisms in the philosophy literature.[38] Thus, such an approach would need substantial discussion to prove its commitment to scientific realism.

The literature on foundations of QM agrees that decoherence does not solve the measurement problem, because the improper mixture obtained at the end of the calculations does not pick out a term that is associated with the measurement outcome. Decoherence does not yet tell you which outcome will be obtained, see (Joos 1996, 4), (Joos 2000, 4), Bacciagaluppi (2013), (Schlosshauer 2007, 55) and more discussion in (Schlosshauer 2005, 1268). However, with regards to the preferred basis problem there is debate. As said before, Zurek and Schlosshauer, amongst others, claim that decoherence solves the preferred basis problem, whilst, for instance, Lombardi and Vanni (2010) disagree. I will argue that there are some worries with regards to decoherence at a prior stage. The issues I will note refer to the terms and conditions of the problems and in what decoherence can be expected to achieve. Thus, my intention is to call for the realist to take extra caution when interpreting physical consequences of decoherence.

In order to show and later critically engage with how decoherence works, I will discuss a well-known model of the 'decoherence of a single qubit', taken from (Zurek 1982, 1864-ff), (Zurek 2003, 730-ff) and (d'Espagnat 1995, Sect. 10.6).

A Standard Model of Decoherence

I have argued that decoherence plays a significant role in accounting for the QM-CM limit, understanding the latter as the measurement problem (the problem of the outcomes in Schlosshauer's nomenclature) and the preferred basis problem. Now I will present a simple model of decoherence, and then I will engage with its criticism from the point of view of realism. Decoherence is discussed widely in the literature in various guises, yet I will follow the seminal paper Zurek (1982), which is still valid and a primary source for this topic. Indeed, (Schlosshauer 2005, Sect. III.D.2) is essentially identical to Zurek's 1982 paper.

Let us consider 1/2-spin particle as our system $\mathcal{S}$ described in the $\hat{z}$ direction with the kets $|\uparrow\rangle$ and $|\downarrow\rangle$ and the apparatus $\mathcal{A}$ also a two-state system – described by the states $\{|p_+\rangle, |p_-\rangle\}$ as before in eq. (3.23). As (Zurek 1982, 1864) explains, the apparatus can be seen as an atom with a ground and excited states with the same energy, which can be formally considered as another 1/2-spin system. The self-energies of the apparatus and the system can be ignored, and only an interaction hamiltonian $H^{\mathcal{SA}}$ is considered.

[38] As mentioned in Chapter 2, Wallace (2016b) has questioned that the collapse really appears in what is called "orthodox" QM. However, as I indicated before, physicists learn the collapse and the e-e link from the early stages in their career. To verify this, it suffices to check any widely used textbook. See page 17.

$H^{\mathcal{SA}}$ operates for a short time and its intensity is determined by g a coupling constant. The interaction $H^{\mathcal{SA}}$ is given by

$$H^{\mathcal{SA}} = g\Big(\left|\uparrow\right\rangle \left\langle\uparrow\right| - \left|\downarrow\right\rangle \left\langle\downarrow\right| \Big) \otimes \Big(\left|p^z_+\right\rangle \left\langle p^z_+\right| - \left|p^z_-\right\rangle \left\langle p^z_-\right| \Big). \tag{3.25}$$

In this situation, $\left|p^z_\pm\right\rangle$ are the two possible outcomes of the apparatus that are correlated to the spin $s_z = \pm$ of the system $\mathcal{S}$ (in units of $\hbar/2$). Now, let us assume a general initial state for the system $\mathcal{S}$ that is a linear combination of the basis states in z:

$$\left|\phi_i\right\rangle = a\left|\uparrow\right\rangle + b\left|\downarrow\right\rangle, \tag{3.26}$$

with $a, b \in \mathbb{C}$ and $|a|^2 + |b|^2 = 1$, and consider $\left|p^x_+\right\rangle$ the initial state of the apparatus $\mathcal{A}$. The hamiltonian in eq. (3.25) will take

$$\left|\psi_i\right\rangle = \left|\phi_i\right\rangle \otimes \left|p^x_+\right\rangle \longrightarrow \left|\psi_f\right\rangle = a\left|\uparrow\right\rangle \otimes \left|p^z_+\right\rangle + b\left|\downarrow\right\rangle \otimes \left|p^z_-\right\rangle. \tag{3.27}$$

This situation is analogous to the final state of the measurement problem discussed above in eq. (3.20). Now, similar to the discussion in eq. (3.24), one could change the basis in which eq. (3.27) is written, and realise that the formalism does not specify which observable is being measured. That is, we have the preferred basis problem, see (Zurek 1982, 1865). At this point the environment enters into the scene.

Decoherence conceives of the system (in this case composite system $\mathcal{SA}$) as an open quantum system that interacts at $t = 0$ with a previously uncorrelated environment $\mathcal{E}$. The environment $\mathcal{E}$ consists of a large number N of 1/2-spin systems, described by bases $\{\left|u_+\right\rangle_k, \left|u_-\right\rangle_k\}_{k\in[1,2,...,N]}$, so that $\left|u_+\right\rangle_k$ and $\left|u_-\right\rangle_k$ are the eigenstates of $S^{\mathcal{E},k}_z$, the component in $\hat{z}$ of the kth spin of the environment $\mathcal{E}$, analogous to $\left|p^z_+\right\rangle, \left|p^z_-\right\rangle$ above.

The initial state of $\mathcal{SA}$ when the interaction with the environment begins (which we now consider as $t = 0$ for simplicity) is the final state of the measurement interaction, $\left|\psi_f\right\rangle = a\left|\uparrow\right\rangle \otimes \left|p^z_+\right\rangle$, which will be now called the 'pre-measurement' state. Therefore, the new initial state $\left|\chi(0)\right\rangle$ of the composite system $\mathcal{SAE}$ that now includes the environment is

$$\left|\chi(0)\right\rangle = \left|\psi_f\right\rangle \otimes \Big(\alpha_1\left|u_+\right\rangle_1 + \beta_1\left|u_-\right\rangle_1 \otimes \alpha_2\left|u_+\right\rangle_2 + \beta_2\left|u_-\right\rangle_2 \otimes ... \otimes \alpha_N\left|u_+\right\rangle_N + \beta_N\left|u_-\right\rangle_N\Big) \tag{3.28}$$

where $\alpha_k, \beta_k \in \mathbb{C}$, $|\alpha_k|^2 + |\beta_k|^2 = 1$. In a more compact notation $\left|\chi(0)\right\rangle$ can be put as,

$$\left|\chi(0)\right\rangle = \left|\psi_f\right\rangle \bigotimes_{k=1}^{N} (\alpha_k\left|u_+\right\rangle_k + \beta_k\left|u_-\right\rangle_k). \tag{3.29}$$

Now we make similar considerations as the discussion about the interaction between $\mathcal{S}$ and $\mathcal{A}$. In terms of the interaction in the system $\mathcal{SAE}$, the first consideration is to disregard, for practical purposes, the self-hamiltonians of each subsystem. That is, we do not consider the self-energy of $\mathcal{S}$, $\mathcal{A}$ and $\mathcal{E}$. The only relevant term is the interaction between the apparatus $\mathcal{A}$ and each kth element of the environment $\mathcal{E}$. However, recall that the apparatus $\mathcal{A}$ is correlated with the system $\mathcal{S}$ as per the state $|\psi_f\rangle$. The interaction between the apparatus and the kth element of the environment is given by

$$g_k(\left|p_+^z\right\rangle\left\langle p_+^z\right| - \left|p_-^z\right\rangle\left\langle p_-^z\right|) \otimes (|u_+\rangle\langle u_+| - |u_-\rangle\langle u_-|)_k, \tag{3.30}$$

with analogous meaning for g_k as a coupling constant. Therefore, the total hamiltonian for the composite system $\mathcal{SAE}$ includes the interaction between the apparatus and each kth element of the environment, and it is

$$H^{\mathcal{AE}} = \sum_k H_k^{\mathcal{AE}}, \tag{3.31}$$

where the kth component is given by

$$\begin{aligned} H_k^{\mathcal{AE}} &= g_k(\left|p_+^z\right\rangle\left\langle p_+^z\right| - \left|p_-^z\right\rangle\left\langle p_-^z\right|) \otimes (|u_+\rangle\langle u_+| - |u_-\rangle\langle u_-|)_k \otimes_{j\neq k} 1 \\ &= \underbrace{\mathbf{1}\otimes\mathbf{1}\otimes\ldots\otimes}_{k-1} g_k S_z^{\mathcal{A}} \otimes S_z^{\mathcal{E},k} \underbrace{\otimes\mathbf{1}\otimes\ldots\otimes\mathbf{1}}_{N-k}. \end{aligned} \tag{3.32}$$

The argument developed by Zurek (1982, 1865) aims at showing that this interaction hamiltonian will preclude the apparatus $\mathcal{S}$ being in a superposition state, which would be the solution to the measurement problem and the preferred basis problem.

Now we use the hamiltonian in eq. (3.32) to evolve the initial state in eq. (3.29) at $t = 0$ to time t. The state will be (consider $\hbar = 1$)

$$\begin{aligned} |\chi(t)\rangle &= a\,|s_+\rangle \bigotimes_k \Big(\alpha_k \exp(ig_k t)\,|u_+\rangle_k + \beta_k \exp(-ig_k t)\,|u_-\rangle_k\Big) \\ &\quad + b\,|s_-\rangle \bigotimes_k \Big(\alpha_k \exp(-ig_k t)\,|u_+\rangle_k + \beta_k \exp(ig_k t)\,|u_-\rangle_k\Big), \end{aligned} \tag{3.33}$$

using the notation $|s_+\rangle = |\uparrow\rangle \otimes \left|p_+^z\right\rangle$ and $|s_-\rangle = |\downarrow\rangle \otimes \left|p_-^z\right\rangle$. Now, consider the following states[39]

$$|\varepsilon_+(t)\rangle = \bigotimes_k \Big(\alpha_k \exp(ig_k t)\,|u_+\rangle_k + \beta_k \exp(-ig_k t)\,|u_-\rangle_k\Big) \tag{3.34}$$

$$|\varepsilon_-(t)\rangle = \bigotimes_k \Big(\alpha_k \exp(-ig_k t)\,|u_+\rangle_k + \beta_k \exp(ig_k t)\,|u_-\rangle_k\Big) = |\varepsilon_+(-t)\rangle\,, \tag{3.35}$$

[39] Following the standard account presented in (Zurek 1982, 1864) and (Zurek 2003, 730).

which are not orthogonal as $\langle \varepsilon_+(t)|\varepsilon_-(t)\rangle \neq 0$, but can help rewrite $|\chi(t)\rangle$ from eq. (3.33) as

$$|\chi(t)\rangle = a\,|s_+\rangle \otimes |\varepsilon_+(t)\rangle + b\,|s_-\rangle \otimes |\varepsilon_-(t)\rangle\,. \tag{3.36}$$

This is analogous to the final state of the measurement in eq. (3.20). The subsystem system + apparatus, $\mathcal{SA}$, is entangled with the environment $\mathcal{E}$. Now, let us take the statistical operator (or matrix operator) for the entire system $\mathcal{SAE}$ in the pure state $|\chi(t)\rangle$ as usual:

$$\begin{aligned} \rho^{\mathcal{SAE}}(t) &= |\chi(t)\rangle\,\langle\chi(t)| && (3.37)\\ &= |a|^2\,|s_+\rangle\,\langle s_+| \otimes |\varepsilon_+(t)\rangle\,\langle\varepsilon_+(t)| + ab^*\,|s_+\rangle\,\langle s_-| \otimes |\varepsilon_+(t)\rangle\,\langle\varepsilon_-(t)| \\ &\quad a^*b\,|s_-\rangle\,\langle s_+| \otimes |\varepsilon_-(t)\rangle\,\langle\varepsilon_+(t)| + |b|^2\,|s_-\rangle\,\langle s_-| \otimes |\varepsilon_-(t)\rangle\,\langle\varepsilon_-(t)|\,. && (3.38)\end{aligned}$$

The cross-terms, the off-diagonal terms in the matrix of eq. (3.38) preclude the interpretation of the pure state of the total system $\mathcal{SAE}$ as a 'classical' state. This state has coherences, off-diagonal terms. The coherence of the overall state in eq. (3.37) is maintained always and cannot change through a unitarian evolution (which is the type of evolution that we assumed throughout, given by the Schrödinger equation). What the process of decoherence obtains is a 'redistribution' of such coherences.

Now, when the system is composite, one can obtain a description of one of the subsystems by tracing out the degrees of freedom of the rest of the total system.[40] In this case, we trace out the degrees of freedom of the environment $\mathcal{E}$, in order to obtain the statistical operator of the subsystem $\mathcal{SA}$:

$$\begin{aligned} \rho^{\mathcal{SA}}(t) &= \mathrm{Tr}_{\mathcal{E}}\big(\,|\chi(t)\rangle\,\langle\chi(t)|\,\big) \\ &= |a|^2\,|s_+\rangle\,\langle s_+| + \langle\varepsilon_+(t)|\varepsilon_-(t)\rangle\,ab^*\,|s_+\rangle\,\langle s_-| \\ &\quad + \langle\varepsilon_-(t)|\varepsilon_+(t)\rangle\,a^*b\,|s_-\rangle\,\langle s_+| + |b|^2\,|s_-\rangle\,\langle s_-|\,. \end{aligned} \tag{3.39}$$

The achievement of decoherence through the monitoring effect of the environment $\mathcal{E}$ on the subsystem composed by the system and apparatus $\mathcal{SA}$ is to "damp out" the correlations, the off-diagonal terms in eq. (3.39).

[40] This is an improper mixture. This is crucial and I will discuss it further below. Unfortunately, literature in current foundations of physics fails to understand their meaning. Nielsen and Chuang (2010, 106) wrongly tell us that partial traces give us a "state about which we apparently do not have maximal knowledge". This is false for partial traces obtain a reduced density operator and this cannot be interpreted in terms of ignorance. That assertion is true for *proper* mixtures, see d'Espagnat (1976, 1995).

Let us note the correlation amplitude $\langle \varepsilon_+(t) | \varepsilon_-(t) \rangle = z(t)$, which as a consequence of previous equations takes the explicit form

$$z(t) = \langle \varepsilon_+(t) | \varepsilon_-(t) \rangle = \prod_{k=1}^{N} \Big[\cos(2g_k t) + i(|\alpha_k|^2 - |\beta_k|^2 \sin(2g_k t)) \Big]. \tag{3.40}$$

The terms of eq. (3.39) that include $z(t)$ are the off-diagonal terms, the coherences or interference terms in the matrix operator, in terms of $\{|s_+\rangle, |s_-\rangle\}$.

Zurek (1982, 1866) obtains – see also (Schlosshauer 2005, 1277) – that $z(t)$ becomes significantly small when taking the limit $N \to \infty$ and the time $t \to \infty$. That is, the average for large times of $|z(t)|^2$ is

$$\langle |z(t)|^2 \rangle_{t\to\infty} \simeq 2^{-N} \prod_{k=1}^{N} [1 + (|\alpha_k|^2 - |\beta_k|^2)^2] \xrightarrow[N\to\infty]{} 0. \tag{3.41}$$

This last formula shows that the correlation amplitude $z(t)$ which is 1 at $t = 0$, drops down and is zero at $t = \infty$. Hence, the off-diagonal terms in eq. (3.39) decrease exponentially fast with the size N of the environment coupled with the apparatus.

As Schlosshauer (2005, 1277, 1279) discusses, the damping happens in a really short time. Even microscopic systems are rapidly decohered by the interaction with the environment, the thermal radiation for instance. The time τ within which $|z(t)| \to 0$ is of a much shorter scale than any practical observation could resolve. In addition, with a similar model of decoherence that the one discussed here, Joos and Zeh (1985) analyse the case of a dust grain floating in the air at room temperature. Defining a coherence length as a distance beyond which no interference should be shown, they find that it is the same that its de Broglie wavelength $\lambda \sim 10^{-14}$ cm, for a dust speck of radius 10^{-5} cm. The time in which the coherence is delocalised for a system in a minimal environment is surprisingly quick. Further calculations are shown in (Bacciagaluppi 2000) and more comments in (Bacciagaluppi 2016).

Another case where decoherence succeeded with establishing the adequacy of quantum predictions is with regards to the dynamics of Hyperion (a classical system), which chaotically orbits around Jupiter. Whilst the time when classical and quantum predictions diverge from each other is around 20 years, the system is about 4 billion years old. Hence, quantum effects should have been observed in its orbit. But this contradicts the empirical fact that Hyperion looks classical. Decoherence solves this disagreement between classical and quantum description of the system, see (Zurek and Paz 1997) and (Bokulich 2008a, 21-27). Zurek and Paz show that the environment secures the seemingly classical dynamic of the moon, because the quantum superpositions decohere.

The tendency of $|z(t)|$ to go to zero so quickly shows the efficacy of decoherence. Under these conditions, any observable belonging to the system+apparatus $\mathcal{SA}$ would have, *for all practical purposes*, the same mean values that would have obtained had the system $\mathcal{SA}$ been a proper mixture described by

$$\tilde{\rho}^{\mathcal{SA}}(t) = |a|^2 |s_+\rangle \langle s_+| + |b|^2 |s_-\rangle \langle s_-| \,. \tag{3.42}$$

Note that the density matrix $\rho^{\mathcal{SA}}$ in eq. (3.44) is, strictly speaking, different to the matrix $\tilde{\rho}^{\mathcal{SA}}$ in eq. (3.42). Their difference lies in the former being an approximation: as the off-diagonal terms decay to zero ($|z(t)| \to 0$), whilst the latter is a proper mixture: a statistical mixture of states $|s_+\rangle \langle s_+|$ and states $|s_-\rangle \langle s_-|$ with statistical weights $|a|^2$ and $|b|^2$ respectively. Despite this conceptual difference, for all practical purposes,[41] measurements on the former subsystem $\mathcal{SA}$ of the total system $\mathcal{SAE}$ will be the same as the latter case where the total system is $\mathcal{SA}$.

Now, how does this help with the measurement problem and the problem of the preferred basis?

With regards to the measurement problem, there is the effect of environment-induced decoherence. This is the fast local suppression of interference between different states of the system. The discussion above shows that there is a typically very short time within which the reduced density matrix in eq. (3.39) including off-diagonal terms, becomes approximately diagonal

$$|z(t)| \to 0, \text{ implies} \tag{3.43}$$

$$\rho^{\mathcal{SA}} \to |a|^2 |\uparrow\rangle \langle\uparrow| \otimes |p_+^z\rangle \langle p_+^z| + |b|^2 |\downarrow\rangle \langle\downarrow| \otimes |p_-^z\rangle \langle p_-^z| \,, \tag{3.44}$$

recall the previous notation $|s_+\rangle = |\uparrow\rangle \otimes |p_+^z\rangle$ and $|s_-\rangle = |\downarrow\rangle \otimes |p_-^z\rangle$.

Although, as said in page 78, the unitary evolution does not destroy the global phase coherence. What happens is that such a coherence is delocalised away from the reduced density matrix that describes the system and apparatus $\mathcal{SA}$ to the degrees of freedom of the environment $\mathcal{E}$, see (Schlosshauer 2005, 1276). The effect of "environment-induced decoherence" is to have arrived at approximately diagonal density matrix for $\mathcal{SA}$, displaying pointer states which remain in spite of the environment, whilst their superpositions lose phase coherence and decohere, see (Zurek 2003, 717). In conclusion, the environment effectively "destroys" the correlation between states that correspond to different eigenvalues of H^{SA}, the environment precludes coherent superpositions. In subsequent Sections I will discuss the extent to which this is a successful response to the measurement problem.

[41] This is a technical term and I will come back to engage with it below.

With regards to the preferred basis problem, the effect of the environment seems more successful since the seminal work in (Zurek 1981). This is called the environment-induced superselection. Decoherence selects the preferred pointer basis, which does not change the system-apparatus correlations despite the interaction between the apparatus and the environment. The apparatus states (the pointer states) are those whose associated projection operators commute with the interaction hamiltonian $H^{\mathcal{A}\mathcal{E}}$, see (Zurek 1981, 1520) and (Schlosshauer 2005, 1278-1279). That is,

$$[H^{\mathcal{A}\mathcal{E}}, \hat{P}_n^{\mathcal{A}}] = 0, \quad \forall n, \tag{3.45}$$

in our case $\hat{P}_1^{\mathcal{A}} = |p_+^z\rangle \langle p_+^z|$ and $\hat{P}_2^{\mathcal{A}} = |p_-^z\rangle \langle p_-^z|$. Then, any correlation of the system $\mathcal{S}$ with the eigenstates of a preferred apparatus observable,

$$\hat{O}_{\mathcal{A}} = \lambda_1 |p_+^z\rangle \langle p_+^z| + \lambda_2 |p_-^z\rangle \langle p_-^z|, \tag{3.46}$$

where $\lambda_1, \lambda_2 \in \mathbb{R}$. Zurek (1981, 1522) proves that the apparatus-system will retain perfect correlation in only one product basis of $\mathcal{S}\mathcal{A}$, $\{|\uparrow\rangle \otimes |p_+^z\rangle, |\downarrow\rangle \otimes |p_-^z\rangle\}$. Hence pointer basis states of the apparatus are $\{|p_+^z\rangle, |p_-^z\rangle\}$. Therefore, the correlations $\mathcal{S}$-system– $\mathcal{A}$-apparatus between pointer states that are not eigenstates of an observable that commutes with $H^{\mathcal{A}\mathcal{E}}$ will be quickly delocalised by the monitoring effect of the environment over the apparatus, see (Schlosshauer 2005, 1279).

Decoherence as a Solution to the QM-CM Limit

At the beginning of Section 3.3.4 and in the preliminary discussion, I argued that there is some disagreement in the literature on the foundations of QM on the status of the terms and conditions of the QM-CM limit. I recalled some relevant opinions to show this disagreement, namely the broad views held by Bacciagaluppi, Schlosshauer and Landsman. Moreover, I also recalled that whilst it is known that decoherence does not resolve all the problems, its achievements are not entirely agreed upon.

With regards to the measurement problem, it is accepted that decoherence does not solve it. Indeed, as Bacciagaluppi (2016, Sect. 2) puts it, decoherence even exacerbates the measurement problem, for "if everything is in interaction with everything else, everything is generically entangled with everything else, and that is a worse problem than measuring apparatuses being entangled with the measured systems". I mentioned this before on page 75 and this is argued in the likes of (Joos 1996, 4), (Joos 2000, 4), (Bacciagaluppi 2013, 2016), (Schlosshauer 2007, 55), (Adler 2003, 136). In the pragmatic take on

QM, which has been commonly called the Copenhagen interpretation,[42] the eigenstate-eigenvalue link provides the correct device for associating the final state in eq. (3.20) with the observed value in the apparatus. A solution to the measurement problem would be expected to achieve the same final state without postulating such ad-hoc prescription nor invoking the collapse of the wavefunction, but decoherence does not deliver on this.

With regards to the problem of the preferred basis, the literature broadly considers that decoherence is a solution. Based on (Zurek 1981, 1517), Schlosshauer (2005, 1280-ff) explains that decoherence does solve the problem of the preferred basis. The preferred basis is not chosen in an ad-hoc manner in order to make measurement records match with our experience, but is done on observer-free grounds, through the interaction between the environment and the apparatus, as I discussed above. The basis that is preferred is that which contains reliable records on the state of the system, which means that the system-apparatus correlation are untouched despite the environment-apparatus interaction. That is precisely what was obtained in eq. (3.44). Such a density matrix has a defined basis and observable being measured on the system. Moreover, there are formal conditions that guarantee the existence of this basis, see (Schlosshauer 2005, 1278).

Now, in the next paragraph I will share the opinion that decoherence does not solve the measurement problem, I will present objections to the view that decoherence solves the preferred basis problem and I will indicate two sources of worry from a foundational or conceptual point of view.

Objections to Decoherence as a Realist Account of the QM-CM Limit

Predictions made through the theory of decoherence have an incredible accuracy and the work of Joos, Zeh, Zurek, Paz, and many others, represent a substantial advance in physics. Even if one disagrees with the merits of the models of decoherence, the empirical accuracy strongly suggests that at least something like the process of decoherence does happen in nature. However, I claim that there are serious concerns for the realist. It often happens that useful and reliable tools for making predictions in physics, are not uncontroversial for the philosopher who considers a conceptual point of view. For example: Dirac's delta function, renormalisation in quantum field theory, or even Feynman's path integral formalism. I claim that one can critically engage with the philosophy of physics and the practice of physicists, without denying the physics itself.

My criticism of the claim that decoherence is the ultimate solution to the problems of QM and a description of the QM-CM limit, involves two issues. Firstly, I argue that

[42] However, for historical and philosophical reasons, it is fair to claim that the Copenhagen interpretation simply does not exist, see (Howard 2004).

decoherence works *only* "for all practical purposes" and that this is problematic, in a special sense that I will clarify and, secondly, that there is a potentially vicious circularity involved. In order to defend these claims, a detailed discussion of the nature of improper mixtures is required.

The distinction between proper and improper mixtures has been known since the 1970s with (d'Espagnat 1976, 1995) and (Hellwig and Kraus 1968).[43] The argumentative force of this distinction is perhaps irrelevant if conceptual issues are of less importance than pragmatic, empirical ones. Indeed, the incredibly successful applications of decoherence-based approaches ignore the distinction. However, the realist is concerned with foundational questions, and for her the distinction between proper and improper mixtures ought to be of crucial relevance, as I will argue. Moreover, this discussion is particularly appropriate here because the final state of the decoherence process, the density matrix in eq. (3.39), is an improper mixture, as I mentioned on page 78. The following comment is well-known in the literature but it is important to recall it in order that I can argue the proper/improper distinction. The bottom line is that it is not possible to simply claim that an improper mixture describes one of the subsystems. Furthermore, it is unclear how to interpret improper mixtures.

Firstly let me recall the distinction between a separable and an entangled state.[44] Following Horodecki et al. (2009, 882), consider a composite bipartite system made of system S and environment E the Hilbert space of which is the tensor product of the Hilbert spaces of the two subsystems: $\mathcal{H} = \mathcal{H}^{\mathcal{S}} \otimes \mathcal{H}^{\mathcal{E}}$, with d_S and d_E the dimensions of each space. Starting with both subsystems in pure states $\rho_S = |\psi_S\rangle \langle\psi_S|$ and $\rho_E = |\psi_E\rangle \langle\psi_E|$, there are essentially two cases: 1) the subsystems have not interacted or 2) they have interacted and we look at the state of the composite system after the interaction has ended.[45] In the first case, both subsystems are described by a pure state and the state of the composite system is a product state, the density operator of which is

$$\rho = \rho_S \otimes \rho_E. \tag{3.47}$$

This state describes subsystems that are uncorrelated. Consequently, observables $\hat{O}_S \in \mathcal{B}(\mathcal{H}^{\mathcal{S}})$ and $\hat{O}_E \in \mathcal{B}(\mathcal{H}^{\mathcal{E}})$ that act on the product space $\hat{O}_S \otimes \hat{O}_E$ have independent statistical values. That is, the mean value of an observable $\hat{O}_S \otimes \hat{O}_E \in \mathcal{B}(\mathcal{H}^{\mathcal{S}} \otimes \mathcal{H}^{\mathcal{E}})$ will

[43] Interesting discussion about the history of the improper mixtures is given in (Bub 1997, Sec. 8.1.) and (Masillo et al. 2009).

[44] There are many sources that discuss this, of course. See, for instance, (Vermaas 1999, 227) or (Horodecki et al. 2009).

[45] There is a precise definition of pure state. A system represented by a pure state is such that there exists a measurement the outcome of which is obtained with probability 1. A pure state can be expressed as a ket or, alternatively, in the language of density matrices, as a one-dimension projection operator $|\alpha\rangle \langle\alpha|$. By contrast, a non-pure state can neither be written as a vector state, a wavefunction or a ket. Non-pure states can only be described by a density matrix, see (Primas 1981, 200).

be "separable":

$$< \hat{O}_S \otimes \hat{O}_E > = \mathrm{Tr}(\rho \hat{O}_S \otimes \hat{O}_E) \tag{3.48}$$

$$= \mathrm{Tr}^E(\rho_S \hat{O}_S)\mathrm{Tr}^S(\rho_E \hat{O}_E) \tag{3.49}$$

$$= < \hat{O}_S >< \hat{O}_E > . \tag{3.50}$$

Therefore, $\rho = \rho_S \otimes \rho_E$ is a separable state. In general, separable states are defined as those states that can be expressed as a convex combination of product states

$$\rho = \sum_j \lambda_j \rho_S^j \otimes \rho_E^j, \tag{3.51}$$

with $\sum_j \lambda_j = 1$ and $\lambda_j \geq 0$.

The second case is where S and E interacted in the past and we look at the system after the interaction. In this case, the state will not be separable, but entangled. The state of the composite system is

$$|\psi\rangle = \sum_{i=0}^{d_S-1} \sum_{j=0}^{d_E-1} \lambda_{ij}^{\psi} |s_i\rangle \otimes |e_j\rangle \tag{3.52}$$

where $|s\rangle \in \mathcal{H}^{\mathcal{S}}$, $|e\rangle \in \mathcal{H}^{\mathcal{E}}$ and $\lambda^\psi \in \mathbb{C}^{d_S \times d_E}$.

Now, in general, it is not trivial to tell whether a state is entangled or separable. If we are given a ket and we are told that it represents a composite system, we know that the composite system is in a pure state and it will look like eq. (3.52). But how do we know whether the pure state is separable or entangled? The Schmidt decomposition provides a criterion to determine this.[46] $|\psi\rangle$ can be expressed in its Schmidt decomposition using a bi-orthonormal basis $\{|\alpha_i^S\rangle \otimes \left|\beta_j^E\right\rangle\}$ of $\mathcal{H}$ such that

$$|\psi\rangle = \sum_{i=0}^{r(\psi)} a_i \left|\alpha_i^S\right\rangle \otimes \left|\beta_i^E\right\rangle, \tag{3.53}$$

where $r(\psi)$ is the rank of the matrix λ^ψ and a_i are its non-zero singular eigenvalues. $|\psi\rangle$ is separable if only one non-zero Schmidt coefficient is obtained, that is, only if $r(\psi) = 1$. If the state is not separable, then there are more than one Schmidt coefficients and the state is entangled. In addition, the Schmidt decomposition is unique if the coefficients are all different.[47]

[46]The case where the composite system is bipartite is the simplest case. However, the question of whether the state is product or entangled is highly nontrivial for systems composed by more than two parts, see (Horodecki et al. 2009, 882-890).

[47]Note that if the coefficients are different and the Schmidt decomposition is unique, the ambiguity that we encountered in the example given by Zurek, discussed in eq. (3.22) does not appear. In the

For example, consider the pure state $|\phi\rangle$ of a compound system of two 1/2-spin systems in the Hilbert space $\mathbb{C}^2 \otimes \mathbb{C}^2$

$$|\phi\rangle = \frac{1}{\sqrt{2}} |+\rangle \otimes |+\rangle + \frac{1}{\sqrt{2}} |-\rangle \otimes |-\rangle \, . \tag{3.54}$$

$|\phi\rangle$ is a pure state and because it is an entangled, there are no states $|a\rangle , |b\rangle \in \mathbb{C}^2$ such that $|\phi\rangle = |a\rangle \otimes |b\rangle$. If you measure on the basis $\{|-\rangle , |+\rangle\}$ on any of the two subsystems, the probability for each result is $(\pm\hbar/2)$ is 1/2, but if you measure on particle 1 and $+\hbar/2$ is obtained, then $+\hbar/2$ will be obtained on particle 2 with certainty. That is, the subsystems are correlated.

Now let me recall the density matrix. The complete description of the composite system is given by its density matrix or density operator, which can be written as

$$\rho = \sum_{ij} a_i a_j^* |\alpha_i \otimes \beta_i\rangle \langle \alpha_j \otimes \beta_j| \, . \tag{3.55}$$

This is such for both separable and entangled states. If the state is separable, there are no correlations between the subsystems and it will be possible to write the state as a product state, as in eq. (3.47). By contrast, in the entangled case the state cannot be written as a product state and there are correlations between the subsystems.

Let us consider that ρ is an entangled state. Hence, it is not possible to consider individual subsystems with individual properties. Nevertheless, there is a way to "separate" it in subsystems, by using improper mixtures. If we are only capable of performing measurements of one of the subsystems only, the observables will be of the type $\hat{O}_S \otimes \mathbf{1}_E$ and $\mathbf{1}_S \otimes \hat{O}_E$. Then we will, at best, be able to reconstruct the density operators given by the partial traces:

$$\rho_S = Tr^E(|\psi\rangle \langle\psi|) = \sum_n |a_n|^2 |\alpha_n\rangle \langle\alpha_n| \tag{3.56}$$

$$\rho_E = Tr^S(|\psi\rangle \langle\psi|) = \sum_n |a_n|^2 |\beta_n\rangle \langle\beta_n| \, . \tag{3.57}$$

However, because ρ is entangled, the product of the matrices in eqs. (3.56) and (3.57) does not obtain ρ. Instead, the product of these two improper mixtures is

$$\rho' = \rho_S \otimes \rho_E, \tag{3.58}$$

case where the coefficients are equal, one can change the basis to represent the state, and that is the problem of the preferred basis. But, if the Schmidt decomposition is unique, there is only one way to write the state of the composite system. Lombardi and Vanni (2010) base their argument in this observation. They claim that Zurek's example is unfair, because it departs from the unlikely case where the coefficients are equal and the decomposition is not unique. The claim that in most cases there is no preferred basis problem because the states with equal coefficients are of measure zero.

with spectral resolution $\sum_n |a_n|^2 \, |\alpha_n \otimes \beta_n\rangle \, \langle \alpha_n \otimes \beta_n|$.

Let me insist that $\rho' \neq \rho$. Their difference is conceptual: ρ' is a separable state which is not a projection operator and it cannot represent a pure state, because $\rho'^2 \neq \rho'$. Whilst ρ describes an entangled composite system in a pure state. The nature of the system cannot change by taking a partial trace, it is still entangled. The difference between ρ' and ρ shows that knowledge obtained from measurements on the subsystems only – from which we can obtain ρ' – is not enough to recover the density operator ρ that describes the composite system. In order to obtain ρ one would require also the correlations between the subsystems. But these cannot be known because, by hypothesis, we can only measure each subsystem separately. The difference between ρ' and ρ is that ρ includes coherences, correlations between the subsystems. These are coded in phases of the coefficients a_i in eqs. (3.56) and (3.57). And precisely those correlations are lost when tracing out the degrees of freedom of one of the subsystems, which is evident by noting that the matrixes in eqs. (3.56) and (3.57) include the absolute value of the coefficients. The correlations cannot be obtained by measuring on the partial systems only. They can only be obtained by measuring on the entire system, see (d'Espagnat 1995, Ch. 7) and (Primas 1981, 144).

The bottom line of this commentary is that, firstly, one cannot unitarily transform a pure state into a proper mixture, see (Landsman forthcoming, Sect. 11.3). Secondly, one cannot claim that the density operators in eqs. (3.56) and (3.57) are the description of the subsystems S and E, respectively. For these two improper mixtures describe the subsystems only in a restricted manner. In relation to the discussion of decoherence, the final state of the system, apparatus and environment $\mathcal{SAE}$ in eq. (3.37) is a pure entangled state. The state for the subsystem $\mathcal{SA}$ in eq. (3.39) was obtained by tracing out the degrees of freedom of the environment. Therefore, it is an improper mixture. One ought not to confuse separable and entangled systems, one ought not to take that the improper mixture describes the subsystem *simpliciter*. As discussed above, by tracing out of degrees of freedom there is a loss of phase coherences (regardless of whether the improper mixture itself is diagonal, approximately diagonal, or non-diagonal). Then, such a density operator cannot be simply taken to be a description of the subsystem $\mathcal{SA}$. That improper mixture provides information relative to measurements performed on $\mathcal{SA}$ that leave the environment untouched. However, the state is still entangled and to regard it as non-entangled is an extra consideration. If the realist takes the formalism at face value, the quantum entangled nature of the system is still there, even if one traces out degrees of freedom.

Let us discuss the consequences of the interpretation of proper mixtures. Following (d'Espagnat 1976, 44), a proper mixture is a statistical mixture of systems in pure states obtained from considering a mixture of N_1 systems in pure state $|\phi_1\rangle$, N_2 systems in pure state $|\phi_2\rangle$, and so on, with the condition that $\sum_\alpha N_\alpha = N$. A proper mixture is described by the statistical operator

$$\rho_P = \sum_\alpha \frac{N_\alpha}{N} |\phi_\alpha\rangle \langle\phi_\alpha| . \tag{3.59}$$

This operator is mathematically equivalent to the improper mixture obtained by tracing out degrees of freedom of a larger system in eqs. (3.56) and (3.57). Presented with a statistical operator only, one cannot know whether it is a proper or an improper mixture, unless further information is supplied. Essentially, all one could know from the mathematical entity, is whether the state is pure (if the statistical operator is a projection operator) or a mixture (if it is not a projection). However, despite their mathematical identical appearance, the physical interpretation of proper and improper mixtures is significantly different.

Proper mixtures are, in a way, analogous to classical mixtures. In classical physics, statistical mixtures are interpreted by ignorance. That is, the statistical mixture expresses ignorance of the observer over a determined state of affairs. When we consider a classical ensemble of many identical systems, and we perform an experiment, we know that the system is an element of the ensemble. We assume – consistently with classical physics – that the system at hand is in one possible state, but we, as agents with limited knowledge, do not have enough information to know in which possible state the system is in. We also assume that all the properties of the classical system are determined, but we do not know their values. Therefore, the probabilistic character of the statements made here relate to *our ignorance* of the state of the system. This is also the case of proper mixture: we know that the system is in a pure state, but we do not know which one. We have a probability, in that the pure state is $|\phi_\alpha\rangle$ with a probability associated with a N_α/N statistical frequency associated with such pure state.

Contrary to the interpretation of the probabilities in the statistical operator representing a proper mixture, the probabilities involved in the improper case cannot be interpreted by ignorance in the way just described. This is widely known in the philosophical literature, see (Landsman 2007, 516), (Friederich 2014), (Holik et al. 2013), (Fortin and Lombardi 2014), (Ladyman and Ross 2007, 177), amongst others. In turn, the physics literature typically neglects the distinction altogether. The standard textbooks do not teach physicists this crucial distinction proper/improper, thus the literature misinterprets the states obtained from tracing out degrees of freedom, see (Nielsen and Chuang 2010, 106) and my footnote 40. However, the philosopher has a critical role in accounting, not only for the practice of the scientist, but for the conceptual implications of the theory as well. Crucially, the mistake in the standard interpretation of the model of decoherence is the *assumption* that the subsystem is really described by a proper mixture $\rho = |a|^2 \left|\uparrow\right\rangle \left\langle\uparrow\right| \otimes \left|p^z_+\right\rangle \left\langle p^z_+\right| + |b|^2 \left|\downarrow\right\rangle \left\langle\downarrow\right| \otimes \left|p^z_-\right\rangle \left\langle p^z_-\right|$, which the model of decoherence obtains in the limit when $t \to \infty$ and $N \to \infty$ and $\rho^{\mathcal{SA}}(t) = \mathrm{Tr}_{\mathcal{E}}\big(\left|\chi(t)\right\rangle \left\langle\chi(t)\right| \big) \to \rho$ as the coherences become delocalised given that the correlation $|z(t)| = |\left\langle\varepsilon_+(t)|\varepsilon_-(t)\right\rangle| \to 0$, see eqs. (3.39), (3.41) and (3.44). However, because of the quantum entanglement between the apparatus A and environment E, one cannot presuppose that the subsystem SA has an individual state. Paradoxically, this misconception does not present an obstacle to the physicists to make accurate predictions which are successful 'for all practical purposes' (FAPP) only.

In order to avoid having to interpret the improper mixtures and include the FAPP-style success of the standard models of decoherence, the realist requires further arguments. Those arguments could indicate that QM is only approximately true in the sense that there is a more fundamental theory that is closer to the truth. However, this argument would be dependent on more fundamental physics, which is yet to be developed. Furthermore, if the realist argued in this way in order to justify the FAPP interpretation of the quantum formalism, she would be utilising, at a crucial level, arguments that rely on conceptions of intertheory relations. And this is problematic if the methodology of Core Realism that I discussed in Chapter 2 is convincing. According to Core Realism, the realist ought to interpret a theory first, and then see how that theory and its realist content (derived from the interpretation) relates to other theories. This represents a worry for the success of the FAPP-based argument.

Now, having discussed in detail the difference between proper and improper mixtures and their interpretative challenges, let me move on to the second worry about the role of decoherence in the realist interpretation of QM. This has been raised by Kastner (2014). Whilst her criticism is aimed at the role that decoherence plays in MWI, her line of thought just assumes that there is only unitarian evolution in QM. Hence, her criticism can be extended from MWI to a criticism of any non-collapse interpretation. She argues that the model of decoherence that intends to explain the vanishing of the off-diagonal terms in eq. (3.39) through the limit $|z(t)| \to 0$ in eq. (3.41), is circular and invalid. This is because the model of decoherence implicitly assumes that the apparatus has decohered from the beginning. In her words:

> macroscopic classicality only 'emerges' in [the MWI] picture because a classical, non-quantum-correlated environment was illegitimately put in by hand from the beginning. Without that unjustified presupposition, there would

> be no vanishing of the off-diagonal terms and therefore no apparent diagonalization of the system's reduced density matrix that could support even an approximate, 'FAPP' mixed state interpretation.
>
> (Kastner 2014, 57)

The underlying argumentative force is the recognition that improper mixtures are inherently distinct from proper mixtures, and that decoherence obtains improper mixutres.

In order to discuss this argument, consider as Bacciagaluppi (2016, Sect. 2) does, that according to the conception of open systems in the model of decoherence, "everything is in interaction with everything else, everything is generically entangled with everything else". That is, everything is entangled with everything else, and there are no pure separable states, strictly speaking. However, as Kastner (2014) points out the theory of decoherence considers that at the instant of time when interaction apparatus-environment is set on, the state of the entire system (system and apparatus $\mathcal{SA}$, and the environment $\mathcal{E}$) is the pure state $|\chi(0)\rangle$ in eq. (3.28). $|\psi_f\rangle$ is product state of the pre-measurement state of the subsystem $\mathcal{SA}$, $|\psi_f\rangle$, and the state of the environment $\mathcal{E}$, $\bigotimes_k \alpha_k |u_+\rangle_k + \beta_k |u_-\rangle_k$. Hence, $|\chi(0)\rangle$ is a product state of these two.

However, if the hypothesis of decoherence is that quantum systems are open and what Bacciagaluppi says is true, then the components of the system must have already decohered completely, previous to that instant $t = 0$. On the other hand, the model of decoherence does not obtain a pure state as a final state, but improper mixtures. Even if one ignores the distinction proper/improper, a mixed state is not a pure state. Therefore, following Bacciagaluppi and Kastner, decoherence uses a conclusion within premises. In a nutshell: if the state before the interaction with the environment is a product state, where did that pure state come from?

With this I conclude the discussion of decoherence. If I am right and decoherence does play a role in the account of the QM-QM limit and it is not a sound account of it, then the realist has to find another way out. In the next section I will reflect on what I have done so far and how to move forward.

3.4 Alternatives to the Received Account of the QM-CM Relation

The theoretical devices that attempt to address the relation QM-CM have, as I have argued, limitations. The most these methods can achieve is to recover something that resembles classical physics, in a limited number of situations. Suppose that I accepted that the explanation of the appearance of the classicalities, and the measurement problem and the preferred basis problem, are resolved incontestably, through the mathematical devices discussed above and the process of decoherence. Then, what about, as Landsman (2007) says, the quantum cases from which no classicalities appear? What about cases where the coherence is retained as long as possible, such as in quantum computation? There, decoherence effects are precisely what the physicist wants to avoid and really complex experimental techniques, such as ion traps, are used in order to provide a long coherence time that would allow developments in the field of quantum computation, see (Cirac and Zoller 1995). Furthermore, there are cases where there is a coherent superposition which has direct experimental results, such as superconductivity. In relation to decoherence as an answer to how QM reduces to CM, there is experimental and theoretical evidence of effects that are purely quantum and with direct expression in the 'macroscopic' realm, see (Brezger et al. 2002; Kovachy et al. 2015) and (Landsman 2007, 418, and references therein).

The existence of relevant quantum physics from which no classicalities emerges evidences that the central philosophical challenges to the realist account of QM ought not to focus on the explanation of the appearance of classicalities. However, what I called the Received View in Chapter 2 considers that the central challenges to the interpretation of QM are the measurement problem, and also the preferred basis problem. These two problems – as I have discussed in detail in Section 3.3.4 – comprise the broad issue of the QM-CM limit.

The arguments in previous sections intended to show that the traditional account of the QM-CM limit has shortcomings and the realist operating within the Received View does not succeed in providing a smooth account of the appearance of classicalities from the quantum. But even if she did, she would still need a realist account of those cases where no classicalities emerge. Such an account is secondary in the methodology of the Received View. In Chapter 2 I discussed it in more detail, but it seems useful to recall here that, for example, Wallace (2016b) essentially considers the many worlds interpretation to be a theory of the macroworld, or that Bohmian mechanics provides an account of QM based on the premise that quantum systems are particles with positions. These two claims defend an account *of* QM *from* an account of the QM-CM limit.

Indeed, Schlosshauer (2005); Landsman (2007); Bacciagaluppi (2013) consider the QM-CM limit, the measurement problem and the preferred basis problem to be *the* foundational problems in the realist account of QM. By contrast, within Core Realism approach to QM, the question is about the way the world is according to QM, even when mathematical limits from QM do not provide classical results (see Section 3.3.1), even if there

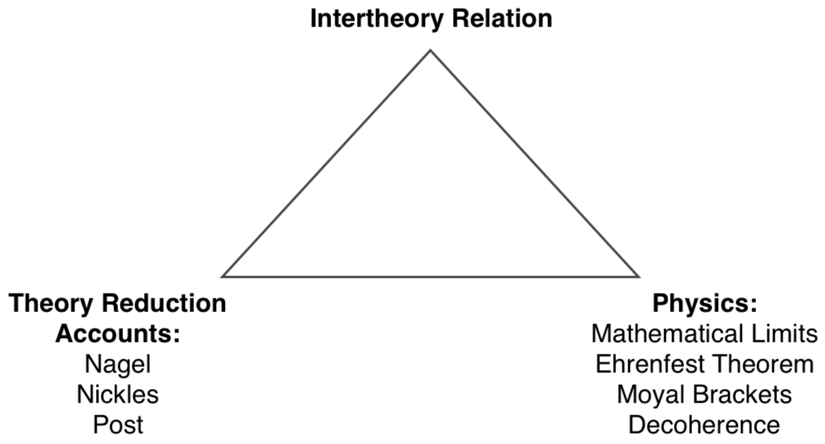

FIGURE 3.3: Triangle of the QM-CM limit. The possible strategies to advance a novel view are initiated by targeting one of the three vertices shown by modifying, improving, criticising them.

is no analog to Newton's equations (see Ehrenfest theorem in Section 3.3.2), even when the Moyal bracket cannot be written as a classical Poisson bracket plus quantum terms (see Section 3.3.3), even before the coherence is delocalised (see Section 3.3.4).

However, the recognition of the overall limitations of the philosophical framework of reduction$_2$ to account for the QM-CM relation, in light of the limitations of the formal theoretical devices developed in physics, is shared by many, see (Bokulich 2008a, Ch. 1). The difference amongst those who agree with such an assessment lies in how to react. What does this indicate and how to fix it? My view is that the debates on the difficulties around the QM-CM limit involve mainly three positive alternatives, arising from modifying one of the three elements in the very debate: firstly, the type of intertheory relation, secondly the philosophical account of reduction and thirdly, the physical account. I illustrate these elements with the triangle in Figure 3.3. To conclude these final remarks, I will mention relevant research focused on each of the three options, although the limits of this dissertation are to engage in critical discussion with one of them only.

One option is to claim that the standard account of the QM-CM limit is insufficient because of the reasons discussed above and because there are relevant physical cases which do not fit at all with such a framework. This view challenges the top vertex of the triangle in Figure 3.3, modifying the type of intertheory relation. More specifically, it considers relevant physical systems in the mesoscopic scale – such as Rydberg atoms in strong uniform external magnetic fields – which cannot be explained by QM alone, let alone fit in the schemes of the traditional account of the QM-CM limit (bottom right vertex of the triangle). Yet, such a system lies in the empirical region between the micro-QM and the macro-CM. Therefore, the alternative here is to articulate a novel conception of intertheory relation. Bokulich (2008a,b, 2012) takes such an approach. She advances a novel intertheory relationship that goes beyond the traditional reduction and pluralism, intending to account for such semiclassical phenomena. I sympathise with this reaction to the situation of the realist account of the QM-CM limit and I will not comment on her work any further for I dedicate Chapters 4 and 5 to critically engaging with her account.

Another alternative is to focus on the bottom left vertex of the triangle in Figure 3.3. That would maintain theory reduction as the appropriate intertheory relation and change the form of such reduction to better capture the relevant physics of the QM-CM relation. The advocate of this view could claim that decoherence – seen as the culmination of the account of the QM-CM limit – is the appropriate device to account for the reduction relation. Yet she will criticise and modify the traditional philosophical views on theory reduction. Relevant and recent research in this direction is being conducted by the likes of Rosaler (2013, 2015, 2016) and I mentioned his view at the end of Section 3.2.2. Rosaler (2016) puts forward a one-size-fits-all account of reduction designed to work equally well for all the different interpretations of QM and is based on decoherence as a template for such reduction.[48]

Now, my distinction between different types of intertheory relations in Section 3.1 is useful to systematically characterise Rosaler's aims. In terms of subject matter, his account of reduction applies to models within a theory. This is meant to build upon the merits of the known approaches – such as Nickles, Post, and also Nagel – which attempt a reduction between theories.[49] His reduction relationship is based on decoherence. It is meant to be an interpretation-neutral approach and this fits with what I called the Received View in Chapter 2.[50] Because Rosaler's approach fits so well within the Received View to the realist interpretation of QM, I do not endorse this alternative philosophical account of the QM-CM limit. More preferable is a view like Bokulich's one, representing a richer and more innovative approach which builds upon novel physics and dares to challenge the type of intertheory relation.

[48]Rosaler considers the different interpretations to be different physical theories "since they differ in the accounts of physical reality (in particular, the laws and ontology) that they take to underwrite the success of the quantum formalism" (Rosaler 2016, 55). Is there a reduction relationship between these different theories? Will the reduction be the same as the one he claims to exist between the various interpretations/theories of QM and CM, or will these two reduction relationships be different? I leave this discussion for future works.

[49]Although as I discussed earlier, Nagel's reduction account is also typically taken to relate to the relationship between disciplines.

[50]My view of Core Realism appears novel given that I note that the forefront of the philosophy of physics which looks at the QM-CM limit is still within the Received View. If the reader agrees with me that the so-conceived Received View is one specific philosophical framework (and thus not the only one), then Rosaler still has to justify further why such an epistemic and ontological intertheory relations are meant to hold, and also the extent to which adopting his approach really answers questions about the interpretation of QM. The realist question that requires urgent answer is about the realist interpretation of the quantum system.

Finally, one could focus on the bottom right vertex of the triangle in Figure 3.3 and challenge the consideration of the physics involved. A reaction to the difficulties of the standard philosophical account of the QM-CM limit based on theory reduction and the physics of decoherence and other devices, could be to modify the physical account of the QM-CM limit itself. Relevant work is conducted by Kastner (2014), which I comment on very briefly. Although she does not directly engage in the debate on theory reduction, we can point out that in the face of controversies around the process of decoherence to account for the appearance of classicalities, Kastner argues that the empirical fact that we observe decoherence is not derivable from a unitary-only dynamics. Hence, she suggests that the relativistic field picture may be promising as a solution to this. She argues that in order to tackle the questions arising within non-relativistic QM, one has to move on to a relativistic approach. Her view considers that the energy-momentum (in the 4-momentum form) basis is more fundamental than position, and thus takes energy/momentum transfers as primary and the spacetime phenomena as secondary and emergent, supervening on the E,p transfer. These are the central elements for her ontology of Possibilist Transactional Interpretation, see Kastner (2012). I insist on non-relativistic QM and hence I do not address her work any further.

3.5 Conclusions

In this chapter I have critically engaged with the standard view of the complex relationship between QM and CM. In the first part I introduced useful analytic distinctions to facilitate a systematic analysis of the traditional views on intertheory relations in the philosophical literature. In the second part, I engaged with the traditional ways that physics expresses how classicalities are seen as a result of the underlying QM. Following on Chapter 2, I argued that the standard account follows the recipe captured by the Received View.

Whilst there are limitations in the philosophical frameworks accounting for the relationship QM-CM, the focus is on the physics. The theoretical devices that were designed with the specific purpose of showing the appearance of CM from QM, only succeed approximately and in a limited number of cases. Accordingly, I have discussed decoherence in detail, arguing that it does not deliver as an account of the QM-CM limit. Furthermore, both the standard philosophical frameworks and the theoretical devices within physics, fail to capture relevant developments that challenge the view that QM is more fundamental than CM and thereby it has to provide an account of the latter. The recent and increasingly relevant developments of modern semiclassical mechanics cannot be accounted for by the outdated accounts of the relationship QM-CM. This motivates revising the very conception of this complex problem. Bokulich's work represents a relevant step towards a novel approach to this philosophical challenge. Consequently, I will dedicate the next two chapters to engaging with her work.

Chapter 4

The Interstructuralist Approach (I): Physical Bones

4.1 Introduction

Bokulich (2008a) develops an interstructuralist account of the relationship QM-CM as a response to the deficient ways in which the intertheoretic quantum-classical relationship had been addressed up to that time in the literature, which I analysed in Chapter 3. In Section 3.4 I mentioned the type of alternative that Bokulich undertakes: her view radically challenges the philosophical underpinning of the intertheory relation. And it directs attention to a novel area of physics where the relationship QM-CM is crucial and that traditional views fail to accommodate. She argues that none of the philosophical views (broadly, reductionism and pluralism) are fit for purpose and, instead, assesses that a new intertheory relation should be conceived. Therefore, Bokulich's view is motivated by the desire to overcome the obstacles that the standard view of intertheory relation, namely, Nickles reduction$_2$ has in light of relevant physics in the region of semiclassical phenomena. One merit of Bokulich's view is that it focuses on concrete examples taken from novel physics that the philosophical literature has not yet sufficiently dealt with. Plus, it is inspired by Paul A. M. Dirac's view on physical theories, bringing interesting elements from the history of physics to current philosophical debates.

In this chapter I focus on two issues. Firstly, I will present Dirac's view for the purposes of engaging with Bokulich's interstructuralism, including her novel reading of Dirac's philosophy. I will engage with this view by contrasting it with the Received View discussed in Chapter 2. Secondly, I will discuss relevant elements of semiclassical mechanics, which is the modern and complex physics that interstructuralism intends to accommodate. This will be essential to understand this novel approach to the relationship between

QM and CM and it will be the basis for my analysis in Chapter 5. This chapter and the next should be considered to be interrelated.

4.2 Open Theories and the Reciprocal Correspondence Principle Methodology

Bokulich's view conceives physical theories, and their interrelation in a particular way. The background conception is inspired by the philosophy of Dirac. For Dirac, there is a structural continuity between theories, meaning, specifically, that there is no sharp break between CM and QM, but rather, QM is seen as an extension in the development of, or a generalisation of, CM.[1] Following Bokulich, the framework that captures Dirac's conception of theories shall be called Open Theories.

Dirac's Open Theories view crucially includes the following elements: Theories are open to future revision, that is, no part of the theory is taken as a permanent achievement. By contrast with a view wherein theories achieve a final form and even small changes entail a wholesale theoretical change, Dirac conceives an openness in terms of theories affording a constant 'reinvention'. Thus, constant revision is welcomed and theories achieve no definite, final form. Indeed, Dirac expresses that even CM is still open to fundamental changes.[2] The revisions and improvements are obtained through refining approximations. This, so Dirac (1962) says, recognises that "science would develop through getting continually more and more accurate approximations, but would never attain complete exactness". Furthermore, there is a significant element of beauty in the theories. Bokulich (2004, 393) argues that beauty in Dirac's sense includes a relevant component of "continuity and structural similarity with classical mechanics". Finally, the openness of theories is embedded in the unity of science. Bokulich's illustration of Dirac's view as 'open' theories can be seen in the following: Dirac conceived that the same 'basic structures' (such as, for instance, equations of motion), appear throughout physics. The open character of the theories is their disposition to constant and gradual development, which tends toward a unified physical theory.

Now, how are the different 'theories' related, given that there is a belief in the unity of the theories of physics? Here Bokulich articulates what is really the key to her own

[1] This reading on Dirac's general view on physical theories is also one of Bokulich's contributions to the philosophical literature, see (Bokulich 2004).

[2] Certainly most physicists and philosophers would agree that QM is not a finalised scientific theory. Perhaps more controversial is to hold that CM is not finalised either. However, CM was not 'over and done with' Newton's work and there has been development ever since. CM is significantly used and keeps being developed. Relevantly for this thesis, consider Chaos Theory, discovered only in the C20th. Another example, from 1951 and many years after scientific community had abandoned the idea of the aether, is when Dirac proposed a relativistic theory in which aether was required. See for discussion (Bokulich 2008a, 59).

view, based on Dirac's notion of intertheory relationships.[3] This is best explained by what Bokulich (2008a, 56) identifies in Dirac as the

Reciprocal Correspondence Principle Methodology: Firstly, development in CM is stimulated by problems in QM. Secondly, the results of that development in CM are fed-back into QM, guiding further development of QM.

Because the theories are 'open' conceptual systems, in the sense discussed above, the scheme can accommodate the interchanging of theoretical features across different theories. For example, dynamical structures, such as classical orbits, can move from CM to QM. Now, a problem in QM can stimulate progress within CM and this be transferred back into developing QM. Indeed, that transfer between the theories is underpinned by the thesis of Structural Continuity, or thesis of transferability, between the theories. This is not trivial, for one could conceive a different scheme with a stronger delimitation to the domain of a theory, or a stronger individuation of theories, whereby the transfer of structures from one theory to the other is more problematic.

Then, Dirac's methodology works in a two-way fashion. One way ($\Rightarrow$), by the use of CM (in the traditional reduction scheme, the predecessor) for the further development of QM (the successor in the traditional view). This is dubbed the General Correspondence Principle.[4] And ($\Leftarrow$), the other way, is when QM plays a guiding role in developing CM, which is called Inverse Correspondence Principle, (Bokulich 2008a, 55, and previous works mentioned too).

These two principles: the General Correspondence Principle and the Inverse Correspondence Principle, are the crucial aspect of Dirac's methodology. In his words:

> if there are new ideas which can be understood on a Classical basis, then one should try to work them out simply keeping to the Classical Theory, and after one has worked them out one can transfer them to the Quantum Theory by using the already established connection between Classical Mechanics and Quantum Mechanics.
>
> Dirac, quoted in (Bokulich 2008a, 57)

[3] See (Bokulich 2008a, Section 3.2).

[4] This aspect is close to the standard views on reduction. Recall Sections 3.2.2 and 3.2.3 in Chapter 3: Nickles' reduction$_2$, involving a strong heuristic component, was characterised by the predecessor setting a guide to the development of the successor, upon the latter establishing a relationship to the former mathematically expressed in various non-competing ways, see Table 3.6. Similarly conceiving the development of the successor in terms of the establishment of a relationship with the predecessor, Post (1971, 228) designed his General Correspondence Principle. A difference between Nickles and Post is that (Post 1971, 225) also emphasises the role of the flaws in the predecessor working as footprints for the development of the successor, whilst Nickles emphasises merely the successes or structural elements in the predecessor. Bokulich (2008a, 55, fn. 14) clarifies that Dirac's General Correspondence Principle should not be seen as similar to Post's.

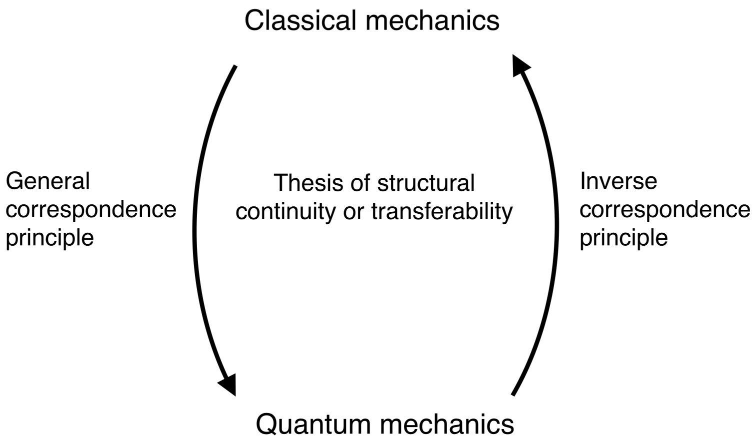

FIGURE 4.1: An illustration of the Reciprocal Correspondence Principle Methodology, applied in the relationship between QM and CM following Dirac's open theories. The arrows represent the direction in the development: on the left-hand side, the General Correspondence Principle captures the development in QM through the use of CM and, on the right-hand side, the Inverse Correspondence Principle allows development in CM through QM (which are then transferred back into QM through the left-hand side arrow). The carrier of this relationships is enabled through the thesis of structural continuity or transferability.

This methodology can be exemplified by the following. The crucial role that celestial mechanics (within CM) played in developing the description of the hydrogen atom (and the whole periodic table), indicates that CM was relevantly used in developing QM. Then, problems in QM, such as the description of the helium atom (not as a hydrogen-like atom, but its exact solution), which QM alone could not provide, stimulated developments in CM. That is, chaos theory and the solutions to the classical three-body problem.[5] That development in CM was subsequently fed back into QM, aiding in the description of, for example, wavefunction scarring or Rydberg atoms in strong magnetic fields. These three examples will be discussed in detail in Sections 4.4, 4.6 and 4.5.

Following the articulation that Bokulich (2008a) presents, I will discuss physical problems and analyse how this methodology captures the practice of physicists working in the 'borderland' between QM and CM. I illustrate this discussion in Figure 4.1. Indeed, the above discussion is an interpretation that Bokulich developed as an underlying framework for her interstructuralism, which is applied specifically to a number of phenomena in the region between CM and QM.

Next, I will introduce the motivations for Bokulich's view, including a systematic analysis of intertheory relations with the analytic tools from Section 3.2 in the previous chapter.

[5]The progress of CM did not even stop there, there are recently discovered phenomena in CM that have relevant consequences for QM, see (Delos et al. 2008). I do not discuss them here but merely point out their existence.

I will also compare interstructuralism with traditional relationships, showing the novelty of Bokulich's account.

4.3 Semiclassical Mechanics: Quantum Chaos and Mesoscopic Phenomena

A natural candidate field of physics that the framework discussed in Section 4.2 can account for, is that of semiclassical mechanics.

Two overlapping major applications of semiclassical mechanics is to tackle problems of quantum chaos and the approach of mesoscopic systems, (Richter 2000, 15). Recall that quantum chaos is the approach to dynamical systems the classical analogs of which are chaotic. Classical chaos can appear in non-integrable systems, where there are less constants of motion than degrees of freedom. For a given energy, the trajectory of a classically chaotic system will cover the entire available 2N-1 dimensional surface in the 2N dimensional phase space, an infinite number of times with an infinite number of momentum directions. Being the classical phase space a continuos space, one can zoom-in or magnify ad-infinitum and always find trajectories. In other words, for any possible phase point, there is an arbitrarily close orbit of the system. Another characteristic of chaotic systems is their sensitivity to the initial conditions: two trajectories starting off with slightly different initial conditions, diverge really quickly from each other. In general, any classical system described by a non-linear differential equation has some chaotic regime. A typical example is a double-rod pendulum, which will cover all phase-space within reach. Classical chaos is a complex discipline and a full characterisation of the types of chaotic behaviour is neither a trivial matter, nor is it necessary for my purposes.

By contrast with chaos in CM, in QM there is no chaos. One way to see this is by comparing the phase spaces of both theories. In the classical case, as discussed above, the trajectories of the system will cover the entire available phase space, whilst in the quantum case the phase-space has a minimal volume of $\hbar^3$, setting a lower limit. Another way to see this is by considering the linear structure of Schrödinger equation, and the superposition principle, which precludes such sensibility to initial conditions, (Connerade 2005, 363). Quantum chaos is a term used to refer to quantum systems the classical analogs of which are chaotic.

The domain of mesoscopic phenomena lies between the micro and macro-scales, the border between quantum and classical domains. Here, purely quantum and purely classical description fail and both theories are combined in order to explain phenomena, (Batterman 2002, 109-110).[6] Typically the system is described by QM, but because of its empirical domain, classical ideas are suitable. If there are complications for the development of a full quantum account, classical ideas can bring simplification. Otherwise, it might just be that the quantum account is not available.

On a first approach one could argue that regardless of the different methods physicists and philosophers make use of for explaining and investigating certain phenomena, it is well-known that quantum particles can hardly be coherently conceived as classical particles following classical dynamics.[7] Yet, Bokulich rightly emphasises significant practices of physicists working in the field of semiclassical mechanics. She claims that the philosophy community should pay more attention to these relevant physical cases because they exhibit a rare example of when quantum phenomena can seemingly be explained *only* by making recourse to classical dynamical structures, such as classical orbit theories. Modern semiclassical mechanics is relatively recent. Its first application to a real problem was done in 1988, see (Delos and Du 1988). However, it has a vast domain of application, particularly phenomena in the mesoscopic scale. I will discuss this in Section 4.3.2.

With the above discussion, I hope to have illustrated three main motivations that Bokulich (2008a, 104) identifies for taking semiclassical mechanics seriously: (i) it is useful in physical cases of interest where a full quantum approach is cumbersome; (ii) in some cases, it can provide 'intuitive' physical insight into the problem, thanks to its use of classical concepts; and (iii) it looks past the fully quantum description of some problems, leading to novel phenomena.

In what remains of this chapter, I will articulate the main ideas in semiclassical mechanics. Critically engaging with Bokulich's philosophical view will be left to Chapter 5. As mentioned above, semiclassical mechanics is a vast field of research and there are many applications in physics on which I could focus in order to critically engage with interstructuralism. For obvious reasons, it will be easier to discuss the examples that Bokulich herself develops (specifically in (Bokulich 2008a)). Thus, I will discuss the Rydberg atom in a strong uniform magnetic field, the helium atom and wavefunction scarring. What these cases have in common is that the physical system is quantum and

[6]These divisions are not philosophically uncontroversial. However, it is commonly understood that 'small' molecules, atoms and 'smaller' particles belong to the micro world described by quantum theory (and subsequent theories like quantum field theory, and so on), whereas 'everyday' objects and their dynamics, are macroscopic phenomena, described by classical mechanics. In between, there is the mesoscopic scale. Arguably, these distinctions are not sharp. Furthermore, in order to consider these scales at all, assumptions about intertheory relations must have been made beforehand. The micro, meso and macro-scales have an empirical aspect of course, but also, they are philosophically laden. I suggest that the very conception of these regions is captured by what I called the Received View in Chapter 2.

[7]This will be discussed further in subsequent sections. Here I am merely introducing the topic.

its classical analog is a chaotic system, thus semiclassical mechanics is the appropriate framework. I will use them for two purposes relevant to this investigation: (1) to assess how well semiclassical mechanics fulfils Bokulich's motivations mentioned above. And (2), to show the Reciprocal Correspondence Principle Methodology in action.

4.3.1 Einstein-Brillouin-Keller Quantisation

The Einstein-Brillouin-Keller (EBK) quantisation method obtains an approximation of the energy levels for a problem which is integrable, that is, that has as many constants of motion as degrees of freedom. EBK is a generalisation of the approximation known as the Wentzel-Kramers-Brillouin (WKB) method, which – so (Batterman 1995, 198) notes – is the ancestor of modern semiclassical mechanics. The classical aspect arises from a formal similarity between the classical Hamilton-Jacobi equations and the Schrödinger equation, and the method works with classical orbits in phase space. As I mentioned earlier, the semiclassical approach goes back to early stages in the development of QM. Indeed, the WKB method was actually developed upon Bohr-Sommerfeld quantisation conditions, one of the first formal aspects of QM. The shortcoming of the WKB method is that it only applies to one-dimensional problems, and although it can be extended to multi-dimensional problems that are separable, the procedure is not unique, (Tabor 1989, 235). The EBK method fixes this since it works for all integrable problems. In turn, the shortcoming of the EBK method is that it cannot be used for non-integrable problems. That is where the trace formula enters.

The underlying ideas to WKB and EBK methods are the same, so let us begin with WKB. The WKB method of approximation obtains the wavefunction for a given hamiltonian and a certain energy for the system. The following is not a rigorous explanation and I only mention results that are well-known in the physics literature. My aim is only to explain enough so I can advance with the analysis of interstructuralism.

The WKB method provides a procedure to calculate approximate energy levels and eigenfunctions for separable and integrable problems in one dimension, in the limit where the action of the problem is very large relative to $\hbar$, that is,

$$S \gg \hbar, \tag{4.1}$$

which is to say in the limit $\hbar \to 0$. There are several sources to develop this approximation and I recall the steps of (Galindo and Pascual 1991, Sec. 9.1),[8] the function

$$\psi(x) = \Big(\frac{dS(x;h)}{dx}\Big)^{-1/2} \exp\Big(i/\hbar S(x;h)\Big),$$

[8] See also Messiah (1961).

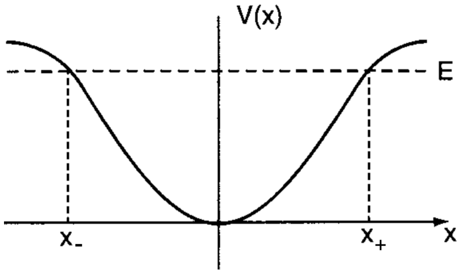

FIGURE 4.2: Potential well for the application of the WKB method. The region I is to the left to turning point x_-, region II is between the two turning points x_- and x_+ and region III is off the right to x_+.

which depends implicitly on $\hbar$ is used as an ansatz into the time independent Schrödinger equation in one dimension:

$$-\frac{\hbar^2}{2m}\psi''(x) + V(x)\psi(x) = E\psi(x), \tag{4.2}$$

obtaining an equation for $S(x;h)$:

$$(S'(x;h))^2 - \hbar^2(S'(x;h))^{1/2}\frac{d^2}{dx^2}(S'(x;h))^{-1/2} = 2m(E - V(x)). \tag{4.3}$$

The proposed solution is

$$S(x;h) = \sum_{n=0}^{\infty} \hbar^{2n} S_n(x). \tag{4.4}$$

By plugging $S(x;h)$ from eq. (4.4) into eq. (4.3) recurrence equations for each S_k can be obtained. In the region $\hbar \to 0$ the first term provides the basic WKB solution:

$$\psi_\pm = \frac{1}{\sqrt{p(x)}}\exp\left(\pm \frac{i}{\hbar}\int^x dx'p(x')\right), \tag{4.5}$$

where $p(x) = \sqrt{2m(E - V(x))}$. This approximation to the wavefunction is acceptable so long as the action can be approximated $S(x;h) \approx S_0(x) = \int^x dx'p(x')$. This entails that the wavelength changes little compared to changes in x, that is, the wavelength $\lambda = h/p(x)$ satisfies

$$\left|\frac{d\lambda}{dx}\right| \ll 1. \tag{4.6}$$

This condition obviously breaks down near the classical turning points, where $E = V(x)$, but there are ways to fix that.

In order to continue, let us suppose a specific potential. A relevant example is the potential well with two turning points x_- and x_+, with $x_- < x_+$ and a classically allowed energy E for the system, as shown in Figure 4.2. At the turning points $E = V(x_\pm)$ and $p(x_\pm) = 0$, which means the wavefunction in eq. (4.5) diverges, the condition (4.6) is not satisfied. The connection formulas are used to match the wavefunction from each side of the turning points and secure the normalisation of the wavefunction, thereby fixing the problem. Then, there are some conditions that the approximation to the wavefunction must fulfil, such as, that the wavefunction is single-valued and normalised. This means the approximation is made locally, dividing the wavefunction by regions relative to the turning points and connecting them. There are a number of non-trivial steps, the details of which are not necessary for our purposes and can be found in (Galindo and Pascual 1991, Ch. 9) and many other sources. Here I just recall the solution.

In region I, far from the turning point x_-, the spatial part of the bound wavefunction is

$$\psi_I(x) = \frac{A}{\sqrt{2m(V(x) - E}} \exp\left(-\frac{1}{\hbar}\int_x^{x_-} dx' \sqrt{2m(V(x') - E)} \right)$$

In region II, $x_- < x < x_+$, we have

$$\begin{aligned}\psi_{II}(x) &= \frac{2A}{\sqrt{2m(E - V(x)}} \cos\left(\frac{1}{\hbar}\int_{x_-}^{x} dx' \sqrt{2m(E - V(x'))} - \frac{\pi}{4} \right) \\ &= \frac{2A}{\sqrt{2m(E - V(x)}} \cos\left(\frac{1}{\hbar}\int_{x}^{x^+} dx' \sqrt{2m(E - V(x'))} - \frac{\pi}{4} + \eta \right),\end{aligned}$$

where

$$\eta = \frac{\pi}{2} - \frac{1}{\hbar}\int_{x_-}^{x_+} dx' \sqrt{2m(E - V(x'))}.$$

In order that a decreasing exponential is obtained in region III, far from x_+, the connecting formulae in (Galindo and Pascual 1991, 95) requires $\sin(\eta) = 0$, hence $\eta = \kappa\pi$ with κ an integer, then

$$\int_{x_-}^{x_+} dx \sqrt{2m(E - V(x))} = (\kappa + \frac{1}{2})\pi\hbar, \text{ with } \kappa \text{ integer } \geq 0. \tag{4.7}$$

Eq. (4.7) is the approximation to the energy levels E_κ^{WKB} for bound states in a region with two turning points. The number κ is the number of nodes in the wavefunction. This approximation is applied to a number of problems, as shown in the literature. The classical aspect also appears by considering the path from $x_- \to x_+$ and back, through a closed orbit. The integral then obtains

$$\oint dx p(x) = 2\pi\hbar(\kappa + \frac{1}{2}), \tag{4.8}$$

where the left hand side is the classical action for a one-dimensional bounded motion through a closed path. And the quantisation condition is also the old-quantum theory Bohr-Sommerfeld condition, see discussion in (Tabor 1989, 234).

There are two limitations to the WKB method: that it applies to integrable problems only and that it applies only to one-dimensional hamiltonians. With regards to the latter, one would expect to be able to apply the method for a more general problem by separating variables. However, there can be non-equivalent ways of separating a hamiltonian. For example, the 3D-isotropic harmonic oscillator can be separated in cartesian and polar coordinates, which differ. Here, the WKB method is not suitable. This motivated the discovery of the EBK method, due to Einstein, Brillouin and Keller, which solved the second limitation by using the canonical action-angle variables.

The idea in EBK is to use the action variables and angular variables as coordinates for the problem: $\{I, \phi\}$. In integrable classical systems the action variables can be calculated

$$I_k = \frac{1}{2\pi} \oint_{C_k} \sum_{i=1}^{N} p_i dq_i, \tag{4.9}$$

where C_k is closed path. For an integral problem with N degrees of freedom there are N different closed orbits in the phase space C_k, $k \in \{1, 2, ..., N\}$, which obtain N constants of motion I_k, $k \in \{1, 2, ..., N\}$, and the system in the 2N-dimensional phase-space is confined within a N-dimensional manifold. The Poincaré-Hopf theorem from topology says that this manifold has the shape of a N-dimensional torus. In a 2-dimensional motion, the topology is like a doughnut, see discussion (Tabor 1989, Sec. 2.5.b and p. 235).

For an N-dimensional torus, there are N topologically distinct closed orbits C_k. If, in traversing C_k one passes through a turning point, or, more generally, a caustic,[9] then there is a phase loss of $\pi/2$ (this observation is also considered in the WKB method). The index α_k counts the number of caustics traversed by C_k, known as the Maslov index. Therefore, the generalisation of WKB in eq. (4.8) to cases that are integrable and not necessarily separable is the EBK quantisation rule:

$$I_k = \oint_{C_k} \sum_{i=1}^{N} p_i dq_i = 2\pi\hbar\Big(\kappa + \frac{\pi\alpha_k}{4}\Big). \tag{4.10}$$

This method gets at better approximation with larger quantum numbers and it does not depend on the detailed knowledge of the trajectory in the phase space. Although it generalises the WKB approximation including non-separable cases, its application is

[9]The caustics are curves that the trajectories graze, are tangential to. That is, they are like envelopes of many trajectories at which the trajectories back over each other, marking the boundary between a classically allowed and classically forbidden region. It can be seen as a generalisation of the turning point in one dimension. The notion of caustic is complex and it is precisely definable in the mathematics of classical orbits. Engaging further with these issues does not concern me here.

limited to integrable systems (as the two cases to be considered below show). This was the next step in the development of semiclassical mechanics.

4.3.2 Trace Formula

That the previous methods to approximate QM with elements from CM, such as WBK and EKB are deficient to approach complex problems was recognised in the early stages of the development of QM. It is well-known that Einstein pointed out this problem in 1917. Gutzwiller undertook this open problem in the 1970s. Indeed, his work represents the foundations of modern semiclassical mechanics and of quantum chaology.[10] And the centre of Gutzwiller's work is his trace formula, which provides an approximation to the quantum density of states or density of levels ($\rho(E)$), of a quantum system whose classical analog is chaotic. There are various methods to obtain an expression for $\rho(E)$ that can be applied both to integrable and chaotic cases.[11] I will focus on Gutzwiller's derivation because it was designed particularly for chaotic problems.[12] By contrast with the WKB method, which starts from the wavefunction, Gutzwiller's trace formula starts from the propagator $K(q'', q', t)$. This is inspired by Feynman's path integral formalism. As it is well known, Feynman's path formalism is widely used in the context of quantum field theories and high energy physics, yet, it was created for non-relativistic QM. Roughly speaking, this formalism is a formulation of QM that starts from the lagrangian rather than the hamiltonian, and it takes the contributions from all the possible paths that a particle can take to go from q' to q'' in the time interval from t' to t''. This approach focuses on the motion of the particle as a function of time, instead of the position of the particle at a specific time. Now, to consider the classical path as the only contribution to the probability amplitude, is the simplest approximation.

In technical terms, the quantum propagator $K(q'', q', t)$ is the solution to the Schrödinger equation written in the following form:

$$\Big(-i\hbar\frac{\partial}{\partial t} - \hat{H}(p'', q'')\Big)K(q'', q', t) = -i\hbar\delta(q'', q'). \tag{4.11}$$

[10]The term quantum chaology seems to have been coined by Berry (1989, 335):" 'Chaology' revives a word which two centuries ago was a technical term describing the branch of theology devoted to what existed before The Creation. I suggest that nowadays we should use it unadorned to mean the study of unpredictable behaviour in deterministic systems, and in the combination 'quantum chaology' to denote the [study of semiclassical, but nonclassical, phenomena characteristic of systems whose classical counterparts exhibit chaos]".

[11]For non-integrable systems, the EBK method and its derivative formulation of the trace formula do not apply. For the case of integrable systems, it is a non-trivial question whether both approaches (EBK and trace formula) lead to similar results, see (Brack and Bhaduri 1997, Ch. 6).

[12]See (Brack and Bhaduri 1997, 207), Main (1999) and (Richter 2000, 28) for discussion of the discussion of applications of the various trace formulas.

The interpretation is that the propagator K takes the particle from q', t' to q'', t'':

$$\psi(q'', t'') = \int d^3q' K(q'', q', t'' - t')\psi(q', t'). \tag{4.12}$$

Now, the Green function is related to the propagator via a change of variables to E from t, via a Fourier-Laplace transform with respect to time t. That is,

$$G(q'', q', E) = \frac{1}{i\hbar} \int_0^{\infty} dt K(q'', q', t) \exp{(iE\hbar/t)}. \tag{4.13}$$

The Green function $G(q'', q', E)$ can be interpreted as the wave produced at position q' by a source of outgoing waves of energy E at position q''. Similar to eq. (4.11), $G(q'', q', E)$ is the solution to the inhomogeneous time-independent Schrödinger equation written in the form

$$\Big(E - \hat{H}(p'', q'')\Big) G(q'', q', E) = \delta(q'', q'). \tag{4.14}$$

Gutzwiller derived his trace formula by working with G and K. He replaced the exact expression of the propagator with a semiclassical approximation, that is, by using classical trajectories. As it is explained in (Gutzwiller 1990, 186), the approximation includes all the possible paths that go from q' to q'' in the time t. Then, that approximation is used into eq. (4.13) in order to obtain the classical approximation to the Green function. From the Green function the density of states for a fixed energy can be calculated via the formula

$$\rho(E) = -\frac{1}{\pi} \int \mathrm{Im} G(q, q, E) dq, \tag{4.15}$$

which is explained in (Brack and Bhaduri 1997, 116).

The density of states $\rho(E)$ obtained from eq. (4.15) contains the information of the spectrum and it is unique for a given hamiltonian. Relevantly, $\rho(E)$ can be separated into two parts: an average slow varying term, and a term that includes the contributions from all the periodic orbits of the corresponding classical system that oscillate as a function of the energy. This formula is the signature of the mesoscopic regime, (Richter 2000, 24), (Berry and Mount 1972, 388):

$$\rho(E) = \rho_0(E) + \rho_{\text{osc}}(E). \tag{4.16}$$

A full description of the smooth term $\rho_0(E)$ can be found in (Brack and Bhaduri 1997, Ch. 4). It is basically obtained by calculating the volume of the accessible phase space, which comes from the contribution to the trace of G in eq. (4.15) in the limit $q'' \to q'$.

The oscillatory part is the relevant part. It takes in the contribution from the trace in eq. (4.15) from paths starting and ending at the same position $q'' = q'$ obtained as mentioned above. The stationary phase method is used to filter out all the trajectories that are not periodic, selecting the orbits with the same momentum p'' at arrival in q'' with the momentum at departure p' in q', $(q'' = q', p'' = p')$. This is why Gutzwiller's approach is referred to in the literature as periodic orbit theory, (Main 1999, 237). Various authors use different expressions of the trace formula and I am broadly following (Brack and Bhaduri 1997). The strict deduction of the trace formula involves complex mathematical considerations that are not necessary for my purposes. In turn, what is relevant is to emphasise the origin of the coefficients that obtain the oscillating part of the quantum density of states, $\rho_{\mathrm{osc}}(E)$. As Brack and Bhaduri (1997, 118) explain, the oscillatory part is crucially determined by classical orbits. The sum can be expressed as

$$\rho_{\mathrm{osc}}(E) = \sum_{\Gamma \in \{\mathrm{PPO}\}} \sum_{k=1}^{\infty} A_{\Gamma k}(E) \cos\left[\frac{k}{\hbar} S_\Gamma(E) - \sigma_{\Gamma k}\frac{\pi}{2}\right], \tag{4.17}$$

where Γ counts the different primitive periodic orbits (PPO) and k counts the repetition around each primitive periodic orbit. Now, the $A_{\Gamma k}(E)$ is a function that depends on the energy, the period and stability of the orbit (explicit expressions of this complex entity are discussed in (Gutzwiller 1990)), then $S_\Gamma(E) = \int dq_\Gamma p_\Gamma(q)$ is the classical action along the trajectory Γ with energy E and $\sigma_{\Gamma k}$ is the Maslov index which can be derived from a count of caustics along the periodic orbit plus a contribution from the stability of $A_{\Gamma k}(E)$, see (Creagh et al. 1990) for technical discussion on Maslov indexes in this context.[13]

It is worth emphasising that the eq. (4.17) is calculated through adopting the approximation that $\hbar$ is negligible if compared with the action S, $S \gg \hbar$. The approximation is best put by Gutzwiller:

> the exact formal expressions on the right [hand side of eq. (4.17)] depend [on classical physics]; but the left side [of eq. (4.17)] is independent of this classical predicament. *The main idea is to use the knowledge of the classical behavior to compute the right-hand side, and then switch sides [to the density of states] so as to draw conclusions about the quantum mechanics of the system.*
>
> Gutzwiller (1990, 282, my italics)

[13] Each of these elements involve complex concepts and I do not intend to discuss each of them in detail. There is sufficient discussion in the references I made.

Evidently, the General Correspondence Principle discussed in Section 4.2 and illustrated in the left hand side of Figure 4.1 captures exactly what is going on here.

Now, note that the right hand side of eq. (4.17) is a Fourier decomposition of the density of states $\rho_{\text{osc}}(E)$, so that the more precision one desires, the more terms have to be added, with increasingly greater action. For chaotic systems the number of periodic orbits increases exponentially with the energy, and this is precisely the most serious problem of Gutzwiller's trace formula. For relevant cases, the sum does not converge and a cut-off to the series has to be set. There are several techniques to solve this, see references in Main (1999). However, if the account of the problem does not demand an exact expression of the level density $\rho(E)$, then the full series is not required. In which case, a 'coarse-grained' expression for $\rho(E)$ will suffice. This has been widely used and known since the early development of the periodic orbit theory, see (Brack and Bhaduri 1997, 223): the idea in the coarse-grained level density is to average finer details of $\rho_{\text{osc}}(E)$ to keep only the leading terms, up to a maximum period. Relevantly, this is what occurs in the application of this theory to the problems that I will discuss below. This will become particularly clear in the example of the Rydberg atom in a magnetic field.

To summarise, Gutzwiller's trace formula provides the semiclassical approximation of the quantum density of states for the spectrum of energy of the quantum system. This is expressed as a superposition of harmonic oscillations as a function of the energy. The frequencies of the oscillatory terms are determined by the periods of the classical orbits associated with the action S, which are found by Fourier-transforming from energy to time scales. The time scale is useful because it allows to observe that the peaks of the actual quantum spectrum coincide with the periods of the classical orbits, and their height corresponds with their relevance in the sum. In turn, the amplitudes and phases of the oscillations depend on properties of the classical orbits, such as their stability and Maslov indices. This is where the role of the classical orbits appear.

***

So far I have discussed the relevant formalism of semiclassical physics. In Section 4.3.1 I mentioned the EBK method and the trace formula in Section 4.3.2. Now I can engage with discussing three significant physical problems that are a difficult to accommodate by the advocate of theory reduction for the pair QM-CM (within the Received View, along the lines of Nickles reduction$_2$ as seen in Chapter 3). By contrast, these problems will be the basis for the justification of the novel intertheory relationship articulated by Bokulich, briefly introduced in Section 4.1.

The three problems will become philosophically central in Chapter 5, where I will engage with Bokulich's form of scientific explanation and how the realist ought to interpret it. It is useful to anticipate the relevance that each of the problems exemplify one of the three motivations that Bokulich (2008a, 104) presents for focusing on semiclassical mechanics:

1. Is it a case where CM aids in the description of a quantum phenomenon that was known but could not be explained within QM alone?
2. Or is it a case whereby a quantum problem stimulated development of CM?
3. Or, finally, is it a case where development in CM can even help into finding novel quantum phenomena that was not known?

I will continue by discussing, in Section 4.4, the case of Rydberg atoms in strong magnetic fields. Although this case has all the elements mentioned in the questions above, I will argue that the Rydberg atom case is predominantly within question (1). Question (2) appears in relation to the helium atom, which I will discuss in Section 4.5. Finally, a case of novelty, and thus more aligned with question (3), is the phenomenon of wavefunction scarring. I will discuss this in Section 4.6.

In these three examples it seems that fictional dynamical structures, i.e. classical periodic orbits, are more than just convenient devices that ease calculations. They seem to be correctly capturing, in their fictional representations, real patterns of structural dependencies in the world and thus it seems that although they involve fictions, they are giving physicists physical insight into the way the world is. These cases can be mapped onto the interstructuralist form of intertheory relation illustrated in Figure 4.1, discussed in Section 4.2.

4.4 The General Correspondence Principle: Rydberg Atoms in Strong Magnetic Fields

I will articulate this case in more detail than the other two cases, in Sections 4.6 and 4.5. Firstly, because according to relevant literature of modern semiclassical physics, e.g. (Gutzwiller 1990, 283), (Kleppner and Delos 2001), (Connerade 2005, 364), among others, the account of the spectrum of the hydrogen atom in a strong magnetic field near the ionisation threshold is the most important accomplishment of this field. Secondly, this problem is relatively simple and there is extensive experimental research in the field, e.g. references in (Brack and Bhaduri 1997, 55). Finally, in my view, this problem has a well balanced mix of the essential elements required to engage with the interstructuralist approach to the relationship QM-CM and the debate over the consequences for a realist take on such a view.

Although both Rydberg atoms in the chaotic regime and wavefunction scarring can be seen as examples of the General Correspondence Principle, the former phenomenon was already known even before the development of quantum chaos, whilst the latter was articulated as an unexpected consequence of the field, as I will discuss in Section 4.6.

Rydberg atoms are such that its farthest electron is in a state with a high principal quantum number n,[14] which causes unusual atomic behaviour. Rydberg atoms play relevant roles in a variety of topics within physics namely, astrophysics, plasma physics, quantum optics and quantum information, see (Dewangan 2012). These applications lie beyond my focus on the QM-CM relationship.

Essentially, the problem is to obtain the absorption spectrum of the Rydberg atom when it is embedded in an external magnetic field. This can be approached by studying the case of a hydrogen atom in a magnetic field. In the classical version of the hydrogen atom, the motion of the electron under the effect of an electric field of the proton becomes chaotic when a magnetic field of strength is added.

Recall that, in Bohr's model of the atom, the radius r of the circular orbit of the electron of charge $-e$ and mass m, around a positively and infinitely heavy charge Ze, is given by

$$r = \frac{n^2\hbar^2}{Ze^2mk}, \tag{4.18}$$

where n is the principal number and $k = 1/4\pi\epsilon_0$, being ϵ_0 the permittivity of free space. For $n = 1$ we obtain Bohr radius $a_0 \approx 5.3\times10^{-11}$ m. Formula (4.18) shows the quadratic dependence of the radius r with the energy level n, $r \propto n^2$, which means that states of high n will have very large orbits.

Now, the energy W of the electron in a state with number n is given by

$$W = -\frac{k^2Ze^4m}{2n^2\hbar^2} = -\frac{Ry}{n^2}, \tag{4.19}$$

where $Ry = \frac{k^2Ze^4m}{2\hbar^2}$ is the Rydberg constant. It can be noticed in eq. (4.19), that the energy that keeps the electron bound to the nucleus (as per the negative sign) and tends to zero as the principal number n increases. The transition energy between two states is given by Rydberg formula:

$$W_2 - W_1 = Ry\Big(\frac{1}{n_1^2} - \frac{1}{n_2^2}\Big). \tag{4.20}$$

[14] Generally if $n \geq 10$ the electron is considered a Rydberg one, with no upper limit. Indeed, the largest value reported is $n \approx 1009$, see (Dewangan 2012).

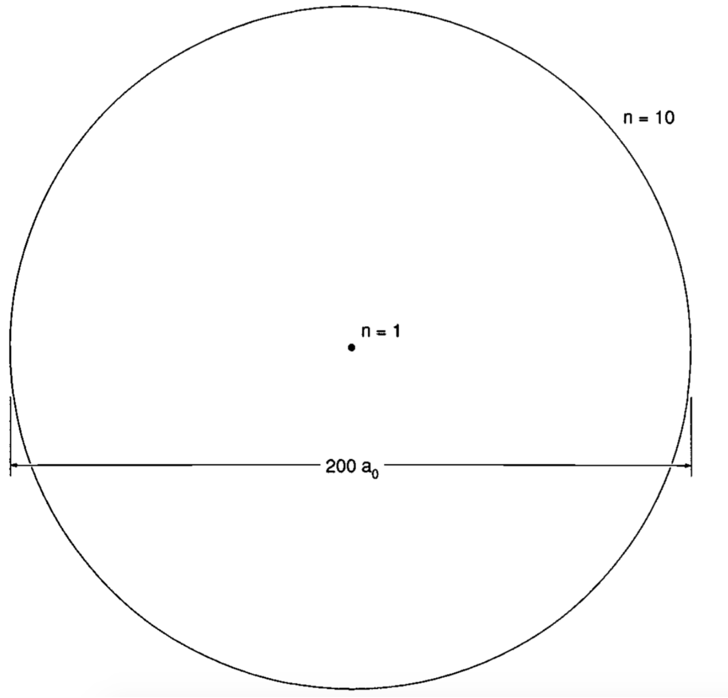

FIGURE 4.3: Illustration of the difference in size of the atom for $n = 1$ (the black spot in the middle) and the size when $n = 10$ the diameter of which is $200a_0 \approx 1 \times 10^{-8}$ m. This image is taken from (Gallagher 2005, 5).

In order to illustrate these considerations I compare the size and energy of a hydrogen atom in its ground state $n = 1$ and a state of $n = 10$: Another useful visualisation of the

n	radius	cross section	energy
1	a_0	a_0^2	$1Ry = 13.6$ eV
10	$100a_0$	$10^4 a_0^2$	$0.01Ry$

TABLE 4.1: Comparison electron in ground state and $n = 10$. Recall $a_0 \approx 5.3 \times 10^{-11}$ m.

atom's size is in Figure 4.3 where the size of the atom in two different states $n = 1$ and $n = 10$ can be appreciated. Consider now $n = 100$, which can be done experimentally and the order of magnitude of the radius is 1 μ m. This is the mesoscopic scale!

Rydberg atoms have many interesting applications and are relevant both experimentally and theoretically, see (Gallagher 2005, 7-9) for a survey of these applications. In particular, a hydrogen-like atom in a magnetic field is a simple case of a non-separable system and this is a paradigmatic system for the study of the quantum chaos, since its classical analog is chaotic. In addition, the case turns non-integrable if the external magnetic field is strong and the study of this problem exhibits general features for all non-integrable quantum systems, see (Wunner et al. 1986, 3261).

As mentioned before, by contrast with the phenomenon of wavefunction scarring, the behaviour and phenomena related to Rydberg atoms has been known since the early stages of the development of QM.[15] Diamagnetic behaviour and quasi-Landau resonances were observed in sodium and potassium Rydberg states for the first time by Jenkins and Segré in 1939. Then, Main et al. (1986) found novel quasi-Landau resonances in highly excited hydrogen atoms in a strong magnetic field, using pulsed, tunable lasers.

Let us appreciate the complexities of this problem in detail. The dynamic of the electron in a hydrogen atom (considering the proton at rest and having infinite mass), in presence of a magnetic field is given by the hamiltonian

$$H = \frac{\mathbf{p}^2}{2m} + V(r) - \frac{e}{2mc}(\mathbf{p}\cdot\mathbf{A} + \mathbf{A}\cdot\mathbf{p}) + \frac{e^2\mathbf{A}^2}{2mc^2} + \frac{1}{2me^2c^2r}\frac{dV}{dr}\mathbf{L}\cdot\mathbf{S} - \frac{e}{mc}\mathbf{B}\cdot\mathbf{S}, \quad (4.21)$$

where the first term is the kinetic energy, the second term is the Coulomb potential interaction $-e/r$ and last two terms are the spin-orbit and the coupling spin-magnetic field. In our case the magnetic field is uniform, $\mathbf{B} = B\hat{z}$, and the vector potential from which it derives, in the symmetric gauge, is $\mathbf{A} = 1/2(\mathbf{r}\times\mathbf{B})$. With eq. (4.21) is

$$H = \frac{p^2}{2m} - \frac{e}{r} - \frac{e}{2mc}BL_z + \frac{e^2}{2mc^2}B^2(x^2+y^2) + \frac{1}{2me^2c^2r}\frac{dV}{dr}\mathbf{L}\cdot\mathbf{S} - \frac{e}{mc}BS_z. \quad (4.22)$$

Here, the diamagnetic term $\frac{e^2}{2mc^2}B^2(x^2+y^2)$ breaks the spherical symmetry, meaning that L^2 is not a constant of motion. Plus it is a non-linear coupling, making the problem non-integrable.

As (Gallagher 2005, 143) notes, the spin can be ignored completely and both the spin-orbit coupling term and BS_z are negligible. Now, using the Larmor frequency $\omega_L = \omega_c/2 = eB/2mc$ (where ω_c is the cyclotron frequency) and rearranging the terms in eq. (4.22), we have

$$H = \frac{p^2}{2m} + \frac{m}{2}\omega_L^2(x^2+y^2) - \omega_L L_z - \frac{e}{r}. \quad (4.23)$$

This is known as Landau's hamiltonian, see (Connerade 2005, 384) and (Gutzwiller 1990, 324). Here we have the terms corresponding to the kinetic energy, diamagnetic quadratic term, the paramagnetic Zeeman term and the Coulomb potential, respectively. The two constants of motion are the energy and L_z.

Now, we can make a qualitative analysis by comparing the terms, see (Gallagher 2005, 143). Let us compare the paramagnetic term, $\omega_L L_z \propto BL_z$ with the diamagnetic term $\frac{m}{2}\omega_L^2(x^2+y^2) \propto B^2(x^2+y^2) \propto B^2r^2 \propto B^2n^4$. The ratio of the diamagnetic term with the term $\omega_L L_z$ scales as Bn^4. Thus, in the low-field case $Bn^4 \ll 1$, where the external field is of a few Tesla and the state is in the ground state or in a low-lying excited state,

[15]See (Gallagher 2005, Ch. 1) for a historical account of this development.

the quadratic term in B can be ignored and then the hamiltonian can be diagonalised in a limited basis and one can approach by perturbative methods, see the approximation of eigenstates in (van der Veldt et al. 1992).

The converse case is more interesting. Here, because $n^4B \gg 1$ the quadratic term cannot be ignored and the classical analog is chaotic. The following qualitative analysis can be made. Recall that the induced current by a magnetic field is proportional to the flux of the external B field, that is, proportional to the area which is perpendicular to the B field direction (in our case, $B\hat{z}$ so the perpendicular area is in the plane (x, y)). Now, we discussed above that, for a fixed B, the diamagnetic interaction scales with n^4 and we can compare this to the Coulomb interaction, the last term Landau's hamiltonian in eq. (4.23), which scales as n^{-2}. Therefore, for large n, the electron is dominated, in the plane (x, y) by the diamagnetic force which is like a two-dimensional harmonic oscillator, whilst the Coulomb potential dominates in $\hat{z}$ imposing a plane wave term $\exp(ikz)$, along $\hat{z}$ the direction of the magnetic field. Typical values entail a slow motion in $\hat{z}$ and a fast circular motion in the (x, y) plane. The energy is expected to depend on these two terms (although the still present paramagnetic term degenerates the energy states):

$$E = (n + 1/2)\hbar\omega_c + \frac{\hbar^2 k^2}{2m}. \tag{4.24}$$

For a fixed k what we have is the spectrum of a harmonic oscillator and the spacing of the levels is $1/2\hbar\omega_c$, these are called Landau levels, see discussion in (Gutzwiller 1990, 327).

The above analysis of the hamiltonian (4.23) in the cases where $n^4B \gg 1$ and where $n^4B \ll 1$ helps us to understand the seminal measurements obtained by W. R. S. Garton and F. S. Tomkins in 1969, see (Gallagher 2005, 127), which I show in Figure 4.4. Their experiment measured photo absorption using σ^- circularly polarised light, which propagates along the magnetic field direction, on Barium atoms. This system allows to exhibit any new structure for the Rydberg states, since the the normal Zeeman spectrum does not appear. I have picked up the spectrum and its discussion from (Connerade 2005, 383). Another strong explanation of the regions, including historical account of its development, can be found in (Gutzwiller 1990, Sec. 18.3).

Let us look firstly at the region (a) in Figure 4.4. The magnetic field can be seen as acting on the electron whilst the potential due to the nucleus is a perturbation to that dynamic. As B increases, going up vertically, the behaviour indicates that the electron is not entirely free from the atom, with the orbits restricted to plane (x, y). Equally spaced structures are seen, typical of a two-dimensional harmonic oscillator. The spacing of the peaks measured by Garton and Tomkins is $1/2\hbar\omega_c$, different to what we would expect from eq. (4.24). However, this was clarified later using the EBK method and it does not

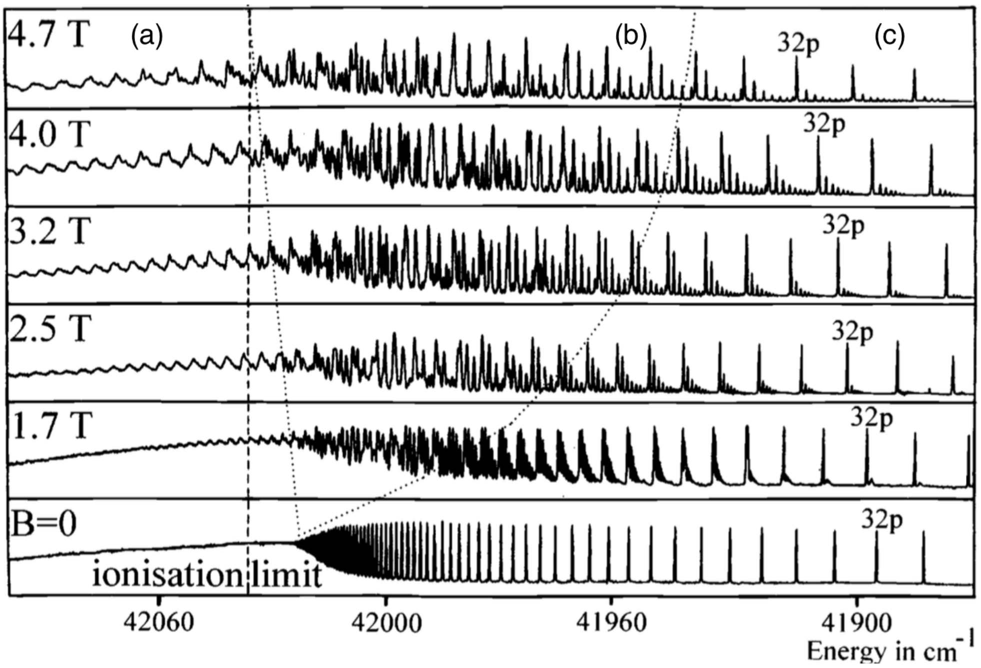

FIGURE 4.4: Spectrum of absorption of Barium for different values of the intensity of a uniform magnetic field B in z, in Tesla units, starting with $B = 0$ at the bottom plot and increasing upwards. On the x-axis the units is energy – the smaller the energy the stronger the bond of the electron to the nucleus. The units on y-axis are arbitrary units to measure absorption. Three zones (a), (b) and (c) are separated by the dotted line: (a) quasi-Landau resonances corresponding to harmonic oscillator in two dimensions, (b) chaotic $n-$mixing region, (c) $\ell-$mixing region. Image taken from (Connerade 2005, 383).

really concern us here, see (Gutzwiller 1990, 327) and (Gallagher 2005, 149). Later on, Garton and Tomkins' measurements of the spacing were confirmed for other atoms too, see references in (Brack and Bhaduri 1997, 32). The peaks in this region are absorption peaks, found even above the ionisation energy threshold only when the strong magnetic field is present.

Looking at the region (c) in Figure 4.4. This region is of low energy and we analyse the spectrum as the B field increases in intensity. As B field becomes stronger, novel lines appear. Their appearance can be taken to arise from the magnetic field perturbing the central symmetry of the atom – and consequently perturbing the conservation of angular momentum – into a spheroidal shape. Hence, within the low energy region, the Rydberg electron is influenced predominantly by the Coulomb field, whereas the magnetic field acts as a perturbation, giving rise to the ℓ-mixed Rydberg states. The name 'ℓ-mixing' effect is given because the effect of the magnetic perturbation couples states with different values of the total angular momentum $\ell\hbar$ (although mixing only ℓ in the same parity). Yet, the levels mixed belong to the same principal number n in proportions that reflect the degree of mixing. In the case when $B = 0$, only one ℓ would be obtained per absorption peak.

The $\ell-$mixing can be obtained under two different conditions. Due to the diamagnetic term $B^2(x^2+y^2)$ scaling as B^2n^4, one can make this term dominate in two ways. Firstly, one can increase the strength of the field B for fixed n. This is generally difficult to obtain experimentally – for it is hard to create a strong and uniform B field. Alternatively, the system can be studied at a fixed B but increasing n. Besides the experimental advantage, increasing n gets the system closer to the semiclassical limit as the size of the atom approximates the mesoscopic scale, see (Connerade 2005, 384).

Between these two regions, in region (b) in Figure 4.4, there is the case of n-mixing (of $n-$levels in the field-free atom), where the spectrum becomes chaotic. That is the region the physical account of which presents a challenge for the Received View (and, to the opponent in Bokulich's case, the advocate of theory-reduction). And it is in this case that methods from quantum chaos play a relevant role and Bokulich focuses on this case in order to develop and apply her interstructuralist approach.

Region (b) in Figure 4.4 shows a spectrum that looks complex and irregular, whereby no pattern seems to show and the signal looks chaotic. However, a Fourier transformation into time scale obtains a strikingly systematic organisation: a pattern which could be physically interpreted. The interpretation was suggested by the theoretical method used to visualise the pattern: that is, the framework of quantum chaos, whereby classical concepts were appropriate. This was particularly due to the work of the Bielefeld group in the 1980s, Holle et al. (1986, 1988); Main et al. (1986), who observed that the distribution of the peaks in region (b) in Figure 4.5 follow the structure of classically allowed closed orbits – according to quantum chaos theory – for the electron moving in the combined Coulomb and magnetic fields. A magnified region of that chaotic area is shown in Figure 4.5, along with the Fourier transform into the time scale. Each peak of absorption there corresponds to a specific closed classical trajectory on the plane (x, y) (drawn on top of each peak).

Now, so far the account of the problem is as quantum as it can get,[16] and because it is not integrable, the WKB and EBK methods cannot be applied. Indeed, the key to solve this problem and obtain a description of the density of states as a function of the energy, comes from modern semiclassical mechanics for non-integrable problems. This is a case of Bokulich's General Correspondence Principle, discussed in Section 4.2, because

[16] I will come back to this. I will argue in Chapter 5 that here we are already in the presence of 'intruding' classical mechanical ideas: does the underlying idea of the hydrogen atom not count as a classicality originated in the quantification of a classical hamiltonian of a planetary system? Is it really a "purely" quantum description?

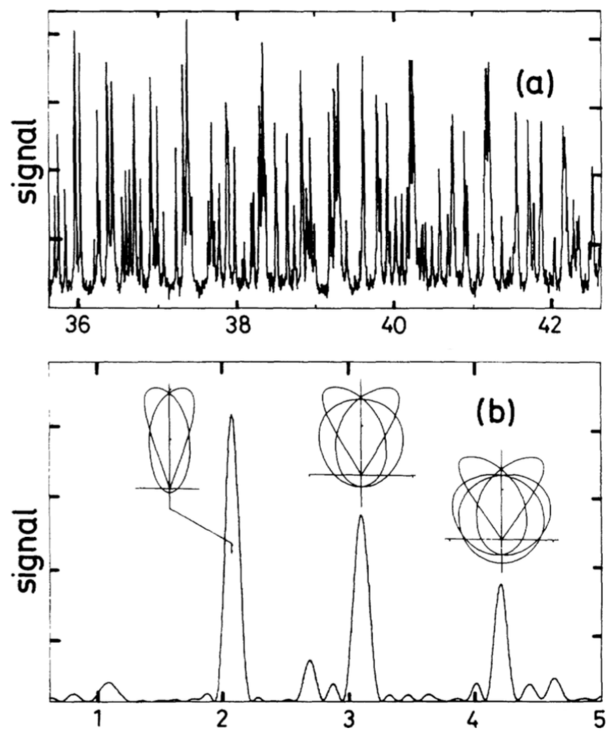

FIGURE 4.5: (a) Absorption spectrum of the Rydberg electron near ionisation energy in the energy scale (x-axis) and an arbitrary signal magnitude on y-axis. The pattern is irregular. (b) Fourier transform of (a) into the time domain suggested by the trace formula. The peaks appear at the return-time of the associated with periodic classical orbits in (ρ, z) projection for the electron. There is only one periodic orbit per peak, there seems to be more but is just that the trajectory passes close to the origin a some times before the final return. Obtained from (Holle et al. 1988, 161).

developments of CM (classical chaos theory) aid in the development of QM (quantum chaos). Let us see how the trace formula helps to explain the quantum phenomenon.

4.4.1 The Trace Formula and the Rydberg Spectrum

The basis for the understanding of the structure of the spectrum in the Region (b) of Figure 4.4 (qualitatively the same as the spectrum in part (a) of Figure 4.5), is the trace formula, developed by Gutzwiller. I discussed it in Section 4.3.2. I follow the model developed in (Delos and Du 1988; Du and Delos 1988a,b).

The classical orbit theory developed by Gutzwiller with the trace formula can approach non-integrable problems. It can calculate the density of states by considering periodic orbits associated with the corresponding classical system.

As we discussed, the semiclassical approximation to the Green function from which the density of states $\rho(E)$ can be calculated, includes, in principle, the contribution of all the classical trajectories. This sum is difficult to use as it includes many terms and its convergence is not guaranteed. The stationary phase method simplifies the calculation, by showing that only periodic orbits contribute significantly. It separates the density of states as the sum of two contributions, from eq. (4.16):

$$\rho(E) = \rho_0(E) + \rho_{\mathrm{osc}}(E), \tag{4.25}$$

where $\rho_0(E)$ is the phase-space volume available and the oscillatory part is

$$\rho_{\mathrm{osc}}(E) = \sum_k a_k(E) \sin\Big(\frac{S_k(E)}{\hbar} + \eta_k\Big).^{17} \tag{4.26}$$

Du and Delos' idea is to calculate the absorption spectrum within a certain resolution ΔE. That is, including only those orbits with period $T \leq T_{\mathrm{max}} = 2\pi\hbar/\Delta E$, for only the spectrum of part (a) in Figure 4.5 is to be considered here (which is similar to Region (b) of Figure 4.4). What is measured is an average absorption of states within the finite-resolution limited by the nature of the experiment, because the resolution of the experiment does not manage to separate individual states (as we saw in the discussion of the n-mixing effect!). Therefore, the aim is not to calculate individual eigenvalues for individual states. Furthermore, Du and Delos emphasise repeated times, that "it is not claimed that any relationship exists between individual orbits and individual quantum states" (Du and Delos 1988a, 1901).

Now, what spectroscopic measurements obtain is not the density of states $\rho(E)$, but a proportional magnitude $Df(E)$ called oscillator-strength density, (Du and Delos 1988a, 1902). When a laser is applied to excite the electron in the atom (the technology of tunable lasers allows this possibility, which is the technique used in the experiments of Holle et al. (1988)), there is a rate of absorption of photons, or, in other words, a rate of production of atoms whose electron passes from initial state $\psi_i \to \psi_f$ (from energy $E_i \to E_f$). In the conditions of the finite-resolution, where the spacing between energy levels of the atom is small compared to the energy width of the laser beam and the transitions occur from one initial state to many final states, instead of between two particular states, it is useful to define the oscillator-strength density (per unit of energy increment), $Df(E_f)$.[18]

As Du and Delos (1988a, 1904) show, the oscillator-strength density becomes

$$Df(E) = Df_0(E) + \sum_{k_m} C_{mk_m} \sin(T_{mk_m} E + \Delta_{mk_m}), \tag{4.27}$$

[17] I am following now the notation in (Du and Delos 1988a).
[18] See eq. (2.8) in (Du and Delos 1988b, 1915).

where Df_0 is a smooth background term which is the oscillator-strength that would have been obtained without an external magnetic field, or, in other words, the term obtained from the waves that propagate from $q' \to q$ within the vicinity of the nucleus (and hence the magnetic field is ignored). The terms in the sum are calculated by using semiclassical considerations of the nature of the orbits, leading to an account of the spectral lines in the empirical results.

Du and Delos (1988a, 1902) provide the central idea in correlating the seemingly chaotic oscillations in the spectrum with closed classical orbits. The illustration of the hypotheses which leads to their model is Figure 4.6. When the laser stimulates the atom embedded in the magnetic field, there is an outgoing Coulombian wave at near-zero-energy, propagating from the initial level near the nucleus (say, orbital 2p, as in Figure 4.6), to a Rydberg orbit distance, i.e. $r \leq 50a_0$ (where a_0 is the ground level distance). This propagation happens in accord with classical periodic trajectories. Eventually, the magnetic field becomes relevant, much more than the Coulombian potential and the wave fronts are returned back to the atom. The incoming waves interfere with the outgoing ones, producing the spectrum in the n-mixing zone, in Figure 4.4. Of course, this is a very simplified version of the story and the article (Du and Delos 1988b) is entirely devoted to these calculations. Relevantly, in the region where the classical analog is chaotic, the Fourier transform onto the time-scale obtains harmonics that correspond to the classically periodic orbits.

Hence, the model developed can predict the wavelength, amplitude and phase of the observed resonances (see Figure 4.5 (b)), and, furthermore, this model can also be used to explain the absorption spectra of other atoms, such as hydrogen, helium, and lithium atoms in strong magnetic fields. Therefore, there is no quantum chaos and the seemingly random absorption spectrum can be described in terms of ordered peaks.

Now I have discussed the account of the Rydberg atom in a strong magnetic field. At the beginning of Section 4.3 I had mentioned the merits and motivations that Bokulich assigns to semiclassical mechanics. The phenomenon of the Rydberg atom discussed here represents a case where (i) semiclassical mechanics provides an account of a problem the quantum explanation of which is unavailable. Plus, (ii) it provides physical insight through the use of the trace formula and the corresponding Fourier transform from the energy scale into the time scale, whereby the seemingly chaotic spectrum in the former scale appears as a sequence of harmonics in the latter scale. Yet, the phenomenon of the absorption of the Rydberg atom in a magnetic field was known well before the development of the theory of quantum chaos and the semiclassical approach (the measurements by Garton and Tomkins are from 1969 and the trace formula was developed after that).

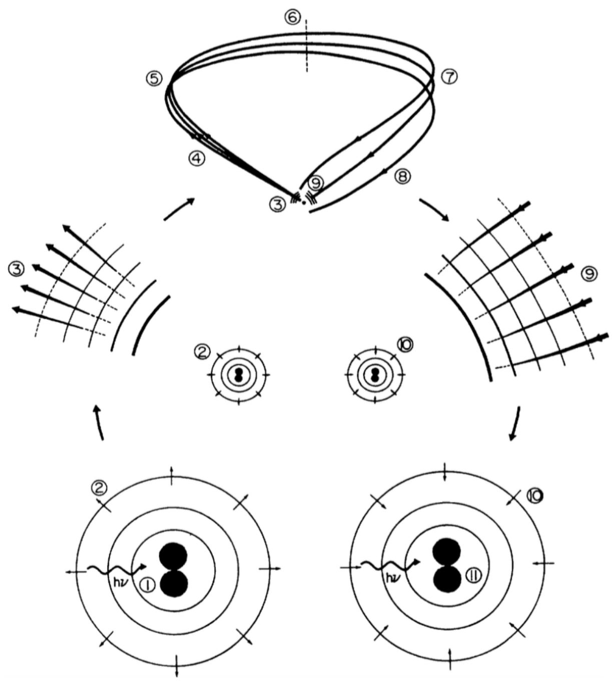

FIGURE 4.6: (Du and Delos 1988b, 1914)'s fictional orbits account of the spectrum near ionisation energy for Rydberg states. "(1) The atom is initially in the 2p, state, with the oscillating field due to the laser present. (2) The oscillating field produces zero-energy Coulomb waves, which propagate outward in all directions. (3) For distances greater than about $50a_0$, a semiclassical approximation is appropriate, and we can propagate the wave outward by following classical trajectories. (4) A pencil of trajectories propagates outward, encounters a caustic (5), a focus (6), and another caustic (7). This group of trajectories started out in such a direction that it turned around and returned toward the atom (8). Around $50a_0$, we describe it as an incoming zero-energy Coulomb wave (9), which continues to propagate inward (10), until it overlaps with the initial 2p, state (11). Interference between steadily produced outgoing and incoming waves leads to oscillations in the absorption spectrum."

In the next section I will discuss the problem of the helium atom, which is a very clear case of a quantum phenomenon that had been known before the novel developments of CM, and explained with the help of those developments. Then, I will discuss the problem of wavefunction scarring, whereby there in application of chaos theory, a development of CM, that leads to the appreciation of novel quantum phenomena: wavefunction scarring was not known before the development of quantum chaos! Yet, I will not develop the next two problems in as much detail as I have done with the Rydberg atom case, because I think that the most important elements for the relevant philosophical debate are already present here.

Then I will have completed to review the material that I need to fully discuss the interstructuralist approach to the QM-CM relation in Chapter 5.

4.5 The Inverse Correspondence Principle: the Helium Atom

The three-body problem was known long before the appearance of QM. Within the development of QM, one of the main obstacles to the 'old' quantum theory of Bohr-Sommerfeld, was its difficulty to account for the helium atom: a quantum version of a three-body problem interacting via a Coulombian potential.

The hamiltonian of the classical problem of two particles with charge e and mass m_e interacting with each other and with a nucleus of infinite mass and charge Z (with units $e = m_e = 1$) is:

$$H = \frac{\mathbf{p}_1^2 + \mathbf{p}_2^2}{2} + \frac{Z}{r_1} + \frac{Z}{r_2} + \frac{1}{r_{12}}, \tag{4.28}$$

where $r_{1,2}$ are the distance from the particle 1,2 to the nucleus and r_{12} is the distance between the particles. The constants of motion are the energy E and the total angular momentum $\mathbf{L}$. Thus, one can confine the motion of the three particles to a plane ($\mathbf{L} = 0$) and then take the three distances between them as the coordinates. This problem is non-integrable.

Leopold and Percival (1980) recognised that the failure of the early developments in QM to obtain the energy of the ground state of the helium atom was not actually due to a flaw in the quantum theory – as it was understood at the time. Instead, what the founding fathers of QM had failed to take into account was the role that the theory of classical periodic orbits plays in problems that are not integrable. This was not fully known at the time, and it was only developed within the framework of quantum chaos. For instance, the conjugate points in periodic orbit theory and the Maslov index, which is from the 1970s.

Indeed, a significant step in the account of the helium atom within the framework of the EBK method is due to Leopold and Percival (1980). Whilst they incorporated semiclassical knowledge in the quantisation of the classical three-body problem (the Maslov index which counts the conjugate points of the orbits), they still did not succeed in approximating the solutions to the problem. And we now know why: the problem is non-integrable and, as mentioned in Section 4.3.1, the EBK method does not apply. Still, they obtained closer agreement to the experimental results compared to previous models and led the way to the correct description of this problem.

The semiclassical approach required to correctly obtain the energies of the helium atom, is fundamentally based in Gutzwiller's trace formula. This was carried out by Ezra et al. (1991); Wintgen et al. (1992). They associated the periodic orbits of the classical system with the density of states and the corresponding eigenvalues of the quantised hamiltonian.

Similarly to the Rydberg atom in a strong magnetic field, the analysis and discussion of the solution to the problem of the helium atom quickly becomes very technical and complex. The introduction of the trace formula and the analysis of the classical orbits involved in terms of their period, stability, and other properties., would require visiting complex issues that are unnecessary in order to engage with the underlying philosophical debate. That is why I offered more detail in the discussion of the Rydberg atom and, for the purposes of my analysis, that will be sufficient.

However, what is useful to recall is that the semiclassical approach provides a description of, and physical insight into, a phenomenon which could not be achieved without the help of the classical periodic orbit theory. The analysis of the periodic orbits allows the physicists to explain the resonances of the dynamic of the helium atom by assessing the stability of the configuration and the orbits that appear in the trace formula, etc. The helium atom could not be satisfactorily solved within the old quantum theory. Relevantly, the reason was a lack of consideration of the classical theory in the classical analog: the three-body problem. The subsequent development of classical periodic orbits in the work of Wintgen and collaborators and Gutzwiller et. al., was relevantly used to approach the quantum problem. This showed not that the old quantum theory was flawed, but that the difficulty had been with the account of the corresponding classical problem.

Before moving on to the next relevant case, wavefunction scarring, which is a phenomenon that was not known before the advent of modern semiclassical mechanics, it is worth questioning the following: how "purely" quantum is the problem of the helium atom, given that it's basic description is the quantum analog to the three-body problem? How much surprise should be felt over the fact that the classical theory appears relevant to the account of the quantum problem, given that the quantum system is the quantisation of the classical system?

On the other hand, the problem of the helium atom in this context, clearly does not fit with the standard account of intertheory relations: the predecessor CM was not abandoned and the successor QM required its predecessor to account for the phenomena in a much more complex way than Nickles' reduction$_2$ could have considered. From this point of view, it is evident that Bokulich's interstructuralism is required.

4.6 A Novel Phenomenon: Wavefunction Scarring

In Section 4.4 I discussed the Rydberg atom in a strong magnetic field, the empirical results of which were known before the discovery of quantum chaos but the problem remained unaccounted for until the development of the trace formula. The case of the Rydberg atom can be mapped onto the left hand side of Figure 4.1, the General Correspondence Principle. That is, the account of a quantum phenomenon aided by CM. Indeed, to a large extent that development in CM can be seen to have been fuelled by a previous quantum problem: the helium atom. In Section 4.5 I discussed that the helium atom was also an unsolved problem since the early stages of QM, and its satisfactory account was a result of further development of CM: the understanding of the classical periodic orbits and the associated complexities (periods, stability and so on). The helium atom represents an example of the Inverse Correspondence Principle, where the development of CM is fed back into QM, in order to describe phenomena like the Rydberg atom. These two cases share that the quantum problem was there, before the development of the classical theory.

In this section I discuss the case of wavefunction scarring. This is a novelty that Bokulich's appropriation of the philosophy of Dirac can account for. The structural continuity thesis allows the interchange of dynamical structures and classical trajectories can play significant roles in the account of quantum phenomena. Wavefunction scarring is a case where the aid of classical ideas that had been stimulated by problems originated in QM, were put back into the quantum, leading to a novel and unexpected phenomenon.

Within the development of classical chaos, a paradigmatic problem that exhibits chaotic features is the so-called 'stadium shaped billiard' problem. This consists of a free moving point-particle that bounces elastically off the walls of a frictionless two-dimensional enclosure that has the shape of a football stadium. The shape of the stadium can vary. A typical case is known as the Bunimovich stadium and it has two semi-circular end caps connected by straight horizontal sections of equal length, as in Figure 4.7.

The relevant question is concerned with the trajectory of the particle. This exhibits the chaotic features that I discussed in Section 4.3. Firstly, given unlimited time the trajectory of the particle will cover the entire stadium an infinite number of times with an infinite number of momentum directions, thus occupying the whole phase-space available. Secondly, two trajectories that start with slightly different initial conditions, will diverge quickly from each other, within a few bounces. These two observations are the signature of the chaotic dynamics. The plot of the trajectories after some finite time is depicted in Figure 4.8.

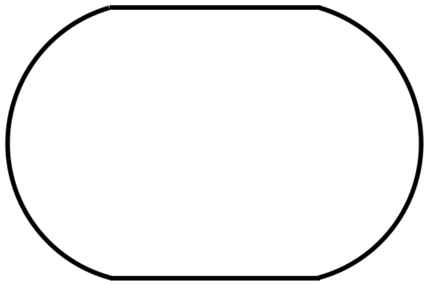

FIGURE 4.7: 2D stadium shaped enclosure, known as the Bunimovich stadium. There is a vast literature on this topic, see Heller (1986); Heller and O'Connor (1987); Gutzwiller (1990).

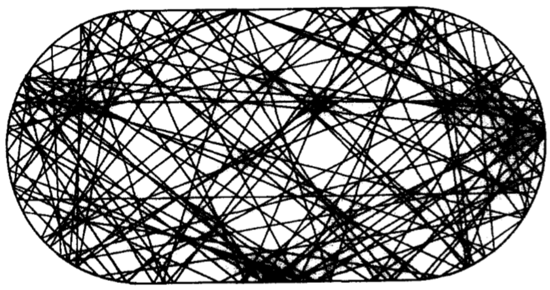

FIGURE 4.8: This illustrates the trajectory of a particle in the Bunimovich stadium. Given random initial conditions, the particle will most likely not follow a periodic orbit. However unlikely, periodic orbits are dynamically possible. Moreover, given unlimited time for the particle to traverse the surface, it will cover the whole region, passing over each point an infinite number of times with an infinite different momentum directions. Image taken from (Heller and O'Connor 1987, 202).

In the possible trajectories of classically chaotic systems there are, mostly, unstable orbits which are not periodic. However, there are also periodic orbits and although there is an infinite number of them, they are of zero measure. Indeed, some of them even have names: 'rectangular', 'bow-tie', 'V', and 'double-diamond', see (Heller 1984),[19] and see Figure 4.9 where I show some of them. They are all unstable, meaning that they are isolated, there are no periodic orbits near them, and the slightest deviation from one of them will exponentially diverge from it with time. Each periodic orbit has different period ω and instability, characterised by the Lyapunov exponent λ, see (Heller and O'Connor 1987, 208).

Now, so far I discussed the *classical* version, which really poses no challenge for physics. Indeed, the problem is to describe the quantum analog to this system! Think of the usual quantisation, we should take the association between a ray and a plane wave, whereby the ray (the trajectory of the particle) will determine the wavefront as the surface that

[19] A nice explanation for this phenomenon is in (Gutzwiller 1990, Sect. 15.6).

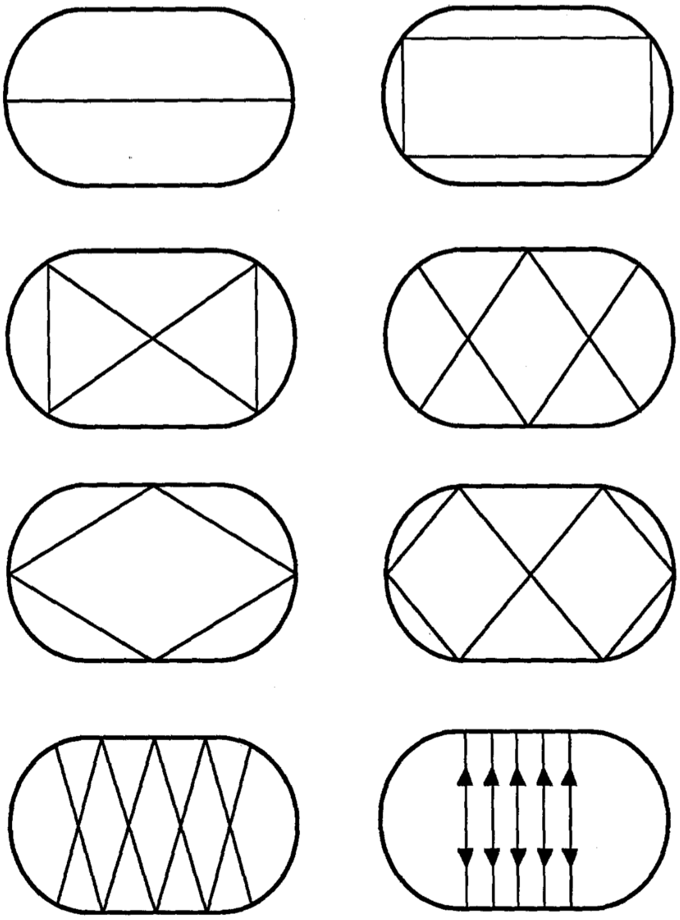

FIGURE 4.9: Some periodic orbits. Image taken from (Heller 1986, 169).

is perpendicular to it. Then, looking at the trajectory in Figure 4.8 we would have the superposition of the same free particle in random directions, following the trajectory of the particle. In terms of the probability density, physicists initially expected to obtain a diffuse plot in the coordinate space, with 'speckles', i.e. spikes of very large amplitude at rare places. This was the 'random eigenstate' hypothesis, articulated by Michael V. Berry, which got to be known as 'Berry's conjecture'. Berry provided a qualitative analysis of his proposal, calculating the resulting wavefunction and its characteristics, see (Heller and O'Connor 1987, 202), (Heller 1984) and references in there. However, this analysis was found to be inappropriate, as Heller and his collaborators found by doing numerical simulations. And here is where the 'surprises' of quantum chaos appear.[20]

Instead of the speckles, what was found were what Heller (1984) called the "scars of the wavefunction": this is the localisation of probability density around those zero measure

[20]In his review of Bokulich (2008a), Berry (2010) attempts to clarify his view on the problem of wavefunction scarring. However, this discussion is not my main concern.

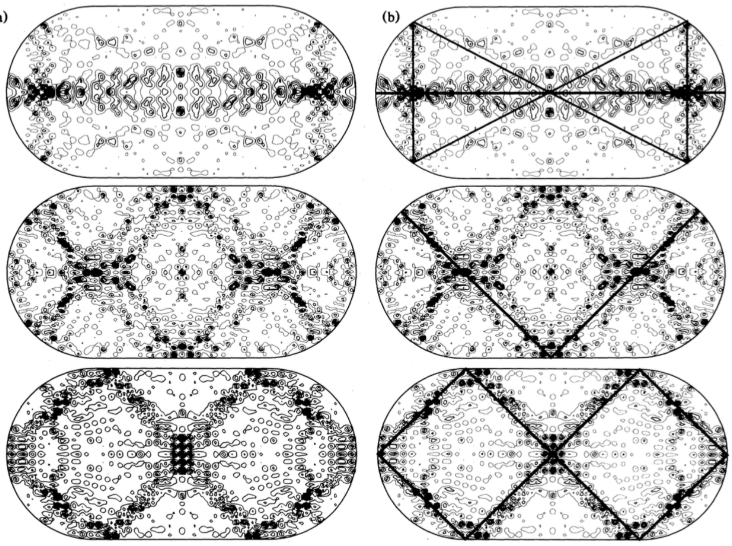

FIGURE 4.10: (a) Three scarred states of the free particle in 2-D stadium shaped domain. (b) The periodic orbits corresponding to the (a) cases. Taken from (Heller 1984, 1517).

periodic orbits. I show some of these scars in Figure 4.10. There are many more in the several articles and books in the citations of this section.

What is surprising is both the appearance of the periodic orbit structures at the level of the density of probability as scars in the quantum billiard problem and the fact their appearance remains even in the limit $h \to 0$, (Heller and O'Connor 1987). There is much more detailed discussion and explanation of this phenomenon that one could deliver, involving the analysis of the appearance of the scars in the Fourier transform of the correlation function between the wavepacket with itself as a function of time $\langle \psi(t)|\phi(0)\rangle$ and how that distribution of peaks (the scar enhancement) depends on the parameters of the classical periodic orbits (its stability Lyapunov exponent λ, the period of the orbit τ, etc. see (Bokulich 2008a, 128-129, and references mentioned). However, for my purposes this suffices to engage with the underlying philosophical debate over Bokulich's view and the relationship QM-CM.

Persuasively enough, the phenomenon of wavefunction scarring shows how a phenomenon within QM (the quantum analog of the classical stadium) can still retain features of classical dynamical structures. Again, although it is known that the phenomenon is not classical, it cannot be explained within QM only, and the theory of classical periodic orbits in the classical problem within classical chaos seems to be playing a significant role in the explanation of the quantum phenomenon. According to Bokulich (2008a,b), the role of the classical trajectories is indispensable in order to explain.

Therefore, this is a case in modern semiclassical mechanics whereby classical ideas play an essential role, act as an investigative tool into new phenomena that was not known before the development of quantum chaos.

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These three physical problems clearly exemplify the type of intertheory relation put forward by Bokulich. Her view is underpinned by her appropriation of Dirac's philosophy, captured by the Reciprocal Correspondence Principle Methodology. The helium atom, a quantum phenomenon that was known and remained unresolved since early stages of the discovery of QM, fuelled the further development of CM with the work of Gutzwiller and others on the non-integrable three-body problem. The development of classical periodic orbit theory was then fed back into the QM domain, where it helped to appropriately account for the helium atom. Hence, this is a case of the Inverse Correspondence Principle, which is one of the two was in which QM and CM relate. This is illustrated by the right hand side of Figure 4.1. Secondly, the spectrum of the Rydberg atoms was known, although Region (b) in Figure 4.4 remained unexplained until the development of the classical orbit theory. The trace formula was used to calculate quantum density of states of the atom. Therefore, this is the second aspect of the Reciprocal Correspondence Principle Methodology, that is, the General Correspondence Principle. This occurs when CM helps to develop an explanation of a quantum phenomenon. It is illustrated in the left hand side of Figure 4.1. Finally, to close the loop, there is the case of wavefunction scarring. The relevant aspect is the novelty. As a result of that feedback loop, physicists discovered a novel phenomenon, unknown before the discovery of classical chaos theory. Bokulich's view rightly captures the practice of physicists with the framework based on the notion of Open Theories and the Reciprocal Correspondence Principle Methodology.

Furthermore, these three cases exemplify the motivations that Bokulich has to taking semiclassical mechanics seriously. Recall them in page 100. Both the Rydberg atom and the helium atom apply to (i) and (ii). That is, they represent cases where (i) semiclassical mechanics provides an account for a problem the quantum explanation of which is unavailable and (ii) they provide physical insight through the use of the trace formula. This is almost an exact quote of how physicist explain the merits of semiclassical mechanics, see (Main 1999, 236). Yet, both phenomena were known well before the development of the theory of quantum chaos and the modern semiclassical approach.

In turn, wavefunction scarring fulfils (iii). That is, semiclassical mechanics provides the means to obtain novel phenomena by looking past the fully quantum description of some problems. Indeed, as discussed above, wavefunction scarring was not known before the development of quantum chaos and the appreciation of the classically chaotic system.[21]

Now, Bokulich takes the fictional character of the explanation of such semiclassical phenomena and provides a philosophical account of it through a novel form of scientific explanation. In her words, the "anomalous resonances and their regular organization seems to be intimately tied to the *fictional assumption* that these Rydberg electrons, instead of behaving quantum mechanically, are following definite classical trajectories" (Bokulich 2008a, 118, my emphasis). For what is worth, this claim also holds for the helium atom and wavefunction scarring.

However, the assessment of these claims requires further analysis.

4.7 On the 'Quantum' Nature of Semiclassical Phenomena

Throughout these discussions of the physical problems, I have remarked that I agree with Bokulich in that the Open Theories approach captures the intertheory relation that the semiclassical mechanics phenomena display. I have shown sympathy with the way the physical account of the Rydberg atom, the helium atom and the wavefunction scarring mirror the scheme depicted in Figure 4.1, in terms of the progress made to account for phenomena, which the standard model of Nickels reduction$_2$, discussed in Chapter 3, fails to take in. However, my view departs from hers in the philosophical consideration of these phenomena in relation to their quantum nature and the role that classical ideas play in their account.

Let us look at wavefunction scarring. Figure 4.10 shows plots of the density of probability obtained from the wavefunction, according to Heller (1984). This is a quantum phenomenon. And it is undeniable that they resemble the classical periodic orbits from the classical analog, as part (b) of Figure 4.10 shows. The classical analog is the free particle in the stadium shaped domain, the Bunimovich stadium, and it is a chaotic system. In almost every case, the trajectory of the classical particle will cover the whole available space without repeating itself, like in Figure 4.8. However, amongst those possible trajectories of the particle, there are an infinite number of periodic orbits (which are unstable, and despite their infinite amount, this is a set of measure zero), some of them were shown in Figure 4.9. Thus, comparing the quantum case with its classical analog, the behaviour of the former is really surprising.

[21]Of course, the semiclassical account of the Rydberg atom also led to novel predictions, allowing to study the phenomenon for different atoms, then participating in (iii) as well.

However, one can question whether the phenomenon of wavefunction scarring is *purely* a quantum phenomenon. Recall that this quantum phenomenon is conceived as the quantum analog to the classically chaotic problem of the Bunimovich stadium. As a quantum phenomenon, it is conceived from a classical one. That is, CM is necessary in order to conceive the wavefunction scarring. More precisely, the very formulation of the physical problem is "there is a quantum particle in a 2D confined region" and this already involves a classical idea. Therefore, should the physicist really be surprised to find classical dynamical structures in such a problem? Or, should the realist take the appearance of classical orbits in wavefunction scarring as a significant example of how QM relates to CM given that the physical problem where the scars appear was formulated in a "mixed language" from the outset? Further investigative questions arising from the discussion of this issue are: is there a quantum phenomenon that has no resemblance whatsoever to a classical phenomenon? That is, is there a quantum phenomenon that is explained without the aid of classical ideas? If there is not, is there a measure of the role that classical ideas play in the account of quantum phenomena? Finally, are classical dynamical structures playing different roles in the semiclassically described phenomena discussed in this chapter, compared to other quantum phenomena?

Let us look at the Rydberg atom, the phenomenon of the absorption spectrum is clearly a quantum effect. See Figure 4.4: to start with, in the case where $B = 0$, there is no external magnetic field and the spectrum is discrete: there are peaks of absorption at certain energies, separated by a specific gap. In a classical system the spectrum would be a continuum and the fact the energy spectrum is quantised for the atom is a signature of QM. The account of such phenomenon is the quantum hydrogen-like atom, which requires no aid of classically periodic orbit theory. Now, in the chaotic region, part (b) of Figure 4.4, the account of the atomic spectrum is given by the theory of periodic orbits and the trace formula connecting the classical orbits with the quantum density of states. Thus, either these classical orbits are really there, or they play a role such that we should take them seriously, or at least insofar as to explain the phenomenon, they are playing a significant role. However, continuous classical orbits (periodic or not) have no real existence in QM. This is recognised in the standard physics literature: "in [QM] there is no such concept as the path of a particle" (Landau and Lifshitz 1965, 2). That is, there are no trajectories in the quantum.[22] Therefore, we need to decide what to do with the classical trajectories in the quantum phenomena: we could take them to be fictional elements playing an explanatory role without being reified as real, or we could take the semiclassical explanation to be flawed/incomplete in some way. Perhaps the model could be improved, so that the classical periodic orbits are eliminated from

[22]Of course, unless one takes up a Bohmian interpretation. But I am so far only engaging with standard QM.

the explanation. Yet, physicists do not follow the latter route and, indeed, for the purposes of the observed spectrum, the trace formula allows them to successfully obtain the description of the peaks in the Fourier transform of the spectrum in the energy scale. Nevertheless, the realist is interested in accounting for the practice of physicists and also, in giving an account of the way the world is according to the theory. The situation requires to critically assess the role of the classical trajectories, which are real according to CM, but that are knowingly non-real in the quantum.

Coming back to the case with $B = 0$ in Figure 4.4: is this really a case where classical ideas play no role? Recall that the hydrogen atom was originally conceived as a two-body system with a central potential, where one of the bodies has an infinite mass and is at rest. The correct account of the hydrogen atom, including the quantum effects (Zeeman effect, Darwin correction, orbit-spin interaction) is a modification of the quantisation of the classical hamiltonian of the two-body system:

$$H = p^2 2m + V(r). \tag{4.29}$$

To quantise this hamiltonian, the potential is taken to be a Coulomb one instead of gravitational one and, then, suitable terms are added to include quantum corrections. That is the basis of the account of the hydrogen atom. If an external uniform magnetic field is added, the hamiltonian is the Landau's hamiltonian seen before in eq. (4.23). Therefore, is the hydrogen atom without the external magnetic field a *purely* quantum case? Is this qualitatively different to the quantum-chaotic region of the spectrum, where the only explanation available is via the trace formula and the classical periodic orbit theory?

Bokulich builds a novel form of scientific explanation around the fictional trajectories that whilst false, are undeniably playing a relevant explanatory role. The situation is remarkable and I certainly agree with the motivation for accounting for this from a philosophical point of view. Before even exploring Bokulich's account of explanation, I will comment on my interpretation of the understanding of these phenomena by the physicists. I identify that the physicists are well aware that the electron in the Rydberg atom is not following a classical trajectory. They also indicate the lack of relationship between the empirical result of the Rydberg spectrum and the individual state of the electron in the Rydberg atom. In a popular science article, von Baeyer (1995) puts it that the electron behaves "like an atomic amphibian, sprinting along the firm ground of classical mechanics before it plunges back into the swirling waves of quantum mechanics". This is a nice story. However, it is essentially erroneous and no realist ought to take it seriously. The key is that no relationship exists between the individual states of the electron and the empirical results suggesting classical orbits. Physicists explicitly state this: "there is no direct relation between the periodic orbits and the spectrum of energy levels" (Gutzwiller 1990, 210); Connerade (2005, 366) explains that, when 'quantum chaos' is set on, the n and ℓ numbers for individual electrons lose meaning as such; Du and Delos emphasise repeated times in their articles that "it is not claimed that any relationship exists between individual orbits and individual quantum states" (Du and Delos 1988a, 1901). These citations indicate that, physicists maintain that the underlying quantum system is being as quantum as it has always been, it does not follow classical trajectories, and it is not thought that they do.

Now, what is making the difference between what the system is really like and how the observations can be interpreted, described and predicted? I indicate that a significant factor here is the underlying hypothesis in the semiclassical model: the finite-resolution condition. The absorption spectrum in the Region (b) of Figure 4.4, and also part (a) of Figure 4.5, are not showing the levels of the atom itself, as they do in the case of $B = 0$. This is because of the resolution of the experiment (or the theoretical calculation), not because anything that is happening to the physical system under study. The electron is always an electron and its quantum nature remains the same, regardless of its interactions. It cannot be otherwise and both the realists and the physicists know that. In the next chapter I will explore these issues in a broader philosophical context.

4.8 Conclusions

Let us take stock and see how to move on. In Section 4.2 I characterised Bokulich's view on physical theories and their intertheory relations, in terms of Open Theories and the Reciprocal Correspondence Principle Methodology. I illustrated this scheme in Figure 4.1. It considers an open character of the theories in the sense that they are open to changes and small modifications are possible. This entails a gradualist conception of progress. The thesis of structural continuity says that structural dynamical entities, can be used in different contexts, allowing for classical trajectories to play a role in the explanation of quantum effects.

I presented semiclassical mechanics, both in its 'old' style EBK quantisation applicable to integrable problems, in Section 4.3.1, and, in Section 4.3.2, the trace formula, which applies to non-integrable problems. The trace formula is the crux of modern semiclassical mechanics. I showed how classical ideas appear in semiclassical mechanics: within the EBK method, CM is used to approximate the solutions to the Schrödinger equation of quantum problems. Similarly, classical orbit theory is crucial in the method of approximation of quantum quantities via the trace formula. This is particularly useful in non-integrable problems, which present chaotic behaviour. I discussed relevant examples in Sections 4.4, 4.5 and 4.6, namely: the Rydberg atom in a strong uniform external magnetic field, the helium atom and the phenomenon of wavefunction scarring, respectively.

By the end of Section 4.6 I discussed that these three problems are captured by Bokulich's account of intertheory relations. Furthermore, I explained how these cases demonstrate the motivations for the philosopher to pay more attention to semiclassical mechanics. As Bokulich (2008a, 104) puts it, these are that semiclassical methods: are useful in cases where a quantum approach is difficult; can provide physical insight; and allow for the discovery of novel phenomena, see page 100.

Then, I introduced the basis for Bokulich's interstructuralism. However, critical philosophical engagement is required to account for this view. In Section 4.7 I raised questions that cue in the next Chapter. These relate to the content of this chapter in relation to the philosophical interpretation of the physical account of those problems. In particular, the realist will have to reflect on the ontological status of the classical orbits in the quantum domain; on the type of explanation that the physicists are employing; on the role of classical orbits in the semiclassical models, whether they are fictional or indispensable for the explanation; plus, on the nature of a purely quantum description of a quantum effect.

Chapter 5

The Interstructuralist Approach (II): Philosophical Flesh

5.1 Introduction

In this chapter I continue to critically engage with the interstructuralist account of the QM-CM relation.

In Chapter 4 I set out the 'physical bones' of the issue. I analysed modern semiclassical mechanics by discussing the EBK method, which applies to integrable problems (where the number of constants of motion equals the number of degrees of freedom), and the trace formula developed by Gutzwiller, which additionally applies also to non-integrable problems (where there are fewer constants of motion than degrees of freedom). The trace formula is relevant for the physical problems upon which Bokulich builds her interstructuralist account. These are quantum phenomena with chaotic classical analogs, such as the Rydberg atom, the helium atom and wavefunction scarring. Unexpectedly, classical periodic orbits play a significant role in their explanation. I anticipated the view that Bokulich proposes and, whilst I raised some worries and made some critical observations, I left it to this chapter to exploring the 'philosophical flesh'.

In the first part of the chapter, in Section 5.2, I will examine the model-structural form of scientific explanation developed by Bokulich. This concludes with Section 5.2.5 where I will develop how this view explains the physical problems discussed in Chapter 4.

In the second part of this chapter, in Section 5.3, I will critically engage with Bokulich's model-structural explanation. In Sections 5.3.1 and 5.3.2 I will engage with interstructuralism and its application to the semiclassical phenomena – with emphasis on the crucial problem of the Rydberg atoms – from within, as it were. I will work with its own elements to explore the role of the fictions and their status. In Sections 5.3.3, 5.3.4 and 5.3.5, I will engage 'from the outside'. These sections will enthuse proposing a modified version of the form of explanation, offering a different interpretation to the goings on in semiclassical physics.

5.2 Scientific Explanation and Interstructuralism

Similarly to scientific realism and intertheory relations, scientific explanation is a vast and complex notion closely related to other big issues in the philosophy of science.

Indeed, there are various philosophical angles to approach the analysis of scientific explanation. Today it is accepted that the account of scientific explanation begins with the deductive-nomological model (DN model), whereby Carl Hempel's work is taken to be the standard version. Other notions of explanation can be seen as reaction to the DN model.

The DN model seems to be crucial in having set the standard distinction between explanandum and explanans: that which is being explained and that which explains, respectively. Indeed, Bueno and French (2012) take the establishment of such a distinction to be a criterion for what counts as an explanation. Yet, they remark that caution must be exercised in taking scientists assertions over what and in what sense elements in their theories and models are explanatory. The philosopher takes a critical attitude towards explanation, and examining and interpreting the scientific practice around this is central in philosophy of science.

My aim is to engage with the form of explanation in Bokulich's interstructuralism. In Section 5.2.1 I will recall the DN model. I will discuss Salmon's causal explanation in Section 5.2.2. Salmon's view is relevant as a reaction to the DN model and also in relation to Woodward's view, which is influential currently and especially in the context of Bokulich's form of explanation. I will recall the main ideas of Woodwardian explanation in Section 5.2.3. I will end this part of the chapter by discussing Bokulich's form of explanation in Section 5.2.4, and in Section 5.2.5 by using this framework to explain the mesoscopic phenomena described in Chapter 4.

5.2.1 Hempel's DN Model

The topic of scientific explanation is discussed in a vast array of literature. Similar to Chapter 3, where I presented forms of theory reduction that arise in relation to the traditional work of Nagel, forms of scientific explanation can be traced back to tensions or associations with Hempel's account. Hence, although most philosophers today reject the traditional view of Hempel, this view is worth recalling.

Hempel's view on explanation is built upon two central ideas. Firstly, explanations must make use of a law of nature in order to explain. Hence, the 'nomic expectability' says that it is by invoking laws that phenomena are expected and therefore explained. Secondly, the explanation obtains in a logical structure, whereby the *explanandum* – that which is being explained, the phenomenon – is deduced or derived from the *explanans*, the set of premises which includes initial and background conditions, plus at least one law. These two components motivate the name of 'deductive-nomoligcal' model of explanation (DN model). In short, the DN model shows the explanandum-phenomenon as deduced from the explanans (which includes a covering law).[1]

Recalling the general features of this form of explanation is relevant insofar as currently advocated forms of explanation are to some extent reactions to the DN model. In particular, reactions to the DN model's failure to capture explanations in actual science. There are various options to engage with the DN model, and I do not intend to discuss them all, see (de Regt 2011) or (Woodward 2008). For the purposes of discussing Bokulich's view on explanation in the context of her interstructuralism, it will be useful to recall two criticisms to DN model. Firstly, the criticism that the DN model fails to captures the scientific practice. This is because according to Hempel's 'empirical condition of adequacy', the explanans must be literary true, (Hempel 1965, 248). However, this is rarely the case since scientists use models to explain phenomena; and models typically include some non-true elements. Hence, there are explanations where the explanans is not true, see (Bokulich 2008a, 139) and (Bokulich 2012, 726). Furthermore, the empirical condition of adequacy entails that according to Hempel's DN model fictions cannot play an explanatory role. This motivates Bokulich to develop her own form of explanation, which will be analysed later on.

Secondly, there is the problem of causality in the DN model. For example, this is illustrated by the famous Bromberger's flagpole, see (Salmon 1989, 47). Consider a flagpole, its height and the length of the shadow that the flagpole casts. In case 1), the length of the shadow casted by a flagpole (explanandum) can be deduced from the explanans including the flagpole's height, the position of the Sun and some basic laws of geometrical-optics. 2) Alternatively, one could invert the example swapping roles in explanans and explanandum. One could claim that the height of the flagpole (now the

[1] Indeed, Nagelian reduction and the DN model are closely connected. One could claim that the less fundamental/predecessor theory is explained by the more fundamental/successor theory, or that the explanans reduces the explanandum, alternatively. I do not explore this relationship, however. See (de Regt 2011, Sect. 8).

explanandum) is explained by the explanans including the length of the shadow, the position of the Sun and the same laws of geometric-optics.

According to the DN model both cases count as explanation. However, there most people will not accept 2) since the shadow of a flagpole cannot really 'explain' the height of the flagpole. The asymmetry between 1) and 2) is that the shadow is caused by the light of the Sun shining on the flagpole. Hence, we ought not to accept the length of the shadow in the explanans and the height of the flagpole in the explanandum. That is because effects cannot explain causes; but causes explain effects. The inability to appreciate causal relations does not allow the DN model to differentiate between 1) and 2). Hempel belonged to the tradition in which causality was a 'metaphysical' and unnecessary concept, and he considered that "causal explanation is, at least implicitly, deductive-nomological" (Hempel 1965, 349). The problem remains, however, and it is accepted that original DN model cannot discriminate the apparently evident asymmetry introduced by causality, see (de Regt 2011, 161).

5.2.2 Salmon's Causal Explanation

The failure of the DN model to incorporate causal relations in the explanation motivated novel views. Salmon (1984a,b, 1989) proposed a form of explanation that is causal. His view appeared explicitly as a reaction to the DN model and it is the most influential account of causal explanation, (de Regt 2011; Woodward 2008). According to Salmon, an event is explained when it is shown that such an event fits within a physical pattern in the world. The patterns are "causal processes, causal interactions, and causal laws [that] provide the mechanisms by which the world works" (Salmon 1984b, 132).

In Salmon's theory, the causal interaction is a persistent 'mark' or change of at least one of the causal processes when two or more processes intersect, such as a collision between two cars that dents both. In Salmon's account explaining an event amounts to describing the causal processes and interactions that have produced it, (de Regt 2011, 162). In addition, Salmon (1989, 86) conceives that "explanations exist in the world". That is, the cause of a fact is the fact's explanation. Relevantly, by contrast with Hempel's DN model, explanations for Salmon are not arguments, but actual objective wordly entities. This highlights the contrast between the ontological and the epistemic conceptions of explanation. In the epistemic conception, scientific explanations are meant to be arguments and explanations aim at providing understanding. Hempel's DN model falls within this conception.[2]

[2]In addition, Salmon also contrasted his view with the modal conception of explanation, whereby the explanation explains by showing the necessity of the occurrence of the events. It is not clear how this differs from the ontic conception, see (Saatsi 2016).

Although in Salmon's view an explanation is also a report of the facts, the previous feature is dominant; and Salmon's view entails that explanation has an objective causal reference to the world. Bokulich (2016) notes that Salmon's view identifies the explanations with the phenomena. Hence, each entity existing in the world is an explanation, insofar as an entity is a phenomenon for which that entity is causally responsible. There is a controversial consequence in that things in the world are scientific explanations and thus explanation is not anymore a 'human activity', (Bokulich 2016, 4).

There are known criticisms to Salmon's account. First, whilst it was designed to capture the explanations in physics, it fails in relevant cases, such as in quantum phenomena. For instance, relevant aspects of entanglement do not involve a causal relation. Secondly, Salmon's view has been criticised for not being able to capture explanations in other sciences beyond physics. Finally, it has been argued that the causal aspect of Salmon's account requires counterfactuals. For example, in the explanation of the cars that crash there is a claim of what would have happened if they did not crash. However, Salmon did not want to include counterfactuals in his account, see (de Regt 2011, 161-162).

For my purposes it is relevant to emphasise that whilst semiclassical phenomena is quantum, it is explained by classical orbits. However, these dynamical structures are considered fictions. Therefore, this can hardly be captured by a causal relation. This motivates Bokulich to conceive a novel form of explanation.

In the next section I will examine James Woodward's notion of explanation, which incorporates the counterfactual aspect that Salmon's view could not.

5.2.3 Woodward's Causal-Counterfactual Account

Woodward's account appeared also as a reaction to the DN model of explanation. By contrast with the DN model, Woodward's notion aims to capture explanations that do not require invoking laws of nature, whilst capturing causal relations. By contrast with Salmon's explanation, Woodward articulates a central role of counterfactual statements in the explanation.

This view falls in the ontic conception, including a specific notion of causality. It is relevant to examine Woodward's proposal because Bokulich's own form of model explanation is built up on it. Despite Woodward's intention on focusing on causal relations, some argue that Woodward's framework can be exported and appropriated into non-ontic forms of explanation, (Bokulich 2008a; Saatsi and Pexton 2013; Saatsi 2016).

One central notion in Woodwardian explanation is the 'manipulationist or interventionist' conception of causal explanation, which is inspired by the "practical interest human beings have in manipulation and control" (Woodward 2003, 10). The causal dimension captures that explanations explain "by showing how an outcome depends [causally] on other variables or factors" (Woodward 2003, 6).[3] This is why Woodwardian explanation is considered to follow the tradition of Salmon's causal explanation.

Another central aspect is the notion of counterfactual.[4] Woodward's idea is that

> an explanation ought to be such that it can be used to answer what I call a *what-if-things-had-been-diferent* question: the explanation must enable us to see what sort of difference it would have made for the explanandum if the factors cited in the explanans had been different in various possible ways.
>
> (Woodward 2003, 11, emphasis in the original)

Let us focus on this aspect. A factor X is causally relevant to Y (X causes Y), relative to a suitable *what-if-things-had-been-diferent* question, 'w-question', for short. This is when we see how, if so, changes in the X are associated with changes in Y, (Woodward 2003, 14). Then, the causal claim X causes Y is true if and only if a relevant counterfactual conditional is true: 'had X been different, Y would have been different'. Here, the antecedent X is made true by interventions. That is, had one intervened on X, Y would have been different. Clearly, X is the explanans and Y the explanandum: the explanans X causes the explanandum Y, if and only if, had the explanans X been different, the explanandum Y would have been different. The causal claim is true if any changes in Y will occur only through intervening on X, (Woodward 2003, 145). Therefore, the notion of intervention is central in picking out the causal characteristics of the counterfactual conditionals. The notion of intervention allows us to put the explanans and explanandum in the right place of the causal claims: 'had the explanans been different, the explanandum would have been different'.

Now, as mentioned before, Bokulich's own notion of explanation is built up on the Woodwardian's account in that it focuses on a counterfactual aspect. However, Bokulich departs from the ontic tradition: "One can, however, reject the ontic conception

[3] Of course, in order that causality is really there in the world the causal account has to be conceived as non-Humean.

[4] I do not engage with the entire literature around Woodwardian explanation since that is not my main concern. However, note that his notion of explanation wants to capture the workings of 'applied sciences' the aim of which is to "represent nature in a way that permits manipulation and control" (Woodward 2003, 12), by contrast with the practice of 'pure sciences' that aim at representing nature in a truthful way. As such, the view on manipulation, intervention and control, seems to resemble an instrumentalist conception of scientific theories. It seems that Woodward's explanation is far away from the main interest of the (metaphysical) realist, whose main concern is with the interpretation of the theory, as I defended in Chapter 2. Of course, other forms of realism avoid the metaphysical depths and remain on the surface. The latter is where Woodwardian view seems to play a significant role.

of explanation (i.e., deny that explanations are things in the world, independent of human theorizing), but endorse the view that many explanations are indeed causal (i.e., involve citing and representing the relevant subset of causes of the phenomenon)" (Bokulich 2016, 4). Indeed the move to abandon the manipulationist/interventionist-causal emphasis whilst retaining the counterfactual pattern manifested in the w-questions is shared by others, e.g. (Bokulich 2008b, 226), (Bokulich 2008a, 145), (Bokulich 2012, 730), (Saatsi and Pexton 2013), (Saatsi 2016). In particular, Saatsi (2016, 17) recognises that although Woodward's account is explicitly an ontic account (a la Salmon) it can accommodate an instrumentalist view on explanation, whereby a successful explanation does not need to be a question of 'getting the fundamental ontology right'.

Woodwardian explanation is placed within the ontic tradition. However, Bokulich takes the counterfactual framework and puts it to work for a different tradition in explanation. Bokulich attempts to emphasise the role of understanding as a result of explanation, which is the characteristic of the epistemic conception of scientific explanation.

***

With the previous sections I have covered the basic elements that play a role in Bokulich's own account of explanation. I will examine her account showing the tensions with the DN model, Salmon's causal account and Woodwardian counterfactual-casual explanation.

5.2.4 Bokulich's own Model-Structural Explanation

Bokulich's central attempt is to articulate a form of scientific explanation that captures the scientific practice in semiclassical mechanics. In Chapter 4 I showed that classical dynamical structures – i.e. classical trajectories – play an significant role in the explanation of quantum phenomena, such as the Rydberg atom, the helium atom and wavefunction scarring. Whilst QM is the theoretical framework for that phenomena, the explanation is achieved by using classical trajectories, which are dynamical structures from CM. Relevantly, if one accepts that physical theories can be ordered by a scale of fundamentality, CM is less fundamental than QM. This is why the notion of explanation that captures the explanation involved in semiclassical phenomena has to include fictions. The classical dynamical structures are not fictional in virtue of them being false simpliciter. Their fictional character is appreciated by the fact that classical trajectories are imported from a their proper classical domain, into the quantum domain. The classical trajectories are fictional in the quantum domain because they do not follow the laws of QM. For wavefunctions are not like trajectories in classical phase space.[5] Indeed, classical trajectories appear in the account of quantum phenomena by the explicit approximation obtained through the quantum Green function that sums over classical orbits, see Section 4.3.2.

Crucially, the accounts of explanation discussed above cannot accommodate this scientific practice. In the DN model, see in Section 5.2.1, the explanans must be true in order to explain according to the empirical condition of adequacy. However, the explanans in the account of the mesoscopic phenomena described by QM in the semiclassical approximation is the density of states, which is calculated as a sum over classical periodic orbits. Hence, the explanans includes false elements, conflicting with the DN model.

In turn, as discussed in Section 5.2.2, in Salmon's causal account to explain means to identify causes: if A explains B, then A causes B. Indeed, this entails that the explanation exists in the world, (Salmon 1989, 86), (Bokulich 2016). Hence, the explanans explains and is real, or does not explain and is not real. This conflicts with the cases that I am focusing here. For example, let us consider wavefunction scarring. This purely quantum phenomenon is explained by classical trajectories, but the 'scars' are the wavefunction. Hence, the fictional character of the explanans entails that Salmon's account does not capture the explanation.

Therefore, Bokulich requires an account of explanation whereby fictional entities can explain without having to be reified as real, as existing or causally interacting with the explanandum-phenomenon. Rather than evaluating theoretical expectations of what an explanation should be according to philosophy of science, Bokulich's aim is to capture the scientific practice. In her view philosophy of science should provide an account of what the physicists actually are taking to be explanatory, for surely that is worthwhile. The dynamical structures that physicists use to explain quantum phenomena, such as the periodic orbits should be considered more than merely fictions since these classical trajectories capture empirical aspects of the phenomena and they can also be, to some extent, measured (e.g. in the wavefunction scarring).

Now, Woodward's form of explanation is partially suitable for Bokulich's purposes. Although there is no causal connection between explanans and explanandum, and the associated emphasis on intervention/manipulation does not apply straightforwardly. Hence, Bokulich only incorporates Woodward's counterfactual aspect. As mentioned before, the move to divorce the counterfactual aspect from the emphasis on causation is recognised in the literature. Whilst Saatsi and Pexton (2013, 623) agree in separating these two aspects, they also worry that Bokulich might be too liberal in terms of conceiving that mere abstract structure of counterfactual relations can be explanatory.

[5] This discussion is neutral in regards to the interpretation of QM; even if one advocates Bohmian mechanics, the trajectories of quantum particles are not classical.

Furthermore, Schindler (2014, 1749) warns that Bokulich abandons the notion of intervention prematurely. He identifies a tension since notion of intervention is crucial in individuating causal relations, which in turn provide relevant explanatory desiderata – such as explanatory asymmetry, discrimination of non-explanatory correlations, explanatory depth. However, the fictions do not cause and Schindler argues that without the notion of intervention, Bokulich is unable to identify the structures in the models as explanans, and the phenomena as explanandum. Hence, in his view, Bokulich fails to distinguish a causal relation from a mere correlation. Consequently, she will be unable to rule out 'reverse counterfactuals' of the form: 'had the explanandum been different, then the explanans would have been different'. This ought to be ruled out since it implies that the explanandum explains the explanans or that the explanandum causes the explanans. I will engage in detail with this criticism in Section 5.3.3.

Bokulich's form of explanation is not meant to be a universal one, but is conceived in relation to the specific physical cases discussed in Chapter 4: mesoscopic phenomena described my modern semiclassical mechanics. The type of relationship between the explanans and explanandum is not causal, but structural; since the explanandum is a consequence of the explanans through its structure.[6] Broadly put, the explanandum is explained by showing that the explanans (such as the trace formula) delineates the types of explanandums that can be expected, the sort of objects, properties, states or behaviours, (Bokulich 2008a, 149). Bokulich's structural explanation is distinct to the previous forms of explanation discussed above, since it is neither a covering law (the DN model) nor involves a causal relation (Salmon/Woodward).

By contrast with the structural aspect of her account, the type of model explanation has been articulated in detail. Bokulich (2008a,b, 2012) characterises the model explanation by the following three aspects:

1. The explanans makes recourse to a scientific model that includes fictional elements.[7]

2. The model explains the explanandum by showing a pattern of counterfactual dependence between the relevant features of the target system and the structures represented in the model (the elements of the model that can 'reproduce' the relevant features of the explanandum).

[6]The structural aspect of Bokulich's explanation is appropriated from Margaret Morrisons' structural dependencies, (Morrison 1999). The structural aspect of her form of explanation remains in-progress.

[7]Bokulich (2008a, 138) distinguishes explanatory fictions from calculational devices. Both are fictional and non-real, and the model can function as a calculational device. However, the model can be more than a calculational device, by being able to capture certain features of the empirical phenomenon, for example. In short, the model explanation is more than a mere calculational device insofar as it explains.

3. Finally, there is a 'justificatory step' that secures that the model is genuinely explanatory.

In Woodward's explanation there is a causal relation associated with the counterfactual dependence between the explanans and explanandum. That is not the case in Bokulich's model-structural explanation. This clarifies point 2. Indeed, in Section 5.2.5 I will show how Bokulich's view interprets the classical structures to explain quantum phenomena without a causal relation and despite their fictional character.

Now, once an explanation has been identified as a model explanation by satisfying these three elements, it remains to say what kind of model explanation it is. Bokulich considers three types of model explanation in the literature and argues that the one involved in her interstructuralism does not fit with any of them. Hence motivating the development of her own structural-model explanation.[8]

The justificatory step in 3. requires further discussion. Essentially, the function of the justificatory step is to show that the model is a 'good one'. Hence, similarly to Hempel's condition of truth, the justificatory step discriminates those models that merely save the phenomena from those models that are the genuinely explanatory. However, it is not trivial how that is achieved, and Bokulich has offered different accounts of the justificatory step.

The first account of the justificatory step involves recognising two strategies: 'top-down' or 'bottom-up'. The former consists in providing an overarching theory that specifies the domain of the model and establishes that the model captures relevant features of the phenomenon. This is the rarest case and typically scientists will follow the latter. In the bottom-up strategy, the justification occurs through various empirical investigations. This applies in cases where the target system is idealised by the model. The justificatory step consists in de-idealising the model, showing that it is genuinely explanatory, (Bokulich 2008b, 226-227). In the case of semiclassical mechanics and the mesoscopic phenomena, the strategy is top-down and the overarching theory is the trace formula. I will discuss this further in Section 5.2.5.

The second account of the justificatory step was developed as a response to (Belot and Jansson 2010). Belot and Jansson argued that Bokulich's view could not discriminate explanatory fictions from non-explanatory fictions. In particular, they claimed this view would take the cycles in Ptolemaic astronomy as explanatory of celestial phenomena, which is unacceptable. In order to address this challenge Bokulich (2016) set out three components in the justificatory function:

[8]Engaging further with this assessment is beyond my concerns. See (Bokulich 2008a, 148) and (Bokulich 2011).

i) The justificatory step includes an external factor since examining the specific model alone is not sufficient to determine whether it is genuinely explanatory or not. Indeed, Bokulich includes a ‘contextual relevance relation’ that is established by the current scientific community. This relevance relation manages to specify the entities, states and processes that could explain the explanandum. See (Bokulich 2012, 736).

ii) The justificatory step specifies the domain of applicability of the model and shows that the phenomenon in the real world to be explained falls within that domain. In other words, this justifies the model as an adequate representation of the relevant features of the world. See (Bokulich 2012, 736).

iii) The justificatory step includes a ‘translation key’ allowing the scientist to turn statements about the fictional structures of the model into correct conclusions about the explanandum phenomenon. See (Bokulich 2012, 735).

Finally, Bokulich has recently supplemented the justificatory step with the notion of ‘credentialing process’. In short, a ‘credentialed fiction’ is one that the scientific community considers that it provides an adequate representation of a certain target system, and provides physical insight and factive understanding of the phenomenon in question, (Bokulich 2016, 15).

With this I have offered an account of the general framework that Bokulich proposes. In short, the explanation involves is a ‘model’ type of explanation in virtue of the three components enumerated in page 142, and a ‘structural’ type of explanation insofar as the relationship between explanans and explanandum is not causal or nomothetical, but involves the structure of the explanans as an active element to connect with the explanandum. This model-structural explanation was designed to account for scientific practice in describing the phenomena that I discussed in Chapter 4. The account of the model-structural explanation supplements the theoretical framework of physical theories and intertheory relations articulated in Section 4.2 of Chapter 4. Next I will discuss how this scheme applies to the cases developed previously.

5.2.5 Bokulich’s Account of Mesoscopic Phenomena

I discussed the Open Theories view and the Reciprocal Correspondence Principle Methodology in Section 4.2. These are interstructuralism’s underlying views on physical theories and intertheory relations. In Section 5.2.4 I discussed the central elements of the model-structural explanation. Hence, I am now in position to put Bokulich’s view to work. In this section I will focus on the Rydberg atom in a strong uniform magnetic field, which I discussed in Section 4.4.

The traditional DN model requires the explanans to be true; Salmon's causal explanation involves a causal connection between the explanans and explanandum; and Woodward's account emphasises a causal relation between explanans and explanandum based on a pattern of counterfactual dependence. Therefore the phenomena explained by modern semiclassical mechanics cannot be captured by any of these forms of explanation.

In modern semiclassical mechanics the explanans is a model built on classical orbits of a classical system whose quantum analog is the target system. Modern semiclassical mechanics describes problems quantum chaos such as the Rydberg atom in a strong uniform magnetic field, whose hamiltonian is not integrable, which is crucial. For the integrable cases such as the hydrogen atom without the external magnetic field, the Schrödinger equation can be treated with standard methods to resolve ordinary differential equations and the solutions can be calculated. The wavefunction of the system is a linear combination of the solutions, which are a basis of the relevant Hilbert space. This provides the prediction and explanation of the phenomena. However, the solutions to non-integrable quantum problems, such as the atom in a strong magnetic field, cannot be obtained analytically, although they exist.[9] Hence, physicists use semiclassical theory.

Let me summarise the problem of the Rydberg atom from page 117. In Section 4.4 I discussed how Gutzwiller's trace formula is the basis for the explanation of the chaotic regime of the Rydberg atom's spectrum in the non-integrable case. The experimental results in Region (b) of Figure 4.4 show a seemingly random spectrum. The theoretical model cannot be solved analytically since the problem is non-integrable. However, transforming the energy spectrum to the time scale obtains a pattern of peaks. I showed this in Figure 4.5.

The quantity of interest is the quantum density of states ρ. Knowing ρ would obtain a description of the spectrum, but this is impossible to find analytically since the problem is non-integrable. Instead, ρ can be seen as the sum of two parts, see eqs. (4.25) and (4.26) in Section 4.4:

$$\rho(E) = \rho_0(E) + \rho_{\text{osc}}(E). \tag{5.1}$$

$\rho_0(E)$ represents the volume of the phase space and it is not relevant for our purposes. The relevant part is the an oscillatory part, $\rho_{\text{osc}}(E)$. Gutzwiller's periodic orbit theory

[9] WKB and EBK methods are also useful when the integrable case is difficult to solve and the approximation suffices.

obtains the semiclassical approximation for $\rho_{\text{osc}}(E)$:

$$\rho_{\text{osc}}(E) = \sum_{\Gamma \in \{\text{PPO}\}} \sum_{k=1}^{\infty} A_{\Gamma k}(E) \cos\left[\frac{k}{\hbar} S_{\Gamma}(E) - \sigma_{\Gamma k} \frac{\pi}{2}\right]. \tag{5.2}$$

The left hand side of eq. (5.2) is a quantum quantity. Instead, the right hand side of eq. (5.2) is a sum over harmonics that are determined by the properties of the classically chaotic system that is the analog of the quantum system of interest. In particular, the times at which the peaks appear in the quantum spectrum in the time scale can be associated with the orbits in the right hand side, see Figure 4.5. Therefore, the model uses the fictional classical orbits. The parameters that determine the approximation of the density of states only depend on the properties of the classical trajectories. This simply summarises relevant aspects of Section 4.4.

Now, I discuss how the model-structural explanation operates here, by comparing the characterisation of the form of explanation in page 142, in the face of the the explanation of the spectrum of the Rydberg atom in a strong magnetic field discussed in Section 4.4 given by the physicists.

- The first characteristic (see page 142) is satisfied since the explanans appeals to the semiclassical model, whereby the behaviour of the Rydberg electron is explained through classical periodic orbits. These orbits are fictional insofar as the electron does not follow classical trajectories. Instead, those trajectories are determined by the properties of the classical system that is analog to the quantum system at hand, as discussed in Section 4.4.
- The second characteristic requires a pattern of counterfactual dependence between the relevant features of the target system and the structures represented in the model. In the case of the Rydberg atom, the model establishes a relationship between the fictional orbits obtained from the classical analog of the quantum system and the peaks obtained in the time-scale spectrum. Indeed, there is a pattern of counterfactual dependence a la Woodward's w-questions: 'had the classical orbits changed – e.g. any of the classical parameters of the classical orbits in the right hand side of eq. (5.2) – the quantum absorption spectrum would have been different'. Note that this counterfactual dependence does not involve a causal relation since the fictions do not cause and the underlying dynamic is entirely quantum. Moreover, one cannot claim that an intervention on the classical orbits will effect a change in the phenomenon. The system is causally independent of the classical orbits that explain it.

Furthermore, the model provides an adequate representation of the spectrum. This was discussed in Section 4.4.1, illustrated in Figure 4.6. There, the harmonics in the Fourier transform in the time scale can be seen as constructive interference between the outgoing and returning waves passing through a phase where they are fictionally represented by classical trajectories (step (3) till (8) in Figure 4.6), see (Bokulich 2008a, 147).

- Finally, the justificatory step. Consider the two possible strategies discussed in page 142: top-down or bottom-up. In this case, the justificatory step is Gutzwiller's trace formula and the associated periodic orbit theory. This was developed in the book 'Chaos in Classical and Quantum Mechanics', (Gutzwiller 1990). This is achieved by a top-down strategy, since the classical trajectories cannot be conceived as idealisations that can be de-idealised by adding further information into the model (as the bottom-up strategy would do). The trace formula is the overarching theory that specifies the domain of the model and establishes that the model captures relevant features of the phenomenon.

  Or, consider the three components of the justificatory step in page 142. The first and second components are specified in that the classical dynamical structures, i.e. the classical trajectories, are shown to be relevant for the explanation of the semiclassical phenomena at the mesoscopic scale, including the Rydberg atoms, the helium atom and wavefunction scarring. The third component is the translation key, which is the trace formula. The trace formula allows the physicist to translate statements about the fictional periodic orbits – right hand side of eq. (5.2) – into statements about the underlying quantum mechanical structure of the phenomenon in the Rydberg atom, represented by the left hand side in eq. (5.2).

  Finally, consider the credentialing process. As discussed in Chapter 4, physicists do consider the trace formula and the associated periodic orbit theory as robust theoretical devices that provide genuine physical insight into, and understanding of, a vast range of phenomena and applications.[10]

Why does Bokulich need to eschew the causal connection and the intervention from Woodward's account? This seems clear now. The physicists know that the orbits are not real nor cause the spectrum. However, the classical orbits are more than mere calculational devices for them, since the semiclassical reasoning does provide physical insight. For example, Kleppner and Delos (2001, 606) reassure us that semiclassical physicists (they) do not contradict QM, and do not claim that the electron follows classical trajectories. This demonstrates that "truth or existence is not a necessary

[10]For example, see the impressive number of developments and applications summarised in (Delos 2016a).

condition for an item to be admitted to the scientist's explanatory store" (Bokulich 2012, 734). However, not all fictional entities can be stored there too, which is why Bokulich develops her framework. She provides a criterion to consider as explanatory only those fictions that adequately represent relevant features of the phenomenon. In a way, they are selected by "the relevant scientific community and will depend on the details of the particular science, the nature of the target system, and the purposes for which the scientists are deploying the model" (Bokulich 2012, 734).

Similar analysis can be made for the case of wavefunction scarring, see (Bokulich 2008a, 147-148).

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I have discussed how Bokulich provides a philosophical account of the explanation of semiclassical phenomena. Next, I will critically engage with this form of explanation. Firstly, I will discus and engage with a recent criticism made to Bokulich's view, namely (Schindler 2014). Secondly, I will assess this form of explanation from the point of view of scientific realism. Thirdly, I will explore the prospects of incorporating a causal element in the explanation by recovering an interventional aspect drawing on Woodward's counterfactual explanation. Fourthly, I will provide a novel interpretation to the merits of the semiclassical mechanics motivating the content of next chapter.

5.3 Scientific Realism and Interstructuralism

In this part of the chapter I will critically engage with the form of explanation defended within interstructuralism. However, I will first recall two core characteristics of the model-structural explanation: the fictional nature of the explanans and its indispensable character. The first aspect has been mentioned sufficiently in previous sections.

The indispensable character of the fictional explanation is argued by Bokulich in several places. Physicists base their models on such a hypothesis. Despite the fact that the phenomena described by semiclassical mechanics are quantum, Bokulich argues that the fictional orbits are indispensable in the sense that they "provide a deeper understanding of the physical phenomena than the purely quantum-mechanical explanations do" (Bokulich 2008a, 137). Furthermore, without using the fictional orbits the explanation would be opaque, (Bokulich 2012, 735). What does Bokulich mean by 'deeper understanding'? This requires some discussion.

There is a long standing debate in the literature about 'understanding' and this is not my main concern. Understanding appears because the central idea in the epistemic conception on explanation is to provide understanding, whereby 'understanding' means that the explanandum is in some way expected, (Saatsi 2016, 9). Despite the subjective and psychological resonance of the term, Bokulich argues that there is an objective concept of understanding based on the depth of the explanation of the phenomenon of interest. 'Explanatory depth' is a technical term that was coined by Hitchcock and Woodward (2003) within a specific view on explanation. This is complex issue and I do not intend to critically engage with this concept. However, I mention the main idea. Explanatory depth relates to the range of w-questions that a certain explanation can provide, it intends to be a measure of the amount of information that an explanans provides about the system of interest. Roughly speaking, if there are two explanations for the same phenomena – something that Bokulich's and Woodward and Hitchcock's views contemplate, although this is a contestable assessment – , the one that provides answers to a greater range of w-questions is 'deeper'. The point where Bokulich's and Hitchcock and Woodward's depart is that whilst Hitchcock and Woodward develop examples wherein the more fundamental theory provides deeper explanations, Bokulich points at the opposite situation. Specifically, Bokulich (2008a, 152) argues that the explanation of semiclassical phenomena based on dynamical structures from CM, a less fundamental theory than QM, provides a deeper explanation than the 'purely' quantum one.

Indeed, the explanation given above on the anomalous spectra of the Rydberg atom is in that sense deeper than the purely quantum explanation. A purely quantum model would be more complicated due to the non-integrable character of the problem, although for energies below zero the electron is still bounded to the nucleus and there are bases of the Hilbert space to expand the solutions. However, these bases would involve a huge number of elements and become ever larger as we approach ionisation threshold.[11] By contrast, the semiclassical models based on the trace formula offer knowledge of relevant structural features of the spectrum, "[facilitating] correct inferences and factive understanding of the phenomenon" (Bokulich 2016, 12), thereby being more than merely calculational devices. In other words: "Classical structures, such as closed and periodic orbits, provide a level of understanding of these phenomena that the purely quantum-mechanical explanations do not" (Bokulich 2008a, 154). Analogously, in the case of the wavefunction scarring the explanation through classical orbits is deeper than the explanation based on numerical calculations, since the latter provides no understanding of the underlying phenomena. Now, despite the possible philosophical contention in the notion of understanding and the falsities in the explanans, the semiclassical models are a great resource to the physicists, they are more than a calculational device. This is how

[11] I am grateful to Professor Delos for clarifying this to me, (Delos 2016b).

I think that Bokulich considers the classical trajectories that underpin the semiclassical models to be indispensable.

In Section 5.3.1 I will offer an interpretation of the role of the classical orbits in the explanation of quantum phenomena. In Section 5.3.2 I will interpret the fictional nature of the orbits challenging the fictional status that Bokulich assigns to them. In Section 5.3.3 I will engage with the criticism raised by Schindler by attempting a response in defence of Bokulich and offering my own assessment. I will defend a form of causal dependence in the explanation in Section 5.3.4. Finally, I will offer my interpretation of the interstructuralist programme in Section 5.3.5, motivating further chapters.

5.3.1 On the Explanatory *Role* of Fictions

In this section I do not challenge that the fictional dynamical structures are indispensable to explain the mesoscopic phenomena, but I note that this presents a tension with realism. Bokulich (2008a, 124) acknowledges this tension. However, so far she has focused on spelling out the fictions' explanatory role.[12] I will offer an interpretation of their explanatory role that does not require reifying them as real. Of course, as I have mentioned before, neither the physicist – e.g. (Kleppner and Delos 2001) – nor the philosopher – cf. (Bokulich 2008a, 124) –, claim that the orbits are real in the quantum context. The explanationist such as Psillos will infer ontological commitment to whatever is playing an indispensable role in the explanation. Given that the fictions explain, how to avoid reifying them as real in light of standard realism?

Therefore, if we accept that fictional dynamical entities play an indispensable explanatory role and if we take upon a standard form of realism, then a tension appears. In order to resolve this tension one requires an argument that enables the classical dynamical structures to be essential in the explanation and yet be fictional. For this, I will re-interpret the role of the fictions in the explanation accommodating their un-real character. I present an analogy between the status of these fictional dynamical structures and the debate around the ontological status of mathematics derived from their role in scientific explanation.[13] The analogy is that neither the fictional dynamical entities nor maths are really desired guests in the ontological meeting, as it were. The benefit from stressing this analogy is that the debate over the status of maths has been explored more deeply in the literature than that of the status of the fictional orbits in semiclassical phenomena.

[12] Recently Bokulich (2016) articulates further how fictions can represent, although not in relation with a specific form of realism.

[13]There is a vast literature on this topic and I do not intend to engage with it nor have an informed opinion of such a debate, see (Colyvan 2001). Here I am merely picking out some elements of that discussion for my purposes, i.e. to engage with the role of fictions in the interstructuralist explanation.

The indispensability argument exposes the tension with respect to mathematics. Glossing over various nuances, the argument says that if mathematics are indispensable in the explanation, the explanationist realist will have to take a realist commitment to mathematical entities, see (Colyvan 2001; Saatsi 2011). In fact, Psillos (2016) – a central advocate of explanationism – will (reluctantly) believe in the existence of mathematical entities based on this argument. However, I would think that the explanationist would like to be able to reject the reality of the (knowingly fictional) orbits in the quantum phenomena, unless she is willing to challenge the view of the entire physics community. Hence, regardless of the outcome in the debate over the ontological status of mathematics we ought to have an argument to secure the fictional status of the classical orbits in the quantum domain.

Saatsi (2016) has recently proposed to distinguish two different types of explanatory roles. Such a distinction is meant to accommodate an indispensable role of mathematics without having to infer ontological commitments from them. Saatsi advocates that not every active explanatory role involves an ontological commitment. In order to defend that view, Saatsi distinguishes between 'thin' and 'thick' explanatory roles.

Once an explanatory feature is recognised as indispensable, one should determine whether they play a thin or a thick explanatory role. The distinction discriminates those explanatory features that pick out real elements from the explanandum from those which do not. Features that play a thin explanatory role are one step away from the explanandum whereas the features playing a thick explanatory role carry a legitimate realist commitment:

> 'Thick explanatory role' is played by a fact that bears an ontic relation of explanatory relevance to the explanandum in question.
>
> 'Thin explanatory role' is played by something that allows us to grasp, or (re)present, whatever plays a 'thick' explanatory role.
>
> (Saatsi 2016, 12)

If Saatsi's account is plausible, then the realist can include idealised models and other kinds of abstracta – such as fictional dynamical structures – as indispensable to scientifically explain, without having to consider that they exist. In order to ontologically commit to the existence of the fictional orbits one would require to assess their explanatory role as thick. However, as mentioned earlier, the tension is not how to accommodate the fictions within the real, but rather how to be an explanationist realist whilst assigning a fictional status to the classical orbits despite their indispensable explanatory role.

Now, Saatsi's argument is designed to engage with the ontic conception of explanation, essentially that of Salmon as I discussed in Section 5.2.2. In the ontic tradition explanatory power is relative to or dependent on stating explanatorily relevant worldly facts. Saatsi argues that the indispensability argument cannot be argued simpliciter, but it requires that a specific conception of explanation is adopted. Furthermore, he claims that the most suitable conception to host such an argument is the ontic conception.

In Section 5.2.2 I noted that Bokulich (2016) characterises Salmon's ontic conception of explanation in that the explanation is real. Bokulich proposes another type of explanation. Bokulich's model-structural explanation belongs in the epistemic conception, whereby the central claim is that explanation serves the purpose of understanding, (Saatsi 2016, 9); and Bokulich emphasises that the fundamental explanatory relevance of the fictional orbits is that they provide physical insight and understanding of the relevant phenomena, see the beginning of Section 5.3. In the epistemic conception, to provide understanding amounts to show that the explanandum is expected. Therefore, the explanatory power does not crucially depend on establishing a relationship between the explanans and the explanandum in the world.

Thus, Saatsi's arguments aim at resolving the tension between the indispensability argument and explanationism by offering a criterion to divorce the explanatory role from ontological commitments. My contention is that his criterion can be used to resolve the tension in the explanation of the semiclassical phenomena and standard realism. The interstructuralist could use Saatsi's argument in order to articulate her form of realism. Alternatively, the explanationist realist could appropriate the form of explanation provided by Bokulich with an ontic twist: she will be pressed to resolve the ontological status of the fictions. As said before, the fictitious status of the classical orbits in the quantum phenomena ought to clarified.

Hence, by appropriating Saatsi's thin/thick distinction I will argue that the explanatory role played by fictional dynamical structures in the Rydberg atom is a 'thin explanatory' role. Consequently, the classical trajectories are representational *relative to* the averaging effect of the laser in the spectrum, but they do not represent any real feature of the underlying quantum system. Therefore, this releases some tension for the explanationist realist.

The explanation given by the physicists based on classical dynamical structures does not replace the dynamic of the electron by a fictionally conceived classical trajectory. Gutzwiller's trace formula directly presents a different problem. The question is not to obtain the eigenfunctions of the Rydberg electron, since the problem is not-integrable. Moreover, the very nature of the experiment indicates that the measurement of the $n-$mixing effect in the chaotic regime does not obtain individual levels, see Region (b) in Figure 4.4. Essentially, each 'line' in the spectrum involves several final states of the electron. What is found is an average absorption through the effect of the laser. The harmonics of the Fourier transform of the density of states are correlated with classical trajectories: the time at which there is a peak is the return-time of the closed orbit, (Du and Delos 1988a, 1899).

Moreover, "in this method it is *not* claimed that any relationship exists between individual orbits and individual quantum states" (Du and Delos 1988a, 1901). Therefore, the physicists do know that the underlying phenomenon is precisely the same opaque (using Bokulich's term) quantum mechanical atomic transition as in any hydrogen-like atom. In this way, the fictional trajectories allow the physicist to grasp a specific phenomenon that is not the result of the 'pure' quantum system. In that sense the fictions are one step away from the underlying 'pure' quantum phenomenon. In my assessment, the phenomenon that the classically closed orbits explain is the phenomenon of the averaging of the quantum spectrum due to the band width of the laser. However, the underlying quantum system has no special relationship with classical trajectories.

Therefore, using Saatsi's terminology the fictional orbits cannot ever (and are not intended to) play a thick explanatory role if the phenomenon in question is the underlying Rydberg atom in a uniform and strong magnetic field. When the atom is excited by the laser characterised by a ΔE, this averages the spectrum of the atom and the empirical result is like the results shown in Figure 4.5. The association between the spectrum and the classical trajectories via the trace formula establishes that the return-time of the orbits are associated with the harmonics of the Fourier transform of the density of states from energy to time, $\rho(E) \to \mathcal{F}(\rho)(t)$. The finite-resolution hypothesis – due to the characteristic period given by the laser: $T_{\max} = 2\pi\hbar/\Delta E$ – provides a limit to the amount of orbits that can be observed. That is, orbits with periods longer than T produce oscillations with wavelength on the energy axis of $E = 2\pi\hbar/T$. If the $\Delta E = 2\pi\hbar/T_{\max}$ of the laser (the energy width) is larger than E (that is, if that orbit's period T is larger than $T_{\max}$), then oscillation will not be visible.[14]

Therefore, the quantum state is still accountable for the ontological content of the Rydberg atom and this is neither directly obtained in the experimental result in Figures 4.4 and 4.5, nor in the semiclassical model described in Section 4.4 of Chapter 4. The semiclassical phenomenon that the classical trajectories explain is the averaging effect of the spectrum of the atom alone as a result of the energy width of the laser.

I have argued that the fictional orbits play a thin explanatory role and the explanationist can appropriate Bokulich's form of explanation without having to reify the classical

[14] I am grateful to Professor Delos for clarifying this to me, (Delos 2016b).

orbits. This is because the underlying quantum phenomenon is entirely quantum, described by the wavefunction and explained as the hydrogen atom is explained in standard QM.[15]

Next I will explore in further detail the fictional status of the classical orbits, for I think that their fictional character should be narrowed down and this extends Bokulich's view on the matter.

5.3.2 On the *Fictional* Character of the Classical Orbits

In this section I offer an interpretation of the fictional status of the classical orbits in the semiclassical phenomena. The focus is on the phenomenon of the Rydberg atom in a uniform magnetic field, explained by classical orbits that begin and end at the nucleus, thereby closed orbits. Some of this section may overlap with previous sections. However, the emphasis is relevantly different. Previously I engaged with explanatory role of the fictions and I argued that despite their indispensable role, the classical orbits should not be taken as real since that role is a thin one (in the sense of (Saatsi 2016)). Now my focus is on the fictional character of the orbits: why are they fictions? Or, what is their fictitious status relative to? What are physicists really saying that these trajectories represent? In Section 5.3.5 I will revisit the interpretation of the fictional orbits.

For the sake of the discussion, let us agree with a typical argument according to which the classical gravitational force serves the purpose of explaining a wide range of phenomena – such as the motion of the planets, our unbearable feeling of heaviness and inability to leave off the ground – but in reality gravity is false. Instead, it is the 4-dimensional time-space manifold that folds and turns according to the masses of the bodies, following the laws of the general theory of relativity.[16] The gravitational force is a fiction that can explain without 'getting the ontology right'. I do not intend to assess these statements, but I question whether the fictional orbits that explain the phenomena discussed in Chapter 4 are fictions in that same way: are they fictions relative to an underlying 'better approximation to the truth of the world'? I will defend a negative answer. In the semiclassical phenomena classical orbits are pure fictions divorced from any 'underlying truth'.

By now it should be clear that it is not questionable whether there is a *real* relationship between the quantum states of the Rydberg electron and the classical orbits that explain the atomic spectrum. For example the Region (b) of the measurements made by Garton

[15]In subsequent Chapters I will return to engage with this: how 'purely' quantum is a phenomenon that is conceived of quantising a classical hamiltonian? Do we have pure quantum phenomena at all?

[16]The reader may find this example debatable. However I merely use it to elicit a common intuition.

and Tomkins in 1969 (shown in Figure 4.4 and discussed in Section 4.4). The classical orbits are used to explain such a spectrum, they provide physical insight and indeed there is an extremely fruitful branch of semiclassical physics involved. See a comprehensive review of the many applications of modern semiclassical mechanics and the periodic orbit theory of the density of states in Delos' webpage, (Delos 2016a). "The construction of quantum wave functions from classical trajectories provides an intuitive picture and a depth of insight that cannot be obtained in other ways" (Delos 2016a). Furthermore, this branch of physics has relevant practical applications, for example in medical physics.[17]

Bokulich defends that the fictional models can represent entities, states or processes and give us genuine insight into the way the world is, (Bokulich 2011, 44). Then, precisely what do fictions represent in the case of the Rydberg atom? I think that Bokulich has not sufficiently clarified the role of the fictional orbits. I defend that they do not represent the behaviour or states of the Rydberg atom simpliciter. Instead, I defend that the classical orbits represent a phenomenon obtained as the result of the laser that averages the very dense distribution of states. I have argued throughout the chapter that the hypothesis (and experimental condition) of the finite-resolution is crucial.

If that is plausible, then the Region (b) of the spectrum in Figure 4.4 does not correspond the way the quantum system is alone. The group of physicists led by Kleppner claim that by "the 'classical trajectory of an electron,' we mean ... the path the electron would follow if it obeyed the laws of classical mechanics. In quantum mechanics an electron is not a localized object moving along a path" (Haggerty et al. 1998, 1592). Indeed, I think that in the theoretical model the tunable laser is doing a significant *intervention* in the actual spectrum, setting the maximum period as $2\pi\hbar/\Delta E$, where ΔE is determined by the properties of the laser which excites the atom to its Rydberg states.[18] Then, the phenomenon at hand does not exhibit the 'pure' quantum nature of the system and it is not true that the electron is being fictionally imagined as traversing trajectories. The 'pure' quantum nature of the system is filtered through the finite-resolution of the laser. Therefore, the model-structural explanation is not explaining the phenomenon of the Rydberg electron. Instead, the classical periodic orbits explain the n-mixing effect, which is not a 'pure' quantum one. The chaotic behaviour arises in the effect of the laser on the quantum system. As the physicists say, the "closed orbit theory relates fluctuations in the atomic photoabsorption spectrum to the system's classical closed orbits (orbits that begin and end at the nucleus)" (Haggerty et al. 1998, 1592).

In conclusion, the classical trajectories are not intended to represent the state of the Rydberg electron. The measurements in the semiclassical phenomenon discussed in

[17] Of course, engaging in discussing such details is far from my purposes here. Exploring a philosophical view over such applications is a task for future works.

[18] This resembles Woodwardian explanation. I will come back to this in Section 5.3.4.

Section 4.4 do not measure individual spectral lines, but many are included due to the finite-resolution method. The fictional status of the orbits is relative to a phenomenon other than the underlying purely quantum phenomenon.

As mentioned earlier, the finite-resolution hypothesis brings to mind the notion of intervention in Woodward's manner. I explore this in Section 5.3.4.

5.3.3 Schindler's Criticism

Schindler (2014) identifies an internal tension in Bokulich's model-structural explanation. He claims that Bokulich ought to provide a case where the counterfactuals captured by the fictional model are not captured by the theory. However, the justificatory step requirement precludes that possibility. Therefore, he concludes that either the model is a mere calculational device that does not explain, or it explains but without justification. The tension is between the claim of the explanatory *autonomy* of the fictions, and the requirement that the model-fictions be *justified* (through the justificatory step characteristic in the model explanation) in order that the fictions are genuinely explanatory.

Schindler (2014, 1746) claims that insofar as the explanatory role of the fictions is justified by modern semiclassical mechanics the fictions cannot be explanatorily autonomous. More specifically, he defends that the claim (his C1): 'had relevant structures *in the model* been different, then relevant features in the explanandum phenomenon would have been different', and the condition that the translation key in the justificatory step connects statements about the model fiction with statements about the underlying structures of the explanandum phenomenon, entail the counterfactual C2, which is problematic. He concludes that (his C2): "had the relevant quantum mechanical features ... of the relevant quantum systems ... been different, then the relevant structures in the model fiction (such as the shape of the electron orbit) would have been different" (Schindler 2014, 1747). C2 would entail a counterfactual dependence between features of the explanandum phenomenon and the features of QM. Consequently, features of QM would be explaining the quantum phenomenon. However, this is something that Bokulich wants to deny. Consequently, she would require a further argument to maintain the the model explains and not QM.

Schindler assessess that both claims – the autonomy of the model fiction and their justification as explanatory – cannot be held in conjunction unless Bokulich provides an argument to the effect of establishing the autonomy of model fictions whilst ensuring their justification.

I will formulate what I think Bokulich could reply and I will attack Schindler's arguments from a neutral viewpoint.

What Could Bokulich Specifically Reply to the Argument?

Firstly, I think Bokulich could observe that Schindler's argument overlooks an important detail. Schindler says that Bokulich's fictional models are meant to be explanatory and also autonomous, meaning that "they sometimes provide insight into phenomena where true theories don't" (Schindler 2014, 1742). This allows him to demand an additional argument for the fact that QM is not doing the real explanatory work but the fictional model is. This is also the key to argue that Schindler's argument does not deliver.

Bokulich's account does not deny that there is a counterfactual dependence between QM and the fictions in the model. This means that Schindler's C2 in (Schindler 2014, 1747) is trivially true. However, this does not present a threat, because Bokulich does not affirm that QM does not provide physical insight into the phenomenon, nor does she claim that QM does not explain the phenomenon. Indeed, Bokulich (2008b) takes Bohr's atom model as explanatory and this does not mean that modern QM is wrong, nor that QM is not capable of providing an explanation.

There is a difference between the fictional account of the spectrum of the hydrogen atom by Bohr's model, and the cases of quantum chaos. In the former, there is an account of the phenomenon via the fictional explanation provided by Bohr's model and also an explanation modern QM. By contrast, in the latter case the phenomena such as wavefunction scarring are only explained via the fictional models, and there is no explanation in modern QM. However, that is a contingent fact and not a flaw in QM (as I think Schindler is trying to claim Bokulich holds) nor a flaw in Bokulich's view. The fact that there is no quantum explanation for the phenomena of quantum chaos does not challenge Bokulich's view. The lack of that explanation is that the problems of quantum chaos are non-integrable. Hence, there is no analytic solution for the Schrödinger equation. However, a quantum explanation would not represent a problem: Bokulich can accommodate several explanations for the same phenomenon. For example, both the Bohr model and the correct QM account explain, they just respond different w-questions.

Schindler seems to have omitted that Bokulich is an explanatory pluralist. Hence, his argument lacks the traction he intends it to have. For example, Bokulich considers that whilst Bohr's model of the hydrogen atom is explanatory and essentially involves fictions, it is not the only possible explanation nor the best. "Bohr's model does genuinely explain the Balmer series, though the explanation it offers may not be as deep as that offered by modern quantum mechanics, and moreover, the explanation offered by modern (nonrelativistic) quantum mechanics may not be as deep as that offered by quantum field theory" (Bokulich 2008b, 44). In fact, that a more fundamental theory does not necessarily provide a deeper explanation follows from her general view on physical theories and intertheory relations. Sometimes the less fundamental theory provides a wider range of w-questions.

Therefore, Bokulich could reply that Schindler's alleged tension is weak and not sufficiently worrisome. I will present a response to Schindler's objections without assuming a defence of interstructuralism.

What Could I Contribute to this Debate?

Firstly, I think that Schindler's presentation of the physical problems is inaccurate. More specifically, his C2 quoted in page 155 seems to claim that the fiction in the model is the orbit of the electron. I claim that this is incorrect. In Chapter 4 I put it that the quantum density of states is approximated by the trace formula through a calculation over periodic orbits. However, it is neither the case that the electron is traversing a fictional trajectory (it is not really traversing any trajectory), nor does the semiclassical model take that to be the case. Instead, the quantum quantity is being estimated through calculations made with the classical analog: a classical particle subjected to the classical action $S(q)$, see the trace formula in Section 4.3.2.

However, the idea that I think he is trying to convey seems plausible. Had the quantum mechanical features of the quantum system been different, then the model fiction would have been different. For example, in wavefunction scarring, had the shape of the stadium been different, the wavefunction would have scarred over different periodic orbits. Whilst these are true I deny that there is a tension. Given that this is a quantum phenomenon, it is expectable that features of the explanandum depend on features of QM. Bokulich would not deny the existence of a quantum explanation, if there was one, as I defended above. The issue with non-integrable systems is that one cannot resolve the Schrödinger equation. That is why the trace formula and the associated fictional orbits provide insight 'where QM does not'. The justification for the explanatory role of the fictions is the semiclassical theory. Gutzwiller's trace formula is the link between the classical orbits (fictional, in the quantum domain) and the quantum density of states, despite Schindler (2014, 1746) resistance. Had the trace formula not been discovered, the physicists would have been forced to use a numerical approach to account for the wavefunction scarring – which is actually the method to simulate the phenomenon. And, although an approximation to the wavefunction could have been the basis for an explanation, it would have lacked the element of understanding that the semiclassical model provides, (Bokulich 2008b, 230). Indeed, the physical insight of semiclassical mechanics is underpinned by the appearance of the classically periodic orbits, a feature that the numerical approach cannot incorporate.

Secondly, I think that Schindler unfairly neglects the crucial role of Bokulich's conception of physical theories and intertheory relations. He seems to overlook how Bokulich conceives these issues. In Chapter 4 I recalled the thesis of structural continuity, which allows dynamical structures of one theory to play epistemic roles in phenomena arising within another theory. That enables classical dynamical structures to explain and stimulate development of QM. Although there is a counterfactual dependence between the quantum phenomenon and QM, it is the model featuring classical dynamical structures – fictional in that domain – that is explaining it.

Hence, Schindler's arguments do not show that there is a tension in Bokulich's model-structural explanation and its application to the semiclassical phenomena.

5.3.4 Woodwardians Return

Schindler worries that Bokulich may have abandoned the notion of intervention in the explanation prematurely, (Schindler 2014). Indeed, I defend that a causal aspect a la Woodward could be recovered.

Physicists explicitly acknowledge that the closed orbits in the semiclassical phenomena relate to the 'fluctuation' in the photoabsorption of the Rydberg atom influenced by the external magnetic field. Thus, I offer a pattern of counterfactual dependence between the phenomenon of the chaotic regime of the spectrum in the likes of the one obtained by Garton and Tomkins in 1969, Region (b) in Figure 4.4 and the laser that excites the atom to its Rydberg states. Hence, I defend that the orbits appearing in the spectrum explain a phenomenon in Woodward's causal manner: such orbits have a direct relation to the type of intervention entailed by the laser that affects the system in a specific way. This type of explanation displays that the relationship between the classical orbits (part of the explanans, part of the model) and the absorption spectrum of the atom in that experiment (the explanandum), holds as a matter of empirical fact and exhibits the type of relationship of manipulation that Woodward (2003, 6) describes.

In Section 5.2.3 I discussed the general features of Woodward's form of explanation. I recalled that the explanation allows us to differentiate effects on the explanandum if relevant features in the explanans had been different. A feature X in the explanans is said to cause the explanandum Y if the following counterfactual condition is true: 'had X been different, Y would have been different'. Plus, the non-anthropocentric intervention plays a central role in evaluating the truth-values of counterfactual conditionals. I focus on the role of the laser in the phenomenon by assessing its 'semantic function', see Schindler (2014).

The semantic function of intervention allows us to distinguish between a mere correlation and a causal relation. This is illustrated by an example, e.g. (Schindler 2014, 1748):

> Correlation: Had the value of the barometer reading been changed below a certain value, a storm would have occurred.
>
> Causation: Had the atmospheric pressure been changed below a certain value, a storm would have occurred.

In the correlation case, the active counterfactual condition is false since a change in the barometer does not affect the weather; but changes in the atmospheric pressure do. I identify a causal relation involved the spectrum of the Rydberg atom influenced by the laser. Near the ionisation energy the actual spectrum of the atom is very dense. In terms of the classical orbits, the classical closed orbits have a return-time to the nucleus that coincides with the position of the peaks of the spectrum in the time scale. The laser has an intervening function. The energy width ΔE is a crucial feature of the laser. The peaks that appear in the Fourier transform of the density of states would change depending on ΔE, since the laser averages over the density of states. The characteristic period $T_{\max} = 2\pi\hbar/\Delta E$ determines which orbits can be seen: orbits with return-time longer than T will not be visible. Of course, other properties such as the spacing of the peaks in the time scale and their intensity are features of the system alone, independent of ΔE. This mimics Woodward's account:

C1: Had the features of the laser changed, the obtained spectrum would have been different.

In fact, C1 can be dissected (I have discussed the validity of the conditions C1' and C2' in previous Sections):

C1' The energy width of the laser ΔE has an averaging effect on the spectrum of the system.

C2' Orbits of period $T > T_{\max} = 2\pi\hbar/\Delta E$ are not visible in the spectrum.

$\therefore$ The averaging process by the laser ΔE has an effect on the observed spectrum.

The counterfactual dependence of C1 can be evaluated by changing features of the laser and observing changes in the spectrum. Indeed, this is what the experiments show: if the resolution is improved, more orbits will appear since the averaging effect is fine-grained. A worse resolution will impose a coarser-grained average showing fewer orbits.

Therefore, the laser is having a causal relation with the spectrum obtained and the semiclassical model explains that phenomenon. Had the ΔE been different, the spectrum would have been different, for different closed orbits would contribute to the spectrum in the specific sense that trace formula indicates.

Bokulich argues that her form of explanation presents a non-causal pattern of counterfactual dependence, with the outcome that “one can say precisely how the quantum absorption spectrum would have been different if the classical closed orbits had been changed” (Bokulich 2008a, 147). I challenge that account by highlighting the intervention of the laser on the spectrum. My analysis seems to mirror Woodward’s framework, see Section 5.2.3.

5.3.5 The Heuristic Approach to Interstructuralism

Let us recall the background discussion. Physicists studying semiclassical phenomena observe the surprising appearance of the classical trajectories in quantum experiments. Despite the fictitious status of the trajectories, they can be ‘measured’ and provide physical insight, allowing physicists to make novel predictions. Indeed, the theory of classical orbits is central to understanding these systems.

Adopting a realist point of view, how should one view the classical orbits that are indispensable for the explanation and fictional in a quantum domain? Previously I articulated the role of the orbits in relation to the averaging effect of the spectrum of the atom. In Section 5.3.4 I put such a relationship as a the causal intervention in Woodward’s way. Then, the classical orbits explain the observed spectrum, which is modified by the intervention determined by properties of the laser that excites the atom. Furthermore, Bokulich (2012, 735) and the physicists recognise that the semiclassical approach provides “physical insight ... into what is otherwise often opaque quantum dynamics”. In this section I will consider the classical orbits in terms of what Spencer Hey (2016) calls a ‘simplifying heuristic’.

Hey (2016, 483) considers the following problem: there is a long, gas-filled tube subjected to a short, violent pressure applied to one end. The question is to explain the behaviour of the gas in response to the change of pressure. The explanation is effectively achieved with thermodynamics assuming that the system (the gas in the tube) is a continuous fluid with little viscosity, shrinking the complicated shock-region down into a two-dimensional boundary. However, the gas is not really a continuous fluid but a complex system of molecules described by statistical QM.

Although the hypothesis of continuity does not capture the ontology of the gas, it is helps to explain. Indeed, the ontology of the system does not matter here: one could think that the gas is idealised by classical massive point-like particles elastically colliding with each other and the walls of the tube, described by CM. Or one could describe it with quantum mechanical statistics, taking its atomic structure (a mono-atomic gas, or else.). Both cases involve unnecessary details for the phenomenon to be explained. Furthermore, this holds through much of fluid mechanics. Physicists are content with the explanation given by the continuum hypothesis. Although the continuum hypothesis is fictional, it is good enough to explain and it provides physical insight. This resembles semiclassical phenomena.

Hey develops the notion of simplifying heuristic. In his view, the continuum limit is a 'minimal descriptive shorthand' that is adequate for the purposes of explaining the phenomenon only:

> A 'minimal descriptive shorthand' is the hallmark of a simplifying heuristic. Applying the continuum limit heuristic to treat the shock as a two-dimensional boundary allows us to ignore irrelevant details about the system. We do not care about the initial configuration of molecules in each possible simulation of the event. What we care about is the way the shock moves and the way it effects the two regions on either side. ... The full, complicated story of how the individual molecules behave in the tube fails to adequately explain the phenomenon precisely because those details do not ultimately make a difference to the feature of the system we care about: the behaviour of the shock event.
>
> (Hey 2016, 485)

Whilst the minimal descriptive shorthand helps to explain the observed phenomenon of those shock waves, the real 'stuff' is the quantum gas. Similarly, in the observed phenomenon of the chaotic spectrum, the real system is the quantum Rydberg electron interacting with the Coulomb potential of the atom, the external magnetic field and being excited by the laser. One could argue that here the classical trajectories are a *simplifying heuristics*, just like the continuous fluid hypothesis. Indeed, if my arguments above are right, the experiment does not even measure the quantum state of the atom and the hypothesis of the trajectories manages to explain the experimental outcomes.

I could rephrase Hey's quote in the following:

> the classical periodic orbit is a simplifying heuristic. Applying the classical periodic orbit theory and the trace formula to the absorption spectrum of Rydberg atoms averaged by the laser allows us to ignore the individual eigenstates of the system, which are irrelevant as they are not experimentally observed: instead, the n-mixing effect is observed due to the effect of the laser. We do not care about the the actual eigenstate of the Rydberg electron, but about the pattern of the absorption spectrum and the way it changes with changes with the external magnetic field, intervened by the laser. The full complicated story of how each individual eigenstate contributes to the spectrum does not explain the phenomenon precisely because those details do not make a difference to the feature of the system we care about. In the n-mixing region different energy levels contribute to the spectrum, the combined spectrum of many eigenstates.

This framework provides the fictional explanation of the semiclassical phenomena with an entrance ticket to the realist's explanation store without any associated ontological commitment. The phenomenon does not exhibit features of the underlying system only; relatedly, its explanation does not need to focus on the "true model". Hence, Bokulich's model explanation is useful. No ontological commitments shall be inferred from the obtained data; the fictional orbits can be evaluated by their heuristic value. Hey (2016, 487) justifies his meta-heuristic approach resolving possible objections by the realist: fundamentally, thermodynamics is false implying that it cannot be explanatory, and/or that the fundamental physical theory must be explanatorily sufficient. Bokulich (2008a, 153) argues that the fictionally conceived closed orbits do explain and deeper, despite their fictional character. The quantum explanation does not provide physical insight into the phenomenon the Rydberg atom. In the same manner, I can justify – and this could also respond to (Schindler 2014) – that the fictional dynamical structures of the closed trajectories unproblematically explain in the way that Hey's simplifying heuristics do.

By applying the strategy of the simplifying heuristic developed by Hey, I have offered an interpretation of the explanation of semiclassical phenomena instrumentally, without inferring realist commitments, despite the model's empirical success and explanatory depth.

5.4 Conclusions

Semiclassical phenomena present two challenges for the view on intertheory relations:

1. Although semiclassical mechanics deals with quantum phenomena, it has stimulated further development of CM, such of chaos theory, see (Kleppner and Delos 2001; Delos et al. 2008; Delos 2016a).

2. Classical dynamical structures such as classical orbits (periodic or closed) have an increasingly 'unexpected unexpected' relevance in the explanation, calculational and predictive power, physical insight into and understanding of quantum phenomena, see (Delos 2016a).[19]

The surprising phenomena explained by modern semiclassical mechanics – the trace formula, the work by (Delos 2016a), by Kleppner's experimental group, etc.– unquestionably presents the influence of CM in the quantum scale.

Traditional views on intertheory relations, such as theory reduction, discussed in Chapter 3, do not have the philosophical tools or mindset to accommodate the novel relationship exhibited in the semiclassical phenomena and its account. Hence, Bokulich's interstructuralism is a substantial contribution to articulating a novel intertheory relationship. Interstructuralism captures the dual aspect of modern semiclassical mechanics mentioned above through the thesis of structural continuity that allows the interchange of dynamical structures from one theory to the other (discusses in Chapter 4), and is supplemented by the model-structural form of explanation (analysed in this chapter).

In this chapter I have critically engaged with interstructuralism. I have explored interpreting the explanatory role of the fictions and their fictional nature. I have defended that the classical trajectories represent a subsidiary phenomenon (the averaging effect on the spectrum by the laser) relative to an underlying quantum phenomenon of absorption (the quantum transition from a higher energy level to a lower energy level). In doing so I have managed to respond to criticisms raised against interstructuralism, such as Schindler's. Plus, I have offered a causal counterfactual pattern a la Woodward. Furthermore, I have shown that Hey's simplifying heuristics can help the realist to accept that quantum phenomena is explained by classical dynamical structures without requiring realist commitments. This is my preferred way to conceive the explanation of the semiclassical phenomena.

In Chapter 2 I defended that there is a Received View in regards to QM. Hence, is interstructuralism captured by the Received View? The Received View includes the

[19]See Section 3.3 for discussion of the metaphor 'unexpected unexpected'.

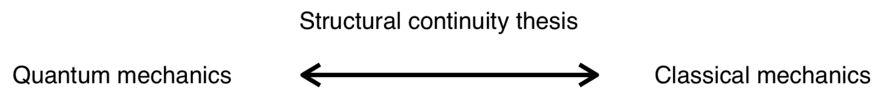

FIGURE 5.1: In Figure 4.1 I showed an illustration of the Reciprocal Correspondence Principle Methodology exhibiting Dirac's Open Theories view, which underpins interstructuralism. Such a view presents a tension with the epistemic commitment of the Received View discussed in Chapter 3. However, interstructuralism conserves the metaphysical commitment from the Received View. This illustration proposes to consider that there is no hierarchy inter-theories and thus separates farther away from the Received View.

hypotheses that QM has to give an account of the appearance of CM because it is more fundamental (epistemic intertheory relation), and that macroscopic bodies are metaphysically related to quantum objects (ontological claim).

From this analysis, Bokulich's view breaks with the epistemic aspect of the Received View, since interstructuralism proposes a novel view underpinned by Dirac's Open Theories view. The Open Theories view includes the thesis of structural continuity (see Chapter 4), which conflicts with the epistemic thesis of the Received View. However, in regards to the metaphysical aspect interstructuralism maintains that there is ontological reduction between quantum and classical properties, (Bokulich 2008a, 4). Hence, the metaphysical claim in the Received View captures interstructuralism. I claim that a novel realist framework to interpret QM could be conceived by departing from the Received View. Hence, there is one more claim to challenge.

In Figure 4.1 I illustrated the framework of physical theories advocated by Dirac that Bokulich appropriates. One could modify that framework abandoning the notion that QM is more fundamental than CM. In Figure 5.1 I illustrate a view including the thesis of structural continuity. This includes the technical tool of the simplifying heuristic, which allows to capture the practice of physicists without inferring ontological commitments, as discussed in this chapter.

I propose that Heisenberg's view can help here. I will outline such a view and leave further analysis for the two subsequent chapters. In a nutshell, Heiseberg's view, the Closed Theories view, is a form of pluralism.

- A closed theory covers a limited domain of phenomena.
- A closed theory provides the final description of phenomena.

- The Closed Theories view entails an anti-gradualist model of theory change.
- It holds that there is no hierarchy at the ontological level in terms of fundamentality.

This view does not contradict the Core Realism that I discussed in Chapter 2; it can be accepted in the realist tradition; and, relevantly, it opposes the Received View of the realist interpretation of QM. I will examine such a view in the next two chapters.

Chapter 6

Beyond the Received View of QM (I): Realist Strategies

6.1 Introduction

In Chapter 2 I outlined a stripped-down version of scientific realism called Core Realism. Core Realism says that the world is independent of us; that our theories capture true features of the world; and that the realist is mainly concerned with an interpretation of the theories: 'how is the world according to the theory?' 'What is the theory telling us the world is like?'. My main target is the realist interpretation of QM. Hence, the application of this debate in this manner to other problems in the philosophical literature or other disciplines is not straightforward and that discussion shall be left for future works.

However, the presentation of Core Realism in Chapter 2 is not enough to be considered as a form of realism. Indeed, it looks more like a stance or the basis of an attitude, and it has to be supplemented with further content. What should be added to the core to become a plausible realist view that could be used to interpret QM? In my view there are three elements that most forms of realism have and that my proposal should engage with: the realist content or realist commitment, a global prescription and a view on intertheory relations.

Firstly, the proposal should include some account of what it is realist about. This typically involves looking at the particular science at hand. In the case of physics and QM, one could just consult the physicists and take whatever they consider the elements of the theory to be, and then be realist about that. However, things are more complex: clearly QM is about electrons, protons, atoms, ions and other quantum system, but what exactly are they as real objects? One should develop a critical interpretation of the physics and the physicists' opinions, and provide an informed view of the underlying goings on. And this – for some, unfortunately – might require the realist to engage with metaphysical questions of a greater and lesser degree of sophistication.

Secondly, and not independently of the first element, forms of realism typically include a global prescription as to the type of entities that are considered. Both epistemically or metaphysically minded forms of realism include some notion of the following type: some realists put forward the view that physics tells us about the world made of objects (think of Psillos' object oriented realism), or dispositions, or structures, among others.

Thirdly, forms of realism typically hold some view on intertheory relations, on the way that science progresses: by accumulating truths, by approximating better to the truth, but latching onto the world, etc.

In regards to the first two elements, in Chapter 2 I proposed the Received View as a characterisation of the main realist interpretations of QM and I noted that there is no agreement over which interpretation (if any) really 'gets it right'. Hence, I consider it a sufficiently motivated working hypothesis that the realist interpretation of QM is a question open to debate. That entails that the first two elements mentioned above are still unsettled discussions, because the outlook depicted in Chapter 2 included that there is neither agreement on the interpretation of QM nor on the metaphysical account of the quantum objects. Additionally, there is disagreement within realism on what the problem is. As Lewis (2016, 25) puts it: "[QM] is a theory in which we have no idea what we are talking about, because we have no idea what (if anything) the basic mathematical structures of the theory represent".

The third element has received more attention in this investigation. Consider Chapter 3, where I discussed the received account of the intertheory relation QM-CM. I argued there that Nickles reduction$_2$ is considered a traditional philosophical account[1] and presented, in physical theoretical terms, such as through mathematical limits, Ehrenfest theorem, Moyal brackets and decoherence. However, I pointed out significant deficiencies in both aspects, concluding that modern conceptions in terms of the measurement problem and preferred basis problem are controversial and not smoothly addressed by decoherence – seen in the literature as one of the best attempts at such problems. This suggested the necessity of a novel intertheory relationship to account for the case QM-CM.

A major attempt to improve our account of the intertheory relation QM-CM is put forward by Bokulich (2008a), and the discussion above can partly explain her motivations. Additionally, an explicit motivation is the practice of physicists. Because none of

[1] Where I also argued for significant similarities and differences with Post's General Correspondence Principle not sufficiently recognised in the philosophical literature.

the traditional views on the relationship QM-CM – i.e. theory reduction in philosophy; decoherence in physics – can account for the increasingly appreciated modern field of semiclassical mechanics. Consequently, I critically engaged with interstructuralism in Chapters 4 and 5. In those chapters I argued that interstructuralism can be seen as an attempt to depart from the Received View of the Realist Interpretation of QM, although of course Bokulich does not put it in my terms. I also concluded that interstructuralism has strong merits and has paved the way towards a novel understanding of QM and its problems, and yet it leaves some issues untouched, such as the form of realism underpinning it and the realist interpretation of QM. That appreciation has led me to call for further reflection on realism and QM. This chapter and Chapter 7 are about such issues.

Hence, considering the above motivations, in this chapter I engage further with scientific realism. Again, the aim is to articulate a view on the broad and vital problem of the realist interpretation of QM. The debate on scientific realism covers a vast, exciting and productive literature, authors and scholarship. In order to engage with a debate around realism and QM in a manageable way I will draw on two recent relevant distinctions: recipe vs. exemplar realism, put forward by Saatsi (2015) and discussed subsequently by French (2016), and deep vs. shallow realism, articulated by Magnus (2012). Considering these distinctions will help me to argue for the supplementation of the Core Realism. I will argue that Heisenberg's Closed Theories view mentioned at the end of Chapter 5 is a possible candidate for a significant contribution to this debate. Whilst in this chapter I will motivate it and discuss its basic elements, Chapter 7 will continue to engage with this realist proposal by contrasting it with other relevant and similar views in the philosophical literature.

With that in mind, I move on next to specify further aspects of Core Realism by discussing Saatsi's distinction between recipe and exemplar realism in Section 6.2 and extending its applicability to other realist questions such as intertheory relations and metaphysics. Then, in Section 6.3 I will discuss Magnus' distinction between deep and shallow realisms. Including the recommendations concluded from these discussions, in Section 6.4 I will present in more detail the Closed Theories View initiated by Heisenberg.

6.2 Strategies in Realism: Exemplar vs. Recipe

In order to organise the discussion, recall the distinction I made in Chapter 2, which divorced two issues. The first issue relates to the debate for and against scientific realism (whether we should believe the content of the theories in toto, or partially, or not at all and instead consider the content of theories as merely projections of our thoughts, and so on). This is also part of the debate of how realism can be justified against its antitheses, such as idealism or scepticism, see Papineau (1996). The second issue is the debate over the scope and meaning of realism assuming that one is a realist. That is, the question of how realism accounts for science and in particular physical theories. In order to narrow down my analysis, I do not engage with the former issue, and I focus on the second. My investigation concerns with what scientific realism should be, what its outcomes should be when approaching a physical theory, and QM in particular. Hence, I assume a realist view from the outset and questioning my view by alleging that I should address possible objections which the anti-realist, idealist, sceptic, and others, may raise, would deny precisely what I assume.

Saatsi (2015) distinguishes between what he calls recipe realism and exemplar realism. I will use Saatsi's distinction and a recent commentary by French in order to specify further my approach to QM derived from Core Realism. To do so I will discuss Saatsi's proposal, then French's commentary, followed by my appropriation of their contributions.

Saatsi characterises recipe realism as a realism that aims to provide a *uniform* account of the way our current theories latch onto the world, which aims to pin down a unified realist sense across the board, across the range of disciplines and areas of theorising. The recipe realist finds motivation in the consideration of a small number of particular case studies wherein the empirical success of science is explained and then alleges to project that recipe or algorithm onto the rest of science. Recipe realism would be successful if it were "capable of distilling the trustworthy aspects of a theory, applicable to any good, predictively successful mature theory" (Saatsi 2015, 2).

It is tempting to attempt to map currently well-known forms of realism onto this characterisation, such as structural realism, entity realism, dispositional semi-realism. It is not my purpose to discuss them in detail, and without going into nuanced debates on each one can sympathise with the idea that each presents a recipe: given a mature theory which is empirically successful, structuralism will claim to obtain the right structure; an entity realist will argue that the success is explained by to getting the right entities; the dispositionalist will insist on the right bundles of dispositions; and so on. What Saatsi identifies that they all have in common is that they follow a recipe. It is in virtue of one specific and unifying sort of entity that theories are latching onto the world.

However, by contrast with debates that argue for the merits of one recipe against another, Saatsi points to the difficulties of arguing for the plausibility of *any* recipe in principle. He notes the evident inhomogeneity in the key aspects of scientific theorising and concludes that it is not viable to search for a unified recipe that would capture the way in which theories' empirical success is correlated with the way they latch onto reality. In addition, Saatsi (2015, 4-5) denies the plausibility of any recipe motivated by the appreciation of the plural variety of explanations which, despite not getting the ontology right (the structure, entities, dispositions, and so on), can be empirically successful nevertheless. An obvious example of that is interstructuralism, which successfully explains phenomena like the Rydberg atoms in strong magnetic fields with fictional orbits, as I discussed in detail in Chapters 4 and 5. The orbits are not real, the electron does not follow classical trajectories and yet, without getting the ontology right such model succeeds to tick many relevant epistemic boxes.

Yet, Saatsi argues that the obstacles for recipe realism understood this way do not entail the end of realism altogether. Realism, he declares, does not *have* to be recipe realism. Indeed, realism can be conceived in a local manner instead of as a search for a general recipe that captures all exemplars. And hence he proposes a more specific, case-dependent form of realism. In fact, he can recognise the merits of each recipe's successful cases, but denies that we can export any one recipe to understand all other theories and disciplines.

In its positive proposal, he recalls the phrasing of the general realist view by Chakravarty, who talks of realism as a positive epistemic attitude with belief in both observable and unobservable aspects of the world. Saatsi has a cautious attitude and as discussed above, claims that no recipe delineates the realist to have commitments across the board, and thus such a 'blanket' recommendation is not required. That objection or observation motivates replacing the belief in observables and unobservables by a weaker positive attitude to 'getting something right about the unobservable world.' Hence, the realist can construe different ways to cash out the success of theories by capturing something right about the world that depends on particular, localised, examples. This is what exemplar realism puts forward.

Hence, exemplar realism is quite different to the strategy of recipe realism. Exemplar realism can be illustrated by the following: what allows the realist to say something epistemically about the various ways in which scientific theories latch onto reality? "In a domain of science like <u>this</u>, with theories or models like <u>that</u>, empirical success in <u>this</u> sense, is accountable in <u>those</u> terms" (even if theories are to some extent mistaken) (Saatsi 2015, 8). The more exemplars the realist explores, the more refined her epistemic commitments. Now, how does she replace the underlined demonstratives? By consulting – and critically assessing the opinion of – the relevant expert scientists.

Now, exemplar realism is not totally local. That is, there is a global aspect that mirrors that of recipe realism but lifted one step up: the global character of exemplar realism is in the conception of realism itself. That is, there is a global desideratum in conceiving realism as an attitude, a forward looking approach to science. An attitude based on the no-miracles argument, in the sense of the intuition that science's empirical success has to do with a "latching onto reality," with getting something right about the world. And yet, this does not entail commitment to a specific recipe. Such a global attitude does not really prescribe what there is in the world nor does it say there should be one type of abstracta – tropes, entities, primitive ontology, or structure and "turtles all the way down," as it were.

Now, so far I have reviewed Saatsi's distinction between recipe and exemplar approaches to realism. By recommending caution as to the basic commitments in realism and identifying that, essentially, to follow a recipe is not included in the basis of realism, Saatsi favours the latter.

Relevantly, Saatsi's exemplar view resonates with my Core Realism. I claim that, the discussion I gave in Chapter 2 can be translated in the language of Saatsi's discussion. Thus, the argument is that my Core Realism does not entail a recipe for what one should be realist about nor does it provide a global prescription on the type of entities that are considered real. Core Realism, I claim, can be seen as an expression of the motivations put forward by exemplar realism.

In Chapter 2 I specifically argued in relation to the realist interpretation of QM that Core Realism does not have a predefined recipe for intertheory relations. I will tackle this issue next. In order to contribute to developing further the distinction exemplar vs. recipe – which is a work-in-progress – I will raise two questions that are interesting for my purposes of supplementing Core Realism as mentioned in Section 6.1.

6.2.1 Exemplar/Recipe and Intertheory Relations

In Section 6.1 I anticipated the elements with which my Core Realism from Chapter 2 should be supplemented. Intertheory relations appeared in the third element and I discussed the particular case of the pair QM-CM in detail in Chapter 3. Here I explore the hypothesis that Saatsi's distinction can be usefully applied to how we, as realists, understand intertheory relations with the dual aim of (i) contributing to expand the domain of application of the distinction itself and (ii) specifying further my approach.

In regards to (i), how could Saatsi's distinction be used to engage with the topic of intertheory relations? I will attempt this by questioning whether the realist has a *recipe* to cash out intertheory relations or whether she looks at different *examples* with an open mind without expecting to extrapolate it. Here I will revisit forms of intertheory relation discussed in Chapter 3. In turn, as to (ii), can I supplement Core Realism by drawing on the exemplar/recipe distinction? The strategy will be to return to discuss the supplementation of Core Realism by opposition with the Received View. This is relevant as the Received View was characterised in terms of intertheory relations.

(i) Let us attempt to distinguish two options for the realist's account of intertheory relations. Firstly, the realist could follow a recipe to account for the relationship between different theories, based on the detailed account of a few examples, in a form of inductive reasoning – e.g. the structural realist will look for structural relations between theories. Secondly, the realist could be open to look at different examples and conceive of intertheory relations that accommodate each relevant example, in a local manner. Recipe realism advocates the former option, whilst the latter expresses an exemplar methodology. It should be expected that an advocate of exemplar realism would also adhere to an exemplar view on intertheory relations, of course. This does not exactly mean that all pairs of theories will be related by different relations, but that each case should be conceived locally instead of by applying a recipe.

Chapter 3 engaged with traditional views on theory reduction, such as Nagelian reduction, Nickles' reduction$_2$ and Post's General Correspondence Principle. Now, after Section 6.2 I argue that these views could be analysed through Saatsi's distinction. Indeed, a first impression is that they should be understood as recipes: Nagel established that the theorems in the predecessor will be proven by the premises of the successor, thereby obtaining a deduction relationship between them; Nickles took it that there are non-competing mathematical relationships, typically mathematical limits, which by connecting the novel successor with the predecessor provide a reduction relationship and a heuristic to the further development and justification of the former; Post constructed a heuristic guide, which as Kamminga et al. (1993, xix) put it, a 'recipe for constructing new theories' based on a series of steps mainly considering that the successor theory must retain the successful parts of the predecessor and that the flaws of the latter are footprints for the development of the former. These proposals do seem to be recipes: general intertheory relations inferred from a number of alleged successful applications. However, once this is recognised, we can either take Saatsi's exemplar/recipe distinction and arguments in favour of the former in order to undermine those intertheory relationship developments and discard them all, or we could otherwise read them in an exemplar key and decide to retain their limited, local successes. Going along the latter route involves a more moderate, engaging and positive attitude, and I think that this is preferable.

Let us see Post's case. One could recommend the whole heuristic approach should be discarded because of the known difficulties that it has to capture the relationship QM-CM.[2] That is, once Post's approach is recognised as a recipe, the realist expecting a recipe will have to discard this view, given that it is does not always succeed. The

[2]See page 50 where I discuss how Post (1971, 233) acknowledges such difficulties but considered them to be a shortcoming in QM. We know that he was wrong in this respect although the general merits of his view remain.

alternative – the only way to save a faulty recipe – is to consider Post's heuristic proposal as an exemplar view and read his approach as a combination of various locally conceived heuristic moves. Indeed, his view was built upon the careful study of specific cases. And one could recognise that the initial criticism that it received has an undermining effect *only* within a recipe strategy and, for instance, take Post's view as an account of the relationship between special relativity and Newtonian mechanics – *pace* (Radder 1991)'s criticism. For instance see the articles dedicated to discussing plausible applications of Post's heuristics in the volume Post et al. (1993).

Hence, I argue that one could read Post's view in an exemplar fashion and claim that he merely presents a sort of tool-box of heuristic resources from which the philosopher can pick the most appropriate tool for the case at hand. And if none of the tools do the job then that might indicate the need to forge a novel tool or it might indicate a more general challenge to philosophy rather than a drawback in Post's view. For, as I discussed in detail in Chapter 3, as much as one could criticise Post's view for not being able to capture the QM-CM limit, neither can the other views!

Similarly, one could analyse the other 'recipes' for intertheory relations that I analysed in Chapter 3, such as Nagelian reduction and Nickles' reduction$_2$. I leave this for future works as my focus now is to move on to (ii). In this respect, there are two further fruitful associations to make between recipe/exemplar distinction and previously analysed issues in this thesis: interstructuralism, and the Received View of the Realist Interpretation of QM and Core Realism.

Let us begin by revisiting interstructuralism from the point of view of the Saatsi's distinction between exemplar/recipe applied to intertheory relations. This connection has not been made before, according to my knowledge. Hence, is interstructuralism following a recipe or does it work in an exemplar way? Bokulich (2008a, 273) emphasises that whilst she suspects that her interstructuralist view could be extended to capture other pairs, such work should be carried out by looking at each example – work which has not been done yet. In addition, she predicts that it would be surprising, if not unexpected, that the same account of intertheory relations applied to all pairs of scientific theories. Hence, an interstructuralist account of the relation QM-CM is a case of an exemplar approach to intertheory relations.[3]

Hence, one could describe interstructuralism by appealing to the exemplar approach to intertheory relation. This would take the following form: interstructuralism looks

[3]I engaged with interstructuralism in Chapters 4 and 5. I argued that although this view presents significant merits in accounting for the relationship between QM and CM, it also leaves relevant questions about realist commitments untouched. Hence, it is not straightforwardly accommodated in the exemplar strategy that Saatsi develops, because Saatsi's articulation relates to realist commitments and not with intertheory relations.

at the pair QM and CM. In particular, it focuses on semiclassical mechanics, which includes mixing quantum and classical features in a way that cannot be accounted for by the traditional 'recipes' by Nagel, Nickles, and Post. By contrast with those views, interstructuralism does not attempt to force the science into a previously conceived recipe, but articulates a view that accommodates whatever is really going on in the practice of physics.

Now let us do the same analysis in order to specify Core Realism. In Chapter 2 I proposed considering a Received View of the Realist Interpretation of QM, and I characterised it as conceiving of the realist interpretation of QM subject to a priorly conceived intertheory relation. For example, that QM has to provide an explanation of CM, that QM reduces to CM, that CM emerges from QM – at the epistemic level –, and that – at the metaphysical level – quantum objects somehow give rise to classical objects, classical properties are a obtained from underlying quantum properties. Then, it can be seen that the Received View – which, I argued, captures the underlying philosophical framework of much of the well-known realist interpretations of QM – *is* a recipe.

Therefore, my argument is that Saatsi's traction in favour of an exemplar realist attitude supports my claims against the recipe entailed by the Received View. Looking at my discussion Received View vs. Core Realism in the context of the exemplar/recipe distinction, I argue that my alternative to the Received View appears as an open minded, global realist attitude towards physics. That is, Core Realism from Chapter 2 can be seen as an the seed for an exemplar form of realism that looks at QM. If my notion of the Received View is sound, and it can be seen as following a recipe, then there is a significant gap in the philosophical market to approach QM, and my Core Realism can be used to patch up that gap.

In relation to QM, the – exemplar – global attitude of Core Realism was expressed in the Table 2.1 where I put it that the Core Realist's concern is to provide a physical interpretation of QM, regardless of preconceived recipes or expectations of how QM should relate to CM; the question of what Ψ represents in the world does not have to be conceived in relation to classical macroscopic objects. I think this nicely represents the global attitude that Saatsi alludes of being an exemplar realist, applied to the case of QM.

Thus, my extension of the initial distinction has recognised that sympathy with exemplar realism entails that a global prescription or recipe should be rejected even as to what direction novel theories should take from the predecessors (even if heuristically, or historically, they were motivated or shaped by predecessors). Following this discussion, the exemplar realist will have no fixed sense in which a successor should latch better onto the *unobservable* reality than the predecessor. One understanding is that science improves – that is, it gets better at capturing the real – by going deeper into the world through studying smaller bodies, higher energies, radiation from farther zones of the universe, and so on. However, this already sounds like a recipe. The realist ought not expect science to advance in a specific direction, for that would be dogmatic. Instead, the global realist attitude applied to local epistemic areas, such as physical theories, should be as open minded as possible.[4]

Therefore, I showed that the recipe/exemplar distinction can be further articulated by approaching questions of intertheory relations and that this helps me to articulate a novel realist approach to QM. This was done by discussing the Received View and characterising it as a recipe. Then, by using Saatsi's arguments in favour of exemplar realism in my debate against the Received View. If the realist should be exemplar a la Saatsi, it should also be exemplar in terms of intertheory relations, and the advocate of the Received View cannot accommodate this. Instead, a view based on my Core Realism can.

As I discussed in Section 6.1, a form of realism that attempts to concentrate in QM also involves some metaphysical content. Saatsi's distinction will help to debate this too.

6.2.2 Exemplar/Recipe and Metaphysics

Let us continue exploring the exemplar/recipe distinction by noting that realism typically involves a global prescription as to what the realist is committed to and how metaphysical that is, as mentioned in Section 6.1.

Looking at Saatsi's distinction discussed above there seems to be no specifications as to the amount of metaphysics that the realist should include in her realism. Indeed, French (2016) questions this too. He argues that even if realism ought to be understood – as Saatsi argues – in the exemplar way, some amount of metaphysics is still necessary: "If our realism is going to be exemplar based then there is even greater need to be clear on what it is we are going to be realist about" French (2016). That is, French argues that metaphysics is still an urgent matter for the realist, even the exemplar one. The realist can otherwise dismiss the need for metaphysics, but that will "leave us with only the thinnest of understandings (indeed, one that is cast in largely negative terms)," one that does not keep a clear distance from the instrumentalist account of the empirical success of science, French (2016).

[4] The question of what is theoretical progress is an issue in itself and I do not intend to debate it, I limit my analysis here to intertheory relations.

French argues for the need for a metaphysical component partly by challenging Saatsi's assessment that structural realism should be understood as a recipe side. And he dedicates much of the article to undermining Saatsi's view on structural realism, but I am not going to engage in this debate. My interest is in the question of whether the exemplar/recipe distinction involves considerations of 'how much' metaphysics should be included.

I can engage with French's argument without entering into a debate on structural realism because I think that the expectation that realist should include some healthy dose of metaphysics (and the discussion of the dose's size) is not particular to French's preferred version of structural realism (the radical ontic-eliminativist one, see French (2014)), nor even particular to more moderate forms of ontic structural realism (defended by the likes of Ladyman and Ross). As French argues, metaphysics is still an urgent matter for the realist who follows either recipe style or even the exemplar one. And here is where the question in regards to realism and its attitude towards metaphysics enters.

Hence, as noted by French, Saatsi's exemplar/recipe distinction and his adherence – as a methodological strategy – to the former does not prescribe on the metaphysical content in realism. French's preferred option is to consider metaphysics as a toolbox, that is, to divorce our realism from any specific recipe-conceived metaphysical view (e.g. monism vs. pluralism or fundamentality vs. gunk, etc.), and instead take the formal content of the theory as conditions to decide which metaphysical concept best fits the theoretical content given by the structure of the theories. That is, broadly taken, what French and McKenzie (2012) refer to when they conceive of the metaphysical toolbox. Now, I do not engage with French's assessments nor with his metaphysical account in terms of structure; I articulate his commentary of the options that the realist has in terms of metaphysical content. And the options that he conceives draw on a distinction made by Magnus (2012). Magnus claims that there is the "shallow" realist who eschews metaphysical content in her realism and, what French prefers, a "deep" realism. I discuss them in the next Section.

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To summarise, I have noted that Saatsi's assessment stimulates the realist to take an exemplar strategy. However, I have noticed two ways to engage further with the exemplar/recipe distinction. Firstly, that forms of intertheory relations can also be distinguished in terms of recipe or the exemplar strategies. I argued that the intertheory relations discussed in Chapter 3 take the recipe form, and that interstructuralism is an example of an exemplar form of intertheory relation. Plus that the methodology suggested in my Core Realism in Chapter 2 also expresses such an exemplar based realist attitude towards QM. This motivates considering Core Realism. Independently, I noted that Saatsi's distinction leaves underdetermined the amount of metaphysics that should be included and, drawing on French (2016), I argued for the inclusion of a healthy dose of metaphysics. Yet, following the exemplar strategy does not tell us what a healthy dose of metaphysics is. Two options are, following Magnus, to go shallow or deep with respect to metaphysics. The next Section discusses this. Having this at hand will allow me to supplement Core Realism, as discussed in Section 6.1.

6.3 Metaphysics in Realism: Deep vs. Shallow

In Chapter 2 I presented a broad form of realism called Core Realism and a more specific methodology characterised as the Received View of the Realist Interpretation of QM. With the content of this chapter I have so far offered Core Realism as a plausible version of exemplar realism – following Saatsi (2016). Additionally, Core Realism is compatible with an exemplar attitude to intertheory relations, although the Received View follows a recipe. Saatsi's arguments support my motivation for Core Realism. But – following French (2016) – there is the underdetermination in terms of the metaphysical content, and the options are two: a shallow or a deep realism. This distinction will help specify further a form of realism, in the manner discussed in Section 6.1.

In a nutshell, shallow realism is an epistemic form of realism, whereas deep realism is a more metaphysical realism. In principle, Core Realism can be used both ways. This will be discussed in Chapter 7 where I will compare my proposal based on Heisenberg's Closed Theories, with other similar views.

The best way to illustrate the deep/shallow distinction is by discussing the metaphysical content of Magnus' realism. Magnus advances a shallow realism based on natural kinds, built upon Boyd's realism, which is relative to accounting for the inductive and explanatory success of science. Its shallow character is emphasised in the observation that the concept of "natural kind" is itself a natural kind. This articulates the epistemic reliability of science's inductive and explanatory practices, (Magnus 2012, 105). Yet, "natural kind" is not a natural kind simpliciter, because it is not a *metaphysical* natural kind. Magnus (2012, 106) presents a conception of natural kinds that "requires both that a kind underwrite explanatory and inductive success (the success clause) but also that it be indispensable for doing so (the restriction clause)".

Let me illustrate Magnus' realism with an example. Let us discuss the sense in which oxygen is a natural kind, given that it has been known to us since a specific time in history and not before, despite science asserting its existence even before its discovery.

Of course, argues Magnus, there was oxygen before Lavoisier's time. It is the *kind oxygen* that did not exist before Lavoisier's work. That is, **natural kind** is, essentially, a term. Hence, there was no term 'oxygen' until the work of Lavoisier, although there was oxygen in nature. By coining the term oxygen, Lavoisier established the relationship between natural kind term 'oxygen' and **oxygen** as a natural kind in nature (the boldface is to distinguish the word and the world relation). The difference is between the kind that is mentioned in our science, 'oxygen', which was coined at a specific time, and the natural kind that describes what there is in the world, despite our discursive activity.

Now, Magnus' view on natural kinds does not engage with *deep* metaphysics. In his conception, natural kinds are meant to be features of the world although there is "no story to tell about the deep metaphysical substance of them" (Magnus 2012, 122). For example, in relation to electrons, he agrees with Hacking in his famous claim that despite the lack of an exhaustive or perfect description them, electrons are real because we can spray them, (Magnus 2012, 123). Hence, Magnus remains agnostic as to the fundamental nature of natural kinds, and this is not a pressing question so long as he remains a shallow realist.

Hence, I think Magnus' is an example view of shallow realism. Shallow realism can be summarised as an epistemological realism that considers that there is no pressing need to describe the nature of the things that science talks about. It remains agnostic and not concerned with describing in the nature of the entities of the empirically successful theories.

Here I can draw a connection with the well-known article by Woodward and Bogen where they state what really matters, what is and what is not to be considered problematic, in their view:

> It should be clear that we think of particular phenomena as in the world, as belonging to the natural order itself and not just to the way we talk about or conceptualize that order. Beyond this, however, we are inclined to be ontologically non-committal. Phenomena seem to fall into many different traditional ontological categories –they include particular objects, objects with features, events, processes, and states. Perhaps some phenomena are best thought of as having a structure more like that traditionally ascribed to facts or states of affairs. ... We have not attempted to characterize a single ontological category to which all phenomena belong, both because we do not know how to provide an illuminating classification of this sort, and because doing so is not essential for the purposes we pursue in this paper. For our purposes, what matters most about phenomena is the distinctive role they

> play in connection with explanation and prediction, the general features they possess which suit them to this role, and the way in which they contrast in these respects with data. For our purposes, anything which can play this role and which has these general features can qualify as a phenomenon, and this is why (like the scientists whose activity we claim to be describing) we are inclined to be somewhat casual about matters of ontological classification.
>
> (Bogen and Woodward 1988, 321-322)

This quote seems to be fully compatible with the attitude that Magnus puts forward in his shallow conception of realism.

On the other hand, Magnus (2012, 119) identifies *deep* realism as one which "[describes] the world as it is, and there are many options; the realists' world might be a collection of entities, substances instantiating universals, a structure of properties, a cloud of tropes, or some other deep metaphysical ontogoria".

It seems plausible to use the distinction exemplar/recipe and the discussion in Section 6.2.2 in this context. It would seem that Magnus is following a recipe to conceive his shallow realism: all there is are shallow natural kinds. Another option, following French (2016), who draws on Saatsi's distinction, is to be a deep and exemplar realist. French's defence of ontic-structural realism illustrates a form of realism concerned with the deep metaphysical content of the theories and obtained from looking at each particular theory without making inferences beyond their reach.

Hence, it seems that a deep realism, that is, one which describes the world as it is in terms of a metaphysical concept (structure, tropes, entities, etc.), does not have to be understood as committing to only one of those categories, as Magnus characterises it. A (deep) metaphysical realism does not have to be a recipe realism and one can take a exemplar based form of realism which includes a deep metaphysical content. Indeed, I take French (2016) to defend that structural realism can be a token of such a type.

Therefore French (2016) might be suggesting a matrix of plausible realist options: one selection is deep or shallow (metaphysics) and another recipe or exemplar (strategy). I illustrate these options in Table 6.1.

Before moving forward, there is a specific example I want to draw attention to, which can both represent a possible application of Magnus' scheme – outside the range of examples he himself articulates – and an association between these discussions on realism and problems encountered in previous Chapters 4 and 5. That is, the challenging status of fictional classical trajectories that explain quantum chaotic phenomena.

		Metaphysics	
		Deep	Shallow
Strategy	Recipe		
	Exemplar		

Table 6.1: Matrix of realist view in terms of deep vs. shallow (metaphysics), and recipe vs. exemplar (strategy).

Let us momentarily return to Chapter 5. Interstructuralism intends to be a realist view in which fictional – knowingly false, non-real – dynamical entities do play a relevant role in the explanation, despite their metaphysical status. In that Chapter I noted the tension that arises when attempting to accommodate this view within a standard form of realism. The tension was that realism typically intends to commit to those elements of the explanation that play an indispensable role. My assessment is that if the interstructuralist wants to specify her realism, she can assume a shallow realism and the associated prescription at a metaphysical level will mitigate such a tension in regards to the fictional explanation.

Within shallow realism, anything with a minimal empirical content can be accepted in the shallow-conceived ontology. The shallow realist can include fictional orbits in her ontology because such orbits explain. In the semiclassical phenomena that I discussed in Chapter 4, classical orbits play a significant role in the explanation. There is a relevant counterfactual account of them due to the averaging effect of the laser which excites the atom and determines which orbits will contribute, as I argued in Chapter 5. The interstructuralist can conceive of the electron as a natural kind without having to specify its fundamental metaphysical structure. This is just the shallow realist view. Here, the electron has any properties that the model assigns and, perhaps, the fallible belief is that the electron cannot undertake a classical trajectory. Because the specific semiclassical model developed by Du and Delos based on Gutzwiller's trace formula explain the experimental results obtained by the Bielefeld group. Indeed, arguments to prevent the electron travelling in trajectories would establish a deeper metaphysical commitment than the shallow realist is ready to consider, hence the fruitful prospect of shallow realism for the interstructuralist.

Therefore, I claim that – according to shallow realism – a pragmatic naturalism can conceive of the electrons traversing classical periodic orbits as non-fictional thus avoiding the "deep" worries that other non-shallow realists would have. Such a shallow conception of realism seems to present a valid route for the sort of approach that interstructuralism puts forward.

However, if the interstructuralist rejects the shallow proposal and undertakes the deep path in realism, the tension on the status of explanatory fictions will tighten. And this will press the interstructuralist to consider my interpretation. By the end of Chapter 5 I considered that one should regard the classical trajectories as not representing the electron in any way. Because actually the trajectories appear as a result of the intervention due to the laser that averages the spectrum of the atom. I considered the trajectories as simplifying heuristics, in the sense developed by Hey (2016).

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To summarise, in previous sections I argued that Core Realism follows the exemplar strategy in realist terms and in terms of intertheory relations. In terms of metaphysical content, I think that Core Realism can be used both by the shallow or deep metaphysical realist. In the next section I will supplement Core Realism by appropriating Heisenberg's Closed Theories view, which I call Core Realism+Closed Theories. More specific discussion will be had in Chapter 7 when I will contrast my Core Realism+Closed Theories with other relevant forms of realism, such as forms of perspectivism and Cartwright's pluralism.

6.4 Supplementing Core Realism: Heisenberg's Closed Theories

In Section 6.2 I set out the distinction made by Saatsi between exemplar and recipe strategies to realism. I established that my Core Realism follows the exemplar one and I began to supplement it by discussing its take on intertheory relations and metaphysics. In Section 6.2.1 I extended Saatsi's distinction to consider intertheory relations. I argued that if exemplar realism is to be favoured *a la* Saatsi, then an exemplar strategy in regards to intertheory relations should be taken too. Plus, in Section 6.3 I discussed the distinction between deep and shallow metaphysics, which, in addition to exemplar/recipe distinction, it can also be used to question and characterise a form of realism. I discussed that Core Realism could be made compatible with either deep or shallow views.

At the end of Chapter 5 I outlined the basis of Heisenberg's Closed Theories view and I proposed that it could be useful in order to conceive a novel attempt at a realist interpretation to QM. Now I will argue in more detail that such a combination provides a basis for developing an exemplar realist approach to QM that does not belong to the Received View, which is a recipe type. In this section I discuss Heisenberg's view in more context and detail and in Chapter 7 I will contrast my appropriation of it with other realist approaches, which despite not being specifically aimed at QM, present useful similarities.[5]

The notion of closed theories was originally advocated by Heisenberg and it can be traced bsck to 1927. In a paper written with Born, they stated at the conclusions that they considered "quantum mechanics to be a closed theory, whose fundamental physical and mathematical assumptions are no longer susceptible to any modification" quoted from (Bacciagaluppi and Valentini 2009, 435). This seem to have been the first appearance of the closed theories conception. Scholars have identified this in many writings by Heisenberg. However, I will focus on two primary sources: the famous book "Physics and Philosophy", (Heisenberg 1958, particularly Ch. 6), and the series of interviews for the Archive for the History of Quantum Physics conducted mainly by Kuhn, in 1963, in particular (Heisenberg 1963a,b,c,d). Although one can find mentions of Heisenberg's Closed Theories in the philosophical literature, see for instance (Hacking 1992, 30),[6] specialised philosophical investigations, discussions and understandings of this aspect of Heisenberg's view are too few, regrettably. Amongst recent commentators, some recognise that the conception of Closed Theories was a crucial element in Heisenberg's thought, such as Bokulich (2004, 2006, 2008a); Camilleri (2009a,b); Schiemann (2009); Wolff (2014) and also (Chevalley 1988; Beller 1999; Scheibe 2001) even earlier. A shared remark across these works is that the philosophical literature has not paid fair attention to Heisenberg's view.

The relevance of the conception of theories as closed in Heisenberg's view remained unrecognised for many years. Indeed, for a long time it was wrongly believed that Heisenberg adhered to the positivist movement or that he even was an instrumentalist, (see (Bokulich 2006, 90) and (Wolff 2014)). There are two significant sources for this mistaken view, which to a large extent are due to Heisenberg himself: the supposition of the existence of a Copenhagen interpretation as an orthodox and homogenous interpretation whose main advocates are Heisenberg and Bohr – mentioned explicitly in Heisenberg (1958) – and the famous anecdote of how Heisenberg, whilst recovering from an illness on the island of Helgoland, discovered the basis for matrix mechanics by focusing on observable quantities only – recalled in his autobiography Heisenberg (1971). However, recent scholars have argued that these two statements should be revised and that Heisenberg cannot be simply accommodated in philosophical traditions. In regards to the first aspect, Howard (2004) has provided historical evidence and philosophical arguments to the conclusion that there was no unitarian view amongst the founding fathers of QM over its interpretation and that "the Copenhagen interpretation" did not exist as such and

[5]Of course, the first thing that comes to mind is to contrast Heisenberg's Closed Theories with the underlying framework of interstructuralism discussed in Chapter 4: Dirac's Open Theories. They indeed seem to be antitheses and such relevant discussion has been presented by Bokulich (2004).

[6]I am grateful to Greg Radick for bringing this up.

should actually be discarded, considered a myth mainly originated by Heisenberg. As to the second, it has been argued by the likes of Darrigol (1992) that the influence of the positivist attitude on Heisenberg's view on QM and physics was not really significant.[7] Nowadays, and especially after Bokulich's works, it is becoming more recognised that the notion of closed theories represented a key feature in Heisenberg's understanding of scientific methodology, theory change, intertheoretic relations and realism.

As a ground-clearing remark, I will be following mainly Bokulich's reading of Heisenberg and my analysis neither intends to be interpretive nor exegetical in reference to Heisenberg's philosophy. So I do not intend to assess the validity of his claims. Instead, my aim is to illustrate Heisenberg's view to extract its underlying framework and then appropriate it. My contribution relates to the commentators' observation that Heisenberg's Closed Theories view is useful for current philosophy of science but insufficient work has been done with it. Hence, I will build upon recent and modern discussions and fruitfully use it for my purposes.

Bokulich (2006, 93) identifies three main claims in the view developed by Heisenberg. Firstly, that a closed theory has a limited domain of applicability. Secondly, that in its domain, the closed theory is perfectly accurate, or as Schiemann (2009, 264) puts it, that a closed theory is "particularly well adapted to the pattern of experience of their realm of application". And thirdly, that a closed theory provides the ultimate description of the phenomena of its domain and no further modifications can be made.

With this conception, Heisenberg (1958, 98-99) conceives of four closed theories: Newtonian mechanics, thermodynamics, electrodynamics (including special relativity) and QM. Newtonian mechanics (what I have referred to in this thesis as CM), is considered by Heisenberg as suitable for the description of mechanical systems, the motion of fluids and the vibration of bodies, and it includes acoustics, statics and aerodynamics. Then there is thermodynamics, the theory of heat. Despite recognising ways to relate thermodynamics with statistical mechanics, Heisenberg considers that "it would not be realistic to consider it as a part of mechanics". Thirdly, there is electrodynamics, special relativity, optics, magnetism, related to electricity and magnetism which he considered to have reached its final form with special theory of relativity. He even included the de Broglie theory of matter waves, but not the wave theory of Schrödinger. Fourthly, he considered QM, the central concept of which is the probability function or the density operator. He includes here "quantum and wave mechanics, the theory of atomic spectra, chemistry, and the theory of other properties of matter like electric conductivity, ferromagnetism, etc.".

[7] An interesting account of Heisenberg's general views on science and physics, and how it has been mistakenly conceived is given by Camilleri (2009a), and also in the references mentioned above.

There are two useful comments to make on that quotation. Firstly, that Heisenberg does force all theories have to be closed in order to be considered, developed and used. From their conception when they are still open, theories can progress and evolve until they become closed. Furthermore, although each closed theory is closed and finalised, physics is not a closed discipline and the list of physical theories does not have a limit. This can be noticed by identifying an obvious omission in his list of closed theories: general relativity is remarkably absent. And the reason is that Heisenberg considered that whilst general relativity is distinctly different from the other theories, it had yet to reach its final form. In other words, he viewed it as not yet closed. It is irrelevant whether we agree or disagree with his assessment over general relativity: what I want to emphasise is that he did consider the possibility that theories develop from an initial stage to a finalised, closed, state and that physics can include novel theories not previously considered.

Secondly, the reason why Heisenberg does not include thermodynamics into Newtonian mechanics, as one might do, is because the former involves a number of concepts, like heat, specific heat, entropy, free energy, among others, which have no counterpart in the latter. And attempting to relate them by associating heat with the average energy of an ensemble – as it is standardly done – would eventually make it look as though thermodynamics is also connected with the rest of the theories as well, electromagnetism and QM. However, their key concepts are not merely reducible to the rest of physics, so Heisenberg claims. And that is a clear statement to the effect that he does not consider there to be a unified conception of physics. However, the reductionist does not deny that there are concepts from one theory that do not feature in another – indeed, recall the heterogeneous reduction conceived by Nagel, see Chapter 3. Hence, the reductionist could see Heisenberg as begging the question leaving him a weak opponent to theory reduction. Yet, there are well-known problems with reduction, specifically in the QM-CM case, as I have argued earlier.

Now, there is a subtle indication that would drive the philosopher to consider Heisenberg to be aligned with positivism, particularly in terms of the notion of intertheory relations initially derived from the Closed Theories view: did he consider theories in isolation? Indeed, the German term used by Heisenberg is *geschlossene Theorie*, which Bacciagaluppi and Valentini translated – in the quotation above – as closed theory, but *geschlossene* can be translated to English also as 'locked' or 'self-contained' and it could mean something like 'there is nothing that could or should be added to the theory to complete it.' This does suggest some resemblance with the logico-empiricism movement, in the following; recall that, for the positivists, only observational statements are meaningful and only these can be used for justifying scientific statements. As Nickles (1977) discusses, the positivists conceived theories "in splendid isolation". Focusing on the theories conceptual and empirical content, positivists precluded intertheory relations playing a role in the confirmation or justification of hypotheses. More precisely, Nickles (1977, 572) assessed that positivists conceived of conceptual external relations and theories as only internally developed: they are "rationally reconstructed in a conceptual vacuum, their only link being through a neutral observational language". And thus one would be tempted to extract a similar structure in Heisenberg's conception of physical theories. However, as mentioned before, current scholarship considers it simply wrong to associate Heisenberg with the positivists and I add that despite the tag 'closed' in his view, intertheory relations are not denied.

I infer that one indication that Heisenberg did not preclude interconnection between theories is the following: Heisenberg (1958, 99) explains that "any coherent set of axioms and concepts in physics will necessarily contain the concepts of energy, momentum and angular momentum and the law that these quantities must under certain conditions be conserved. This follows if the coherent set is intended to describe certain features of nature that are correct at all times and everywhere; in other words, features that do not depend on space and time or, as the mathematicians put it, that are invariant under arbitrary translations in space and time, rotations in space and the Galileo– or Lorentz–transformation". Hence, different theories can be closed systems and yet present relationships between them, tied to these concepts.

Finally, let us comment on the way a closed theory can capture its domain and other theories cannot. Heisenberg illustrated this in the following way:

> As soon as you come to velocities, near the velocity of light, then it is not only so that Newtonian physics does not apply, but the point is that you even don't know what you mean by 'velocity.' ... That, I think is a very characteristic feature of what I mean by [closed] system; that is, when you have such a system and you get disagreement with the facts; then it means that you can't use the words anymore. You just don't know how to talk.
>
> Heisenberg (1963b)

That is,[8] concepts are meaningful within their domain of applicability only, and their limited applicability is determined by the closed theory having a limited domain. Of course, one can worry about how the closed theory achieves closure or the sense in which its domain is limited. These issues will be revisited in Chapter 7.

Heisenberg left many questions about his view unresolved, particularly in relation to how to establish the closure of the theory. What we know is that he considered the

[8]The above quotation resembles Kuhn's paradigms. Indeed, the quotation is from a conversation between Heisenberg and Kuhn. The philosophical discussion between them has been analysed in (Bokulich 2006), among others, and I do not intend to engage in such debate.

axiomatisation of the theory to be central: "by a closed theory we mean a system of axioms, definitions and laws, whereby a large field of phenomena can be described, that is mathematically represented, in a correct and noncontradictory fashion" quoted from (Bokulich 2004, 379).[9] Now, Bokulich (2004), and Scheibe (2001), argue that this emphasis on closed theories as axiomatic systems would leave little space for an account of intertheory relations. I have noted above that he also considered there to be significant commonalities across theories, which would be the basis for some intertheory relation.

In addition to my comment above on the possibility of intertheory relations in Heisenberg's view, Heisenberg's pluralist rhetoric did not prevent him to make correspondences between QM and CM. But the axiomatic structure of the closure of theories indicates that if there is any relationship it will be more "static". By contrast, a form of intertheory relations emphasising heuristic aspects, and also the Open Theories view advocated by Dirac, presents a "dynamic" form of intertheory relations, that are"evolving, and are continuing to be developed and extended in new ways" (Bokulich 2008a, 173).[10]

Nevertheless, we can say that Heisenberg regarded closed theories as tight, distinct systems, which cannot be improved on in a piecemeal fashion and cannot afford small changes unless the whole system is transformed.

Although I clarified before that critically engaging with the view that Heisenberg himself had is not my intention, I will establish some clarificatory points of disagreement. I disagree with Heisenberg's assertion that QM can be considered to be a closed theory. As I argued in Chapter 2 my assessment is that QM is still open in the sense that there is no clear answer to the basic questions: what is the world according to QM? What is a quantum system really? What I do want to indicate is that Heisenberg's approach can be useful to complement Core Realism, particularly the realist interpretation of QM. Indeed, Heisenberg's underlying framework of Closed Theories seems to fit well with the exemplar strategy discussed in Section 6.2. I will address this association in Section 6.5.

I think that the above paragraphs provide a sufficient illustration of the type of conception that Heisenberg put forward. In regards to the questions that might arise in relation to this conception's robustness, coherence and related issues, I will come back to it in Chapter 7. For now I want to continue by showing that the framework of Closed Theories can be used to supplement Core Realism.

[9]Many would agree that a theory achieves its final form when it is axiomatised, within the tradition of the syntactic view. Today, the semantic approach seems to be more advocated than the syntactic view. However, even within such a model theoretic view to theories (i.e. theories regarded as families of models), philosophers could argue that theories could be considered closed too. However, da Costa and French (2003) – in the semantic approach – consider theories as open. There may be more to discuss about the success of Heisenberg in establishing the closure of the theory by axiomatising it and the possibilities of accommodating such a view in the semantic approach. However, I do not intend to engage in the general debate about syntactic vs. semantic views or the role of axiomatisation.

[10]I introduced the Open Theories view advocated by Dirac in Chapter 4.

6.5 An Exemplar Realist View of QM: Core Realism+Closed Theories

As discussed previously, Core Realism should be supplemented. Drawing on useful recent distinctions, I anticipated some of the characteristics of the form of realism I prefer to approach QM: a novel realist view of QM would have to be broadly conceived within an exemplar realist strategy, as per Section 6.2.1. My appropriation of Saatsi's distinction entails that there is no one recipe to account for intertheory relationships and then one should look at specific pairs to decide how they relate. Furthermore, this literally precludes questions of intertheory relation precede the central interpretative questions for the realist who looks at QM. My view secures that the central interpretative question is actually the one that Core Realism presents: what would it take for the world to be the way QM says it is? Or, what is the world according to QM? This discussion of the two methodologies was illustrated in Table 2.1, discussed in Chapter 2.

In terms of metaphysical commitments, in Section 6.3 I indicated two alternatives – deep and shallow – and I noted that Core Realism is wedded to neither of them. I will return to this question in Chapter 7 by presenting contrasts with other relevantly similar forms of realism.

Now I want to argue that the supplementation Core Realism requires can be achieved by adopting the underlying framework of Heisenberg's Closed Theories view discussed in Section 6.4. I will call this Core Realism+Closed Theories and I will present relevant contrasts with the Received View.

Closed Theories appears as a global attitude that allows us to consider each physical theory on its own. How theories latch onto the world does not follow a recipe but is particular to the theory in its domain of applicability. By contrast with the exemplar-based approach that I offer, the main realist interpretations discussed in Chapter 2 follow the recipe of putting the interpretative problem of QM in terms of questions of intertheory relations. That is my way to explain why most realist interpretations consider that they have to focus on the explanation of the 'appearance of classicalities, given that the world is quantum'. That framework is underpinned by recipes that force the realist to answer the problematic question: how does the quantum explain the appearance of the classical? Instead, adopting the Core Realist stance, supplemented by Closed Theories view, the realist manages to approach the question of QM in a different way. A realist following the exemplar approach questions what would the world be for QM to be true, or, more specifically: what is the wavefunction representing in the world?

Core Realism+Closed Theories view takes an exemplar attitude to intertheory relations as well: the view of Closed Theories does not require novel theories to account or recover the successes of previous theories, as Nickles' or Nagel's recipes do, nor does it require the flaws of previous theories to develop the novel theory further, as in Post's recipe. At least when it comes to interpreting the theory, to establish the relationship between the theory and the world, intertheory relations do not play a crucial role as they do in the Received View. This does not contest studies in the history of the development of the theory. Perhaps the heuristics do follow Post's proposal, but that is a different question from the interpretational question: what does the wavefunction represent?

Provided with a physical theory, such as QM, the Closed Theories view develops an interpretation of its (limited) domain of applicability. As Chevalley (1988) puts it, Heisenberg conceived, through his notion of Closed Theories, that "that the new theory should essentially be concerned with the 'kind of reality' of its objects, and that one had to break with the former conception of the structure of physical theories". With this global attitude, one considers that quantum objects be construed quantum mechanically regardless of, or metaphysically independent of, classical objects. Each closed theory captures its own metaphysics. And this latter statement is key in presenting a contrast with the Received View discussed in Chapter 2, which conceives the nature of quantum objects as subsidiary to provide an account of macroscopic objects: how does QM explain the classical appearance of the world? Why do we not find quantum superpositions?

More specifically, most current advocates of the many worlds interpretation (MWI) of QM would consider themselves as having achieved an interpretation of QM by successfully establishing the appearance of the macroscopic world from the quantum. As I mentioned earlier, Wallace (2016b,c), a main proponent of MWI, considers that MWI is a successful interpretation of QM and that it is a theory of the macroworld. Moreover, as discussed in Chapter 2, decoherence plays a crucial role in securing the success of MWI. Yet, it is not obvious the sense in which MWI has clarified the conception of the quantum system before it has decohered. Their argument is that such time is "for all practical purposes" irrelevant and, in any case, QM is merely approximately true hence that question is not pressing. Their recipe is satisfied with connecting the formalism of QM with the macroworld of the everyday appearances, despite underdetermined response to a purely quantum question: what is a quantum state in the world – even when it is in a superposition that has not delocalised its coherences? That is, even when Wallace – as one of the main proponents of a version of MWI – considers the problem to have been solved, there are some surprisingly relevant questions unanswered! By contrast, Core Realism+Closed Theories considers that the central question is the interpretative question about QM in the exemplar way, following Heisenberg, in relation to the class of phenomena that QM relates to.

Now, in addition to the contrast between Core Realism+Closed Theories with the Received View in terms of intertheory relations, one can discuss such a contrast with the type of intertheory relations put forward by interstructuralism. I pointed out the novelty in Bokulich's interstructuralism and extensively engaged with it in Chapters 4 and 5. In Section 5.4 I discussed that her view evidently challenges the vertical hierarchy QM $\rightarrow$ CM (bottom-up) defended by the Received View at the epistemic level. Appropriately motivated by physical and philosophical arguments that are based on relevant physical phenomena and capturing the practice of physicists, the interstructuralist provides a novel relationship between QM and CM. Interstructuralism can be characterised as considering an horizontal relationship in epistemic terms, as depicted in Figure 5.1. This is articulated by adopting Dirac's philosophical view of the Reciprocal Correspondence Principle Methodology, see Chapter 4. However, there is the question of which approach the interstructuralist adopts at the ontological level. And here Bokulich's view maintains the standard metaphysical reduction: "Throughout this book, the criticisms I raise against reductionism should be understood as being against theory reductionism or explanatory reductionism. At no point am I challenging ontological reductionism, or what philosophers sometimes call materialism. So, for example, I do not think that there are emergent properties that are not just the result of fundamental physical properties, their organization, and complex interactions" (Bokulich 2008a, 4, fn. 1). Therefore, her view challenges the epistemic aspect of traditionally accepted relation between QM and CM, but conserves the ontological commitment of it. This is aligned with one of the two components of the Received View, presented in Chapter 2.

Therefore a novelty could result from moving further away from the Received View and challenge that classical objects have to be somehow ontologically dependent on quantum ones despite knowing that a full ontological account of quantum objects is far from unproblematic or a closed issue. Whilst my view does not actually deny such ontological dependence entirely I claim that, methodologically, the realist should have a clear account of the quantum ontology, and then explore how it is related to classical.[11] My approach can accommodate that novelty.

Amongst current realists aligned with the Received View, physical theories are generally conceived as universal.[12] For example, the advocate of wavefunction realism – such as Albert or Maudlin – relies on the conceptual wavefunction of the entire universe, literally,

[11]The worry could arise: 'well, Bohmians do have a clear ontology for QM'. It is not so straightforward. Essentially, the nature of the Bohmian particle is not as easily conceived as some claim. In particular, it is not a classical particle. A classical particle is not governed by the Bohmian guidance equation, but by CM. A classical particle is in classical phase space, whilst Bohmian particles live in configuration space. Bohmians argue that there are particles *with the intention* of having similar ontologies for QM and CM. My approach says that no such a recipe should be imposed. There is no a priori reason why both theories should involve the same real content.

[12]Indeed, the measurement problem requires that QM is universally applicable.

as discussed in Chapter 2. This recipe-based statement is captured by the Received View: it postulates that QM is universally valid and expects from it an account of the entire universe, literally.[13] By contrast, my exemplar based approach, Core Realism+Closed Theories, does not require such a far reaching claim. The Core Realism+Closed Theories view offers a more local and focused approach. The task is to account for the real content of QM in relation to its empirically determined domain of applicability, not with the entire universe. Core Realism+Closed Theories focuses on the interpretation: what is the theory telling us about the way the world is? It does not follow the recipe that a physical theory should be accountable for all phenomena in the universe.

Now, this discussion and the defence of the Core Realism+Closed Theories view does not provide solutions to the interpretation question of QM. It is a contribution towards the conception of a philosophical framework where the solutions to the realist challenges of QM could be addressed. Indeed, there are unresolved questions as to the Closed Theories view itself, questions that the commentators, such as Bokulich, have identified. Heisenberg's description of the limited nature of the domain is not fully satisfactory, given that he did not provide an independent account of how to determine where certain concepts apply and where they do not. He says that the concepts apply in their domain and the domain is determined by the applicability of the concepts, but with this circular definition he does not really answer how to know what the domain is, see (Bokulich 2008a, 34).

Nevertheless, this proposal is relevant at least as a framework that differs from the Received View. As discussed in Chapters 2 and 3 the account of the relationship QM-CM is at the centre of the focus of most realist attempts at QM. The question of why should one consider the question of intertheory relation prior to merely giving a physical account of QM, or why these two issues should be so closely related, cannot be avoided unless one abandons the Received View. Within Heisenberg's view, the motivation from Core Realism can be carried out without modification: the realist looks at QM, interprets it, and then attempts to find how QM relates to CM. Clearly, this is a simple yet complex question, and I have not attempted to provide an answer. The problem I identified is that even asking the question requires substantial philosophical analysis. My contribution by adopting the underlying ideas of Heisenberg's Closed Theories view plus the core realist conceptions, is to provide a fruitful framework.

[13] For instance Monton (2013, 154) explicitly argues that QM is false because it does not correctly predict the behaviour of precise clocks in strong gravitational fields. Or in a different context Donald (1998, 221) states that "the ultimate goal should be to analyse a universal wave-function $\psi \in \mathcal{H}$ which would be an uncollapsed state arising from the big bang".

Indeed, there are still relevant questions to answer in order to further specify the view of Closed Theories+Core Realism. I will do so in Chapter 7 by contrasting this scheme with other realist views found in the philosophical literature.

6.6 Conclusions

The basic elements of realism discussed in Chapter 2 in terms of what I called Core Realism were meant to be a basis for the development of a novel realist approach to QM, but that was not sufficient. The Core Realism view discussed in that chapter appeared more as a stance. The novel element was presented by contrast with crucial features that I found common to main realist interpretations already known in the literature, in what I called the Received View. This chapter has dealt with the further specification of Core Realism, developing that basic stance into a more sophisticated framework that could interpret QM. I have done this firstly, by identifying three relevant features that any form of realism typically involves: the realist commitment, a global prescription of the type of entity it considers and, a form of intertheory relation. I have dealt with these issues by drawing on recent, novel and relevant distinctions in the literature. That is, Saatsi's distinction between exemplar and recipe realism, and Magnus' distinction between deep and shallow realism. In regards to the former distinction, I have contributed to articulating further the distinction itself, by arguing that an exemplar realist should commit in an exemplar way to issues of intertheory relations, and I utilised the distinction in order to further characterise Core Realism. In regards to the latter, I have provided two options for the development of Core Realism.

Besides the discussion of Core Realism, I have revisited issues that were relevant in previous chapters, such as interstructuralism as an account of the QM-CM relation. I argued, through discussing in terms of the two relevant distinctions mentioned above, that interstructuralism can be characterised as following an exemplar strategy towards intertheory relations at an epistemic level. This would place interstructuralism outside the Received View. However, because interstructuralism adheres to ontological reduction, it still belongs to the Received View. Furthermore, I used the shallow/deep distinction to provide a possible solution to the difficulties to consider interstructuralism within standard realism, that were discussed in Chapter 5. I claimed that the interstructuralist could adopt a shallow realism in order to release some weight from her shoulders and shallowly interpret the status of the fictional orbits in the semiclassical models.

In addition to engaging with these stimulating distinctions, I have contributed to supplementing Core Realism with a conception of physical theories, Heisenberg's Closed Theories view. This is not a new view, although it has been so far mostly neglected by the literature. I had already pointed out the motivation for looking at this view at the end of Chapter 5.

I presented the context and central elements of Heisenberg's view and I argued that using it to supplement Core Realism is promising. By drawing again on the previous distinctions, I argued that my combination Core Realism+Closed Theories is the realisation of a form of realism that follows the exemplar strategy that Saatsi advocates. I defended that the Received View of the Realist Interpretation of QM should be considered a recipe and thus suspended, if, following Saatsi, recipes should be abandoned. Consequently, this is a strong challenge to the main realist approaches to QM. My proposal is that Core Realism+Closed Theories view is a possible option for a realist approach to QM. That is if, following Saatsi, an exemplar based approach should be preferred.

Yet, I emphasised that this discussion does not resolve the question about the interpretation of QM. What it does is to offer a novel framework whereby a new interpretation could be designed. If my conception of the Received View is not a philosophical straw man, then my Core Realism+Closed Theories will appear as novel and relevant by contrast with the Received View. However, the novelty is at the level of a framework to approach QM, a 'meta-level'. Further investigations should really put my framework into work. This present investigation has had to go a long way to *motivate considering* a novel framework, and it is a preliminary work for a novel interpretation.

In the next chapter I will continue articulating Core Realism+Closed Theories as a realist view, considering that its central feature is to advocate a form of pluralism. Further philosophical work should specify the nature of such pluralism. One question that so far has been undecided is whether this view should be considered a shallow or deep pluralism. I will do so by contrasting my view with relevantly similar realist views already known in the philosophical literature.

Chapter 7

Beyond the Received View of QM (II): Other Realisms

7.1 Introduction

In Chapter 3 I argued that the Received View faces notorious difficulties to account for the QM-CM. Furthermore, in Chapter 4 I discussed relevant physics at the mesoscopic scale that cannot be accommodated in the standard view. Therefore, I argued for the need for a novel view. In Chapter 5 I critically engaged with Bokulich's view of the relation QM-CM, interstructuralism. I concluded that there is sufficient motivation for developing a novel view of QM.

My alternative approach to QM is my Core Realism+Closed Theories view. I introduced Core Realism in Chapter 2 and supplemented it with Heisenberg's Closed Theories in Chapter 6, where I made use of current and relevant distinctions in the philosophical literature on realism.

Previously I argued that the Core Realism+Closed Theories view includes a pluralist component. Drawing on Saatsi's (2015) exemplar/recipe distinction, I argued that my view is exemplar, which allows us to consider the way that theories capture or latch onto the world in a case-by-case basis. No recipes are required and each (closed) theory is applicable to its own domain. Furthermore, no recipe dictates how different theories relate with each other. Simply put, whilst my view does not deny the history of science, neither does it advocate that the realist content of theories ought to respect the successor/predecessor relationship historically conceived. My view considers the interpretative question at the centre: what does QM tell us about the way the world is?

In this chapter I will contrast my Core Realism+Closed Theories view with other relevant realist views. I will specify my view by comparing it with the perspectival views articulated by Ronald Giere (2006) and Michela Massimi (2012, 2016). Plus, I will discuss the pluralism of Nancy Cartwright (1983, 1999). By presenting similarities and differences, I will be further articulating what I think my Core Realism+Closed Theories view amounts to.

7.2 The Core Realism+Closed Theories View and Other Realisms

As an approach to QM, my Core Realism+Closed Theories view may have appeared very different compared to other realist frameworks for QM. However, there are relevant realist frameworks that were not specially designed for QM and yet are similar to my view.

In the next section I will discuss relevant pluralist versions of realism and contrast them with my Core Realism+Closed Theories view.

7.2.1 Realism and Perspectives

There are views within realism that explicitly incorporate into the account of the scientific knowledge the basic fact that knowledge is produced by humans in communities, situated in specific historical periods, and utilising specific instruments and theoretical devices which thereby are *idiosyncractic* to that given community. Perspectivist views include that relative aspect of science within the analysis of science itself.

In Chapter 2 I identified key debates and questions concerning realism: I considered what realism amounts to distinguished from what its justifications are. The broad debate of incorporating the human perspective into realism covers these two questions too. Typically, a view of this kind presents a discussion about the meaning and scope of the human factor by debating against competing views. Thus, in order to specify what perspectivism amounts to, the perspectivist compares her view against another type of realism, namely, objectivism. The core of objectivism was famously expressed by Van Fraassen (1980, 8): “science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a theory involves the belief that it is true”.

I will consider two forms of perspectivism: Giere’s initial view, which I will refer to as *scientific perspectivism*, and a derivative and separate project proposed by Massimi, namely, *perspectival realism*. I will contrast their views against my appropriation of Heisenberg's Closed Theories view within my Core Realism.

7.2.1.1 Giere's Scientific Perspectivism

Giere conceives his scientific perspectivism (SP, although at times he also talks of perspectival realism) on the grounds of interpreting the role of scientists in how science works. Particularly, he focuses on the human visual system and articulates a perspectival view thereof, a procedure that can be extended to other senses and scientific instruments. The visual system is designed to respond to inputs of electromagnetic waves within a range of frequencies. Hence, "scientific observation is always mediated by the nature of the instruments through which we interact with selected aspects of reality" (Giere 2006, 43). This works for vision and for scientific observation: we do not have an image of the brain in and of itself, what we obtain is the image scanned by a magnetic resonance imaging apparatus. Giere provides a comprehensive discussion of relevant examples to justify his perspectival approach, that I do not intend to rehearse. I am concerned with his framework and how that can help to articulate my Core Realism+Closed Theories view, presented in Chapter 6. For a broader discussion of Giere's view and its context, see (Massimi 2012; Votsis 2012; Giere 2006, 2013; Chakravartty 2010).

Giere's SP is articulated against an objectivist understandings of scientific realism. Indeed, he intends to present a middle-way position between objective realism and relativism. Giere (2006, 92) claims that whilst debates considered in philosophy of science present a tension between realism and constructivism, such a debate assumes – in his analysis – the controversial conception that realism must be objectivist, precluding the possibility for a perspectival non-objectivist realism.

There are two broad commitments at the core of SP, according to Giere. Firstly, the perspectival commitment that scientific claims are conditional relative to "a set of humanly constructed concepts, a *conceptual scheme*" (Giere 2013, 53). And secondly, the realist attitude that "some claims generated by scientific practice are claims about the world. They are not merely claims about beliefs about the world" (Giere 2013, 53).

Giere's idea is to take stock and be as much a realist as possible, recognising the human aspect of science, without having to take scientific claims to be merely socially determined. He takes SP to be "a methodological naturalism that supports scientific investigation as indeed the best means humans have devised for understanding both the natural world and themselves as part of that world. That, I think, is a more secure ground on which to combat all pretenses to absolute knowledge, including those based on religion, political theory, or, in some cases, science itself" (Giere 2006, 16).

There are two further intertwined features that characterise Giere's SP: (1) his emphasis on representation and modelling and, (2) the notion of perspectives. (1) SP explicitly considers the scientist within the process of representation of the world via models. According to Giere, representation cannot be conceived as a relation between two elements, for it has to include the scientist within the scheme. The elements are: an agent, user or scientist S, her purposes P, the model M, and the aspect of the world, W. They each centre on the notion that science represents through modelling. Giere's view includes a strong ascription to the semantic approach, the model-theoretic conception of science, where, basically, instead of individual theories and laws, the philosopher considers families of models. Hence, representation is a relationship between these elements, such that S uses [M] to represent W for purposes P, (Giere 2006, 60).[1]

(2) Giere considers the observational and the theoretical perspectives. It is in the perspectives that models originate. Giere (2006) develops a comprehensive detailed account of the perspectival aspect of observation with human vision, but his conclusions are meant to extend to the entire scientific knowledge. Rather than critically engaging with his case study, I focus on his conclusion, that to say that scientific observation is perspectival "is to say that claims about what is observed cannot be detached from the means of observation" (Giere 2006, 48). The observational perspective provides the basis for assessing the fit of the representational models mentioned above.

In turn, where do theoretical models come from? The theoretical perspective deals with this. Theoretical models are obtained from general fundamental principles. Principles in Giere's view are what more standard accounts identify as fundamental laws. For Giere these principles do not actually tell us anything about the world. Instead, they are used to make such claims via the models that they generate. And the principles characterise the theoretical perspective: Newton's laws characterise the perspective of CM, the laws of Maxwell characterise the perspective of electromagnetism, Schrödinger's equation characterises a quantum mechanical perspective, and so on, see (Giere 2006, 14). In short: "all theoretical claims remain perspectival in that they apply to some aspects of the world, never with complete precision" (Giere 2006, 15).

The tension with the objectivist realist is exhibited by the crucial aspect of how theory and observation are related in Giere's SP. "Given the assumed observational and theoretical perspectives, [model] M exhibits a good fit to the subject matter of interest. There is *no basis* for going further to a supposed more objective, nonrelativized, *claim that this is how the world is*, period" (Giere 2006, 92, my emphasis). Therefore, Giere presents

[1]The issues around representation and modelling are not within my central concern and discussing them with sufficient justice would require going beyond my limits in this investigation.

a relationship between models of the data (observational perspective) and representational models (theoretical perspective). The tension with the objectivist is that she would like to make a claim about the way the world is simpliciter, whilst SP recognises the representation of an aspect of the world.

For example:

> When I say we have a colored perspective on the world, I do not mean that we experience colored representations of the world. I mean we experience an interaction with the world that, given our biological nature, results in our being able to distinguish objects and other phenomena by their apparent colors. To put it another way, we perceive aspects of the world itself, which aspects being determined by our particular sensory capabilities. How this happens for colors is being explained by color science.
>
> (Giere 2006, 35-36)

Further discussions could engage with a number of complex concepts in Giere's view. One could discuss the notion of similarity and how it is the scientist (through the representational practice above) rather than the model, who finds the similarity in the process of representing; one could analyse the notion of truth involved in such a representation relationship and the associated notion of fitness of the models; the notion of law of nature involved (the law of the objectivist is here a principle); among others. What I am interested in having at hand are those notions that can engage with my appropriation of the view proposed by Heisenberg, what I called the Core Realism+Closed Theories view.

Giere's Scientific Perspectivism and the Core Realism+Closed Theories View

Rather than critically engaging with Giere's view, my aim is to utilise SP to further specify my Core Realism+Closed Theories view. For criticism of Giere's view, see (Votsis 2012) and (Massimi 2012), among others.[2]

Considering the metaphysical aspect, both SP and the Core Realism+Closed Theories views agree with the existence of a world independent of humans. In Core Realism+Closed Theories there is a tension between that metaphysical commitment and the

[2]The distinctions discussed in Chapter 6 could be used to critically engage with Giere's view. For example, one could argue that Giere's view is a recipe form of realism, which based on the case study of colour science, inductively accounts for the whole of science. I postpone developing this analysis for future work.

consideration that closed theories apply in a limited domain and different domains might not overlap. For, how could one single world be secured from a metaphysical pluralism? SP partly has this tension too, and Giere (2006, 35) resolves it by claiming that the "methodological principle that there is only one world in which we all live", is compatible with the perspectivist aspect of his realism. Core Realism+Closed Theories can appropriate that, claiming that there is a single world as a methodological presumption.

At an epistemic level, there is some disagreement between SP and Core Realism+Closed Theories, although both equally disagree with the standard view. Giere puts the success of science in terms of a representation relation. Beyond matching the observational and theoretical perspectives, the representation relation also includes the scientist and her purposes. Within SP modelling represents perspective-relative true features of the world, not merely true of the world itself. In contrast, in the Core Realism+Closed Theories view, closed theories capture true features of the world that are in the domain of the theory.

Let us put this in terms of QM and CM. According to Giere's SP, CM represents classical features of the world in the perspective of CM. That is, the principle of CM provides theoretical models which successfully fit with the observations obtained through the observational perspective. QM picks out real features analogously. Because there is no individuation theory for the principles (Schrödinger equation and Newton's laws, seen as principles), there is no trouble in terms of intertheory relations. In Giere's account, the QM-CM limit is unproblematic and something that scientist will deal with. Because in practice physicists can translate predictions from QM and CM. This might be the case, and probably Giere is right in his assessment. However, my issue is, in practice, how do we establish the relationship QM-CM? In Chapter 3 I examined the difficulties to do so, showing that there are specific individual cases where the relationship is accounted for, but not free from controversies. I think that Giere's view misses the appreciation of the difficulties in establishing the physical relation QM-CM.

In my view, the core realist question in QM is: what is a quantum system? Core Realism+Closed Theories can frontally face this question, but Giere's view struggles. For he considers that the challenges are the comparison between models of data and theoretical models, whether both match and how well they do. Consequently, in regards to QM, the interpretative question is pushed one step away. For instance, let us consider the well-known double-slit experiment in QM.[3] According to Core Realism+Closed Theories, the double slit experiment exhibits the challenge of conceptualising quantum systems: how can the world be such that something is a wave and a particle? This

[3] I assume the reader broadly familiar with this famous problem. See (Feynman 1965, 1-1), (Landau and Lifshitz 1965, 1), or (Lewis 2016) for a recent discussion.

cannot be, but if quantum systems are neither particles nor waves, what then? These questions affirm realism. It is assumed that QM is telling us something about the world and that prompts the realist to interpret the experiment and the model in relation to physical reality. However, insofar as Giere's challenge is the matching between the observational perspective and the theoretical one *for the purposes of the scientist*, there is no crucial problem in the double-slit experiment. He could say that the data model is unproblematically obtained through known empirical procedures, that Schrödinger equation generates the theoretical model, and that both data and theoretical model successfully match. However, this would be difficult to maintain given the disagreement on the interpretation of QM, as I discussed in Chapter 2.

According to the Core Realism+Closed Theories view, CM captures real features of the world and QM captures real features of the world as well. Furthermore, CM and QM do not have to relate with each other in any specific way because the view itself follows no recipe. However, whilst we know how to interpret CM by the familiar ontology of macroscopic objects and other intuitive concepts, no clear response to the interpretative question about QM has been obtained so far.[4] Therefore, my view is a more appropriate realist framework for interpreting QM than Giere's, because whilst the interpretation question here is central, it is not a problem for SP. But the interpretation of QM is an open question, hence Giere's view is in this respect deficient.

In short, it seems difficult to identify how Giere's SP conceives of the interpretation of QM. If we accept that the crucial feature in his view is to be perspectivist, which distinguishes his view from standard realism, then that central feature pushes the interpretative question in QM away; and that is undesirable. Instead, my Core Realism+Closed Theories view can incorporate the interpretative question of QM.

Now, let us recall the account of QM by the standard view. In the standard view (or, my Received View discussed in Chapter 2) the problem of interpreting QM is translated into questions of intertheory relations. The standard view holds that theories are universal and can be sorted by a relation of fundamentality. Therefore, QM is universal and a more fundamental theory than CM, which entails that QM has to recover CM. Then, one issue at a meta-level is whether there is an end to the fundamentality scale, and if so whether we will ever actually find it. Whether that is the case or not is debatable, from a philosophical viewpoint. For instance, some structural realists claim that there always

[4] I appreciate that relevant debates in the literature can consider the interpretation of CM to be problematic too, namely (Jones 1991). However, I broadly assume that CM is interpreted in terms of macroscopic objects that exist and have determined properties. I consider CM to be about the 'everyday'. And it is by comparing these features of CM that the problems of QM arise. Indeed, QM is generally characterised in negative terms relative to classical features: 'quantum probabilities are not classical and are not interpretable in terms of ignorance', 'quantum logic is not classical, since it is non-Boolean', 'quantum properties are not determined', and so on. Hence, even if one accepts debate in the account of CM, the realist question about QM is notoriously more problematic.

is more fundamental structure in the world to be discovered, with the famous slogan 'it is turtles all the way down'; other realists claim that although there is a fundamental bottom, we might never reach it. In turn, on the physics side the most fundamental theory of physics is still to be developed, if that is even a plausible theory.

A second issue that appears in the standard view is actually articulating the account of CM from QM. I argued in Chapter 3 that this task is notoriously problematic. Indeed, the general view on intertheory relations whereby the predecessor is accounted for by its successor is challenged by current physics. Chapter 4 discussed recent and relevant developments in physics where CM significantly appears in the account of quantum phenomena, thereby challenging the validity of the standard vertical hierarchy (QM-low level; CM-higher level).

These two issues – about fundamentality and about the account of the CM from QM – neither appear in SP nor in the Core Realism+Closed Theories view. In SP they do not appear because the focus is to account for the representation practices by the scientist. This downplays the role of the interpretative question about QM.[5] In turn, within my Core Realism+Closed Theories view these two issues do not appear at this stage. Methodologically, the Core Realism+Closed Theories view is meant to tackle firstly the interpretation of QM: what is the quantum Ψ? Then, the account of intertheory relations can be considered after.[6]

	Scientific Perspectivism	Core Realism+Closed Theories
Metaphysical Realism	Yes	Yes
Epistemic Level	Relative truth	Closed theory applies in limited domain
Focus	Representation in practice of scientist	Interpretation of QM: what is Ψ in the world?
Representation	Includes the agent in representing	Relationship between the world and the theory
Intertheory relations	No inter-perspective relation	Does not commit to a recipe; intertheory relation to be adjudicated once theories are interpreted

Table 7.1: Similarities and differences between SP and Core Realism+Closed Theories.

[5]I suspect that SP could be a nice realist framework that could accommodate interstructuralism. Because SP can cope with swapping dynamical structures from one theoretical perspective to map data model from other 'theories'. However, I leave the analysis of this speculation for future work.

[6]Recall the methodology of Core Realism, see Chapter 2.

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Whilst Giere perspectivist view is characterised by the inclusion of the scientist *within* the notion of representation, there is another relevant perspectivist view. Next, I will engage with Massimi's perspectival realism in order to specify my Core Realism+Closed Theories view.

7.2.1.2 Massimi's Perspectival Realism

Massimi puts forward a perspectival view that is relevantly different to Giere's. As discussed above, Giere's SP conceives that statements have a relative truth-value, whereby the scientist is included within the notion of representation: the representation articulates a relationship between the model, the agent and her purposes, and the world. Hence, true scientific claims are true relative to the perspective involved in the representation relation (conceived as a matching between an observational and a theoretical perspectives). But taking truth and falsity to be relative to a perspective and denying the 'God's eye objective knowledge' threatens to preclude objectivity. That is, according to Massimi (2016), one worry with Giere's variant is the threat of relativism. In turn, her perspectivist aspect relates to the conditions under which realists are justified in believing the truth of scientific claims. Massimi's is an epistemic form of perspectivism.

Massimi's perspectival feature enters at the level of the agent, into the manner in which scientists come to know facts: "the agent is able to self-reflect on her beliefs, on the sources of her beliefs, the way beliefs cohere with one another, no less than the way in which they, individually and jointly, are anchored to the empirical ground via reliable methods", but there is still commitment to the idea that "there are facts of the matter that make our beliefs about nature either true or false" (Massimi 2012, 48-49).

Therefore, this perspectivism emphasises epistemic realism. Massimi (2016) argues that perspectivist views are mistaken if they equate the rejection of objectivity with the claim that the nature of worldly states of affairs is relative. Indeed, she contends that the acceptance of the plural character of scientific enquiry, whereby there is no unique, objective and privileged epistemic perspective, is not incompatible with acceptance of a metaphysical mind-independent (perspective independent) world.

Hence, Massimi's perspectivism concerns epistemic and methodological aspects. There is a neo-Kantian tradition here, limited to recognising – as Kant did, according to Massimi – the "acknowledgement of the human vantage point (as opposed to the God's eye view) from which only knowledge of nature becomes possible for us" Massimi (2016). That is, knowledge is perspectival in the sense that it is situated in the human vantage point and all questions about nature that are asked thereof.

To understand the difference between Giere's and Massimi's view consider the following. Both views reject the objectivist "God's eye view of the objective world". Giere's emphasis is in the relationship between scientific knowledge with the *objective world*, claiming that scientific practice obtains truths that are not simply true of the objective world, but relative to a perspective. Contrarily, Massimi challenges the objectivist notion of *God's view*. Her perspectivism denies that there is a view that can look at the objective world simply, and affirms that the way in which claims are true of the objective world is 'situated' within a human perspective.

In terms of defining her view, Massimi (2016) puts her perspectivism as a form of realism (hence, perspectival realism, PR) underpinned by the following claims:

1. PR endorses the realist metaphysical tenet about a mind-independent (and perspective-independent) world.
2. PR endorses the realist semantic tenet about a literal construal of the language of science.
3. Finally, PR endorses the realist epistemic tenet in that acceptance of a theory implies the belief that the theory is true (and even shares the realist intuition that truth is correspondence with states of affairs in the world).

Yet, this is not enough to distinguish PR as a realism on its own. Indeed it looks like the three standard aspects of realism articulated by Psillos (1999). The novelty and difference is in the notions of *perspectival truth* and *scientific progress across perspectives*, that do not deny the existence of perspective-independent states of affairs.

Perspectival truth establishes a correspondence with states of affairs of the world that are not dependent on humans, but also that are contextual. That is, the truth-maker is still the perspective-independent-world, but the epistemic access is situated in a scientific perspective. For example, consider the viscosity of water. Massimi explains that it is ultimately the world that makes the claim 'water is a liquid with dynamic viscosity of 1.983 x 10^{-5} Pa s' either true or false. However, "our ability to know these states of affairs (and hence to ascribe a truth-value to the relevant knowledge claim) depends inevitably on the perspectival circumstances or context of use" (Massimi 2016).

PR considers that scientific knowledge claims are perspectival, that is, are held in particular communities at particular historical times, working within well-defined intellectual traditions. Hence, perspectival *standards of performance-adequacy* in the original context of use define whether a scientific knowledge claim is true, and thereby merely perspectivally true.[7] To do this, Massimi explains, scientists deploy different contexts for asserting the truth of the viscosity of water. One could check whether samples of water satisfy the Navier-Stokes equations, look at non-Newtonian theories of fluids to predict the mechanical behaviour of water under the action of particular forces and stresses, or run experimental tests, among others. However, this is not all, for PR wants to establish claims about a perspective-independent reality.

Those perspectival standards of performance-adequacy are not sufficient for establishing the truth or falsity of a scientific claim, because "scientific perspectives cannot sanction their own scientific truths" Massimi (2016). And this is where the notion of scientific progress across perspectives enters. Indeed, scientific perspectives act as contexts of assessment, whereby cross-perspective claims can be evaluated. For instance, claims from a historical predecessor perspective will be assessed from the current perspective. Depending on whether they continue to satisfy their original standards of performance-adequacy from the current perspective, they will be retained or withdrawn. As an example, Massimi gives us Fresnel's equations. From our current perspective they are still delivering by their own standards, hence they are retained. By contrast, the celestial spheres from geocentric cosmological models have been withdrawn from our current scientific perspective according to their own original standards they have lost their performance-adequacy. Why? Because since the seventeenth century, they have been incapable of delivering, on their own original standards, the apparent motions of the fixed stars and the planets, for the planetary orbits turned out to be elliptic and not circular, among other issues.[8]

With the notion of scientific progress across perspectives, Massimi intends to achieve a dual aim. Firstly, it gives the realist a view towards the establishment of true features of the world that are perspective-independent. Through scientific progress, worldly states of affairs across scientific perspectives can be tracked. Secondly, this also detaches PR from considering our current perspective as a privileged one. This is because our current perspective is just one among others past and future ones, providing that our current perspective is the best up to our knowledge.

One worry with this form of perspectivism is that in order to assess the perspective-independent truth of scientific claims the scientist will look at different perspectives. And this involves a number of elements that could be distinguished as theoretical or observational, as per the examples mentioned above: either different theories of fluids that could explain the properties of water, or experimental tests. To adjudicate this

[7]The notion of perspectival standards of performance-adequacy is still in progress, see forthcoming papers in (Massimi 2016).

[8]See more discussion of this example in (Massimi 2016).

precisely Massimi invokes some "perspectival standards of performance-adequacy". Unfortunately, those standards have not yet been provided by her, see forthcoming papers in Massimi (2016).

It can be characterised that her PR conceives that different models or theories are contextual and plural, and can be applied to make true or false claims of a perspective-independent world.[9]

Massimi's Perspectival Realism and Core Realism+Closed Theories

The similarities between my Core Realism+Closed Theories view and Massimi's PR lie, I believe, in the unresolved questions that both views share. The reader who reads my Chapter 6, immediately questions: but how are the borders of the applicability of the closed theory delimited? How are closed theories individuated? What is the relationship between different closed theories? How is it compatible such a scheme with metaphysical realism?

Similarly, PR leaves open similar questions: how is a perspective defined? What is the individuation of perspectives? If they are historical, when is the transition from one perspective to the next? Are there co-existing perspectives? Given the disagreement on the interpretation of QM, do we currently have a consistently conformed perspective or are there open perspectives?

PR seems to be a global attitude to realism. This is because it readily proposes a view on the progress of science. By contrast, the Core Realism+Closed Theories view was specifically designed to the case of QM.

PR and my Core Realism+Closed Theories view have different conceptions of scientific progress. The former considers progress as a matter of cross-perspective questions. As discussed above, if claims belonging to historical previous perspectives continue to satisfy their original standards of performance-adequacy from our current perspective, they will be retained, or else withdrawn. By contrast, the Core Realism+Closed Theories view considers historically previous stages of science as moments in the development where the theory was not yet closed. For example, Ptolemaic theory of the motion of planets

[9]Massimi's view could be characterised by what Bokulich (2008a) calls pluralism type-II. That is, a form of pluralism that conceives of a multiple competing theories or models in describing the same (single) domain of phenomena. For example, the view defended by Hacking (1983) is of this type. He asserts that whilst the realist has enough evidence that the quantum objects exist, there is no one unique way to describe it which is completely truthful: "different and incompatible models of [quantum objects] which one does not think are literally true, but there are [quantum objects], nonetheless" (Hacking 1983, 26). More generally, his realist and pluralist view accepts the simultaneous description of a unique type of object by incompatible theories. Further work to discuss the differences between this view and Massimi's is left for the future.

was an instance in the development of current CM. Once CM achieved its closure, it is established that CM the final account of its domain. Previous accounts of phenomena in that domain are not relevant.

In PR, Massimi wants to retain an epistemic humility when it comes to truth and progress of science. That is, she does not want to suggest that our current perspective is the final account of the mind-independent world. By contrast, in the Core Realism+Closed Theories view, a closed theory *is* the final account of the phenomena it applies. The humility appears at a different level. Once that domain is the domain of a closed theory and no further changes can be made, the progress will come by obtaining a novel closed theory that will apply to another closed domain. For example, CM is the final account of classical phenomena. But this does not preclude the development of QM. The latter theory will apply to another domain of phenomena, period. Because the commitment to metaphysical realism is that the world exist independently of humans, but there is no a priori knowledge of what that world is or what structure it takes. There is no necessity to claim that there is only one domain of phenomena that is real.

Massimi puts her perspectives as "historically and culturally" situated. That is, perspectives change in time and culture. However, the perspectival aspect in the Core Realism+Closed Theories view is determined by the closed character of theories applying within a limited domain. Yet, since closed theories pick out true features of the world definitely, previous stages in the development of a theory are just that and can be analysed as the steps that were required in order to arrive at the final stage of the development of the closed theory.

Massimi's PR and my Core Realism+Closed Theories view represent different forms of pluralism. PR considers an epistemic pluralism, where there are multiple ways of knowing the same historically and culturally evolving kinds. For example, the knowledge of water in statistical mechanics and in hydrodynamics belongs in different perspectives. However, water is a perspective-independent truth-maker. By contrast, the form of pluralism in the Core Realism+Closed Theories view takes it that each aspect of the world is accounted for by a closed theory, and there is no overlap between different closed theories. Consequently, QM describes quantum properties of physical systems, and in principle QM should not be made accountable for the classical properties of physical systems, for QM and CM apply in their own distinct domains.

7.2.2 Cartwright's Metaphysical Pluralism

Cartwright advocates a unique form of realism. For my purposes, I will characterise her view in the following way: (i) Cartwright's view denies the hypothesis of fundamentalism,

(ii) it focuses on models and not on laws, and (iii) it draws explicitly on scientific practice.

(i) Cartwright denies fundamentalism and the idea of the unity of science. Indeed, she does not recognise that there is a worry in asserting that theories have a limited domain of applicability, without a precise definition of that. In her words: "the theory is successful in its domain ... Theories are successful where they are successful, and that is that" (Cartwright 1999, 31). A relevant consequence of this for my purposes, is that she denies the need for intertheory relations at the theoretical level: "nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way" (Cartwright 1999, 31). Note that this does not logically preclude intertheory relations, it merely denies their necessity.

Similarly to Giere, Cartwright bases her view of science on (ii) scientific models and (iii) scientific practice. Indeed, she takes scientific laws to be neither true not representative of the world. For her, "the fundamental laws of physics do not describe true facts about reality. Rendered as descriptions of facts, they are false; amended to be true, they lose their fundamental, explanatory force" (Cartwright 1983, 54). Instead, she says that it is statements of *models* that are true, and which represent. Models represent the real things in the world and their behaviour, whilst theories are one kind of model-building tool, similar to mathematics or instrumentation, (Cartwright et al. 1995, 140). In the example of the construction of the model for superconductivity, Cartwright et al. (1995) explain how, in practice, the modelling is not so theory-driven but phenomenology-driven. That is, they assess that the scientific practice that represents the physical world, is best captured by the model. The fundamental laws are true of the model and not of the world; models are true of the world. Cartwright et al. (1995) claim that the superconductivity model was constructed at a phenomenological level, independently of the theory. Thus the divide between models and theories.[10]

The typical example illustrating Cartwright's view is that of Neurath's bill, developed in (Cartwright 1994). Consider a banknote being swept away by the wind. The question is to predict where it will land. Evidently, Newton's laws by themselves are unable to provide a model that accounts for this phenomenon, and hence they are not true. Because considering $F = ma$ on its own does not specify what forces are involved. If only gravity is used, this will give an inaccurate prediction (free fall). In turn, the law itself does not allow us to consider the force exerted on the note by the wind. Rather, it is

[10]Da Costa and French criticise this view. They consider two consequences of identifying models of the phenomena with phenomenological models: "(1) Models of phenomena are regarded as 'independent' of, or autonomous from, theory, and (2) models of phenomena are regarded as 'true' or 'true to the phenomena', whereas theoretical models 'lie' and are false" (da Costa and French 2003, 71-72). Instead, they defend the semantic view whereby a theory is defined by a set of models, and no model is simply true but only 'quasi-true', see (da Costa and French 2003, 74). In addition, they also criticise Cartwright's approach on the grounds that she does not really provide an account of what are we mean to understand by 'true' when she says that laws are true of the model, or models are true of phenomena.

fluid dynamics that is needed. This shows that the theory is true in virtue of providing an empirically adequate model which describes the aforementioned phenomenon. Moreover, Cartwright (1994, 360) claims that "fluid dynamics can be both genuinely different from and genuinely irreducible to Newtonian mechanics. Yet both can be true at once because both are true only in systems sufficiently like their models, and their models are very different". This is based on her conception of laws as *ceteris paribus* clauses. This is to say that, for example, $F = ma$ "is true so long as no influences on the acceleration occur that can not be modelled as a force" (Cartwright 1994, 360). Consequently, the domain of applicability of the law is restricted to its domain of validity. What describes the world is the models.

There is significant literature discussing the view of Cartwright. My concern is with QM, and a difference between her view and Giere's and Massimi's, is that Cartwright has done specific work in the domain of the interpretation of QM.

Let us discuss how she conceives of the relation QM-CM. It is easy to identify Cartwright's view by contrast with theory reduction, particularly considering Nagelian model. Because Cartwright denies fundamental theories or laws, there is no need for the Nagelian deduction: "quantum states and classical states can live peacefully in the world together" (Cartwright 1994, 363), that is, without having to reduce one another. In addition, this is not a view only about QM-CM, indeed: "Nature is not reductive and single minded. She has a rich and diverse tolerant imagination and is happily running both classical and quantum mechanics side by side" (Cartwright 1994, 361). Both classical and quantum models give us good predictions in certain real world situations and are frequently employed in cooperation.

Furthermore, consider her illustration of the different scientific disciplines and their relation given in (Cartwright 1994). In the balloon image of the relations in science, she puts one balloon per theory, illustrating her pluralist conception. The balloons are independent and no special relationship bears among them and all tied to the empirical world. One balloon could be QM and another CM. They both generate models that are true of some experiments. And that is all that is needed for Cartwright to realistically consider a model.

Cartwright's Pluralism and Core Realism+Closed Theories

Core Realism+Closed Theories has fruitful connections with Cartwright's pluralism. Indeed, Bokulich (2008a, fn. 2, 29) has noted the relationship between Heisenberg's and Cartwright's pluralisms. She says that both views agree in conceiving that there are distinct domains in nature that are captured by distinct theoretical frameworks (classes of models or laws). And that their difference is that Heisenberg has the extra hypothesis that a closed theory is an accurate and final description of its domain, whereas no such hypothesis underpins Cartwright's view. Now, clearly Cartwright's and Heisenberg's views contrast with the previously discussed forms of perspectivism, where either truth is relative to a perspective (with Giere) or is an objective truth but situated within a historical/cultural perspective (with Massimi).

In Chapter 2 I put the standard view of QM in the Received View. Both Cartwright's and Closed Theories plainly reject the standard view on the relationship QM-CM. In particular, Cartwright rejects the relevance of the problem of recovering classicalities from QM:

> There are both quantum and classical states and the same system can have both without contradiction. It is important here that I say classical states, not quantum analogues of classical states. There is no contradiction built in because we have no theory (nor even a good programme for such a theory) of the relation between quantum and classical characteristics. As with all cases of genuine theoretical pluralism, what we have to do is look for what connections there are and where they are. The job we have to undertake is not that of solving but rather of hunting the quantum measurement problem.
>
> (Cartwright 1994, 362)

Indeed, Cartwright explains that the standard view to QM wrongly attempts to obtain macroscopic reality from QM via the partially tracing out degrees of freedom of a subsystem, reducing the superposition to a mixture.[11] Because QM requires quantum superpositions. Hence she denies that the problem is to replace the superposition with a mixture. Rather, she says that the problem is "to explain why we mistakenly believe that a mixture is called" (Cartwright 1974, 229). This point is crucial in contrasting my view and Cartwright's.

My Core Realism+Closed Theories view is compatible with her assessment. Similarly, my Section 6.4 in Chapter 6 argued that the realist's task should be to interpret the quantum state without requiring to replace, recover, obtain, classical features. Perhaps recovering classicality from the quantum may be possible, but that is not logically required in order that an interpretation of QM is obtained. Furthermore, the realist who tries to account for CM from QM encounters the difficulties that I discussed in Chapter 3. Those difficulties are an additional motivation to consider a novel vantage point, beyond the lack of logical necessity of the standard QM-CM relationship.

[11] This was discussed in technical detail in Chapter 3.

However, my Core Realism+Closed Theories view disagrees with Cartwright in how to respond to that assessment. Cartwright (1983) considers that classical objects can be described by QM. Thus, she allows mixing domains. But in my Core Realism+Closed Theories view, each closed theory has a limited domain of application where the theory is the final account of the domain. Hence, cross-domain is a no thoroughfare. According to Core Realism+Closed Theories, QM describes quantum systems, quantum features of the world; CM describes classical systems and explains classical phenomena, classical features of the world. The notion here is that closed theories apply in domains. Domains should not be understood as ranges of a variable, such as "for low energies", nor as spacial localisation. They should be understood similar to Massimi's notion of perspective.

Cartwright claims that the reason why the collapse of the wavefunction is rejected is because the formalism is being taken 'too seriously'. The measurement problem is denied by her: "There is no real problem because there are not two different kinds of evolution in quantum mechanics. There are evolutions that are correctly described by the Schroedinger equation, and there are evolutions that are correctly described by something like von Neumann's projection postulate. But these are not different kinds in any physically relevant sense" (Cartwright 1983, 198).

Instead, the Core Realism+Closed Theories view does take the formalism of the theory seriously, and considers that the collapse and its non-unitarian time evolution should be rejected. And the problem then is to interpret the formalism, without adding foreign conditions, as the e-e link for instance. Recalling the various interpretations I discussed in Chapter 2, one recognises the familiarity of this scheme with the project of the modal interpretations. I will come back to this in Chapter 8.

In short, a central difference between the Core Realism+Closed Theories and Cartwright's views is that the former does not presume that we know already what the world is like. Cartwright presumes that we already know the world and the relevant task is to find the models that map it. In her famous balloon illustration recalled above, she illustrates how she takes the world to be. She knows already that all there is to the world is that nice park and the theories are attached to the parks entities. Rather, my view is that we learn the ways the world is via scientific knowledge obtained from science. In Core Realism+Closed Theories, the realist has an open mind and she is ready to learn novel features of the world, unknown to us beforehand. QM captures features of the world, which are not classical, and the question for interpretation is to make sense of QM in relation to a physical reality that we are discovering.

7.3 Conclusions

I have discussed and specified my framework of Core Realism+Closed Theories as a form of realism that intends to approach the challenging interpretative question of QM in a novel manner. The basis my view is the Core Realism framework discussed in Chapter 2. In Chapter 6 I supplemented Core Realism with the view of Closed Theories based on Heisenberg's philosophy; hence obtaining the Core Realism+Closed Theories view. This chapter has specified what my framework amounts to in relation to other relevant forms of realism that were not particularly designed to interpret QM, however.

By contrast with Giere's scientific perspectivism, I showed that my view agrees with a metaphysical commitment to the existence of a world that is independent of humans. Yet, my view and his disagree in the epistemic realism: Giere's view advocates that science provides relative truths, not truths about the world. His view account for the world 'from a perspective'. In turn, according to my Core Realism+Closed Theories view, closed theories capture features of the world independent of us.

I also presented a contrast with Massimi's PR. Massimi's is an epistemic view, whereby theories capture true features of the world, but that world is known perspectivally since theories are historically and culturally situated. In my view, theories are closed by arriving at their final stage of development and applying to a limited domain. I discussed that both my view and hers share unresolved questions that require further research: what is the theory of individuation of perspectives/closed theories? Is there an inter-perspectives/inter-closed theories relation?

Finally, I presented a contrast with Cartwright's view. She holds a significantly similar view to my Core Realism+Closed Theories. Both views consider that there are different domains in nature. A difference between my view and Cartwright's is that hers conceives cross-domain interchanges, so she allows that QM can describe classical systems. That is not allowed in Closed Theories, for a closed theory applies in a limited domain and it is the final account of that domain. Cartwright also requests that we do not take the formalism of the theory seriously, and thus there is no measurement problem since, according to her, it just happens that some events do not follow Schrödinger unitarian evolution. However, she admits that something like the collapse happens. My view also dissolves the measurement problem, but for another reason. In my analysis the measurement problem is underpinned by the idea that QM has to recover CM, which I reject. Instead, the interpretative question about the nature of quantum systems does not assume that QM is a universal theory that has to account for the domain of ordinary macroscopic objects.

In the next chapter I will present the general conclusions of the thesis and I will return to discussing issues related to QM specifically. I will revisit questions about one of the interpretations mentioned in Chapter 2, Modal Interpretations. I will argue that my Core Realism+Closed Theories view can provide a life saving attempt at this abandoned research project. Furthermore, I will revisit two topics discussed in Chapter 3 and 4, namely, decoherence and the Rydberg atom.

Chapter 8

Conclusions and Prospects

The current disagreement amongst the realist interpretations of QM is substantial. If a quantum object is something real and its nature does not actively depend on humans, what kind of thing is it? The answer to this question must come from an interpretation of the theory. A central claim in this dissertation is that, despite their differences, the main realist interpretations follow the specific methodology of the Received View. In Chapter 2 I proposed that the Received View suggests the realist interpretations to spell out the nature of quantum objects indirectly. The Received View's concern is to explain how classical objects arise from quantum objects, whatever their quantum nature is. I showed that such a framework captures the interpretations by making explicit reference to primary sources from the many interpretations. The appreciation of the philosophical character of that Received View enables me to question its basis.

Indeed, I have tried to shift the debate. I have tried to free realism from the demand of having to capture the classical from the quantum. My main argument is that the Received View is mistaken because the realist's first question ought to be 'what is a quantum object?' and not about the relation between quantum objects and tables and chairs. That is an issue of intertheory relations, and we know that quantum objects and tables are distinct types of objects. Thus, it is only at a second stage that the intertheory question could reasonably appear: 'and now that we know what quantum systems are, how do they relate with tables and chairs?'. If the realist intends to provide an account of the nature of quantum objects by assuming that the familiar macroscopic ontology arises from it, I think she is putting an extra hypothesis in the quantum. My view, the basis of which is my Core Realism, rejects the priority of such a claim. However, it does not deny the intertheory relation entirely. Core Realism says that the nature of the quantum objects should be clearly and agreed-upon before addressing the relation between QM and CM. Given that the nature of quantum objects is not yet easily understood, there are no firm grounds to demand that quantum objects have to 'recover' tables and chairs.

Indeed, in Chapter 3 I discussed the difficulties that certain theoretical devices have in order to spell out how CM appears as a result of QM. In particular, does decoherence recover classical states or does it obtain states that could be approximately conceived as resembling a classical nature, but are not classical at all? It is questionable to claim that although improper mixtures are precisely what the formalism gives us, they approximately describe an objective entity whose properties are disentangled form their environment. That would depend on the belief that there is an underlying description of that object. But no such description exists because the probability claims derived from improper mixtures cannot be interpreted as originated in a lack of existing information (i.e by ignorance). I argued that the well-known conceptual difference between an improper mixture and a description of a classical object has to be considered by the philosopher. Yet, decoherence plays a crucial role in cutting-edge physics despite the conceptual difficulties in interpreting QM, and despite the generally recognised criticisms of decoherence as a solution to the fundamental problems. Therefore, evidenced by the robust empirical success achieved by modern physics, at least something like decoherence has to happen.

However, even if the traditional view with decoherence and so on succeeded in showing how CM appears in the limit of the QM, there is a further problem. Chapter 4 discussed a significant and successful field of physics based on something that the Received View cannot accommodate. In modern semiclassical mechanics, quantum phenomena is explained by classical trajectories that, whilst knowingly unreal in that context, cannot be de-idealised or eliminated. Indeed, classical trajectories play an indispensable epistemic role in explaining phenomena that is meant to be quantum. To capture this counter-intuitive feature is the defining merit of interstructuralism, the novel view on intertheory relations specifically designed for the pair QM-CM articulated by Bokulich. In Chapter 5 I critically engaged with the philosophical questions involved in interstructuralism, such as the assessment of its associated notion of explanation. However, I concluded by pointing out that, even if one takes interstructuralism to definitely account for the QM-CM relation, the question remains: what is a quantum system? Of course, interstructuralism does not have to answer that question. However, in line with my criticism to the Received View and given the evident uncertainty in accounting for quantum objects within realism, Core Realism undermines the project of establishing the intertheory relation before answering the more important interpretative question. Therefore, I concluded that a novel realist framework that does away with the tradition is required. Drawing on (Bokulich 2004, 2008a), I proposed that the philosophy of Heisenberg could help us.

This thesis motivated considering an alternative approach to the Received View and the associated realist interpretations. The core of realism was supplemented with a view on physical theories. I critically examined the philosophy of Heisenberg, namely, the Closed Theories view, and proposed a novel approach to interpreting QM by combining it with Core Realism. I called it the Core Realism+Closed Theories view. In Chapter 6 I assessed it through a novel criterion articulated by Saatsi (2015): the distinction between 'exemplar' and 'recipe' forms of realism. Following Saatsi's criterion, I concluded that my view should at least be granted some sympathy. However, I noted that further discussion was necessary in order to distinguish my view from other forms of realism that also involve some notion of plurality. In Chapter 7 I specified my view by relating it with others, namely, Giere's and Massimi's perspectivalisms and Cartwright's metaphysical pluralism. From these discussions I hope to have conveyed better what my proposal is and what it is not. Nevertheless, I did not propose a novel interpretation of QM. I made the claim that my Core Realism+Closed Theories view is a framework within which the crucial questions about QM could be properly addressed.

Having summarised the most important considerations in this thesis, I want to conclude with two discussions that will make reference to other content in this thesis not mentioned above and indicate areas for future research. Firstly, my Core Realism+Closed Theories view has to accommodate the empirical success of decoherence, regardless of the conceptual issues that I raised in Chapter 3. Secondly, Modal Interpretations were abandoned mainly because they did not match with the properties selected by decoherence. I will argue that the view defended in this thesis could resuscitate the Modal Interpretations, or at least their associated research programme.

8.1 Decoherence-Up and Rydberg Atoms-Down: Simplifying Heuristics

Recall that in Chapter 2 I characterised the Received View of the realist interpretation of QM in terms of the priority given to addressing issues concerning intertheory relations. The Received View follows a recipe: it says that however quantum objects are conceptualised, they need to provide an account of the appearance of the macroscopic world. Instead, my Core Realism+Closed Theories view, proposed and examined in Chapters 6 and 7, interprets each theory independently, case by case (in an exemplar fashion). This is compatible with the realist intuition according to which the question for interpretation is concerned with the relationship between the theory and the world, not between the theory and the extent to which the world is classical. Thereby Core Realism+Closed Theories presents a clear contrast with the Received View.

Nevertheless, despite the philosophical arguments that I have deployed throughout this thesis, the Received View enjoys substantial empirical merits: it can account for the process of decoherence, as discussed in Chapter 3. In addition, with the aid of interstructuralism, semiclassical mechanics and its associated mixed phenomena can also be accommodated by the Received View, as discussed in Chapter 4.

Within the Received View decoherence exhibits evidence of an instance of a transition from the quantum to the classical. Quantum correlations between a system and the environment do not disappear but are delocalised into the degrees of freedom of the environment, and the apparatus is forced onto an einselected pointer basis, as discussed in Chapter 3. That is a case of the classical appearing from the quantum. This is compatible with the traditional view in which QM is more fundamental than and a successor to CM. The intertheory relationship involved here goes from the ground-up.

Yet, there is also the 'converse', phenomena that are quantum but the classical is necessary for their explanation. The tension is that the latter case is not straightforwardly accommodated by the Received View. The Received View conceived of the relationship from the ground up, but the semiclassical phenomena and their explanation in terms of classical trajectories indicate a top-down direction. The paradigmatic case is exhibited by the spectrum of Rydberg atoms (a quantum phenomenon), which can only be explained through classical trajectories theory. I discussed this in detail in Chapters 4 and 5. Bokulich's interstructuralism is an account for the explanation of such unusual phenomena. I have argued that despite its unique exemplar-based conception of intertheory relations, its ontological commitment keeps interstructuralism within the framework of the Received View.

Therefore, the Received View has significant success. Hence, if I propose a different view, such as Core Realism+Closed Theories, I should also provide some account for those phenomena that the Received View successfully accounts for. It will be illustrative to focus on decoherence and Rydberg atoms, as they undeniably appear in the practice of physicists.

In order to indicate how my Core Realism+Closed Theories view could account for the empirical power involved in decoherence and Rydberg atoms, recall a discussion from Chapter 5. There I incorporated the meta-tool that Hey (2016) developed to account for models in science that, whilst they get the results right, do not capture the 'world' correctly. I argued that the models of Rydberg atoms could be seen in terms of Hey's 'simplifying heuristics'.

Hey's simplifying heuristics are helpful with regards to decoherence too. To consider decoherence as a simplifying heuristic can allow my Core Realism+Closed Theories view to accommodate the empirical success of decoherence without having to infer ontological commitments from that success, whilst appreciating the conceptual challenges involved, which I discussed in Chapter 3. The challenge is essentially that the crucial step in explaining decoherence involves tracing out the degrees of freedom of the environment. This obtains a mixed state that allegedly represents the system. However, the density operator that is obtained by tracing out degrees of freedom of one of the subsystems is a very particular type of density operator. It is an 'improper mixture'. I noted that the physics literature does not always recognise this notion and physicists are not trained to understand the conceptual implications. An improper mixture does not represent the subsystem simpliciter. It provides the correct empirical predictions for observables that pertain to that subsystem only, which is fine for practical purposes. However, the subsystem is still entangled with the environment and the improper mixture cannot distinguish this, because they are mathematically the same. That is, if you are given a density operator, you cannot know whether it is a proper or improper mixture, unless extra information of the physical system is known too. Moreover, the probability statements derived from the improper mixtures cannot be interpreted by ignorance. That is, they cannot be conceptualised in relation to existing information that is available but inaccessible. And there is no current understanding of the meaning of this type of probability: what do they mean?

However, the Received View cannot resolve the conceptual issues involved in the account of decoherence. Because the advocate of the Received View is content with a theoretical device that works 'for all practical purposes'. This argument manages to neglect the conceptual issues. If the realist is content with merely empirical success, then she is forced to be satisfied with decoherence. The reason that decoherence 'works' is that those conceptual issues mentioned above do not matter for the purposes of accounting for the experimental results. This is clear in the following way: the measurements are performed on the subsystem and the improper mixtures correctly predict the outcome of those measurements. Indeed, complex experiments with ion traps can be explained by considering decoherence.[1] Therefore, mimicking Hey's account, I can say that the details of those conceptual questions that are concerned with the 'real' physical events are irrelevant for empirical purposes. Namely, that decoherence obtains an improper mixture does not present an obstacle to physicists. It still results in successful empirical predictions. The fact that quantum probabilities cannot be interpreted as a measure of ignorance of existing information of the system does not deter the empirical success of physicists' models.

Yet, there is a benefit in recognising decoherence and classical trajectories in Rydberg atoms as simplifying heuristics. The benefit is that in my view it is acceptable to be

[1] In Chapter 3 I cited relevant physics articles.

concerned with the conceptual challenge of the improper mixtures. Consequently, this allows the realist to pursue a realist interpretation of QM, maintaining the interpretive question in the centre of attention: 'what is a wavefunction representing in the world?' or 'how could the world be the way QM says it is?'. The realist in the Received View cannot approach those questions because she is merely looking for an explanation of the classical from the quantum and thus cannot distinguish the conceptual challenge. Therefore, the realist in the Received View cannot account for her own problems. This is evident given the substantial disagreement between the interpretations, and the limited success in obtaining an account of the classical world from QM.

8.2 Resuscitation Strategy: Bringing Modal Interpretations Back to Life

As discussed in Chapter 2, MIs involve various interpretations of QM, but they share the following characteristics:[2] they are based on the standard formalism of QM and deny the collapse of the wavefunction. That is, only the accepted evolution is given by Schrödinger equation, even when measurements are performed; they do not take QM as the theory of the microscopic realm, but of nature as a whole; the property ascription rules do not simply ascribe one set of properties (in contrast with the standard eigenvalue-eigenstate link), but a number of sets of properties ascribed together with associated probabilities; finally, each set determines possible properties possessed by the system and the probabilities that these properties are actually possessed by the system.

In Section 2.2.6 I recalled that the research programme of the MIs is today considered obsolete. The most important reason is the mismatch between the properties specified by MIs and the quantities selected by decoherence interactions. This was mainly developed by (Donald 1998) and (Bacciagaluppi 2000). There are other reasons that MIs fell out of favour, such as the various no-go theorems, see (Lombardi and Dieks 2016). Relevantly, I noted that the strength of the argument against MIs crucially relies on the idea that the quantities selected by decoherence have to be considered if a 'classical' world is to emerge from QM. That is, the strength of the attack against MIs is relative to adopting the philosophical framework of the Received View. That demand is the characteristic feature of the Received View. However, if the Received View is abandoned and another realist framework takes it place, then the main argument against MIs dissolves. Indeed, my novel view of Core Realism+Closed Theories does not set recovering the classical from the quantum as a necessary condition.

[2] There are more but here I recall the main ones. A general reference on MIs is (Vermaas 1999).

Therefore, if my analysis of the Received View is at least plausible, then it will be possible to revisit the MIs' research programme with a more optimistic attitude. The Core Realism+Closed Theories view affirms a relationship between theories and the world, such that closed theories apply in a limited domain. QM captures quantum features of the world. It is still an unresolved question how to describe the limits of that aspect of the world that is quantum. What the community of scientists has is a range of experimental results and technological applications that work for all practical purposes. However, it is not clear how to say what a quantum system is.

From the characteristics of MIs mentioned above, perhaps the only one that requires discussion in light of Core Realism+Closed Theories is that QM is meant to be 'a theory of nature as a whole'. I do not take this to mean that QM is universal; I do not think it means that everything in the universe is quantum and every fact can be described quantum mechanically. In my view, that QM is theory of nature means that it is applicable to any physical system, which entails that what QM picks out/describes/represents from the system are its quantum features. Hence, QM does not necessarily have to recover the system's classical features.

Hence, if the Received View is abandoned and there is no specific requirement to recover classical reality from QM, then MIs can be retained as a research programme. My Core Realism+Closed Theories view can be a framework within which MIs can be considered. Within the Received View, MIs cannot be held, because the latter does not satisfy the necessary conditions of the former. But, as I have argued, recovering CM is not a philosophically necessary requirement. Indeed, my Core Realism+Closed Theories view does not require that.

A further challenge within MIs was that there are different accounts of the sense of modality involved. The crucial notion of modality was not mature enough and different proponents of MI disagreed on this, see (de Ronde 2011). The notion of modalities in the MIs prompt the realist to develop a metaphysics of it. If we free the MIs from having to recover the classical features of the world, then we would be offering the possibility of exploring a range of metaphysical tools, such as versions of 'modality'. Perhaps none of the tools that current metaphysicians consider can accommodate the notion of modality involved in QM. Indeed, a major criticism of metaphysics is that it does not acknowledge the developments of science. However, as French and McKenzie (2012) argue, this is not a reason to abandon metaphysics altogether. Neither will it mean that MIs are wrong. On the contrary, this would suggest a fruitful collaboration between philosophy of science and metaphysics: metaphysicians should forge a novel tool based on the conditions that the philosopher of physics assesses as necessary for the purposes of a realist interpretation of QM.

Engaging further with MIs and metaphysical notions of modality is left for future works.

Appendix A

Quantum Superposition

Let us discuss the difference between quantum and classical superpositions. I am following (Cohen-Tannoudji et al. 1977, 252-255). See also (Schlosshauer 2007, Sec. 2.4) and many other references.

In QM, because of the linear character of the Schrödinger equation, the linear superposition of solutions is also a solution. In fact, a vector in the Hilbert space can be expressed in different bases, as linear superpositions. Furthermore, there is nothing formally special about a vector described as a one-term vector. The basis in that case contains that vector. If the system is described by that vector, the superposition is of one term only. In other bases it may be described as superposition of more terms.

Take it that $|\psi_1\rangle$ and $|\psi_2\rangle$ are two orthonormal states that are eigenstates of observable B with respective eigenvalues b_1 and b_2 (non-degenerate eigenvalues). Let us say the system is in state $|\psi_1\rangle$, and let us calculate the probability that (non-degenerate) eigenvalue a_n with associated eigenstate $|u_n\rangle$ is obtained when measuring observable A on the system in state $|\psi_1\rangle$.

$$P_{|\psi_1\rangle}(a_n) = |\langle u_n|\psi_1\rangle|^2. \tag{A.1}$$

Analogously, the probability of finding a_n when A is measured on system in state $|\psi_2\rangle$ is

$$P_{|\psi_2\rangle}(a_n) = |\langle u_n|\psi_2\rangle|^2. \tag{A.2}$$

Now consider a superposition

$$\lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle , \tag{A.3}$$

with $|\lambda_1|^2 + |\lambda_2|^2 = 1$.

Now, say again that we measure A. If the state $\lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle$ did represent a statistical mixture of systems (a proper mixture) in $|\psi_1\rangle$ and $|\psi_2\rangle$, we could think that in an ensemble of N systems, N$|\lambda_1|^2$ would be in state $|\psi_1\rangle$ and N$|\lambda_2|^2$ in $|\psi_2\rangle$, which in turn would mean that the probability of finding a_n would be

$$P(a_n) = |\lambda_1|^2 P_{|\psi_1\rangle}(a_n) + |\lambda_2|^2 P_{|\psi_2\rangle}(a_n). \tag{A.4}$$

Yet, this is mistaken, as it entails inaccurate physical predictions. The superposition in eq. (A.3) cannot be understood as it describing a situation where there is probability $|\lambda_1|^2$ that the state is $|\psi_1\rangle$ and probability $|\lambda_2|^2$ that the state is $|\psi_2\rangle$. The superposition in eq. (A.3) entails the following probabilities:

If we measure A, the probability to find u_n if the system is in state (A.3) is given by

$$\begin{aligned} P(a_n) &= |\langle u_n| \Big(\lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle\Big)|^2 && \text{(A.5)} \\ &= |\lambda_1|^2 P_{|\psi_1\rangle}(a_n) + |\lambda_2|^2 P_{|\psi_2\rangle}(a_n) + \\ &\quad \lambda_1\overline{\lambda_2} \langle u_n|\psi_1\rangle \langle\psi_2|u_n\rangle + \lambda_2\overline{\lambda_1} \langle u_n|\psi_2\rangle \langle\psi_1|u_n\rangle . && \text{(A.6)} \end{aligned}$$

The last two terms are the quantum interference terms, which depend crucially on both the moduli $|\lambda_{1,2}|$ and their relative phase!

Ignoring details of the thought-experiment, the state of the poor little cat in Schrödinger's paradox is $\frac{1}{\sqrt{2}}(|\text{dead}\rangle + |\text{alive}\rangle)$. Yet, the cat *cannot* be thought as being dead or alive, not can it be a statistical mixture of N/2 systems with a live cat and N/2 systems with a dead cat. It is in a quantum superposition of both dead and alive.

A[1] statistical mixture cannot be described as a vector in the Hilbert space like the case we discussed above, in eq. (A.3). Statistical mixtures are described in terms of density operators (called also statistical operators, or density matrices) – which form also a Hilbert space, a Hilbert space which operates on the Hilbert space of the vector states. Yet, density operators can also be used to describe pure states. In the case of a pure state $|\phi\rangle$, the density operator is

$$\rho = |\phi\rangle \langle\phi| . \tag{A.7}$$

This operator has many interesting properties discussed widely in the literature (e.g. the cited (Cohen-Tannoudji et al. 1977)). For our purposes it suffices to remind that in the pure case ρ is a projector operator with $\mathrm{Tr}(\rho) = 1$. If the trace of the density matrix is < 1 then it is not a pure state and it represents a mixture.

We discussed above that if the state of the system is a pure state given by $\lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle$ this is a quantum superposition and it cannot be understood as a mixture of systems in $|\psi_1\rangle$ and states in $|\psi_2\rangle$, with proportions given by the coefficients $\lambda_{1,2}$. This case can

[1] Here I am following (Cohen-Tannoudji et al. 1977, 297-ff).

also be described by a matrix operator

$$\rho = \Big(\lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle\Big)\Big(\overline{\lambda_1} \langle\psi_1| + \overline{\lambda_2} \langle\psi_2|\Big) \tag{A.8}$$

$$= |\lambda_1|^2 |\psi_1\rangle \langle\psi_2| + \lambda_1\overline{\lambda_2} |\psi_1\rangle \langle\psi_2| + \lambda_2\overline{\lambda_1} |\psi_2\rangle \langle\psi_1| + |\lambda_2|^2 |\psi_2\rangle \langle\psi_2| . \tag{A.9}$$

Of course, in this case there are off-diagonal terms in the density operator, which entail coherence or interference between certain components. Obviously, because this is a pure state, there is a way to write this density operator as one term, by defining a vector $|\chi\rangle = \lambda_1 |\psi_1\rangle + \lambda_2 |\psi_2\rangle$ in the basis. In that case the density operator is $|\chi\rangle \langle\chi|$. This should not be taken to mean that this state is 'classical' or that it does not have quantum properties, see (Schlosshauer 2007, 35).

Now, it can be the case that our quantum system is a statistical mixture. This can be due to having insufficient information about the state of the system. We might know that the system is in a pure state but we might not have enough knowledge to decide which one and we might have just a probability for different pure states. In this case the system cannot described by a pure state in the same Hilbert space as the quantum superposition in the previous case eq. (A.3), but it can be described by a statistical operator. If we consider an ensemble of N systems, with Nλ_1 in state $|\psi_1\rangle$ and Nλ_2 systems in $|\psi_2\rangle$ λ_1 then the system is described by

$$\rho = |\lambda_1|^2 |\psi_1\rangle \langle\psi_1| + |\lambda_2|^2 |\psi_2\rangle \langle\psi_2| . \tag{A.10}$$

In this case, the probability of finding a_n when measuring A would be

$$P(a_n) = \mathrm{Tr}(\rho |u_n\rangle \langle u_n|) \tag{A.11}$$

$$= |\lambda_1|^2 |\langle u_n|\psi_1\rangle|^2 + |\lambda_2|^2 |\langle u_n|\psi_2\rangle|^2 \tag{A.12}$$

$$= |\lambda_1|^2 P_{|\psi_1\rangle}(a_n) + |\lambda_2|^2 P_{|\psi_2\rangle}(a_n), \tag{A.13}$$

which is the case we mentioned in eq. (A.4).

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