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Causation, Realism, Determinism, and Probability in the Science and Philosophy of Max Born

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Abstract

In this thesis I will examine the philosophy of the physicist Max Born (1882-1970). As well as his scientific work, Born wrote on a number of philosophical topics: causation, realism, determinism, and probability. They appear as an interest throughout his career, but he particularly concentrates on them from the 1940s onwards. Born is a significant figure in the development of quantum mechanics whose philosophical work has been left largely unexamined. It is the aim of this thesis to elucidate and to critically examine that work.

I will give a defence of presentist historiography in the history and philosophy of science and a (relatively) brief biography of Born. With regards to causation, the thesis will argue that he holds that there exist principles regarding causal relations that have guided the development of physics and have, in the modern formulation of the subject, been confirmed as having an empirical status. I will argue that he is a selective realist, initially with regards to invariant properties and, later on, a structural realist. With regards to determinism, I will argue that Born has produced an argument, compatible with modern philosophical definitions of determinism, that we were never entitled to conclude from the success of classical mechanics that the world was deterministic. Finally, I will argue that Born holds an objective interpretation of probabilities in quantum mechanics which, due to his strong belief in the physical reality of quantum-mechanical probabilities and his apparent disbelief in the superposition of the wave-function, is most likely a long-run propensity theory.

Causation, Realism, Determinism, and Probability in the Science and Philosophy of Max Born

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Table of Contents

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Introduction

In this thesis I am going to explore the philosophy of quantum scientist Max Born (1882-1970). Born was instrumental in the development of the new quantum mechanics, in particular giving the statistical interpretation of the wavefunction (1926a). He was the head of the theoretical physics group at Göttingen from 1921-1933 and worked closely with Werner Heisenberg (1901-1976) and Pascual Jordan (1902-1980) to develop matrix mechanics. Although he was not jointly awarded the Nobel Prize in the 1930s for his work with Heisenberg, he was awarded the 1954 prize for his development of the statistical interpretation of the wavefunction in 1926.

As well as his scientific work, Born wrote on a number of philosophical topics— causation, realism, determinism, and probability. They appear as an interest throughout his career, but he particularly concentrates on them from the 1940s onwards. Born is a significant figure in the development of quantum mechanics whose philosophical work has been left largely unexamined. It is the aim of this thesis to elucidate and to critically examine that work. My general methodology is to first try to work out Born's position on a particular topic and then to examine modern philosophical positions, with the aim of determining which, if any, is the best fit. The thesis is structured as follows:

Chapter 1—Historiographical Prolegomenon. The historiography of this thesis is presentist in nature: I aim to use contemporary philosophy to elucidate and understand Born's views. This is a methodology that is frequently regarded as problematic by historians of science. In this chapter, I will examine the potential problems that can (not must) arise from presentist history and offer a defence of it. I will do this by examining arguments concerning Whig history, triumphalism, and chronologically wide-ranging historical surveys, as well as looking a case study in the history of alchemy and early modern chemistry. I will also examine the historiography of scientific biography through the lens of three biographies of Isaac Newton.

Chapter 2—The Born Identity. This chapter will give a (relatively) brief biography of Born. Although the primary focus of this thesis is Born's work, it is also worth giving an overview of his life. This is both because he is not so well-known as other major figures in the history of quantum mechanics—for example, there exists no academic biography of him, only a popular one—and because it is worth examining his education and trying to set his work in context.

Chapter 3—Born on Causation. This chapter is a critical examination of Born's book, *Natural Philosophy of Cause and Chance* (1949a). Originally delivered as the 1948 Waynflete lectures at Magdalen College, Oxford, it deals with Born's views on causation in physics. Born argues that causal relations in physics ought to obey two principles: contiguity, which ensures causal connection; and antecedence, which ensures causal priority. He argues that these principles have both affected the development of physics, and have in modern physics gained an empirical, rather than metaphysical, status. In this chapter I will give a critical examination of his argument and then look at what sort of philosophical status he means his principles to have. This will be aided by looking at a 'zoo' of potential sort of principles. This zoo contains Kant's synthetic a priori principles, Chang's principles of intelligibility, Meyrson's Principles of Identity, Watkins' confirmable and influential metaphysics, and Zahar's heuristically superior metaphysical principles. I will argue that they are best regarded as combination of Zahar's and Watkins' positions. Finally, I will examine whether or not certain aspects of modern physics—spooky action-at-a-distance and relativistic time-travel—might cause problems for Born's thesis and will conclude that they do not.

Chapter 4—Born on Realism. This chapter will examine Born's position with regards to scientific realism. Born engages explicitly with the topic in a number of papers, arguing against positivism and for a position termed 'invariant realism'. Invariant realism argues that we should only be realist about such physical properties, along with the entities which bear them, that are invariant under transformation. This position will be examined and it will be argued that it meets the criteria—metaphysical, semantic, epistemic and progressive—to be classed as variety of selective realism. I will also examine a later paper of Born's (1966) in which he advances a position that is much more in line with epistemic, but not ontic, structural realism than his earlier work.

Chapter 5—Born on Determinism. This chapter will examine Born's paper *Is Classical Mechanics In Fact Deterministic?* (1955). In this paper, Born argues that we did not have good reason to think that the success of classical mechanics implied that the world was deterministic, and hence there is far less reason to worry about the indeterminism of quantum mechanics. His argument rests on two claims: firstly that there is a necessary non-zero error in all classical measurements which leads to classical determinism being empirically indistinguishable from indeterminism, and secondly that any such indistinguishable claims must be considered to be physically (but not necessarily semantically) meaningless. Born's argument will be examined in detail. This examination will be aided by some modern work on chaotic systems. Finally, a reconstruction will be offered in terms of contemporary definitions of determinism.

Chapter 6—Born on Probability. This chapter will examine Born's position with regards to the interpretation of probability in physics and, in particular, quantum mechanics. Although Born does discuss probability a number of times across his work, he is not explicit about how he interprets it (although he is certainly aware that there exists a debate about the matter). As such, I will start by giving an account of the standard interpretations of probability—the classical/logical interpretation, the subjective interpretation, the frequency interpretation, and the propensity interpretation —as well as looking at what positions on Born's part would indicate that he holds one of those interpretations. I will conclude that it is quite obvious that Born holds an objective interpretation of probability, but in order to say which (frequency or propensity) we need to look in detail at his work on quantum mechanics. Hence this will be followed by an examination of *On the Quantum Mechanics of Collisions* (Born 1926a). Via an argument of Cartwright's (1987) concerning Born's work on quantum mechanics in the 1920s, supported by an examination an exchange of letters between Born and Einstein on the subject of EPR correlations (Born 2005), I will conclude that it is most likely that Born held a long-run propensity view of probability in physics. I will finish by taking a detailed look at Cartwright's (1987) claim that Born holds something like a modal interpretation of quantum mechanics, concluding that his views are at least related but that there is insufficient evidence to place him as being aligned with any of the full-blown contemporary modal interpretations.

Chapter 1—Historiographical Prolegomenon

1   Introduction.

In this project I want to examine Max Born's position on a number of philosophical issues. These include his views on causation in physics—this involves looking at his own work but also that of contemporary philosophers. I also want to look at how he interpreted probabilities in quantum mechanics—this involves examining his writings and comparing his position to those in the philosophical interpretation of probability. I'm going to give an argument that Born was a scientific realist—this involves looking at some contemporary philosophy.

Questions like this can be extremely problematic. It might look like I'm engaging in Whiggism and presentist history. I'm happy to admit that I am presentist, but I do not think that presentism necessarily involves all of the many and varied sins of Whig history—the assumption of the inevitability of the course of history; the assumption of continual progress as that course is followed; the judging of historical actors by the standards of the present; an unwillingness to understand the past on its own terms.

I am a presentist in that I use contemporary philosophical ideas to in my examination of Born. However, I do not want to force modern terms upon him, but rather to see where, if anywhere, his ideas fit within a modern framework. It is also true that I am interested in Born's work precisely because of its historical consequences—in effect my history is motivated and informed by the present. On the other hand, I want to avoid a 'Viking' approach to the history of science, in which philosophers of science see history as a toolbox to be plundered for useful examples, as French (2014 50) regards analytic

metaphysics to be (I say nothing on whether or not metaphysics is an appropriate target for philosophical freebooters).

In this chapter I am going to examine present-centred history and the problems that it can lead to—Whiggism, triumphalism, the assumption of the continuity and transmission of ideas across history—and argue that none of these are unavoidable. I'm going to discuss historiographical methodology and offer a defence of presentism within historically focussed philosophy of science and integrated history and philosophy of science. Specifically, I'm going to argue that although presentism, construed as the study of the past with reference to the concepts and ideas of the present, can be problematic, it is not necessarily so. I do not want to argue that we *ought* to study the past in this way, and will happily concede that historians of science may perfectly legitimately find such studies uninteresting. But I do want to argue that philosophers of science ought to be allowed to ask and answer questions in this way and that this can be done in a rigorous and historiographically defensible way.

I'm also going examine some elements of the debate between William R. Newman and Ursula Klein regarding the influence of alchemy on the development of chemistry. This debate is illustrative because both Newman and Klein make criticism of each other of the kinds that are usually aimed at presentist history.

This thesis is primarily a discussion of a single figure, and part of it is biographical in nature. Because of this, I will discuss particular problems arising from the historiography of scientific biography, examining this question through the lens of a comparison of various biographies of Newton.

Now, philosophers of science have in the past been criticised for the way they treat the history of science. In fact L. Pearce Williams was so ticked off by Joseph Agassi's biography of Michael Faraday that he titled his review of it 'Should Philosophers be allowed to write History?' (Williams 1975) - the answer is a rather loud NO. Williams' complaint is not just about shoddy history, but also the tendency of philosophers to use history to shore up their own views on the nature of science, and to run roughshod over good historical practice in doing so. We can also see this in a criticism that Hasok Chang's makes of Stathis Psillos: that in surveying the history of caloric whilst arguing

for preservative realism, Psillos has only picked out those elements of the theory that act as obvious of modern scientific positions. Chang diagnoses the tendency of philosophers of science to do this as 'precursoritis' (Chang 2003 906) [Chang actually writes that this is what Steven Brush would call it, but does not supply a reference].

So what are the pitfalls that can endanger an unwary (or uncaring) philosopher of science?

2 Problems for Presentism

2.1 Whig History

Whig history was first defined by Herbert Butterfield in the 1930s in his monograph *The Whig Interpretation of History* (Butterfield 1931). The term refers to the tendency of the nineteenth century Whig party to write history as a sort of tale of the inevitable, and morally correct, rise of Protestantism and Liberalism (Hall 1983). Butterfield writes that the goal of the Whig historian is to produce 'a story which is the ratification if not the glorification of the present' (Butterfield 1931 2).

> 'It is part and parcel of the Whig interpretation of history that it studies the past with reference to the present; and though there may be a sense in which this is unobjectionable if its implications are carefully considered, and there may be a sense in which it is inescapable, it has often been an obstruction to historical understanding because it has been taken to mean the study of the past with direct and perpetual reference to the present' (Butterfield 1931 11)

And so Whig history is damned. As far as possible one must study the past on its own terms, and only on its own terms. We must cast out our present-centred preconceptions of the past, and seek not to impose our thoughts and methods upon it. Of course we

should also draw attention to the second clause of the first sentence—that there is a sense in which the study of the past with reference to the present can be unproblematic.

There are of course problems with Whiggish history. Indeed it is almost tautologically problematic—Whig history is almost defined as bad history. This does not mean that one cannot do present-centred history, and it does not even mean that one cannot do excellent present-centred history. Whig history is historiographically problematic when, as Butterfield writes 'it [is]taken to mean the study of the past with direct and perpetual reference to the present' (Butterfield 1931 11). The Whig historian is incapable of taking the past on its own terms.

Within the history of science, Whiggism tends to manifest itself in the assumption that past science leads, linearly and progressively, to modern science. Revolutions and theory changes are 'good' if contemporary science accepts their results, and they are 'bad' if it does not. There is also a tendency to mythologise certain areas of and figures in science, particularly in the aforementioned case of theory change. Parts of historical science which are not precursors in modern science are often treated with contempt or simply ignored. Again, think of Chang's example of Psillos ignoring those parts of caloric theory that do not act as precursors to modern chemistry (Chang 2003 906). We can illustrate this problem precisely by taking a look to a historical preamble to the chapter on particle physics in Young and Freedman's 1600 page doorstop of a textbook, *University Physics*:

> The idea that the world is made of fundamental particles has a long history. In about 400 BC the Greek philosophers Democritus and Leucippus suggested that matter is made of indivisible particles that they called atoms...This idea lay dormant until about 1804, when the English scientist John Dalton (1766-1844), often called that father of modern chemistry, discovered that many chemical formula could be explained if atoms of each element are the basic, indivisible building blocks of matter. (Young and Freedman 2011 1465).

This is a pretty wrong headed view for several reasons—firstly it utterly ignores the fact that many, many people discussed atoms in the 2000 or so years between Democritus

and Dalton, and, more pertinently to the precise point at hand, it links the atoms of Democritus and the atoms of Dalton when really the two concepts share little. It is true that the both sorts of atomism express the concept of bigger things being composed of lots of smaller things but if this is the sense in which atomism is meant, then we really ought to include (at least) the early-modern proponents of the mechanical philosophy in this lineage. If it is not, then we ought to point that the atoms of Dalton are the bearers of chemical properties and the atoms of Democritus are not (Chalmers 2014). There is a definite teleological sense to this view of history: 'the Whig historian knows the moral of his tale before he has sat down to tell it' as Hall puts it (1983 46). The precise problem is one of misinterpretations stemming from pathologically searching for precursors—the only reason that Democritus is given as a precursor to Dalton is that it has been assumed that a) he must have had precursors, and b) a precursor can be identified merely by the use of similar language and concepts.

We can see a criticism of a Whiggish style of history in Pearce's review of Agassi. Williams writes that the picture of Faraday presented by is one of 'a slum kid with a crippling cockney accent, struggling against bitter odds to "make it" in the high-toned world of English science at the beginning of the nineteenth century' (Williams 1975 246). Williams takes Agassi's Faraday to be a man who struggled for recognition because of snobbishness, a 'rebel confronted by the orthodox "conservative" establishment' (Williams 1975 249). He thinks this is wrong, or at least that there is little evidence for it. For one thing, Faraday became part of that establishment. For another, Williams thinks that Faraday's peers struggled to accept his ideas simply because his work was difficult to understand (Williams 1975 250). Where's the Whiggishness in Agassi's account? It's in the idea of (incorrectly) presenting Faraday as a hero who struggled against orthodox snobs to have his (correct, by the lights of modern science) ideas accepted, rather than as a successful scientist whose contemporaries were slow to adopt his ideas because they were difficult to understand. A consequence of the Whiggish narrative of progress can be to treat historical actors involved in the development of successful theories as heroes and villains, depending on their positions with regards to those theories. I think we can see this in Williams' criticisms of Agassi.

Nevertheless, I think it is an error to conflate Whiggism and presentism. Whiggish presentism would read something like this: the present is better than the past, we are only interested in the past with respect to the present and "the past constitutes a linear progressive lead-up to the present". There is no reason why presentist historians would be incapable of taking the past on its own terms. It is true that presentism studies the past with respect to the present; it does not have to be true that it studies that past *only* with respect the present. That a presentist historian might be interested in the development of some science does not mean that they must start out by assuming that that development is entirely linear and progressive. We might, as Oreskes (2013) argues, be interested in some aspect of the past because of our present interests, without necessarily seeking to impose our present views on the past. Present-centred history can and ought to ask such questions whilst letting the past speak for itself.

2.2   Triumphalism

A sin of a different nature is that of triumphalism, or the preferential treatment and scholarly neglect of successful scientific theories to the detriment of those that failed. This can seem to be bound up in a Whiggish view of history—a theory which is not currently adopted by the scientific community is seen as a poor theory, and thus unworthy of study. However, what really marks Whig history as Whig history is the judging of past science by the standards of the present. It is certainly possible for a successful theory to be 'worse' in a Whiggish sense than the competing theories that it triumphed over.

Equally, as Hasok Chang has pointed out one can be triumphalist without necessarily being Whiggish. The example he gives for this is the standard historiography of the chemical revolution at the end of the eighteenth century. In his paper *We have Never Been Whiggish (About Phlogiston)* (Chang 2009a), he argues that the celebration of Lavoisier's theories over Priestley's can hardly be Whiggish as Lavoisier's work is just as "wrong", if not more so, when compared to modern science than Priestley's. The focus on Lavoisier is mere triumphalism, rather than Whiggism. Lavoisier is celebrated as the father of chemistry, but the central components of his work—the caloric theory of heat and the oxygen theory of acidity—bear no closer a resemblance to modern scientific theories that does phlogistonic chemistry (Chang 2009a 239-40). Chemistry

itself moved on fairly quickly, dropping much of Lavoisier's theory. There were probably parallel developments from the theories of both Lavoisier and Priestley to later chemical theories. Chang's point is that Lavoisier is celebrated because his theories were victorious over his opponents' *at the time*. This is not to say that Whiggism is not triumphalist—it often is—but to point out that the two can come apart.

Triumphalism is problematic because it tends to assume that theories that were not adopted by the mainstream scientific community at the time are simply not worth looking at. This not only leads to the neglect of what may be very interesting and informative historical science, but also reinforces the Whiggish conception of the history of science as one of linear and inevitable progress. The disregard for unadopted theories smacks of the assumption that things could have gone no other way than they did, implied by the view that the historical progression of science is inevitable. At the very least, it could never challenge this view.

Additionally, triumphalist history will just *miss* things. Often before a new theory is adopted and an old one displaced, there are a number of alternatives floated around (Priestley's chemistry, alternative periodic tables and genetics, transitional ideas between the 'old' and 'new' quantum mechanics, not to mention the old quantum theory itself, to mention but a few). Simply because these ideas do not echo down the (branching, chaotic and decidedly non-linear) corridors of history in the manner of their more successful contemporaries does not mean that they were not significant and influential at the time. In fact studying and reconstructing non-adopted theories is surely essential if one wishes to understand why other theories *were* adopted. Thus a good presentist historian, who wishes to discern the path of development that lead to the modern sciences cannot afford to be a triumphalist.

2.3 Presentism, Causation and Diachronic History

Simply put, presentism is history motivated and influenced by the concerns of the present. The presentist historian asks such questions as: how is it that we ended up here, with the science (or whatever) that we have today? One also can ask philosophical

questions: how do the thoughts of historical figures fit with what is thought today? How do their philosophical ideas stand up to rigorous inquiry?

Why might this be a presentist history? Well, it may well be that the kind of person who is interested in asking these questions (and by "person" I probably mean "philosopher of science") is also going to want to use the philosophical and scientific tools of the present to perform such an examination. This kind of stuff crops up in the history of philosophy quite a lot. It also appears in HPS when someone asks questions about the "philosophy" of a particular historical scientist (whether Boyle, Newton, Einstein or Bohr). Examples of this might be Dugald Murdoch's (1987) book on Niels Bohr's philosophy of physics or Arthur Fine's examination in *The Shaky Game* (1986) of Einstein's philosophical attitudes. It's also the kind of question that's going to arise when looking at why a particular theory was successful, particularly with regards to understanding on what that success was contingent. The results of modern science and mathematics, and methods and concepts of modern philosophy might well come into play here. This is history informed by the present: it is presentist history.

Certain methods and assumptions may appear characteristic of presentist history. Some of these are the use of diachronic historical surveys and the assumption of a form of historical causation. Diachronic histories are historically wide-ranging surveys of how ideas or sets of ideas change over time. Any presentist history that asks how a particular contemporary theory ended up how it is today is (again, unless it's looking at something very recent) is going to engage in such a history. It has to do this because in virtue of surveying a concept across its history, we must examine that concept in different historical periods. We might think of Alan Chalmers' *The Scientist's Atom and The Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms* (2009), a history of atomism that ranges from antiquity to the twentieth century, as an example of this.

By historical causation I mean the assumption that there is some chain of causation, in principle identifiable by the historian, running through the history of an idea. It also means assumption of the continuity of an idea itself through history, and not just in an immediately antecedent sense. This is relevant to presentism for a couple of reasons. Firstly the possibility of it is going to be a necessary assumption for histories which

seek to explain the development of a theory or idea from the past to the present. If, for example, we want to give an explanation of why we have the particular understanding of the concept 'species' in terms of that concept's development from Linnaeus onwards, we will presumably have to assume that we are able to identify causal inferences across history. If we don't make this assumption, then it does not seem possible to answer the 'why' and the 'how' of the question. Secondly it's also an assumption in the selection of an area of historical study motivated by that area's later effects: by deciding to study, say, Newton because of one's interest in contemporary physics, one presumably assumes that Newton is causally relevant to the history of physics. If one can't assume historical causation (albeit of a very weak kind in the second example) then it doesn't even make sense to do this.

We can see here that there are two categories of presentist question emerging. Nick Tosh (2003) makes a distinction between presentism in terms of the criteria historians of science use to select their areas of study, and presentism in terms of the methodology used in those investigations. One form of presentism is the use by historians of categories not available to historical actors in order to pick out those actors (or their work) as relevant objects of study for the history of science. Tosh subdivides this sort of presentism into two types: *'selection by actor intentionality'* and *'selection by later effect'* (Tosh 2003 647). The former uses the aims or goals of some historical actor to determine whether they are a relevant object of study; the latter the later historical consequences of some theory. In general we might refer to this as presentism in selection criteria.

In his paper *Getting the Game Right*, Andrew Cunningham (1988) argues against drawing in to the history of science historical actors for whom the category 'science' (in anything like the modern sense) was unavailable. This sort of anachronism, according to Cunningham leads to problems: the ascription to those historical figures of motives and practices associated with the modern sense of the word. Science, he argues, is a human activity and, more importantly, it is an intentional human activity (Cunningham 1988 374). He argues that it is therefore an error to ascribe to some historical actor the intention of doing science if it is the case that the concept of 'science' was unavailable to them: someone cannot intend to perform an activity if they have no idea what that activity consists in (Cunningham 1988 378).

Cunningham also here argues against the assumption of the continuity and transmission of ideas across history. Ideas for him are strongly linked to the people and the historical context from which they arose (1988 388). Cunningham argues that it is an error to treat the history of science as 'the history of discrete "ideas"' (1988 389). He thinks that historical ideas are both heavily situated in their historical context, and that ideas only exist in the minds that hold them. Cunningham thinks that it simply doesn't make sense to talk about the transmission of an idea across differing historical contexts. For Cunningham, the problematic sort of history of science is one which treats ideas about science as existing in some objective manner outside of the minds of scientists. The resulting histories are then tales of those ideas as they are discovered by historical scientists (Cunningham 1988 387-8). The problem with this argument is that is is question-begging with regards to one side of a philosophical debate that is not settled: that of realism versus anti-realism about science. More that that, if Cunningham's argument is the sort of thing that we are concerned about, then we also ought to worry about linking ideas held by the same person at different times. People's attitudes and beliefs change over the course of their lives. If we are to take Cunningham seriously, then it would seem we ought to also worry about the fact that it is far from clear what sort of continuity of identity individuals have over time when we write history. This seems rather absurd. We might legitimately worry that we can fail to understand some particular concept as a historical did, and thus produce mistaken historical claims. That worry is very different from stating that there is no objective fact of the matter outside of the mind of a historical actor about what, say, an atom is, and that therefore we cannot therefore study the history of atomism across different thinkers.

Tosh is sceptical of Cunningham's denial of historical causation, pointing out that the assumption of even a very low level of historical causation allows us to select our area of study based on its later effect. A historian of physics can study Newton because of the later effects of his work. It certainly seems to be a reasonably minimal assumption that Newton played some causal role in the development of physical theories. Tosh goes on to point out that the selection criteria that we use to define our area of study 'need not affect *how* the selected material is investigated' (2003 656). Cunningham is right to point out that problems can arise from anachronistic use of categories, but this is merely a difficulty, and not an insurmountable one.

But this issue of causation is also at play in doing diachronic historical surveys. And Cunningham is not wrong in raising the issue here, though I am going to argue that he takes it too far. I think we can illustrate this point—how presentism with a strong assumption of historical causation can be problematic—with reference to the aforementioned Young and Freedman (2011) view of the history of atomism, linking Democritus with Dalton. Now the problem here is not quite the assumption of historical causation itself, but the assumption that because there are similar ideas in different parts of history then one *must* be able to make some kind of historical connection between them. Generally speaking, what we can say is this: it is problematic to *assume* that there exist links between ideas in different periods of the history of science; and we should require historical evidence beyond two thinkers both using the word 'atom' for the establishing of such causation.

2.4 Presentism Defended

So we have a number of potential problems that we might worry presentism will run into: assumptions of historical inevitability, leading to supposed blind alleys in history being ignored; 'precursoritis'; the assumption of progress; low empirical standards for establishment of historical causation; an unwillingness to take the past on its own terms. We can see how presentism might seem to chime well with Butterfield's description of Whiggish history as 'the study of the past with direct and perpetual reference to the present' (1931 11). But remember that Butterfield also points that this can be unproblematic, provided that one is careful.

Naomi Oreskes offers a defence of some forms of presentism in her paper *Why I am a Presentist* (2013). She notes that most historians of science, although rarely stating it outright, consider present-centred history to be problematic (2013 595). This, she argues, is due to a worry that if historians engage in presentism, then they may take a Whiggish view of history, or fail to understand the past on its own terms, and thus drawing incorrect conclusions about history (2013 599).

Oreskes argues that it is a mistake to conflate all presentist histories though, and offers some distinguishing classifications. *Substantive Presentism* is 'the belief that the present is the key to the past, in the literal sense that the past is substantively like the present' (Oreskes 2013 600). *Methodological Presentism* is the thesis that past events are best understood by studying present ones (Oreskes 2013 600). Oreskes offers the example of studying the Arab Spring in order to understand the French revolution. She argues that both of these sorts of presentism are problematic: there are good reasons to think that the past was not substantively like the present; and, whilst we might think that comparing revolutions may well be interesting, it seems unlikely that we would come to understand much about the French revolution by studying the Arab spring (Oreskes 2013 600-601).

Presentism is not problematic in every aspect though, Oreskes thinks. She asks 'is it not possible to acknowledge the concerns of the present and be motivated by them, without succumbing to the view that history is a tale of triumph leading to the present?' (2013 603). Her answer is that it is indeed possible. In fact, Oreskes argues that all historians are to some extent motivated by the concerns of the present, be it out of a desire to explore one's own cultural history, a personal or social interest in a particular area, or simply to increase one's chances of getting tenure (2013 603). This is Oreskes' third type of presentism, *Motivational Presentism*.

To deny or decry the existence of motivational presentism in the history of science is something that Oreskes thinks is both mistaken and potentially damaging to the discipline (Oreskes 2013 604). To decry motivational presentism is mistaken because it fails to acknowledge that it is inevitable, as discussed above. It can be damaging to the discipline because to deny it is to deny historians the ability to argue that there is value to us in the study of history, that the study of the past is of value to the present (Oreskes 2013 604).

It might seem like this project is engaging in some problematic form of methodological presentism, but I want to argue that it is not, or at least it is not my intention to do so. My methodology is to try to understand what it is that Born's views are with regards to some particular philosophical debate, then to try and find which philosophical position those views are best aligned with. The point here is that my starting point is what Born

actually wrote, *not* contemporary philosophy. It's true that I am interested in what Born thought about, say, realism because I am interested in both the history of physics and contemporary philosophy of science, but that is simply motivational presentism and so unproblematic.

I can think of two potential criticisms of this position. The first is that I am still engaging in a problematic form of presentism by even trying to ask questions about Born's views on something like realism or the interpretation of probability for the reason that those are contemporary categories which I am imposing on his thought. The second is that whether or not Born's views on realism are aligned with, say, ontic structural realism is not an interesting question in that it doesn't tell us anything about the historical Born because those categories were not available to him.

My reply to the first criticism is that none of the questions that I ask about Born in this thesis concern categories that he was unaware of. Born wrote a book on causal principles in physics. He engages with critically with positivism and offers arguments for how we can gain objective knowledge of an external world. He discusses classical determinism several times. He is concerned himself with probabilistic physics and is quite aware that there exists a debate concerning the interpretation of the probability calculus.

My reply to the second criticism is that using contemporary positions can help clarify to us exactly what Born thinks. Even if this does not tell us about the pure historical Born, from an HPS point of view it is still interesting in and of itself. I'm perfectly happy to accept that some historians of science might not be interested in such questions. That doesn't mean that such questions ought not to be asked.

Presentism might also be criticised because it is not capable of fully immersing itself in the past and that historians should avoid the use of contemporary science when studying the past (Kuhn 1970 3). We have to be careful when suggesting this sort of thing. For one thing, it is not, I think, entirely possible to completely immerse oneself in the past. Historians today exist in the present. This is not something we can do anything about. There is always going some manner in which even the most careful historian will interpret the thoughts and actions of historical actors in terms of his or her

contemporary intellectual frameworks. What is crucial is constantly bear this in mind and not try to impose views or systems of thought onto past figures anachronistically.

In any case, even if one were to somehow forget all one knew about contemporary physics (a blow to the head, perhaps, which induces Hollywood-style amnesia) allowing one to truly understand the nature of Maxwell's electromagnetic theory, then how precisely would one go about relearning it in manner which is not at all anachronistic? Lecture notes from Cambridge at the time? But in what year, and from what teacher? 19th century scientific books and papers? Without any discussion and influence from one's 19th century peers? There is no way, short of a time machine, to really and truly immerse oneself in the past. Any attempt will really be a sham, and it runs the danger of not realising that it is a sham. And it might turn into a boring one at that. Go too far down this path and one relegates the role of the historian to that of a mere scribe, cataloguing the events of the past. This does not mean that there is no point to a historian learning to solve problems in the manner of 19th century physics (c.f. Jed Buchwald). The point is to be *careful*. One can know as much about the area of study as possible. This is simply good scholarship. One must also be wary of from judging historical figures against inappropriate criteria and constantly consider, and try to avoid, anachronisms.

3   An Example From Alchemy

In *Atoms and Alchemy* (2006), William R Newman traces a lineage of atomic and corpuscular theories from the writings of medieval alchemists through to the mechanical philosophy of Robert Boyle via early seventeenth century alchemist/chymist Daniel Sennert..

3.1 Newman

Newman argues for a historiographical revision of the importance of alchemy for the scientific revolution, claiming that, far from representing a break with some unscientific

and mystical tradition, early modern 'science' owes much to the methodology and theory of the medieval alchemists, particularly with respect to the development of corpuscular and atomic theories Newman 2006 6-12). Although not precisely a presentist historian (he is, after all, writing about the sixteenth century), the kind of diachronic survey he undertakes is open to precisely the same historiographical criticisms as presentism is.

Alchemists, according to Newman, adopted the Scholastic interpretations of the atomic theories of Democritus, and crucially applied these to the practice of their craft. This is exemplified in the *Summa Perfectionis* of Geber, identified by Newman as probably being the alias (derived from a Europeanisation of the name of Arabic alchemist Habbir ibn Hayyam) of Italian monk Paul of Taranto (2006 35). In the *Summa Perfectionis*, pseudo-Geber (as Newman refers to him) links Aristotelianism to atomic theory, arguing that metals are made up of Sulphur and Mercury particles in a very strong composition *(fortissima compositio)*. These particles are in turn made up of a very strong composition of the four Aristotelian elements: Earth, Air, Fire and Water. Other metals are formed due to excess unfixed particles of Sulphur and Mercury and can be reduced to their noble components by removing these in a variety of processes. Thus *Summa...* presents a system which is alchemical, in that it allows the transfiguration of metals, but assumes that they have common building blocks which remain unchanged throughout reactions (Newman 2006 27).

This philosophy interacts with alchemical practice in that it provides an alternate account for reversible reactions to that of the Thomists—the reversibility results from the recovery of the original components, still present in the composition, rather than their regeneration.

Geber's crown is passed down to Daniel Sennert, who adopted a similar theory of matter, arguing that the recovery of materials after dissolution provides evidence of corpuscles as form of "building block" which persists through change. Sennert uses this to defend the existence of substantial forms in the face of growing opposition to the idea —it is these that survive reversible chemical reactions. Sennert's primary example of the substantial forms thesis is an experiment in which silver is dissolved in Aqua Fortis (nitric acid) and then recovered via the addition of salt of tarter (potassium carbonate)

and heating. The silver corpuscles survive this process despite the destruction of the solid form (Newman 2006 112).

Newman then proceeds to argue that these theories, and this experiment in particular, were inherited by Robert Boyle, significantly influencing the development of his mechanical philosophy. Although Boyle's anti-hylomorphism is evidently different from Sennert's defence of substantial forms, Newman makes the case that Boyle borrowed strongly from Sennert's experimental work. I'm not going to go into further detail about this point but suffice it to say that Newman makes a convincing case involving identical terminologies and experiments, and other sources which record that Boyle was reading Sennert at the time he was writing his essay, *Of the Atomicall Philosophy* (Newman 2006 160). Boyle effectively takes Sennert's work and reinterprets the consequences of his experiments to argue for his version of corpuscularism. Alloys and dissolutions of all metals can be reduced to their original components. Mechanical reductionism relied heavily on Sennert's experimental techniques.

3.2 Ursula Klein

Klein has criticised Newman's position on a number of grounds, some specific and others more general. She argues that Paul of Taranto's use of the Aristotelian term 'mixt' is indicative of a lack of corpuscularism (Klein 2007 249). Newman counters this by arguing that Paul is simply using Aristotelian terms that have been redefined by him (Newman 2009).

Klein eschews Newman's interpretation, that of knowledge passed down by the medieval alchemists to the early modern chemists, in favour of a contextual approach. Klein's version of the history of chemistry and atomism in the scientific revolution is that these ideas and their development came not from philosophers and alchemists, but from artisans and technicians and, in particular, the writers of chemical textbooks, whose knowledge was largely practical (Klein and Lefevre 2007). Whilst there almost certainly some truth in this assertion, I don't think that it is necessary to completely write out the influence of alchemy in the way Klein does. In fact her criticism of the fact that Newman concentrates of texts rather than society/ culture as a whole, is strikingly

similar to Skinner's criticism of presentism and wide ranging historical surveys. Whilst Newman is not precisely a presentist historian, his work on atomism is methodologically similar—he looks at a long period of time in an effort to ascertain the origins of a particular theory. Skinner (1969) argues that it is extremely difficult, even impossible to master a period of history spanning centuries. As Tosh describes Skinner's (1969) argument 'Any historian attempting to reconstruct the genealogy of an idea will be forced to rely on his own categories and his own concerns. His resultant history will therefore be "a pack of tricks we play upon the dead"'(2003 651).

Both Klein's preference for a small-scale heavily and actor-focussed history, and her criticism of Newman's interpretation of terminology as over-enthusiastic seem remarkably in line with this view of history. Of course, as has been mentioned earlier, it is not by any means in principle impossible to do history like this. It is difficult, and requires care and diligence, but that is all. The other issue at play here is that of historical causation. Historical causation is the thesis that causal links between events in differing historical periods can be established by the historian.

Newman even accuses Klein coming 'perilously close' to triumphalism (Newman 2007 249) with respect to disregard for the influence of alchemy. A triumphalist view, which Newman thinks was held by scholars such as Hall and Butterfield (2007 212) would run something like this: alchemy had largely lost the scientific battle by the eighteenth century, and much of eighteenth century chemical knowledge was not derived from alchemy, so it is not worth studying. I don't for a minute think that this is a deliberate oversight in Klein's part—she is certainly not Whiggish. The point here is that it is clear that although neither Newman nor Klein are presentist historians, the criticisms that they level at the other are the criticisms that are made of present-centred history. This, I think, suggests that the criticisms of presentist history are a) not unique to it (i.e. we could not simply avoid bad history by not being presentist), and b) are not identical with it (i.e. the problem with presentism is not precisely that it is present-centred but that that present-centredness can lead to other problems). If we grant b), then I think, given what else I have argued, we can reasonably make the claim that it is at least possible to avoid those problems.

4   The Historiography of Scientific Biography.

Once section of this project will be biographical in nature. There is a need to set the work of the man in context; both in the context of the world around him and in the context of his life. And besides, Born lead an interesting life. But there are two things here: the life of the man, and the work of the man. The available biographies of Born try to do one or the other—Greenspan's (2005) book largely covers his life, and is somewhat light on the physics. His Royal Society biographical paper (Kemmer and Schlepp 1971) is separated along these lines: the first half covers his life, the second his science. Precisely how to combine the two is something of a historiographical conundrum. Does one attempt, at one extreme, a full integration of science and life, writing of the influences of the subjects non-academic experiences on his academia, and vice-versa? Or does one, at the other, present the scientist as a Janus-figure, a two faced god (with a two-volume biography): one, the scientist; the other, the man. Both of these extremes can be problematic.

Helge Kragh notes other dangers of biography in the history of science: over-identification with the subject and the tendency to 'present the portrayed scientist as a hero' (Kragh 1987 168). The biography is a medium in which it is easy for the 'mythical history of science to flourish' (Kragh 1987 168). It is only a short step from a biography to a hagiography.

In this section I will examine three biographies of Isaac Newton and discuss how particular methods and biases used in reconstructing his life can both distort significantly not only the picture of the man that is presented but also later scholarship.

Due to Newton's iconic status in the history of science, writers wishing to promote a particular view of science or scientific ideology can utilise Newton's authority via biography. Biographers can construct a picture of Newton in which it is implied that the great man himself would have agreed with whatever positions they hold, thus apparently lending themselves a powerful ally. This approach characterises the first two

biographies I will examine: David Brewster's (1855) mid-Victorian hagiography and Louis Trenchard More's (1934) polemic against early 20th century physics. The third biography, Frank E. Manuel's *A Portrait of Isaac Newton* (1968), is a different beast. It is not a portrait that casts Newton in a particular light in order to support the biographer's particular agenda, but it is deeply entwined with the historiography it uses —Eriksonian psychoanalysis—in a way which shapes the whole work.

In an essay on Brewster as a historian of science, J.R.R. Christie comments that 'It was he too who thoroughly dualised Newton, into scientist and alchemist, the rational and the unintelligible' (Christie 1984 56). Christie maintains that there has not been any great advance on this concept: 'Humpty Dumpty is not yet put back together again' (Christie 1984 56). Brewster, in his biography effectively framed the questions of Newtonian scholarship. Most books about Newton at least mention Brewster's *Memoirs*, even if only to criticise it. Probably every academic who has studied Newton has at least glanced at the book. Despite its obvious flaws it is still, historically speaking, one of the foundations of Newtonian biography. It is not inconceivable that the systems of thought that Brewster used to model the man still 'infect', as Christie puts it, the way we think about Newton,(Christie 1984 56). Certainly this criticism has been levelled at Manuel: Hall refers, rather contemptuously, to Manuel's Newton as 'a combination of computer and psychotic Peter Pan' (Hall 1982 307)—a dualistic identity if ever there was one. The chosen texts will be examined in the light of this concept of duality, both attempting to elucidate how and why Brewster creates this duality and how (if) the concept is continued in later biography.

A relatively important factor in early Newtonian biographies appears to have been access to the Portsmouth archives. These archives, owned originally by the Earl of Portsmouth, contained massive amounts of Newtonian data: unpublished manuscripts, letters, notes and books (Hall 1989 188-91). In order to gain a good picture of the man it is probably necessary to examine these. If nothing else, most of the documents relating to Newton's pursuit of alchemy and his unorthodox religious views were contained here. Before they were auctioned off publicly in 1936, access to them was by request to the Portsmouth family only. Whether for this reason or otherwise, it is not apparent that many biographers of Newton before this point consulted them (Hall 1989 189). This is perhaps another reason, along with general Whiggishness, that certain aspects of

Newton's life were not really discussed until relatively recently. Anyone who had consulted the archives had the resources at their disposal to paint a fairly complete picture of Newton. Any lack of discussion, therefore, of particular aspects of Newton by such-equipped biographers cannot easily be explained as simple ignorance. At the very least it is shoddy scholarship and it requires no great leap of faith to believe that it represents a conscious decision to ignore certain sources.

4.1 David Brewster and the 'Memoirs of Isaac Newton'

> The writings and life of Sir Isaac Newton abound with the richest counsel. Here the philosopher will learn the art of patient observation by which alone he can acquire an immortal name; the moralist will trace the lineaments of a character exhibiting all the symmetry of which our imperfect nature is susceptible; and the Christian will contemplate with delight the High Priest of Science quitting the study of the material universe—the scene of his intellectual triumphs, to investigate with humility and reverence the mysteries of his faith (Brewster 1855 3)

Such writes Sir David Brewster in his 1855 'Memoirs of Sir Isaac Newton', a biography which could be described, perhaps a little uncharitably, as two volumes of hyperbole, sycophancy and inappropriate capitalisation. Perhaps it seems a little Whiggish to talk like this but the modern perspective is not unique in passing this judgement: The Times in its review of the 'Memoirs' was 'forced to the conclusion that Sir David is a good Christian and a bad biographer' (The Times, 1855 9). The Times' anonymous reviewer certainly held the opinion that Brewster wrote from a perspective that was, at the very least, somewhat biased. But Brewster is not interesting simply as an historian of science writing a Whiggish hagiography. Brewster is interesting as a not insignificant figure in nineteenth-century physics who clearly was in thrall to his image of Newton.

Brewster was not professionally separated from Newton's domain, having worked in the field of optics. His work survives in modern physics in the form of Brewster's angle, the

incident angle of light on a surface at which the reflection becomes linearly polarized. He also made contributions to the theory of colours and absorption spectra and invented the kaleidoscope. Although Brewster was perfectly prepared to refute Newton's theory of colours, he insisted, rather unfashionably, upon adhering to the corpuscular theory of light (Shapiro 1993 330-54). It must be admitted that an admiration for Newton is not the only reason for maintaining a belief in particulate light. There were still some scientific disputes over the nature of light in the 1850s, although by this point they were running down. Brewster also had methodological qualms about light waves— particularly the assertion of the existence of the ether (Cantor 1984).

So why did Brewster feel the need to write a biography of Newton, and why did he write it in the manner he did? Brewster, although respected as a scientist, receiving both the Copley and Rumford medals from the Royal Society, appears to have often felt maligned at the way he was treated by society in general. He suffered from financial worries throughout much of his life, supporting himself through his writing and editing the *Edinburgh Magazine*. He even considered declining his knighthood when he heard of the fees (up to £240) involved (Shapin 1984 19). He should have earned significant sums of money from his invention of the kaleidoscope, but due to British patent laws he received relatively little for his work. This added to his feelings of resentment at the treatment of scientists and inventors in Victorian society.

Brewster's involvement in setting up the British Association for the Advancement of Science (BAAS) was an attempt to rectify these problems and pressure the government into recognising science as a true profession (Morell 1984 25-29). However Brewster rapidly became sidelined within the organisation and it became clear that the BAAS was not fulfilling the role he had intended for it. Brewster eventually acquired a permanent position at the University of St. Andrews as the principle of United College. Here he attempted to enact significant reforms on the university system (Anderson 1984). His biography of Newton, written during his years of disputes over the reforms and published towards the end of his tenure as principle, must at least partly represent his attempts to portray how science should be performed and rewarded, utilising Newton's authority to push his vision of society.

Brewster enters into an extended analysis of Newton's correspondence, particularly with Locke, in order to demonstrate that Newton had been engaging in theology and analysis of scripture throughout his life. So, having saved Newton's reputation as a serious theologian, Brewster goes into a discussion of Newton's theology, particularly his views on revelation and scriptural corruption. Brewster takes pains to emphasise the links between the scientific investigation of nature and the investigation of scripture. He writes whilst criticising Voltaire's portrayal of Newton—(the writings of Frenchmen on his hero seem to particularly irk Brewster) 'it has surprised us that other [in this context other means non-sceptical] authors should have regarded the study of the Scriptures as incompatible with scientific research' (Brewster 1855 Vol II 355). He goes on to praise England patriotically for its abundance of such thinkers ('protestant' thinkers, Brewster specifies) before writing that 'From the study of the material universe—the revelation of God's wisdom, to the study of his holy word—the revelation of his will, the transition is neither difficult nor startling' (1855 Vol II 355). He then provides an extended (two pages long) and rather flowery metaphor relating astronomical inquiry to the interpretation of the book of revelation. But what he wants to say here is clear: that science and Christianity are not only amiable partners, but natural ones. Science is the revelation of god's wisdom—the study of nature has theological value. Equally he seems to suggest that the study of nature and the study of scripture are intellectually similar activities.

However, what he does not do here is directly link science with scripture. He writes of how 'the antiquity and authenticity of the books which compose the sacred canon … have been demonstrated to all who are capable of appreciating the force of historical evidence' (Brewster 1855 Vol II 358). What he does not mention is scientific evidence. In fact in his other writings, Brewster often condemns attempts to mix science with scripture, such as the catastrophist geologies which attempted to explain geological features of the Earth as being caused by the Biblical flood. As Paul Baxter has pointed out, Brewster, in his review of William Buckland's Bridgewater treatise condemns the intrusion of religion into scientific matters, and takes pride in the Evangelical movement's defence of the independence of geology from scripture (Baxter 1984 48-49).

Science, Brewster thinks, can tell you about God by way of studying his creation, but it can't tell you about scripture, and scripture can't tell you about science.

We can see that Newton's Christianity and serious theological work are clearly of great importance to Brewster and his program. Newton is held up as the ideal of the scientist and the Christian. So how does Brewster deal with what others had suggested, and what he must have realised when he examined the Portsmouth archives in detail: that Newton was probably guilty of the Arian heresy, and denied the divinity of Christ? How did he deal with, as J.R.R Christie imagined it 'the mounting horror as to what they [the manuscripts] revealed'? (Christie 1984 55). It seems that he tries to cover for Newton's potential heresy, whilst being very careful to not explicitly contradict any historical sources that might be later brought up, effectively covering for himself. The effect is rather transparent.

He writes 'Sir Isaac Newton has been regarded […] as antitrinitarian; but this opinion is not warranted by anything which he has published' (Brewster 1855 Vol II 337). In the light of the unpublished manuscripts that Brewster had examined, the word 'published' becomes significant here. A few pages later he writes with reference to his earlier biography of Newton, written in 1830 before he had looked through the Portsmouth archives, that 'in the absence of all direct evidence I had no hesitation […] in coming to the conclusion that he [Newton] was a believer in the trinity' (Brewster 1855 Vol II 340). Again, Brewster refuses to discuss what he may have found in the archives. He then goes on to write about how he thinks that one should not 'burrow for heresy among the obscurities of thought' (Brewster 1855 Vol II 340). Effectively, he says that there is no published material to suggest Newton was unorthodox, and if one won't accept that, then in any case it's no one else's business.

There are minimal references to Trinitarian controversies when Brewster discusses his archive findings, despite extended discourses on several manuscripts. All he is willing to say with regards to this is that he has found a manuscript 'which may throw some light on the opinions of the author' (Brewster 1855 vol 2 350). The manuscript is not reproduced or discussed in the main text, instead being relegated to the appendix. Again Brewster just refuses to discuss the issue. It is fairly clear that Brewster, from reading the Portsmouth Archive material, came to the conclusion that Newton held heretical

views, but was unable to bring himself to sully his hero's reputation in the public arena. This is something that may go beyond mere Whiggishness. It is a blatant refusal to discuss a controversial (to Brewster at least) area of Newton's life based on personal feelings. This desire to segregate elements of Newton's personality from his identity as a scientist also forms part of the 'dualistic' nature that Christie described.

This speaks, one suspects to more than a simple desire to protect the reputation of a personal hero. It's about Brewster's personal religious convictions as well. Here he goes to great pains to prove (as it were) that Newton was a devout Christian and that his Theological works were deeply serious and not the products of a lunatic or a man in his dotage. Indeed as I mentioned earlier, perceived aspersions of this kind cast on Newton's character by the likes of Biot and Voltaire formed part of the initial impetus to write the book (Christie 1984 54).

4.1.1 The High Priest of Science—Newton as a Religious Figure

> To have been the chosen sage summoned to the study of that
> earth, these systems, and that universe,—the favoured lawgiver
> to worlds unnumbered, the high priest in the temple of
> boundless space,—was a privilege that could be granted but to
> one member of the human family;— and to have executed the
> task was an achievement which in its magnitude can be
> measured only by the infinite in space, and
> in the duration of its triumphs by the infinite in time. That Sage
> —-that Lawgiver—that High-priest was Newton. Let us
> endeavour to convey to the reader some idea of the revelations
> which he made, and of the brilliant discoveries to which they
> conducted his successors. (Brewster 1855 Vol I 279)

This image of Newton as the 'High-Priest' of science is one that recurs throughout the *memoirs*. In the light of Brewster's views on science and religion I think it is fair to say that this is not just a metaphor emphasizing Newton's importance within the canon of science—there is a real religious element to this picture. Brewster seems to genuinely regard science as a vehicle for revelation—something through which god can be revealed, with Newton as one of the Elect.

Later on in volume one, in a discussion of the development of celestial mechanics after Newton's death, Brewster launches into an argument from design based around the stability of the solar system (1855 Vol I 312). This is not, I think, a hypocritical attempt to marry science and scripture, but rather a rationalist argument for God that to be used as technique to convince sceptics—something that would fit well with Brewster's evangelical leanings.

So how does all this fit in with Natural Theological trends amongst Brewster's fellow evangelists? It seems that there is a good fit, at least for some of Brewster's ideas, with Thomas Chalmers, the noted Scottish philosopher and evangelist preacher. Brewster and Chalmers were old acquaintances. Indeed it may even be possible to credit Brewster for Chalmers evangelical conversion, which occurred during the time that Chalmers was writing the Edinburgh encyclopaedia article on Christianity that Brewster had commissioned him to do (McCosh 1874 395). Chalmers was even known, McCosh tells us in *The Scottish Philosophy*, for bringing up Newton at inappropriate times (McCosh 1874 400).

We can also see this duality clearly when examining how Brewster deals with Newton's alchemical researches. His treatment is itself somewhat confused, probably reflecting what again must have been a shocking discovery: that the great Newton devoted massive amounts of time throughout his life to the pursuit of horrible, irrational alchemy. Initially, he attempts to excuse Newton's interest in the area by suggesting a rational basis for it, explaining that the derivation of gold or silver from base ores and the extraction of aluminium from clay must have seemed unbelievable. Thus 'we need not wonder that the most extravagant expectations were entertained of procuring from

the basest materials the precious metals and the noblest gems' (Brewster 1855 vol 2 372). It is possible, therefore, that Newton may have been misguided or lacking in knowledge about the nature of minerals, but certainly he was not irrational.

It seems, in the end, that Brewster cannot really convince even himself of this. A few pages later he writes 'we cannot understand how a mind of such great power […] would stoop to be even the copyist of the most contemptible alchemical poetry' (Brewster 1855 Vol II 374-5). He attempts some further justification by arguing that everyone was up to it, specifically Boyle, Locke and Leibniz. This seems like a weak argument. Brewster, one feels, simply could not overcome his revulsion at Newton's alchemical studies and refuses, once more, to deal with a controversial area. Brewster's refusal to treat Newton's alchemy seriously is evidenced by the detailed analysis earlier in the chapter given of a chemistry manuscript (Brewster 1855 Vol II chapter xxv) found within the same collection of papers. Brewster will not characterise the alchemy, choosing instead to compartmentalize it. He separates off what he considers to be an irrational chunk of Newton's personality from the rest of him, emphasising once more the construction of the dualistic nature.

4.2 Louis Trenchard More and 'Isaac Newton: A Biography'

L.T. More was a physicist of the late nineteenth- and early twentieth- century, based primarily at the University of Cincinnati. He became very concerned with what he felt to be the metaphysical and profoundly unscientific nature of much of modern science, especially the general theory of relativity. In 1915 he published a monograph entitled 'The Limitations of Science' (More 1915) in which he argued just that. Modern science, he thought, had become too laden with unprovable hypotheses and should be concerned with only what can be observed. Scientists had lost the ability to find 'the distinction between the creations of nature and the creations of their imagination' (More 1915 17). More wished to re-establish that distinction. He wished, as indicated by the prefacing quotation, (More 1915, preface) that scientists would stop feigning so many hypotheses. And who better to bring modern science back onto the right path than Mr Hypothesis Non Fingo himself, the esteemed Sir Isaac Newton (with a little biographical aid from More).

More's biography is not quite a hagiography written by a mid-Victorian who wishes to present Newton as the perfect man. Indeed in the preface, More attacks Brewster's biography for its lack of criticism (More 1934 vi). On the other hand, as Hall has noted (Hall 1999 189), More's work is not much of an improvement on Brewster's: More's Newton is still the perfect scientist. The text is also full of explicit attacks on what More disliked about contemporary science, revealing his purpose in writing it.

More presents his work with a strong positivist bent, as a reviewer (Perry 1917 205-6) of *The Limitations of Science* (More 1915) notes. This is not unsurprising given his opinions and context. He explicitly disliked, as has already been stated, what he regarded as metaphysical claims.

The Whig view of the history of science is still there: More still wants to demonstrate that modern physics naturally descends from Newton, though his motives are different from Brewster's. He writes of Newton's theories regarding the corpuscular nature of light 'This supposition of the mutual attraction of matter and light has been revived by Professor Einstein in his theory of the gravitational field' (More 1934 113). This link between corpuscular light and general relativity is rather stretched. It is not reasonable to claim anything more than a superficial resemblance between Newton's corpuscles and the photons of quantum theory—this might remind us of the problematic way that Young and Friedman link the atoms of Democritus with those of Dalton (2011 1465). Despite his deep dislike of the theory of relativity, it would be difficult for More to deny the gravitational bending of light: by the time the book was written it was regarded as well confirmed by experiment. However, by arguing in this manner that Newton's theory of light is in a significant way a precursor to Einstein's, More could save the observed phenomena (the gravitational bending of light) whilst at the same time implying that Einstein's theories were at best derivative of Newton's.

A common theme in More's book is that Newton would have disliked modern science, and that modern science is anti-Newtonian in its methodology. He complains that Newton 'would have renounced science today altogether as most litigious and dogmatic of task masters' (More 1934 121). The essence of More's argument is that Newton was the creator of the 'correct' scientific method and contemporary science had lost its way. He goes even further than this at points, suggesting that modern science is not only

misguided, but is actually opposed to how things should be done. More thinks that the *Principia*, which is one of 'the two most stupendous creations of the scientific brain', is 'under attack […] by the relativists in physics' (More 1934 332). But it gets worse! If modern science proceeds along the dangerous course espoused by heretical thinkers like Einstein then 'it ultimately evaporates into scholasticism. And if it persists, it will cause the decadence of science as surely as the medieval scholasticism preceded the decadence of religion' (More 1934 333). Science itself will collapse, and knowledge will return to the dark ages. And all because we didn't listen to Newton! This is pretty clear ideological leakage. An unsteady argument in any context, it seems completely inappropriate in a biography with any claims towards actually being a work of history.

4.3   Frank E Manuel and 'A Portrait of Isaac Newton'

Manuel (1910-2003) was, in contrast to Brewster and More, a professional historian. What is of great importance here is that Manuel, as a historian, explicitly considers contemporary trends in historical thinking. Consider the context in which the book was written. It was published in 1968 in the wake of Thomas Kuhn's *Structure of Scientific Revolutions,* and at a time when the use of psychoanalysis was popular. Manuel himself writes in the preface that 'on more than one occasion I steered my way between a Scylla of historians of science and a Charybdis of psychoanalysts' (Manuel 1968 ix). He freely admits his reliance on theories of the unconscious, and acknowledges the debt he owes to 'psychoanalysts, psychologists, and sociologists'(Manuel 1968 ix). This approach has certainly attracted criticism, particularly from Hall, who regards Manuel as falling into 'the trap of psychoanalytic or any other ready-made analysis of personality' (Hall 1982 306). How, specifically, does Manuel utilise psychoanalysis, and how does this influence the portrait he paints of Newton? Is his use of psychoanalysis necessary for his argument, or is it merely a theoretical language in which he chooses to frame his writing?

The use of psychoanalysis in Manuel's description of Newton's upbringing and childhood is obvious. Newton is described as an 'ambivalent neurotic'. He refers directly to 'Erikson's schema' (Manuel 1968 66): the psychoanalyst's categorization of

the stages of the growth of the ego into the 'eight ages of man' (Erikson 1963 pp247-74). Manuel characterizes Newton's early life literally in Erikson's terminology: Newton suffers from 'mistrust, shame, doubt, guilt, and inferiority' (Manuel 1968 66). He also notes Newton's 'initiative' and 'industry'.

When Manuel characterises Newton's lack of self-esteem as having 'its origins in an infantile failure to be satiated at the breast', there is a clear echo of Erikson's first age. Erikson writes 'the first demonstration of social trust in the baby is the ease of his feeding'. To someone working within this 'schema' it must seem obvious that something which interferes with breast feeding will result in damage to the ego. When Manuel subsequently refers to the separation anxiety caused to Newton by his mother leaving him, we can again see how Manuel's analysis is framed by psychology, quite possibly again Erikson's. We see it again later on, when he comments that '[t]he fixation upon his mother may have crippled Newton sexually' (Manuel 1968 28).

The portion of the book describing Newton's early life is the most psychoanalysis-heavy. It is referred back to in the closing pages of the biography, when Manuel writes, of some of Newton's reminiscences to John Conduitt, that in them he hears 'the echo of his first anguished puzzlement as an abandoned child bewildered about the meaning of his mother's appearances and disappearances' (Manuel 1968 389). This again is reminiscent of Erikson's first age. Manuel thinks that even as a dying man, Newton felt that he had not really found 'what he had been seeking', still were 'his great queries unanswered' (Manuel 1968 390). In this, there is an echo of Erikson's final age, that of Ego Integrity vs. Despair.

We can also see the old model of Newton as a dualistic personality in Manuel. In the closing statement, he describes him as a Janus-figure. Newton was a man capable of 'pure joys' and great discoveries, but also of 'destructive forces' (Manuel 392) Manuel's dichotomy is not between rationality and irrationality, but between the healthy and the pathological psyche, between, as he puts it, 'the capacity for good and evil' (Manuel 392).

The use of psychoanalysis raises questions more complex than the Whiggishness of earlier history. Manuel's aim is to model the psyche of Newton, to describe him as a

person. Psychoanalysis is his tool, the way he frames his investigation. He is perfectly candid about this, admitting that if some of the theories he uses are incorrect, then some of his arguments will fall down. It might well be informative to utilise psychology to describe how and why Newton behaved as he did. Unless we are to reject psychological insights as being unhistorical for whatever reason, then Manuel's analysis should not be rendered illegitimate simply because he used the prevailing psychological theory of the time. In his disclaimer about his assumptions, Manuel is probably being overly modest (or at least shielding himself from critics). The use of psychoanalysis does not run so deep that his conclusions are inseparable from it.

5   Conclusion

So, what insights or lessons can we draw from this?

I think we can draw a number of strands of criticism of 'poor' present centred history of science here. One is the assumption that there is a particular narrative to history prior to the historian writing it. We can see this in Whiggish history. Another is the recruitment of historical figures as supporters of one's own views. This is evident in both Brewster's and More's biographies of Newton, who respectively try to use Newton's shadow to argue for a particular relationship between science and Christianity, and to argue against relativity and quantum mechanics. There are also the practical difficulties in producing diachronic histories—history that ranges across large stretches of time—and of demonstrating that there are causal links of influence between different periods of history. We worry about this largely because it is very difficult to do: we have to immerse ourselves in a number of different contexts and try and trace links between them. Some historians think that ideas are necessarily linked to the historical context in which they arose. For this reason, they think that there is little point in attempting to draw links between ideas in different contexts. This is part of Klein's criticism of Newman. Finally, there is the worry that the presentist will not take the past on its own terms: that the presentist is more interested in the present that the past and so shows insufficient interest in the historical context and what the actors therein actually thought they were doing.

I fully agree that all of the above can become problematic, although it is certainly not clear that diachronic histories will necessarily be so. I do not think, however, that the project that I propose is particularly susceptible to them. In essence, what I am trying to do is to work out what Born's thoughts were on a variety of philosophical positions and see how (or not) those positions align with contemporary ones. I'm not trying to argue that Born's work on quantum mechanics was part of some inevitable arc of progress (nor, incidentally, am I trying to argue that it wasn't). I'm also not trying to trace a line of thought across different periods of the history of science. Nor am I trying to corral Born for support in, say, the scientific realism debate. I do not think that demonstrating that Born held a particular philosophical position makes that position more plausible.

Where I do engage in presentism, I do so because I think it clarifies Born's position to classify it as being aligned with some particular position in the space of contemporary philosophy. I think that this finding what sort of position Born's views are aligned with is interesting enough in and of itself but it also adds to our understanding of what he thought by allowing us to situate his positions in a wider philosophical context. For example, in Chapter 6: Born on Probability, I will argue that he holds a propensity interpretation of probability in quantum mechanics. This terminology wasn't available to Born when he wrote on probability, but I still think that it is clear that this is the position that he holds It should also be noted that it doesn't have to be part of some argument that one should hold such a position.

I will on occasion engage more critically with Born's position (for example, asking how he might respond to later developments in physics that he was not privy that may create a problem for his arguments regarding causation in physics and trying to reconstruct an argument of Born's concerning determinism with the addition of chaos) because I think that he is worth engaging with as a philosopher. To some extent this is also what the presentist analysis aids us in doing. I also fully appreciate that such critical engagement might tell us little about the historical Born. I do think that these sorts of questions are interesting to philosophers of science and can be answered in a way that takes the history of science seriously. Some historians of science may not be interested in the answers, but so be it.

Chapter 2: The Born Identity

1   Introduction

In this chapter I'm going to give a (relatively) brief biography of Born in order to both give an account of his life, which was eventful and interesting—he was born in a part of Germany that is no longer Germany, lived through two world wars, studied under a man who is often considered to be the greatest mathematician of the 20th century, was at the heart of the development of quantum mechanics in Göttingen. He fled his home country for the UK when the Nazi Party came to power, where he became a naturalized citizen and was made a member of the Royal Society, returned to Germany after winning the Nobel Prize in the 1950s, and devoted much of his time to trying to prevent the nuclear armament of the country of his birth, along with writing the philosophical works which are the main focus of this thesis.

The main sources that I am going to use are Born's autobiography, *My Life: Recollections of a Nobel Laureate* (Born 1978); his Royal Society obituary paper (Kemmer and Schlapp 1972); and science writer Nancy Thorndike Greenspan's biography *The End of the Certain World: The Life and Science of Max Born* (Greenspan 2005).

The purpose of the chapter is to give an account of the details of his life, which are still not that widely known and to give an overview of his scientific work. I'm not going to go into detail on every aspect of his work—Born was extraordinarily prolific, publishing some 360 papers and books during his career (Kemmer and Schlapp 1971 52)—instead concentrating on his education and work on quantum mechanics. I'm not going to try and draw

psychological lessons from the great man's childhood, nor do I really want to make inferences about his scientific work based on his life outside of science.

1.1   The Biographies

I'm first going to say a little about these three accounts of Born's life, before moving on to my own. Born's autobiography (Born 1978), contains interesting detail on his education and on his scientific work, but cuts short of a detailed discussion of his life after the end of the second world war. We might interestingly note that Born died in January of 1970 and so wonder at exactly when it was written. Gustav Born (Max Born's son), writing in a preface and postscript, explains that the book was originally written as a series of 'recollections' for Born's children and grandchildren. He relates that Born had started to write them in 1940 and continued working on them until 1946. The part of the book written in this period gives an account of Born's life from his childhood up until 1925. The rest of the book was written after 1962, when Born had retired and moved back to Germany. The 'recollections' were written in English—Born's children and grandchildren grew up in Britain, after he moved there in 1933. Gustav relates that because it was not written for a wide audience, the style might be somewhat idiosyncratic when compared to an autobiography written for general publication, and further notes that it has received some editing to make it more comprehensible to English-speaking readers. 'Although my father's English was good, minor lapses of idiom have been anglicized' (Born 1978 vii). Although we get some interesting detail on Born's activities, we don't hear much about his inner thoughts: Gustav writes in the postscript 'These recollections give away little about my father's inner life. He was not in the least secretive in the ordinary sense, but his deepest feelings were hidden from everyone until he began to open up a little towards the end of his life, mainly to his children' (Born 1978 297).

The *Biographical Memoirs* (1972) paper was written by the nuclear physicist Nicholas Kemmer and solid state physicist Robert Schlapp. Both Kemmer and Schlapp spent time in

Göttingen during the period that Born ran the department of physics there. It contains an account of his life and a separate account of his work in physics, dealing with both his early work on the dynamics of crystal lattices and his later work on quantum mechanics. They also discuss the textbooks written by Born, several of which are still in publication today.

The End of the Certain World (2005) was written by Nancy Thorndike Greenspan. It contains a detailed and heavily researched account of his life, but does not have a huge amount of detail on his actual scientific work and certainly no analysis of what his philosophical positions might have been. This is not to criticise it, but simply to state what it is not - a piece of intellectual history. Writing a review in Annals of Science, Robert J. Deltete, describes it initially as 'superb' (Deltete 2009 p434), but goes on to note that it is somewhat sparse on detail with regards to his scientific work, although there is nothing in the book that is 'precisely wrong'. Bernstein (2005) gives a much more stringent criticism of the descriptions of physics in the book. Frankly, Bernstein is quite rude and so I don't want to quote him on Greenspan. Still, for this reason, I'm not going to draw on Greenspan with regards to Born's scientific work, but will use it for some details of his life.

2   Early Life

Max Born was born on 11 December 1882 in Breslau, Silesia, then part of East Prussia. It is now the city of Wrocław in Poland. His parents were Jewish-Germans, Gustav Born, a professor of embryology at the University of Breslau and Margarete Born, née Kaufmann, the daughter of middle-class textile factory owners. Born does not give us the impression the family were practising Jews: as there was no Jewish religious education at his school, Gustav had Max attend Lutheran religious classes instead, 'having not particular preference for the Jewish religion, but none for the Christian either' (Born 1978 20). Gustav had remained a Jew (as opposed to converting) 'mainly from the standpoint of

liberalism' as he considered religious questions to be a purely private matter - 'what was good enough for his ancestors he thought was good enough for himself' (Born 1978 20).

Born tells us that when they met, his mother's family did not easily accept his father, as he was not a rich man, but rather a young lecturer with a small salary. However, when she 'got her way' the Kaufmann's deemed it necessary to 'fit the young couple up in a way worthy of the wealth and importance of great merchants' (Born 1978 4). The home Born grew up in was a well-appointed apartment in a city-centre building owned by Born's maternal grandfather, next to the Royal Palace, that was full of 'rather pompous' Victorian-style furniture (Born 1978 4).

Margarete died from gall stones when Born was four years old, and he relates that he does not think that he had 'any direct recollection of her' (Born 1978 6), knowing little of her other than of her love for music (Born 1978 6). He had in his possession his mother's photo album, which contained pictures of a number of famous musicians—Sarasate, Joachim, Brahms, Clara Schuman, Brahms, Max Bruch and a number of unnamed (by Born, at any rate) Wagnerian opera singers from Bayreuth. Born's grandparents would frequently entertain and host musicians visiting Breslau to perform, and his mother 'did not waste this opportunity' (1978 6).

After his mother's death, Born and his sister were mostly taken care by the family's servants. He does not remember much from this period, but writes of taking great pleasure in watching drilling soldiers on the parade ground that the windows of their apartment looked out on. These were mostly daily drills, but there were on occasion rather spectacular events, including times when the Emperor Wilhelm II visited the Royal Palace next to their apartment building (Born 1978 7).

He seems to have had a happy childhood despite the early death of his mother. He tells us that he and his sister were rather spoilt by the Kaufmanns, his mother's family, having 'glorious' birthday celebrations with 'heaps of toys and sweets from the grandparents,

uncles and aunts' (Born 1978 8). As a child, he also spent much time in his grandfather's house in Kleinberg, set in magnificent gardens, which he describes as 'our little paradise' (Born 1978 10). He also fondly recalls spending summers in the Kaufmann's house in Tannhausen, a village on what is now the Polish-Czech border that was home a textile factory that they owned.

Born's father remarried in 1890, during his early schooling. He tells that Gustav had longed for some measure of financial independence from the Kaufmanns after Margarete's death. The Kaufmanns insisted that he maintain their somewhat aristocratic lifestyle, and this compelled him to accept their financial assistance (Greenspan 2005 16). Although he seems to have been a well-regarded scientist, he repeatedly failed to get higher paid positions. The higher profile jobs usually went, as Born explains, to someone who was not Jewish (Born 1978 24). To this end, some friends of Gustav's arranged a correspondence between him and another friend of theirs, Bertha Lipstein, the daughter of a Russian-Jewish timber merchant who, despising the virulent anti-Semitism in Russia at the time, had sent his children to be educated in Germany and Switzerland. The correspondence eventually resulted in an engagement. Born seems to have liked his new stepmother—he remarks that 'kindness and honesty' were her main features (Born 1978 25).

Born was educated in a traditional German manner. He writes 'In my youth German schools were mere places for learning, in an abstract and dull manner; there was hardly any attempt at a general education' (1978 29). The curriculum was humanistic in nature, and centred around Latin—something that Born remembered little of after leaving school (1978 30). He was not in general a particularly gifted scholar of languages. Mathematics was also a central part of the curriculum. Born was not particularly interested in the Euclidean-centred teaching that formed the first few years of his mathematical education— he was, he thought, simply too young to properly appreciate and understand it at that time (1978 31). However, he took interest in the analytic geometry taught in the higher forms— it was, he thought, a powerful tool whose ability to reduce geometrical figures to algebra appealed to him (1978 31). Born also learned some natural sciences, developing a deep

interest in physics. This was noticed by his teacher, who invited him to help prepare the lesson's experiments. He writes that 'I spent many a happy afternoon with him in the physics classroom connecting and switching electric circuits or adjusting lenses on an optical bench' (Born 1978 32).

3   University—Breslau, Heidelberg, Zurich and Göttingen

On July 6th of 1900, Born's father passed away from a heart attack, at just forty-nine years old (Greenspan 2005 20). The next year he started University, initially attending Breslau. The German system of the time was far less strict that the British—there were no examinations apart from a final oral exam for a doctorate, which was the only degree awarded apart from a state teaching certificate (Greenspan 2005 22). Students were free to attend different universities over the course of their education. Born attended Breslau for two years, taking the summer term in each of those in Heidelberg and Zurich, respectively, before attending Göttingen, where he took his doctorate.

For his first two semesters at Breslau, his courses consisted of astronomy, mathematics, experimental physics, chemistry, zoology, general philosophy and logic (Born 1978 49). Born found the experimental physics course to be initially dull. The lecturer was Victor Emil Meyer (not the same person as the chemist Viktor Meyer), an ageing and ill man whose lectures were dull and whose experimental demonstrations were perennially unsuccessful to the extent that students would attend his hydrodynamics experiments with raincoats and umbrellas. Born found that he could not learn much from him beyond what he had already done so from school and so 'stayed away' (Born 1978 51). Meyer retired after Born's first semester and was replaced by Ernst Neumann, a mathematician. Born did not find him to be much of an improvement on Meyer—although Born had the impression that Neumann took great care over his course, the experiments were still not very successful and so he still did not attend regularly (Born 1978 51).

He did not much enjoy his courses on zoology and chemistry either. Zoology contained too many facts that had to be memorized and too little understanding of causal connections between them for Born to maintain interest in it. He had a similar reaction to chemistry. He writes 'there was too much of the kind of stuff I could never digest: accumulations of almost incoherent facts which had to be memorized—at least so it appeared to me' (Born 1978p52). He did however regret not taking a course in laboratory chemistry, lacking the time to do so—some of Born's later work on lattice dynamics connected with chemistry and he admits having to rely on the advice of friends and being somewhat uncomfortable when being asked to lecture to chemists (Born 1978 52).

He also found his philosophy course to be a disappointment—in fact he describes it as 'the greatest disappoint of all my courses' (Born 1978 52). The course was taught by a Catholic priest (Born could not remember his name, but thought it was Baumann) and mostly consisted of Aristotelian logic and metaphysics. Born found the Aristotelian syllogism to be 'the epitome of triviality', a position that he never found cause to alter. Born did learn some logic later on from studying mathematics and attending David Hilbert's lectures on the foundations of logic (Born 1978 52).

Although his other courses were a disappointment, Born enjoyed the lectures he attended on mathematics and on astronomy. In the four semesters he spent in Breslau he laid a 'solid ground' for his mathematical education (Born 1978 52). In the mathematics course, Born was introduced to group theory and matrix calculus, something he would use when developing matrix mechanics with Heisenberg and Jordan many years later in Göttingen, Heisenberg being unaware of the nature of the strange commutation rules that he had produced.

Born's interest in mathematics was something of a turnaround from school, where he had not enjoyed the subject. He explains his fascination by way of contrast with a book of philosophy by Heymans that he and some of his fellow students had read. From this he had

become acquainted with the paradoxes of infinity that occur with regards to discussion of the attributes of God and became 'seriously worried that the whole of metaphysics seemed to be in an awful muddle' (Born 1978 p 53). Not so with mathematics. What in philosophy appeared to be 'veiled in a mist of paradoxes' (Born 1978 53) was presented in in a clear and precise manner according to the occasion. This, for Born was an important discovery 'that such words mean nothing unless applied in a definite system of ideas to a definite problem where they can be made significant for that problem' (Born 1978 p53). So the precision of mathematics and its ability to render complex concepts graspable appealed to Born.

Astronomy, says Born, appealed to him in a different way. It was a practical science, about 'careful handling of instruments, correct reading of scales, systematic elimination of errors of observation and precise numerical calculations' (1978 54); it 'gave one the feeling of standing on solid ground' (Born 1978 54). I would note here that Born, despite being a theoretician, always had an interest in and a respect for the importance of experimental physics throughout his career (See Experiment and Theory in Physics Born 1943). He was so drawn by astronomy that he once considered making it his career, but soon realised though that he did not have the inclination for it. The dream was 'shattered by the horror of computation' (Born 1978 p55), as he puts it. Astronomy in the early 20th century required the completion of vast amounts of calculation to predict planetary orbits, and Born knew that he was no good at this kind of work. Looking back on these days, he reflected that had he known that there was a different kind of astronomy, astrophysics, whose goal was the study of the universe with all of the tools of modern physics, he might have elected to follow that path. But he was not aware of it, and by the time he was introduced to it by Schartzschild in Göttingen it was too late to change paths (Born 1978 55).

Born spent the summer term of 1902 in Heidelberg, persuaded to attend a semester there by his cousin Hans. He found the physics course there just as uninteresting as at Breslau, but enjoyed the mathematics (Born 1978 65). He studied differential geometry, and determinants, which gave him a good foundation for the matrix algebras he studied at

Breslau in the following term (Born 1978 66). Of his time in Heidelberg, Born considers the most important thing to be the start of his friendship with James Franck, whom he would later appoint to the position of director of experimental physics at the institute in Göttingen (Born 1978 68).

Born spent the following winter back in Breslau (1978 69), before deciding to spend another summer term away, this time in Zurich. There, he studied more mathematics, in particular a course on orthogonal functions that he considered to have been of great use in his career as a physicist (Born 1978 73). Once again he was repulsed by the physics courses—he attended a course given by an astronomer (Wolfer) but found it 'so dull that I soon gave up' (Born 1978 73).

After wintering back in Breslau in 1902/3, Born decided to move to Göttingen. The reasons for this were partly personal—he had few friends of his own in Breslau and, although his sister lived there, he did not particularly enjoy the company of her 'set' (Born 1978 79). There were also professional reasons. After the lectures he had attended in Zurich, those at Breslau had started to feel a little 'stale'(1978 79), as he puts it. His friend Otto Toeplitz (1881-1940), later of Toeplitz matrix fame, told him that if he wanted lectures of the standard that he had received in Zurich, then he must go to Göttingen (Born 1978 79-80). His stepmother also gave Born a letter of introduction to Hermann Minkowski (1864-1909), whom she had met some years before at dancing lessons in Königsberg. Born describes Hilbert and Minkowski, both professors of mathematics in Göttingen at the time as 'the Castor and Pollux of the mathematical world' (Born 1978 p80). Born considered the faculty at Göttingen to be more progressive than most others in Germany, having hired two Jewish professors—Minkowski and Schwartzschild.

Born's personal life in his first year in Göttingen did not make much of an impact on him, but he writes that his 'scientific life was inspiring and interesting from the beginning' (Born 1978 81). He took courses in mathematics and in physics taught by David Hilbert (1862-1943) and Woldemar Voigt (1850-1919).

$$pq - qp = \frac{h}{2\pi i}\mathbf{1}$$

Where **1** is the unit matrix. This equation, Born considered to be his most important contribution—at any, rate it is inscribed upon his tombstone.

Born also attended courses by Hilbert on the logical foundation of mathematics and the quadrature of the circle. Under Hilbert's suggestion, Born started research on the latter topic for a doctoral thesis—he focussed on attempting a proof of the transcendental roots of the Bessel functions (wave functions with a decaying amplitude). However, he met with no success in this endeavour. Frustrated and dismayed, he went to Hilbert and explained that he was certain that he was 'no true mathematician' and that he 'would never again attempt to solve a problem from the domain of numbers or any other branch of pure mathematics' (1978 1978 84). Hilbert told Born not to worry and that similar things had happened in his youth. This assuaged some of Born's worries and he saw that there were plenty of other problems to which he have fewer difficulties applying himself (Born 1978 84).

Born also attended mathematics courses by Minkowski on the geometry of lines and spheres and attempted to sit through a number of Klein's courses, but always gave up half-way through (Born 1978 85). Born found Klein's treatment of physics uninteresting—the problems he treated were rendered abstract. This was also down to Klein's lecturing style, which he contrasts with Hilbert's:

'Hilbert was like a mountain guide who leads the straightest and best way to the summit; Klein was more like a prince who wants to show his admirers the greatness of his territory, guiding them in endless winding paths through apparently impenetrable territory and halting at each little hilltop to give a survey over the area covered' (Born 1978 85).

Born wanted to reach the summit as quickly as possible, and so preferred Hilbert's style, although he notes that his tastes changed over years and he now prefers to read Klein over Hilbert (Born 1978 85). In this period he additionally studied set theory in a course given by Zermelo (Born 1978 86).

Once again, Born did not enjoy his initial foray into physics at Göttingen. He writes of Johannes Stark (1874-1957), the lecturer of the first physics course he attended there 'what Stark did seemed to me pure dogmatics. He did not demonstrate experiments but described them in rather obscure terms and the results were formulated like articles of faith without any attempt at explanation' (Born 1978 86). Born notes that he came across Stark later on, who following the first world war began to denounce theoretical physics as 'Jewish' science and eventually joined the Nazi Party (1978 86-7).

But there were other physicists at Göttingen. Born attended Voigt's lectures on optics. He initially had considered giving them up, finding the number of calculations involved tedious, but was persuaded not to by a friend, Max von Laue (who would later win the Nobel prize for his discovery of x-ray diffraction in crystals). Born came to enjoy Voigt's course and what he learned there would form the basis of his own optics text book (Born 1978 88).

During this time Born became acquainted with the neo-Kantian philosopher Leonard Nelson (1882-1927), who tried to derive liberal political views from a priori principles. Born writes that at this time he was 'still under the spell of Kant' (1978 93). His mathematical studies had strengthened his position at at this point he held that there were 'fundamental facts' which were out of the reach of empirical proof but which could be arrived at in an a priori manner. He found Nelson's position that liberalism was the only rational political stance to be 'a shock' (Born 1978 93). Born's acquaintance (and his father's old friend) Dr Lachmann and Göttingen mathematician Erhard Schmidt both seemed to have convictions as strong as Nelson's but were a socialist and a conservative, respectively. He found this 'all rather bewildering' (Born 1978 93) and it made him

gradually reconsider his position on Kant. He writes 'Kant's teaching has a great attraction for one who is inclined towards rationalism...But it did not work for me. However, it was a slow process which made me acknowledge this failure and led me to a standpoint which might be classified as a kind of empiricism' (Born 1978 93).

Born also attended some courses of Edmund Husserl's (1859-1938), who was teaching at Göttingen at the time. He attended a lecture course of Husserl's near to the end of his study, but writes that he 'found them so dull he had to give up' (Born 1978 95). He had better luck with his seminar course, which dealt with philosophy of mathematics. Husserl discussed the epistemological validity of the mathematical axioms, offering a phenomenological solution. According to Born, this solution was comprised by 'the conviction that by a proper kind of contemplation and reflection on the meaning of a notion you can approach the 'phenomenon' itself, obtaining in this way perfect evidence' (Born 1978 96).

He was, for a while, impressed by Husserl, but soon began to consider his position hollow. He thinks of it as 'a kind of a priorism, like that of Kant, but a mystical one' (Born 1978 96). For all of his misgivings about Kant, Born appreciated his position, that 'there are principles or categories of thinking which are the conditions of actual knowledge and which you can discover by investigating the structure of this knowledge' (Born 1978 96). His thinks that this is actually not so different from what theoretical physics does, with the primary difference being that physics does not claim that it's 'latest analysis is a final and irremovable law, but just another step to a remote truth'. This is an interesting point in of itself—it suggests that Born accepts a kind of increasing verisimilitude view of progress in science. But he thinks that to try to find immutable truths via introspection alone is 'irreconcilable' with science (1978 96).

I also want to next highlight a small confusion in Born's *Recollections*. He writes:

As a matter of fact, the phenomenologists in Göttingen were a little group of conceited fellows who did not appeal to me. One of them, Heidecker, tried for some time to convert me to their creed, but in vain. I disliked not only his philosophy, but also his personality. He later became Husserl's successor in Freiburg and wrote a book which appears to me the summit of senseless accumulation of words (Born 1978 p96)

We might think that this means that Born encountered Martin Heidegger (1889-1976) at Göttingen and is simply misremembering his name. After all it was Heidegger who succeeded Husserl at Freiburg. But Born's Heidecker cannot be him. Heidegger was born in 1889 which would have made him 14 or 15 at this point, far too young to be part of the phenomenology group at Göttingen when Born was there. So I think that Born must simply be accidentally conflating two individuals with similar names. Still, it's clear from the passage that Born was not a fan of Heidegger's philosophy.

4 Doctoral Thesis

Next I'm going to discuss Born's doctoral thesis. He writes that 'it was in my second year in Göttingen (1905) that my scientific future was determined, not by my own deliberate choice, but by a series of events in which I was involved' (Born 1978 98). What happened was this. In Born's second year, two advanced seminars on mathematical physics were held, one taught by Klein and Runge on elasticity and one taught by Hilbert and Minkowski on electromagnetic theory. It was the latter that held Born's attention. He was fascinated by the failure of the various ether-detecting experiments which were discussed in the seminar. This seminar was also the starting point for Minkowski's work on the electrodynamics of moving bodies and the transformations which became part of relativity theory. This was what Born wanted to study and research.

He was however denied that opportunity, at that point at least, because of something that happened in the elasticity seminar. For each seminar a problem was assigned and from a the students a speaker and deputy speaker assigned to present and discuss solutions. Born was assigned the role of deputy speaker, whose job was mostly to discuss the paper of the main speaker if they were prevented from attending the seminar (Born 1978 99). The problem for Born's seminar concerned the stability of the elastic line. A week before the seminar on the elastic line, the main speaker, a student called Arthur Haas, came to Born and informed him that he would be unable to present as he was overworked and ill (Born wonders if in fact he was suffering from 'a kind of stage fright', something that was apparently not uncommon in students called to present before Klein). Born therefore had to present the problem himself (Born 1978 99).

Born attempted to examine the available literature on the topic but found that he was unable to get to grips with the details. However he did know that the problem was an example of a kind that he had attended one of Hilbert's courses on and had well understood. Applying these ideas, Born succeeded in giving the stability conditions for the elastic wire for a general case and showed that some of the existing solutions were examples of his general formulation. Born presented his results at the seminar and expected that to be the end if it (Born 1978 99).

However some weeks later, when at home for the Easter break, he received a letter from Klein. Klein wrote that he had liked how Born had approached the problem in looking for a general mathematical solution rather than examining specific examples. He had suggested that the subject annual academic prize of the philosophy faculty for the year 1905/1906, of which the mathematics department was a part, be 'The Stability of the Elastic Line' and expected that Born would compete (Greenspan 2005 31). The prize was rather prestigious. The subject was allotted to each of the departments in turn, and so any individual department could compete for it only very infrequently. The subjects were deliberately chosen so that at least some students would enter and so Klein selecting the

elastic line as the subject represented him personally selecting Born as a competing student (Born 1978 100).

Born writes that he 'ought to have felt honoured and pleased' by this (Born 1978 100). However, he was not. He was not really interested in the problem, preferring electrodynamics. He wrote to Klein to this effect, telling him that he had no 'deep interest' in the problem and would rather not enter his work for the prize. When Born returned to Göttingen for the term, he found that his reply had offended Klein greatly and it had made an enemy of him. This, Born says, could be the end of a mathematician's career in Germany (1978 100).

Born, therefore, went to Klein to humble himself and apologise, offering to do as Klein suggested and compete for the prize. But it did not work. He writes that Klein 'treated me like a naughty schoolboy' 1978 100). Born asked Klein if he would accept his work on the elastic line as a doctoral thesis. Klein refused, telling Born that he did not think he knew enough geometry to pass the oral examination as Born had not attended Klein's lectures on the topic (Born 1978 100).

All of this meant that Born would not be able to take his doctorate in pure mathematics, but would instead have to take it in applied mathematics. For this he would be examined by a mathematician and two scientists. For the examination, Born had to choose a second scientific subject to go along with physics, and so he attended Schwartzschild's course on astronomy (Born 1978 101).

I'd like to take a moment just to point out the astounding collection of talent at Göttingen that Born studied under: Hilbert, Klein, Minkowski, Carathéodory, Zermello, Husserl and Schwartzschild.

Born's progress on his thesis was slow (a shocking state of affairs…) but eventually he was spurred into action by the belief that he would be unable to join the Mathematics

Society, of which many of his friends were members, as he was convinced that Klein would veto any application he made. However, he believed that if he won the academic prize and got his doctorate Klein would be unable to deny him. Born notes that on later reflection and knowledge of Klein's character, he would not actually have prevented him from joining (Born 1978 103).

Born was not particularly interested in the precise problem at hand—how wires bend—but he was interested in the type of problem it was—one of stability. He writes that these sort of problems are the key to understanding not only natural and man-made structures but also catastrophes within such systems. By 'catastrophe', Born means situations like the sudden snapping of a wire during oscillation. Born was unable to solve the problem for a thin wire and so simplified it by considering steel tape (like that used in a tape measure). In this he could study instability in a plane. He was able to obtain practical results via applying methods from pure mathematics to the problem and eventually ended up with a series of predictions for the conditions under which catastrophes would occur. These he tested experimentally and found them to be accurate, a process he found deeply satisfying (Greenspan 2005 35). The

The work was duly handed, accompanied by a quote from Faust on the subject of the inescapability of error (Greenspan 2005 35). Born won the prize for his work and took his examination a few weeks, inn the summer of 1906, which went without incident and he was passed *magna cum laude* (Born 1978 105).

Born had acquired a taste for science from his experiments and wanted to learn more, although he also desired to leave Göttingen. Before he was able to undertake any further scientific work, he had to participate in compulsory military service, in which he served in a Berlin cavalry regiment, the 2nd Dragoon Guards. Usually military service was a year long, but Born was discharged in January 1907 after six months due to his weak lungs—he was asthmatic (Born 1978 106-114).

After his discharge, Born returned to Breslau for a short time before heading to Cambridge in March. He wanted to learn to speak English better and had been informed about English experimental physics by his friend James Franck, who spoke of it admiringly (Born 1978 115). To this end, he travelled to Cambridge. There, he was enrolled as an advanced student at Gonville and Caius. Here, he undertook language lessons as well as attending courses by JJ Thomson on experimental physics and Joseph Larmor on electricity and magnetism. Born found Larmor's lectures incomprehensible because of his 'strange pronunciation' which Born attributes to an Irish accent (Born 1978 117). He enjoyed Thomson's experiments, finding them 'fascinating' (1978 117) but the subject matter was a little beyond him and so he took an elementary course in electricity. Born stayed in Britain over the summer and did some sightseeing, visiting the cathedrals in Peterborough, York, Lincoln and Durham (!), and then spending a few days in Edinburgh (Born 1978 120). He returned to Breslau in August 1907.

5   Habilation and Lectureships in Göttingen and Berlin.

Born had made up his mind to leave mathematics and become a physicist (1978 121). This process was slightly disrupted by a second call up for military service. Born had been called for another medical examination by the German consulate whilst in England and had unfortunately been taken as a malingerer by the officer he saw there (1978 119). He served another month (October 1907) with the Cuirassier regiment before once again being discharged on medical grounds, thanks to a friendly doctor (Greenspan 2005 40).

Following the disruption, Born became a research worker in the physics laboratory at Breslau. There he performed research on black-body radiation. He also started research on relativity, after being asked by Reiche, the head of the physics school in Breslau if he had come across some work by Albert Einstein (1879–1955). Born had not heard of it at that point, but on learning that it concerned the fundamental principles of optics and

electrodynamics he decided to study it with Reiche (1978 130). The topic quickly became his chief interest. On becoming stuck on some particular problem, Born wrote a letter to Minkowski in the summer of 1908 asking for his advice. Minkowski replied that he was interested in the same problems and would Born like to return to Göttingen to collaborate with him? (Born 1978 130-1). And so Born returned to Göttingen.

Sadly, a few weeks after Born's arrival in Göttingen in December 1908, Minkowski fell ill with appendicitis and passed away in January 1909 (Greenspan 2005 43). Born worked on Minkowski's posthumous papers as well as some ideas of his own concerning the self-energy of the electron (the potential it feels due the interactions between it and the vacuum field), a problem which, at least when he was writing the *Recollections,* he did not feel had been solved properly (Born 1978 p133).

Because of these interests, Born tried to derive relativistic equations of motion for an accelerating electron. This lead him to consider the problem of accelerating rigid bodies in a relativistic system, on which he published a paper in *Annalen der Physik* in 1909. This work also formed the basis of the thesis which earned him his Habilation in the summer of 1909 and thus qualified him as a lecturer in the German university system (Born 1978 137).

Much of Born's work in this period was focussed on the lattice structure of crystals and was done in collaboration with Theodore von Kármán (1881-1963). Born explains that their focus on this was due to some work being done in Göttingen by Erwin Madelung (1881-1972) on lattice vibrations. Kármán considered Madelung's handling of the problem to be poor (Born 1978 141) and suggested to Born that they consider it. Their first collaboration (Born and von Kármán 1912) introduced a number of the fundamental concepts of lattice dynamics: the association of the degrees of freedom of the crystal with the normal modes of vibration of the whole body; the use of Fourier analysis; the periodic boundary condition (Kemmer and Schlapp 1971 25). Kemmer and Schlapp note that Debye came to very similar conclusions at the same time, but his methods of approaching

the problem were very different, preferring to use simplified models that made no pretence of physical reality (Kemmer and Schlapp 1971 26). They further note that although Debye's work made a greater impact at the time of publication, it eventually became clear that Born and von Kármán's method was the deeper one and gives a more accurate picture of specific heats at a wider range of temperatures (Kemmer and Schlapp 1971 26).

In 1911 Albert Michelson (1852-1931), one of the two collaborators on the famous of Michelson-Morley experiment, came to Göttingen as a visiting professor and became friends with Born. This lead to Born receiving an invitation from Chicago, Michelson's home institution, to give a course of lectures there on relativity. So in the summer of 1912, Born headed off the USA. There he gave his lecture course and worked with Michelson in the laboratory photographing line spectra (Born 1978 148).

It was also in 1912 that Born and his eventual wife, Hedwig 'Hedi' Ehrenberg, met. Dinner parties were often held by the Göttingen professors and the young lecturers were invited along. It was at one of these that Born met Hedi, the daughter of a professor in the law faculty (Greenspan 2005 57). Shortly afterwards, in May of that year, they announced their engagement and were married in the summer of 1913. There was some dispute about the venue. Hedi, whose father was a Jew who had converted before marriage, wished for Born to do the same so that they could have a church wedding. Born did not wish to do this. A compromise was made by holding the wedding in the (rather palatial) house of his sister with a Lutheran priest performing the ceremony (Greenspan 2005 58).

Born continued his work on crystal solids for the next few years and in 1914 was granted a contract to write a monograph on the subject entitled *Dynamik der Kristallgitter*, half the royalties for which he was forced to accept in books from the publisher. It was in May of that year that his daughter Irene was born (Born 1978 159).

In 1915 Born moved to Berlin to take a position as Professor Extra-Ordinarius, having been recommended for the post by Max Planck. It was here that he struck up his friendship

with Einstein (Born 1978 167), which would continue via correspondence (see the Born-Einstein letters, Born 2005) for the rest of Einstein's life.

In June of 1915, Born joined the German Army, becoming part of a unit of scientists headed by Max Wien who were working on wireless communications for aeroplanes. He notes that he was also asked to join a unit under Fritz Haber working on gas warfare, but he refused to have anything to do with it (Born 1978 168). A few months after joining the unit, Born once again fell ill due to asthma after drill-marching in poor weather (Greenspan 2005 71). He left the wireless group and instead joined a technical institution within the military called the A.P.K. (Artillerie-Pfrufungs-Komission or Artillery Testing Commission) to work on detecting the position of artillery by measuring from different positions the arrival of the sound of a gun firing. It was here that he had his first exposure to Gestalt psychology, a theory with which he became somewhat enamoured (see, for example, Born 1948 208), from two psychologists, Wertheimer and von Hornstobel who had also joined the unit (Born 1978 173). In November of 1915, Hedi gave birth to the Borns' second child, Margarethe (Greenspan 2005 77).

After the war, Born settled back to scientific work and began to try to work out the chemical consequences of his theory of ionic crystals. Born calculated the lattice energy of such crystals from his theory, but found that there was no empirical data to check it against. This brought Born once more into contact with Haber who informed him that no one had ever calculated chemical energy before and that his attempts looked promising (Born 1978 189). Born managed to use the ionization potentials of the molecules to predict the heats of reaction for alkali halides (Kemmer and Schlapp 1972 30 and Born 1919). Fritz Haber's presentation of this would lead to what is known as the, the Born-Haber cycle (Kemmer and Schlapp 1972 30).

In the winter on 1919, Born moved to Frankfurt to take up a position as a full professor there. In Frankfurt he wrote that the department 'was dominated by an idea of Stern's (Born 1978 195). This idea was to investigate the physical properties of atoms and

molecules in a gas using molecular rays. It was for this work that Stern won the Nobel prize in 1946 (Stern 1946). Born took an interest in this and, along with Elisabeth Borman, worked on an experiment to determine molecular collision cross-sections (Born 1978 195). With Gerlach, Stern went on to perform the famous Stern-Gerlach experiments to measure the magnetic moments of atoms and confirm some of the consequences of Sommerfeld's work on quantum theory, in particular quantisation of electron orbits. Despite the economic difficulties in Germany at the time, Stern was able to perform the experiments due to a donation that Born managed to acquire from Henry Goldman, of the bank Goldman-Sachs (Born 1978 197).

6   Back to Göttingen; Quantum Mechanics

In 1921, Born was offered a chair at Göttingen as head of the whole physics group, both experimental and theoretical. He was somewhat uncomfortable with this, considering himself to be not be a sufficiently talented experimentalist to run the lab at Göttingen. He resolved this difficulties by persuading the ministry of education to appoint his old friend James Franck as professor extraordinarius in charge of the experimental group. Born hired Wolfgang Pauli as his assistant when he arrived in Göttingen. When Pauli left in 1923 to take up a professorship, Born hired Werner Heisenberg as his replacement. Kragh (1999 159) notes that Born had turned Göttingen. into a 'world-centre' of quantum theory. There were also close links with the other centre in Germany, the department at Munich under Sommerfeld—Pauli and Heisenberg were both former students of Sommerfeld's (Kragh 1999 159). It was during the summer of 1921, a few months after Born had arrived in Göttingen, that his son, Gustav, was born (Greenspan 2005 107).

The focus of Born's work during this time was the development of a quantum theory of the electronic structure of atoms (Born 1978 214) and in particular to what extent Bohr's theory of the atom could account for the facts. The first problem that Born and his team tackled was whether or not Bohr's work could be generalized from the s single-electron

systems such as a hydrogen atom in which it worked well to a multi-electron system (1923).

In a paper with Heisenberg (Born and Heisenberg 1924) he applied perturbation methods (as he again would do in his 1926 collisions paper), developed in astronomy to the problem and met with some limited success. He succeeded in producing qualitative results which agreed with the 'main properties' of molecules but failed to accurately reproduce the correct spectroscopic results. It was at this point widely accepted that there was something not right with the Bohr-Sommerfeld formulation of quantum theory (Kragh 1999 159). Born writes that 'we became more and more convinced that a radical change of the foundations of physics was necessary, and thus a new type of mechanics, for which we used the term quantum mechanics' (Born 1978 p215).

Born recalls that this term 'quantum mechanics' term first appeared his paper, Quantum Mechanics/ *Über Quantenmechanik,* (1924/2007) which he regards as an 'essential' step towards the development of theory. In this paper he aimed to translate classical formulae into their quantum analogues (Kragh 1999 159). It was known that vibrational frequencies, corresponding with absorption frequencies for radiation, behaved differently from the classical model. Instead of being dependent on a single orbit, they were dependent on energy differences between stationary states and could only take precise values. This was true for all physical quantities in the new field—they are dependent on transition quantities between two stationary states. Kramers (1924) had succeeded in giving an account of the optical properties of atoms purely in terms of these transition quantities. Born describes this, somewhat dramatically, as 'the first step from the bright realm of classical mechanics into the still dark and unexplored world of the new quantum mechanics' (Born 1978 p216). Born writes that he made the next step by if one could find in a similar manner a theory for the interaction between two electronic systems in terms of transition quantities. Indeed, one could. The whole Göttingen group was involved in the discussion which lead to this work. Due to his expertise in perturbation methods, Born was the natural person to carry

out this step (Hendry 1984 59), which were used to carry out the re-interpretation of the classical equations into quantum mechanics.

Next Born and Jordan worked on developing a fully quantum mechanical version of Planck's theory of radiation, which had used classical theories to model the interaction between light and matter (Born 1978 216). Here Born and Jordan gave a quantized analysis of classical multiply periodic systems (Kemmer and Schlapp 1972 33).

Born also writes that they were 'struck by the fact that these 'transition quantities' appearing in our formulae always corresponded to squares of amplitudes of vibrations in classical theory' (Born 1978 216). He suggested In the daily meetings held by Heisenberg, Jordan and himself, he suggested the idea that such 'transition amplitudes' could be formulated and might well be central to the theory. I suspect that Born's point here is that the idea of working out the transition amplitudes was at least partially his, and not purely Heisenberg's.

During this time, Heisenberg worked on his own ideas whose purpose Born describes as 'somewhat dark and mysterious'. This work culminated in a manuscript that he sent to Born to look over in July 1925. Born was fascinated upon reading it: Heisenberg had developed a calculus for transition amplitudes.

He also offers a criticism of Heisenberg's famous requirement (Heisenberg 1925/2007) that quantum mechanics should eliminate classical quantities which cannot be observed and that the theory should be built only from observable quantities. He writes 'It has often been interpreted as the requirement that all quantities should be eliminated which are not directly observable. Yet I think that in this general and vague formulation the principle is quite useless, even misleading. The question of which are the redundant quantities can only be decided by the intuition of a genius like Heisenberg' (Born 1978 217).

 Given that in later work he praises this principle as used by Heisenberg and what he takes to be a variation on it used by Einstein (although the autobiography was published in 1978, this section was written during the second world war). I want to take a moment to think about what Born means here. Firstly, we might take it as a note of caution. Born thinks that it is difficult to apply properly and needs to be accompanied by a developed physical intuition. It should also be pointed out that there is a world of difference between something that is directly observable and something that is unobservable even in principle. What I think that Born is getting at here is that it is a mistake to engage in the semantic elimination from our theories of such quantities that are only measurable indirectly. We might still apply the principle in order to argue that we should not use in principle unmeasurable quantities in our theories or even that we might still use them if it is useful to do so without taking them to represent real facts about the world.

After Heisenberg's paper was sent off for publication, Born writes that he became fixated upon Heisenberg's symbolic notation, feeling that there was 'something fundamental behind it, the consummation of our endeavours of many years' (Born 1978 217). After puzzling over it for a few days, Born realised that the system Heisenberg had developed was actually the matrix calculus that he had studied at university. He writes 'I suddenly saw light: Heisenberg's symbolic multiplication was nothing but the matrix calculus, well known to me since my student days' (Born 1978 217). Hendry (1984 70) thinks it unlikely that Born, having been educated as a mathematician that Born would have puzzled over the matrix element of the paper, and suspects that he was more concerned with the physical interpretations of the paper.

It quickly became clear to Born that the matrix products for *p* and *q,* the momentum and position operators, did not commute. He found that Heisenberg's work gave values to only the diagonal elements of the matrix *pq-qp.* These were all equal to h/2πi. He convinced himself that the off-diagonal elements must be equal to zero, and thus produced the commutation relation:

$$pq - qp = \frac{h}{2\pi i}\mathbf{1} \quad \text{(Born 1978 218)}$$

The work of proving this was left to Jordan, whilst Born went on holiday to Switzerland (Hendry 1984 71). The resulting paper, *Zur Quantenmechanik* was published in September 1925 (van der Waerden 2013 277).

In the Autumn of 1925, Born, Heisenberg and Jordan collaborated on the famous 'dreimannerarbeit', which was published in the spring of the following year (Born, Heisenberg and Jordan 2013). Born writes that his main contribution to the paper was 'the relation of our theory to Hilbert's work on quadratic forms of an infinite number of variables … Here the matrices are used as vectors (in a space of infinite dimensions, the so-called Hilbert Space)' (Born 1978 220).

At the end of October Born travelled to America for a second time to give a series of lectures at MIT. These concentrated on his work on crystal dynamics and on the new quantum mechanics that had just been developed at Göttingen. These lectures were collected and eventually published as *Problems of Atomic Dynamics* (Born 2004). Whilst there, Born continued to work on quantum mechanics, collaborating with Norbert Wiener on a paper which Born thought almost produced a version of wave mechanics prior to Schrödinger. Wiener had recognised that matrices could be generalized as operators, which were similarly non-commutative but had wider application (Hendry 1984 81). Although Born had some qualms about the mathematical legitimacy of Wiener's methods, he worked with him to produce an operator calculus for quantum mechanics (Jammer 1966 221). As to what they missed, Born writes 'We used the differential operator *D=d/dt* and identified it with *(2πi/h)W*, where *W* denotes the energy, but we failed to see that in the same way *d/dq* represents *(2πi/h)p*, where *p* is the momentum belonging to the coordinate *q*' (1978 226). Had they done this, Jammer (1966 223) (and Born) argue, they would have ended up with an operator version of wave mechanics prior to Schrödinger.

After finishing his MIT lectures, Born embarked on a lecture tour of the American west, speaking at Pasadena and Berkeley. He met a number of young American physicists who promised to visit Göttingen to study the new theories with the group that discovered them. Indeed, when Born returned to Göttingen. there was indeed an influx of physicists eager to learn quantum mechanics. Born held a private advanced seminar at his home. Those who attended included EU Condon and Robert Oppenheimer from the USA as well as Friedrich Hund, Enrico Fermi, Paul Dirac, Leon Rosenfeld, Eugene Wigner, John Von Neuman, Vladimir Fock and Egil Hylleraas (Born 1978 225-27).

In 1926, Born published his paper on collisions in quantum mechanics, utilising both wave and matrix mechanics to give a statistical interpretation of the wavefunction (this paper is discussed in some detail in Chapter 6). Born writes that he was 'taken by surprise' by the appearance of Schrödinger's wave mechanics papers, which he regarded as a theory of 'fascinating power and elegance' (Born 1978 229). Heisenberg seems to have been less impressed by Schrödinger's work, calling it 'disgusting' (Kragh 1999 166). The theory of electron waves, developed by de Broglie was familiar to Born when Schrödinger's papers were published. He writes that 'at that time it was clear that the proper interpretation of quantum mechanics must be of a statistical type' (Born 1978 p231). A number of attempts were made to do this using matrix mechanics. Born writes that he hit upon the idea of using aperiodic collision processes to solve the problem for, as he writes 'incoming and outgoing particles could be counted and regarded as empirical probability values' (Born 1978 p232).

When he read Schrödinger's papers, he saw how to do this. He was also guided by an idea of Einstein's (or at least in 1961 when he wrote this part of the book, he recalls that he was): that one could interpret the intensity of a wave as representing the number of photons, at least statistically. Born writes that these ideas 'led immediately to the conjecture that the intensity of the de Broglie wave i.e. the (absolute) square of Schrödinger's wave function must be regarded as the probability density, which is the probability of finding a particle in a unit of volume' (Born 1978 232). Neither Heisenberg

nor Pauli were happy with Born's use of wave mechanics, with Heisenberg being angry at Born's use of classical concepts (Hendry 1984 93).

After his initial 1926 paper on collisions, Born applied these ideas to deriving the adiabatic principle (proposed by Ehrenfest) in quantum theory, collaborating with Fock (Born and Fock 1928). He also spent some time working out a practical example of the collision theory involving an alpha particle colliding with a helium atom (Born 1978 233).

In the winter of 1928, Born suffered a breakdown and went to convalesce in a sanatorium on Lake Konstanz, on the German-Swiss border. Unfortunately, the other patients were all supporters of the Nazis and on opposing their views, Born was 'treated as an outcast' (Born 1978 240). He went from Konstanz to the town of Königsfield in the Black Forest, where he had a much better time (1978 240). He relates how upon walking past the church in town one evening he heard a masterly performance of a Bach Fugue. Playing it turned to be Albert Schweitzer, the famous theologian, philosopher, physician and organist. He and Born became friends and spent much time walking and discussing humanistic matters, which seems to have greatly aided in his convalescence (Born 1978 241).

In the spring of 1929, Born returned to work in Göttingen. In the early 1930s, Born gave a lecture series to an audience of students from all faculties as part of a university initiative to counteract the increasing specialization of the different academic departments. Born presented his work from what he describes as a 'general philosophical point of view'. He regrets (as do I) that the notes from these talks have been lost as he 'should like to see how I presented the philosophy of science at that time' (Born 1978 246). He also gave a series of lectures in Berlin, published in 1933 as *Moderne Physik,* later translated into English as *Atomic Physics* (Born 1935).

In 1932, Born was elected Dean of the Faculty of Science for the year, the only time of his life that he held a primarily administrative post (Born 1978 247). He did not enjoy it. Germany's economic woes had reached their height and Hitler's power and the

corresponding anti-Semitism was growing. During this period, he managed to avoid the sacking of all non-permanent members of staff as ordered by the government by getting the senior staff to pool some of their wages to hire the junior members as assistants (Born 1978 248). Also during that year, Rutherford visited Göttingen. to receive an honorary doctorate which Born, as dean of the faculty, conferred on him. A recording of the Born's address and Rutherford's lecture was made by Robert Pohl, another Göttingen physicist and later published (It can be heard here: http://www.robinmarshall.eu/goettingen.html) (Born 1978 249).

7   Leaving Germany; South Tyrol; Cambridge; and Edinburgh

On 30 January 1933, Hitler became chancellor of Germany. Born at this time began to investigate ways of leaving Germany and Hedi, his wife visited friends in Switzerland to ask their advice. This was unanimous: to get out of the country whilst they still could (Born 1978 251). On the 25th of April a list of civil servants who were to be dismissed from their positions was published. Amongst those was Born and other Jewish professors. In May, Born and his family left Germany for the town of Selva in South Tyrol where they had booked a holiday home, although they had no intention of returning home (Born 1978 253).

Born's salary was still being paid and so he had no immediate financial problems. In any case, he was soon made a number of job offers for positions at Columbus Ohio, Paris, Belgrade, Cambridge and Oxford. He accepted the Cambridge position, although it was temporary, travelling there at the end of September 1933. He writes that a somewhat unnerving situation greeted him there - written on all of the hoardings were the words 'The Man Born to be Hanged'. Of course, it soon turned out that this did not refer to Born but rather was the title of a recently released film based on a crime novel. Born notes that

he found a copy and rather enjoyed it (Born 1978 265). It is in this spirit that I have included a number of puns in the thesis.

Born gave a course of lectures on the non-linear electrodynamics that he had developed whilst in Selva (Born 1978 267). He also continued work on this, trying (ultimately unsuccessfully) to work out a quantum field theory. He also studied some nuclear physics himself, an area into which he had previously not looked (Born 1978 267).

Much of his time was dedicated to trying to secure jobs for Jews in Germany who had lost their positions employment elsewhere. He succeed in getting a number of scientists hired by the Academy of Science in Lima and some more by its Colombian counterpart. He also talks sadly of failing to find Alfred Wittenberg, a well-known violinist of the time, a position in the UK (Born 1978 269).

In the autumn of 1935, Born travelled to Bangalore for six months at the invitation of C.V. Raman, the Nobel Prize winning Indian physicist. Raman tried to arrange for Born to have a permanent position there (his Cambridge position was nearly over) but did not succeed due the rather unpleasant intervention of the English professor of engineering at the university, Aston who told the faculty that a 'second rate foreigner who was driven out of his own country was not good enough for them' (Born 1978 277).

On returning to Cambridge in 1936, Born writes that he had an uncomfortable time of things—it was not clear that his position would be extended and with all of his property and most of his savings in Germany and so largely inaccessible, his financial situation was precarious. In the summer of that year, he received a letter from Peter Kapitza offering him a position in Moscow. Born was uncertain about moving to Russia, but considered the offer as it was not certain that it would be possible for him to remain in England. In the end, Born was offered the Tait Chair of Natural Philosophy at Edinburgh after it was vacated by Charles Galton Darwin, an old friend of his, and accepted this in preference to

the Moscow position—a move that he points out was fortuitous given the treatment of foreign intellectuals in Stalin's purges in the years to come (Born 1978 281).

Born undertook some teaching at Edinburgh at a level that he considered rather low (Born 1978 282-3). He also had some research students. Amongst these were Klaus Fuchs, who would later work on the Manhattan project and pass nuclear secrets to the USSR (Born 1978 284). A number of Chinese research students also studied under Born at Edinburgh, including Kun Huang who collaborated with Born on a book concerning lattice dynamics (Born and Huang 1954). He also worked with the American physicist Edward Corson and Herbert S. Green.

8   Retirement From Edinburgh and Later Life; Nobel Prize; Pugwash

Born's *Recollections* does not cover his life from this point, but there are accounts of it from his son, Gustav and the *Biographical Memoirs* paper as well as his correspondence with Einstein. Having reached the age of 70, Born retired from his chair at Edinburgh in 1953 and moved back to Germany. This was several reasons, both personal and financial. As Born had not worked in Britain for most of his life, his British pension was small and not enough to live on. As part of restitution efforts after the war, he was entitled to his full pension from Göttingen as well as restoration of his property, but this money could not at that point be moved outside of Germany (Born 1978 296). Gustav also writes that Born hoped to use his position as a prominent physicist to persuade Germany not to arm itself with nuclear weapons. Born used the money he had to build a house in Bad Pyrmont, a spa town in Lower Saxony that was not too far away from Göttingen (Kemmer and Schlapp 1971 23). Bad Pyrmont was also home of the headquarters of the Religious Society of Friends in Germany—Hedi had become a Quaker whilst in Edinburgh (Born 2005 197).

Einstein seemed to find Born's decision to move back to Germany mystifying even if it was compelled by financial circumstances—he writes of Born's return to the 'land of the mass-murderers of our kinsmen' (Born 2005 195). Born replied that he probably would have moved back even if he did not have to—his wife was homesick and the country around Bad Pyrmont beautiful (Born 2005 200). Born did not really agree with Einstein's characterisation of the German people. He certainly shared that opinion when the horrors of the concentration camps became known, but revised it upon learning of the situation of Germany post-war (Born 2005 195). He wrote in reply to Einstein '[the German Quakers have their headquarters in Pyrmont. They are no 'mass-murderers', and many of our friends there suffered far worse things under the Nazis that you or I. One should be chary sic of applying epithets of this sort. The Americans have demonstrated in Dresden, Hiroshima and Nagasaki that in sheer speed of extermination they surpass even the Nazis' (Born 2005 200).

In the commentary on the letter, Born writes that he stands by this position and can see little difference between Hiroshima and Nagasaki: 'the usual reasoning is the following: the former case is one of warfare, the latter of cold-blooded slaughter. But the plain truth is that the people involved are in both instances non-participants, defenceless old people, women and children, whose annihilation is supposed to achieve some political or military objective' (Born 2005 201). I mention this to make clear the strength of Born's opposition to nuclear weapons and their use. It is not that he thinks that such mass warfare targeting civilian populations is somehow worse that the Holocaust, it is that he thinks both equally monstrous.

Born castigated those scientists who worked on weapons of war and their application. He writes of Frederick Lindemann (1886-1957) , a physicist who Born had known in Berlin and who became an advisor to Churchill and advocated the bombing of civilian populations 'Lindemann did base things and opened the gates of hell for other men of his type, men efficient and clever, but not profound and wise, who later became leaders in science and its applications to politics and war' (Born 1978 262).

In 1954 he was awarded the Nobel Prize in Physics, sharing that year's award equally with Walter Boethe. Born was given the prize for 'for his fundamental research in quantum mechanics, especially for his statistical interpretation of the wavefunction' (Nobel Prize Committee 1954). One might ask why Born was not awarded the prize in 1933 along with Heisenberg [Heisenberg was awarded the 1932 Nobel prize in 1933 after the prize was reserved for a year as the committee felt that none of the 1932 nominations met the appropriate standards (Nobelprize.org 2014)], Schrödinger and Dirac, instead having to wait 28 years. This certainly seems to have affected Born at the time. In the commentary on a letter from Einstein complementing him on winning the Nobel (the date is not supplied for the letter but Born replied in November, so it is presumably at some point in the year before this) Born writes 'The fact that I did not receive the Nobel prize in 1932 together with Heisenberg hurt me very much at the time' (Born 2005).

His own supposition was that opposition to the statistical interpretation from significant physicists of the time—he names Einstein, de Broglie, Schrödinger and Planck—led the Nobel committee to avoid awarding him the prize until his interpretation had become more widely accepted. There may be something to this, although Born cannot be quite right, as the committee's records state that de Broglie nominated Born for the Nobel in 1946 (Nobelprize.org 2014). There simply is not available information to determine if this was indeed the case. He stated another reason in his reply to Einstein (he was not so rude as to write to Einstein and say that it was his fault that he had to wait so long for a Nobel), that 'the intention was to honour something which has no immediate practical application, something purely theoretical' (Born 2005 225). He notes that Linus Pauling 'a man known for his upright political conduct and his rejection of the misuse of scientific discoveries' also received the Nobel Prize for chemistry at the same time. He goes on to say that 'This could be chance, but it does appear to have been done on purpose, and that would be gratifying' (Born 2005 p225). What Born refers to here is opposition to nuclear weaponry and the use of physics to develop weapons of war in general. He refers later in the letter to

an article he written opposing the building of more atom bombs. As with his other supposition, it is possible that the prize committee decided to award the Nobel to Born because of his anti-nuclear position, but we cannot know.

Born's anti-nuclear stance should be mentioned here. He was one of the original signatories on the Einstein-Russell manifesto (see https://pugwash.org/1955/07/09/statement-manifesto/), the founding charter of the Pugwash Conference. We can see his attitude in the introduction to *Natural Philosophy of Cause and Chance*, where he writes 'It is true that many scientists are not philosophically minded and have hitherto shown much skill and ingenuity but little wisdom. I need hardly to enlarge on this subject. The practical applications of science have given us the means of a fuller and richer life, but also the means of destruction and devastation on a vast scale' (Born 1948 2). I think that it is clear that Born is referring to the development of the atom bomb here, as well as earlier uses of science to wage war such as the use of poison gas. Much of his retirement was dedicated to this sort of writing, along with some of the philosophical work that this thesis discusses.

He passed away on the 5th of January 1970, survived by his widow Hedi, two daughters and a son.

Chapter 3: Born on Causation

1   Introduction

It is an oft-argued claim that there is no genuine causation in physics (see, for example Russell (1912/1989) and more recently Norton (2003)) and that philosophers are mistaken in claiming that there exist causal principles in the discipline. Russell writes in *On the Notion of a Cause* that 'we found first that the law of causality, as usually stated by philosophers, is false, and is not employed in science' (1912 26). Norton in *Causation as Folk Science* tells us that 'At a fundamental level, there are no causes and effects in science and no overarching principle of causality', although it is the case that 'in appropriately restricted domains our science tells us that the world behaves just as if it conformed to some sort of folk theory of causation' (2003 21). The relevance of this point is not limited to the philosophy of science, or even metaphysics: it is, for example, of considerable importance in the mental causation debate. Crudely put, if causation is absent from physics on a basic level, then there is one less reason to worry about how mental causes could have physical effects. This of course does not end that debate—even if causation is absent from fundamental physics, it could still be an emergent feature of the world.

Against this background it seems interesting to examine the of Born's thoughts as someone deeply involved in the development of quantum mechanics who believed that 'scientific work will always be the search for the causal interdependence of phenomena' (1949a 18).

This chapter will examine Born's thoughts on causation and the role that his causal principles, contiguity (causal connection) and antecedence (causal priority), are meant to play in theory construction and development. This will be done by examining firstly the definitions he gives and secondly his treatment of the status of the principles in the history of physics. I will then examine a 'zoo' of options for the status of these principles in physics: Kantian synthetic a priori principles; Hasok Chang's discussions of ontological principles and intelligibility (2001, 2009); Eli Zahar's discussion of metaphysical principles in Einstein's work (1973), Emil Meyerson's principles of identity (Zahar 1989); and John Watkins confirmable and influential metaphysics (1958).

2   Born's Causal Principles

There are, Born notes, two distinct ways in which causality is used and understood in ordinary language. The first is as a timeless relation of dependence—a causal law, if you will. This notion is expressed via statements such as 'Wars are caused by the economic conditions' and 'Chemical reactions are caused by the affinity of molecules' (Born 1949a 5).

The second is a relation of dependence between specific events, fixed in time and space. This is expressed by statements like 'The American war of secession was caused by the economic condition of the slave states' and 'The destruction of Hiroshima was caused by the explosion of an atomic bomb' (Born 1949a 5).

According to Born, both of these are meaningful uses of the term causation: each expresses a different sense of the notion. What is shared between them—what makes them both expressions of the notion of causation - is dependence. What, for Born, is dependence? How can it be established? The notion is clear, he thinks, in mathematics where it is

equivalent to the word 'function' (Born 1949a 6). But this is not what Born thinks dependence in causality refers to—it should refer to dependence between concrete objects: 'real things in nature' (1949a 6). What this means, Born acknowledges, is not a simple matter.

Science, Born thinks, uses repeated experimentation and observation as a criterion for establishing relations of dependence. Of course, as he points out, one can never perform enough observations/experiments to establish such a relation with certainty and so one must employ inductive principles. The justification for their use is ultimately, Born thinks, that they work. There is no way of proving induction. Born says '…there is no logical argument for doing so; it is a question of faith. In this sense I am willing to call induction a metaphysical principle, namely something beyond physics' (1949a 7). On the other hand, the application of the principle works—'science has worked out a code, or rule of craft, for its application. This code has been entirely successful, and I think that is the only justification for it' (1949a 7). Born does not think that this is in any way good grounds for rejecting the use of induction in science—'I do not hesitate to call a man foolish if he rejects the teachings of experience because no logical proof is forthcoming' (Born 1949a 7).

 Born stresses that even if the causal character of a relation between events is neglected, there may still exist some predictable regularity. Born uses the example of a train timetable—one can use it to accurately predict the time of arrival of a train, but one cannot say that the arrival of the train was caused by the timetable. Thus 'the law of the timetable is deterministic' (although in the present day train times appear to be modelled more on notions of quantum indeterminacy, in 1949 such concepts had yet to be applied to the transport system, which remained rigidly Newtonian in character). It allows us to make predictions, but gives no sense of why those predictions might be correct. Therefore, Born says '...one should not identify causality with determinism' (Born 1949a 8). That there exist rules which allow one to predict from one event A the occurrence of another

event B does not entail that there exists a causal relationship between them. At the very least, predictability by itself is not enough to do the job.

Born goes on to give his definitions of determinism, causality, antecedence and contiguity, which I shall state below in full:

> **Determinism** postulates that events at different times are connected by laws in such a way that predictions of unknown situations (past or future) can be made.
>
> By this formulation religious predestination is excluded since it assumes that the book of destiny is only open to God.
>
> **Causality** postulates that there are laws by which the occurrence of an entity B of a certain class depends on the occurrence of an entity A of another class, where the word 'entity' means any physical object, phenomenon, situation or event. A is called the cause, B the effect.
>
> If causality refers to single events, the following attributes of causality must be considered:
>
> **Antecedence** postulates that the cause must be prior to, or at least simultaneous with, the effect.
>
> **Contiguity** postulates that cause and effect must be in spatial contact or connected by a chain of intermediate things in contact. (Born 1949a 9)

The definition of causation itself is a relatively minimal one—there is causality in just the case that there exist laws by which some event, entity or phenomenon is dependent on another. This, Born seems to think is the essence of causation—'a timeless relation of dependence' (1949a 6). Born has already said that he thinks the concept of causation is applied in a second way, as a relation between two events fixed in space and time. It is to

relations between such specific events that the principles of antecedence and contiguity are meant to apply.

So we can see that for Born causation is a kind of law-like physical dependence. It gets more specific when we examine individual instances of causation—in these cases, those instances must obey certain principles in order to be causal. These principles seem intended to provide a way to constrain individual instances of causal processes—it's only laws that act at the level of timeless and spaceless laws of dependence. The definitions seem to have some common-sense basis. Born writes that 'it is always presumed that cause precedes effect; I propose to call this the principle of antecedence. Further, it is generally regarded as repugnant to assume a thing to cause an effect at a place where it is not present, or to which it cannot be linked by other things; I call this the principle of contiguity.' (Born 1949a 8). Antecedence ensures that cause is always temporally prior to effect—effectively blocking backwards causation from counting as causation. Contiguity, by ensuring spacial connection between causal relata, blocks action-at-a-distance and—presumably—ensures locality of causal relations.

3 The Causal Principles in the Development of Physics

Now I want to take a look at some specifics regarding how Born deals with his principles of contiguity and antecedence in the history of science.

3.1 Contiguity

Born starts his discussion of the contiguity principle by writing:

> Although I maintain that neither causality itself nor its attributes, which I called the principles of antecedence and contiguity, are metaphysical, and that only the inference by induction transcends experience, there is no doubt that these ideas have a strong power over the human mind, and we have evidence that they have influenced the development of classical physics. Much effort has been made to reconcile Newton's laws with these postulates. (Born 1949a 17)

Born goes on to give a brief overview of the history of contiguity and antecedence in physics. Contiguity, he writes, is linked to introduction of contact forces and forces within bodies, then with forces within the electromagnetic ether and fields of force. The application of the contiguity principle lead to Newton's theory being superseded by Einstein's. Respect of the antecedence principle is bound up with time-irreversibility in the equations of the theories of physics. The first example of this, for Born, comes in thermodynamics. He tells us that the 'reconciliation' (Born 1949a 17) of it with Newtonian mechanics led to statistical physics. Statistical physics is also bound up in the development of atomic theory. Atoms are first hypothetical and the search for them and the investigation of their properties first confirms their existence and then discovers that there are no Newtonian particles at all. In the final accounting, quantum theory preserves antecedence and contiguity to what Born considers to be 'a considerable degree' (Born 1949a 18).

So we can see in this already a couple of indications as to what status these principles are meant to have. Firstly there is Born's assertion that they are not metaphysical (by 'metaphysical', Born simply means 'something beyond physics' (Born 1949a 7). He doesn't say any more in the introduction, but does elaborate a little in an appendix. Here he notes that although he does not like what he terms the 'speculative philosophy' associated with the term, which 'pretends that there is definite goal to be reached and often claims to have reached it' (Born 1949a 209) there do exist metaphysical problems which he thinks

'cannot be disposed of by declaring them meaningless, or by calling the other names, like "epistemology"' (1949a 209). These problems are those that are, as he says previously are 'beyond physics' and furthermore 'demand an act of faith' (1949a 209).

The implication here seems fairly clear: that Born's causal principles do, in some sense, belong to the realm of physics; that we not require an act of faith to accept them, I.e. they have some kind of empirical status. However it is not clear that this can simply explain the guiding role that they have had on physics. We couldn't have discovered them prior to the successes of relativity and quantum theory, yet they still have some influence over our minds, Born says (1949a 17).

Born discusses starts his discussion of contiguity and antecedence by looking at their status in Newton's theory of mechanics. According to Born (and I think correctly), Newton's theory also fails to satisfy the principle of contiguity. He writes 'Newton's forces, the quantitative expressions for causes of motion, are supposed to act through empty space, so that cause and effect are simultaneous whatever the distance' (Born 1949a 16). Not only is there no demand for contiguous causal contact in Newton's theory of gravitation, there seems explicitly to be no causal contact whatsoever. But it is important that this does make Newton's dynamics an unacceptable or unintelligible theory. Born notes that 'In spite of these difficulties, Newton's dynamics has served many generations of physicists and is useful, even indispensable, to-day' (Born 1949a 16). It's acknowledged that the pre-Newtonian theory of Cartesian cosmology does provide a contiguous description of celestial mechanics, but this on its own does not make it a preferable theory (1949a 16). It's also suggested that Newtonians had good reason not to worry about the lack of contiguity in the theory: 'the language of facts led unambiguously to his [Newton's] results' (1949a 16). It was only when 'new facts' about the finite velocity of the propagation of forces were discovered that physicists were able to deal with the notion of contiguity in gravity in the general theory of relativity.

Born goes on to discuss why the violation of the contiguity principle by Newtonian mechanics was accepted and then later rejected. Born notes that 'it suffices to remember that the power of analytical mechanics to describe and predict accurately the observations led many to the conviction that it was the final formulation of the ultimate laws of nature' (Born 1949a 18). The implication here is clear: the success of the Newtonian programme was sufficient to alleviate conceptual worries regarding it. But, Born thinks, the situation did change, and this was due to the development of terrestrial, rather than just celestial, mechanics.

3.1.1 Contact Forces

Born argues that contiguity had its first proper introduction into classical physics via the investigation of elastic solids and the continuum mechanics of Augustin-Louis Cauchy. He notes that Cauchy initially tried to model solids as aggregates of particles, acting on one another in a non-contiguous manner. However, he writes that 'in the physical application all traces of them were obliterated by averaging' (1949a 19). He doesn't elaborate on this point, but presumably he means something like that individual particles actually do no work in the model. Born thinks that this suggested to Cauchy another approach—modelling matter as a mathematical continuum. He notes that although this might 'from our modern standpoint' seem like a step backwards (moving away from a discontinuous, atomistic model of matter to a continuous one), this approach is not only more contiguous but also forms the basis of field models of the electromagnetic forces.

In Newton's formulation not only is there no concept of a field though which a force can propagate, forces which act on bodies do not propagate through them contiguously. A force acts on one end of a body, and then instantaneously through it at the other end. In Cauchy's system it is assumed that all the properties of the matter composing the body or bodies in question are distributed continuously in the space that they occupy. The density, ρ, of the body is therefore a function of space. The quantity of mass which passes through unit

surface area in unit of time is the current of mass. Assuming the conservation of mass leads (via a more complex derivation given by Born in an appendix (Born 1949a 134) to the famous continuity equation:

$$(1) \quad \dot{\rho} + \operatorname{div} \boldsymbol{u} = 0 \qquad \text{(Born 1949a 20)}$$

In order to calculate the forces acting on such a continuum, one considers the forces acting upon a volume element We regard the substance as being separated into many elements each separated by a surface through which each element exerts a force upon that adjacent to it. This force is represented by the stress tensor **T**,

$$(1.1) \qquad T = \begin{vmatrix} T_{xx} & T_{xy} & T_{xx} \\ T_{yx} & T_{yy} & T_{yz} \\ T_{zx} & T_{zy} & T_{zz} \end{vmatrix} \qquad \text{(Born 1949a p20)}$$

we then get:

$$(1.2) \qquad \dot{\rho}\frac{d\boldsymbol{v}}{dt} = \operatorname{div} T \qquad \text{(Born 1949a 20)}$$

where div **T** is a vector with the components:

$$\left(\operatorname{div} T\right)_x = \frac{\partial T_{xx}}{\partial x} + \frac{\partial T_{xy}}{\partial x} + \frac{\partial T_{xz}}{\partial x}, \dots, \quad \text{(Born 1949a p21)}$$

and d/dt the operator

$$\frac{d}{dt} = \frac{\partial}{\partial t} + v_x \frac{\partial}{\partial x} + v_y \frac{\partial}{\partial y} + v_z \frac{\partial}{\partial z}, \quad \text{(Born 1949a p21)}$$

Born writes that these equations, (1) and (1.2) 'are the new equations of motion which satisfy the postulate of contiguity' (Born 1949a p21). They satisfy contiguity because they provide a physical model for how forces propagate through a continuous medium, something which is simply absent from previous formulations of mechanics.

3.1.2 Electromagnetic Fields

Born then discusses the notion of contiguity in electromagnetism, although not in quite so much formal detail as he does with contact forces. Early formulations of electromagnetic laws such as Coulomb's violate contiguity in the same way as Newton's law of gravitation. Born's narrative—explicitly—is one of scientists working against a Newtonian background to gradually and empirically develop a contiguous physics. He writes that Faraday tried to understand 'electric and magnetic phenomena with the help of contact forces' (Born 1949a 23). He used concepts from the theories of elastic bodies, derived from Cauchy's continuum mechanics. He writes though that there were' considerable and somewhat strange modifications' (which he does not specify) that 'made it difficult for his learned contemporaries to accept his ideas and to discard the well-established Newtonian fashion of description' (Born, 1949a, 23). Faraday's preference for explanations in terms of contact forces was, Born claims, due to physical intuition rather than mathematics.

The next step was James Clerk Maxwell's work which, according to Born 'made it impossible to accept forces acting instantaneously over finite distances' (1949a 24). Maxwell did this by using a field model to show that EM forces propagate with finite velocity. Born concludes with the following 'I only wish to stress that the use of contact forces and field equations, I.e. the establishment of contiguity, in electromagnetism was the result of a long struggle against preconceptions of a Newtonian origin. This confirms my view that the question of contiguity is not a metaphysical one, but an empirical one' (Born 1949a 25). Why would Born think that this confirms contiguity as an empirical principle, rather than as a metaphysical one? Because it grew out of good physical theories that were more accurate than their predecessors, not out of non-physical background metaphysical assumptions. Now it is of course by no means apparent that Born is correct in this surmise—we could equally posit that the move towards contiguous explanation was due to the influence of some metaphysical research program that encouraged the search for such explanations.

3.1.3 Relativity

Born finishes the section on contiguity with a discussion of its status in the theory of relativity. Born notes that at the start of his career Newton's mechanics with its instantaneous non-contiguous action-at-a-distance existed 'more or less peacefully, side by side' (Born 1949a 26) with the contiguous theories of Cauchy's mechanics and Maxwell's EM theory. The problem was how to reconcile Newtonian gravitation with contiguity. The solution was the general theory of relativity. He gives a brief overview of the nature and development of relativity theory before turning to the 'philosophical problem' (Born 1949a 29) at hand, making two relevant points. The first is that relativity makes physical geometry ('the geometric aspect of the behaviour of actual bodies' (1949a p29) subject to the cause-effect relation. The second is that it obeys contiguity.

Not only are Cauchy's continuum mechanics and Maxwell's EM theory more accurate versions of earlier theories, in that they provide better models and mathematically more precise results, but they also respect the principle of contiguity. For Born, these two things are linked. He writes that, referring to the introduction of contact forces throughout materials 'much effort has been made to reconcile Newton's laws with these postulates [of contiguity and antecedence]' (1949a 17). The question here is precisely why Born thinks that this means that his principles are 'empirical'. Born doesn't really elaborate here, but I think that we might speculate in the following way: the introduction of contiguity into theories is desirable for two reasons. Firstly, those theories without contiguous descriptions of forces conceptually lack something—causal descriptions of individual events. Secondly it is the case that the contiguous theories that, in reconciling the principle with Newtonian physics, we are pushed to search for are better or more accurate theories than those within that paradigm that did not respect contiguity. This I think is the lesson that Born means us to learn from Cauchy's development of continuum mechanics and it is, I think, what Born means when he calls the contiguity principle 'empirical'. It must be noted however, that this would imply that the contiguity principle is something like a universal law drawn from

an induction over the history of physics—for every physical relation that expresses some functional relationship between two quantities there exists a contiguous explanation or description of that relation. It is not at all clear that such a principle falls purely within the realm of the empirical, despite Born's claims that it does. On something Zahar's view (Zahar 1973, see also later discussion in section 4 of this chapter), a heuristic principle that guided us to search for a contiguous theory would be grounded in some metaphysical principle that if x influences y then something must propagate between them.

3.2   Antecedence

Born next examines the place of his principle of antecedence, that cause should come prior to, or be at least simultaneous with, effect in physics.

3.2.1 Antecedence in Newtonian Mechanics

In Newtonian mechanics the relationship between the motion of a body at one time and its motion at a later time is symmetrical. Newton's theory in fact goes even further than this— the whole state of a system at one time is symmetrically related to the whole state of the system at another. Newton writes in Scholium One of the Principia 'Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without relation to anything external' (Newton 1689). Born notes that despite this apparent description of time as a uniform flow, nothing of the kind can be drawn from the actual mechanics of Newton's physics. Time is merely an independent variable—to substitute t for -t makes no difference to the equations. Thus we cannot claim that it follows from the physics that the

earlier state of the system must specify the later state of the system. One can claim with as much physical justification that the later state specifies the earlier.

This all implies something about what sorts of conditions must be fulfilled in order for Born to consider his postulates respected. Nowhere does Newton actually suggest the possibility of backwards causation or of any physical significance to the time-symmetry of his mechanics, but it is not enough that for Born that his postulates are not actively broken. They must be actively fulfilled in such a way that the theory makes it impossible for any kind of causal reversal to take place. It also seems to be the case that absolute time itself is not enough to satisfy the postulate. This is because Newton does not actually use the absolute time do any empirical work—removing it from Newtonian physics would make no difference to the formalism of the theory. For Born, it is not enough for a theory to simply independently stipulate some principle that bans backwards causation or enforces time-asymmetry. This is key for understanding what it means for Born to think that some theory does respect the principle of antecedence —there must be some aspect of the formalism of the theory that prevents the symmetric transformation of t into -t, i.e. it is the equations or the definition of the variable that must do the work of respecting antecedence.

It's worth noting that Newtonian mechanics does seem to contain metaphysical principles that allow some intelligible notion of causation—the description of time in Scholium 1

> Absolute, true, and mathematical time, of itself, and from its own nature, flows equably without relation to anything external, and by another name is called duration: relative, apparent, and common time, is some sensible and external (whether accurate or unequable) measure of duration by the means of motion, which is commonly used instead of true time; such as an hour, a day, a month, a year. (Newton 1726)

The flow of time here seems to ensure causal direction, satisfying Born's antecedence principle. The notion of flow implies direction, and if time can only flow in one direction we have good reason to reject the –t solutions that make it impossible to determine whether event A depends on event B or event B on A. Indeed this bears something of a resemblance to Chang's definition for an ontological principle. However it still does not satisfy Born. Why? Well, Born says of Newton 'Whatever he says about the notion of time (in *Principia*, Scholium I) as a uniform flow, the use he makes of it contains nothing of a flow in one direction' (Born 1949a P16). So it takes more than statements of principle to satisfy Born. We cannot simply assume principles of causation, problematic though their lack might be. They have to come from the physics; theories have to be constructed in such a way that relations in them cannot violate them. They cannot simply be added in as additional constraints.

One other way to understand what Born requires here is via Bas van Fraassen's arguments about absolute space in Newtonian mechanics (van Fraassen 1980 57). Van Fraassen points out that Newton's theory is empirically adequate (I.e. it saves all of the phenomena) for any value of constant motion of say, the sun, through absolute space so long as the values of all relative motions (i.e. that of bodies with respect to one another—what we might call Galilean relativity) are preserved. Hence all versions of Newtonian mechanics with differing values of the (collective and isomorphic) motion of bodies with respect to absolute space are empirically equivalent (van Fraassen 1980 46). Furthermore, and because of this, we can also consistently believe that all of those versions are false, even though they are all empirically adequate, if we believe that their falsity consists in the common component that goes *beyond* the phenomena—in this case, absolute space (Van Fraassen 1980 47).

Now Born is not talking about absolute space and he is not really discussing empirical adequacy or equivalence, but the relevant point is this—we can easily see that some theories have excess content that is irrelevant to the job of saving the phenomena. Absolute

time, for Born, seems to fall into this category—whether or not we state that time flows absolute like a river, the dynamics given by the mathematical models of Newtonian mechanics will be time-symmetric. Similarly to absolute space, Newtonian mechanics plus absolute time is empirically equivalent to Newtonian mechanics without absolute time. In order for a theory to respect antecedence, at least some of the *working* parts of a theory— its dynamics—must be time-asymmetric.

3.2.2 Antecedence in Thermodynamics

Born argues that antecedence is fulfilled by the development of thermodynamics and statistical physics. He writes: 'We now have to discuss the experiences which make it possible to distinguish in an objective empirical way between past and future or, in our terminology, to establish the principle of antecedence in the chain of cause and effect. These experiences are connected with the production and transfer of heat' (Born 1949a 31).

Born initially discusses the differential equations for the flow of caloric through bodies. He notes that this was treated with mostly the same methods used for describing the flow of liquids. However caloric fluids are considered to have negligible inertia (Born 1949a 31). From the continuity equation

we can derive the expression:

$$c \frac{[\partial T]}{[\partial t]} = \kappa \Delta T,$$

Where c is the specific heat, κ is the coefficient of conductivity, T is the temperature and t the time. (Born 1949a p31).

This is distinct from liquid flow by being a first-order, rather than second-order, differential equation in time. From the perspective of antecedence, the importance of this

equation is that one cannot substitute t for –t. Born illustrates this by considering the solution for the temperature distribution in a thin wire along the x-direction:

$$T - T_0 = \frac{C}{\sqrt{t}} e^{-(cx^2/4\kappa t)}$$

The equation tells us that an initially high temperature at $x=0$ will gradually spread and level out over time. It is clear that one is unable to substitute t for $-t$ without the equation becoming unphysical (I.e. there is a solution but it describes an exponentially increasing imaginary temperature along the x direction). This, Born says, is 'an obviously irreversible phenomenon' (Born 1949a 32).

Born then discusses the development of thermodynamics proper. The crucial development for the antecedence principle is the second law of thermodynamics. Born notes that the second law is derived from models of thermal machines which transform work into heat and heat into work or move heat from one place to another. (Born 1949a 38). It was known from engineering, and particularly the operations of steam engines, that certain tasks are impossible to perform, namely the complete transformation of heat to work and, in the absence of additional work, the movement of heat from a colder area to hotter one. It is from these concepts that the second law was originally derived.

Born gives a derivation with a different method to that used by Kelvin and Clausius—that of Caratheodory—which he regards as 'much more satisfactory' (Born 1949a 38) because whereas Kelvin's and Clausius' derivations follow from a wide range of impossible processes, Caratheodory's method follows from the idea that one can derive the second law from a single such process. Born notes that we know from James Joule's work that there are such processes. If we take an adiabatically enclosed system and input work to transform it from one equilibrium state to another we do not get back our work simply by reversing the process. This is the case no matter how close the two states are. Thus Born concludes that 'there exist adiabatically inaccessible states in the vicinity of a given state' (1949a 39). So, for any state of an adiabatically enclosed system there exists at least one

state than is inaccessible. It is from this principle that Caratheodory derives the second law of thermodynamics. Born goes through the full derivation, which I will not reproduce here, but we end up with the following:

$$dQ = TdS$$

Where Q is the heat, T the absolute temperature is defined as

$$T(\vartheta) = Ce \int g(\theta)d(\vartheta)$$

and S the entropy, defined as

$$S(\phi) = \frac{1}{C} \int \Phi(\phi)d\phi$$

Where C is fixed by defining T_1-T_2 for two states of some substance (100° for water if T_1 is the boiling point and T_2 the freezing point) (Born 1949a 42).

For dynamical processes that are not simply sequences of equilibrium states we must consider transitions from an initial state V_1^0, V_2^0, S^0 to a final one V_1, V_2, S, where V is the volume and S the entropy. We can affect such a transition in two ways: firstly by adiabatically and quasi-statically varying the volume with the entropy remaining constant or secondly by changing the state adiabatically but with constant volume, which changes the entropy.

In order to prevent a violation of Caratheodory's principle, we must ensure that not every value for the entropy can be reached from every other (I.e. that we cannot get to every S from every S_0). We do this in the following way: For each process we say that either $S \geq S_0$ or $S \leq S_0$. The sign is defined by the choice of constant C and must remain the same for all initial states due to continuity. If C is such that T is positive, then the entropy can never decrease.

The development of thermodynamics proper leads, via the concept of entropy, to a truly irreversible system of physics, thus satisfying the principle of antecedence. But, as Born notes, 'this gain is paid for by the loss of description which ordinary dynamics of continuous media supplies' (Born 1949a 44). Why? Most of thermodynamics is concerned with equilibrium processes. Born explains that we can give some dynamical descriptions of irreversible processes, but these descriptions merely give us the increase in entropy or the decrease in the free energy of the system. So we have a system of mechanics which obeys the principle of antecedence, but cannot give detailed descriptions of the systems it describes.

Born then gives a detailed description of the development of statistical mechanics and the introduction of irreversible processes via probabilistic treatments of systems, which again I will pass over. He returns to the topic of irreversibility when discussing Boltzmann's equation:

$$\frac{df(1)}{dt} = \frac{\delta f(1)}{\delta t} - [H, f(1)] = C(1)$$

where $f(1)$ is the probability density for a particular particle, H the Hamiltonian, and $C(1)$ the collision integral given by:

$$C(1) = \iint (f'(1)f'(2) - f(1)f(2)) |\xi_1 - \xi_2| d\boldsymbol{b} \, d\xi_2$$

The collision integral represents the dropping of the assumption that the motions of the molecules in a gas are independent from one another and models the change a particle crossing some volume of space whilst that motion is interrupted by collisions with other molecules. In Born's formulation f(1) and f(2) refer to two particles before collision; f'(1) and f'(2) after collision; ξ_1 and ξ_2 their velocities; and d$\boldsymbol{b}$ the collision cross-section.

Born writes 'Does the equation (6.23) [the collision integral] really indicate an irreversible approach from any initial state to a homogeneous equilibrium? This is in fact the case, and a very strange result indeed: the metamorphosis of reversible mechanics into irreversible

mechanics with the help of probability' (Born 1949a 57). I.e., is it the case that we have derived some time irreversible process via the introduction of probabilistic considerations in into the kinetic theory of gases?

Born discusses how such a result arises: the collision integral results from averaging over the behaviour of all of the other molecules in the system. Born thinks that we are forced to give this averaging because of our ignorance of what is precisely going on in a microscopic system. He writes that 'mixing mechanical knowledge with ignorance of detail leads to irreversibility' (Born 1949a 59). Is this justified, though? Born thinks that it is. He comments that one of the results of Einstein's work on Brownian motion is there is a limit in the accuracy that measuring equipment can reach, due to random motion in the molecules of the equipment itself. He writes 'There is a limit on observability given by the laws of nature themselves' (Born 1949a 64). Of course, as Born points out, one can always reduce the temperature of the system in order to increase the accuracy of one's equipment —as you do so, the magnitude of the Brownian motion will decrease. Of course, as he says 'later developments in physics proved this rule in physics also to be ineffective' (1949a 64). Born does not tell us precisely what he means here, but presumably he refers to the zero-point energy that is present even at absolute zero.

Born concludes his discussion of classical physics with a chapter entitled *Chance and Antecedence*. He starts by asking what it is that we can learn from the preceding discussion. It is that 'the introduction of chance and probability into the laws of motion removes the reversibility inherent in them; or, in other words, it leads to a conception of time which has a definite direction and satisfies the principle of antecedence in the cause-effect relation' (Born 1949a 71). Born is not alone in arguing that there exists some thermodynamic arrow of time—Reichenbach (1953), Grünbaum (1973) and Albert (2000) are some examples of others who do. Unfortunately, Born does not elaborate on his argument and so we are forced to do a little reconstruction of our own in order to look at what his position might actually be.

Something to note here is that if we take Born's statement about the direction of time at face value, the irreversibility of thermodynamic processes leads us to an irreversibility not just in the particular cause-effect relations described by those theories, but also in time itself. This might imply that we would only need one fundamental time-asymmetric physical process in order to ensure that all processes (if we presume that physics is consistent) are actually time-asymmetric regardless of formalism, as we have managed to derive some kind of thermodynamic arrow of time. It's not clear precisely what metaphysical view of time Born has. The above statement is all we have to go on with regards to that. However, two claims of his might help us here. The first is his assertion that his principles of causation are empirical. The second is his assertion that Newton's stipulation that regardless of dynamics time has some direction, does not in fact give temporal ordering of causal relata in physics. Born's view might therefore be that time is not separate from the processes that take place in it—i.e. all that we have to go on when considering the direction of time is how the time variable behaves in the theories of physics

However it is not clear from this that Born is actually proposing a true reduction of the direction of time to the direction of the increase of entropy (of the sort discussed in Sklar 1985 305-326), especially as he does not seem to discuss time elsewhere in his work. The alternative to this view is that Born is merely talking about the 'conception' of the time variable in thermodynamics and kinetic theory only, and not more generally. So in that sense, we can reconstruct what he says as being something like 'the introduction of probability into the laws of motion in kinetic theory leads to a time irreversibility in them, and hence a view of time within thermodynamics that is irreversible and hence satisfies the principle of contiguity'.

As I have said, Born does not repeat this reasoning or indeed discuss the direction of time elsewhere, instead discussing the ordering of causal relata. I think therefore, that we are unable to come to any firm conclusion regarding precisely what he means here.

Born himself moves swiftly on to giving a formal derivation of that time asymmetry. He starts by defining the entropy, S, as follows:

$$S = -k \frac{\int f \log(f) \, dp \, dq}{\int f \, dp \, dq}$$

Where f is a distribution function, f_1 in the kinetic theory of gases referring to a single molecule and f_N in statistical mechanics, where it refers to a distribution in 2-N phase space.

For a gas we can use Boltzmann's collision integral to show that:

$$\frac{dS}{dt} \geq 0$$

i.e., S, the entropy of the system, always increases. This irreversibility is not, Born argues, in contradiction to to the reversibility of mechanics. This is because the distribution function for colliding molecules satisfies:

$$\frac{\delta f}{\delta t} = [H, f] + C$$

The reversible model for non-interacting particles does not contain the probabilistic collision integral. Thus this irreversibility is 'a consequence of the explicit introduction of ignorance into the fundamental laws' (Born 1949a 72).

Born notes that these 'considerations' hold for any system (1949a 72). He tells that if we solve the entropy equation for a system of closed particles, we obtain $dS/dt = 0$, i.e. for such a closed system, entropy is constant. This irreversibility, he says, 'can only be understood by exempting part of the system from causality.' What does this abandonment

of causality involve? It involves dropping the requirements that the system in question be closed 'or that the positions and velocities of all particles are under control'—i.e. fully determined—by introducing a single particle not under control to the system. We then obtain the result that entropy is either 'constant or increasing' (1949a 72). It might seem a little odd that Born describes this specifically as an abandonment of causality, rather than of determinism or the epistemic possibility of complete information. For Born, determinism and causality are not bound up—they are separate concepts—so it cannot simply be a matter of the system merely not being modelled deterministically.

But think back to Born's definition of causality: 'there are laws by which the occurrence of an entity *B* of a certain class depends on the occurrence of an entity A of another class' (1949a 9). In what sense are we abandoning a nomological description of the system? There is a sense in which allowing an open system leads not only to that system being indeterministic, but also to it being anomalous. This is because one cannot give fully law-like descriptions of open systems because of the possibility of intrusions from outside of the system that cannot be accounted for by any internal description. Why do we say that not all of the particles are 'under control' rather than merely saying that we cannot predict their positions? Because the individual particles are still supposed to behave deterministically and so in order to say that we cannot know their state, then we need to exempt them from the nomological description of the system. As Born goes on to say later in the book, we achieve irreversibility in kinetic theory via 'a deliberate act of averaging, a kind of fraud or falsification from the standpoint of determinism' (1949a 110). The general point to be made here is this: because classical mechanics is at its core supposed to be deterministic, the achievement of time asymmetry via the introduction of probabilistic methods must be the result of a kind of fudge.

Born says this: 'You must violate mechanics in order to obtain a result in obvious contradiction to it' (Born 1949a p72). We violate by dropping the demand that the state of each and every particle can always be determined. Born acknowledges that we might

regard this simply as being necessary for the pragmatic reasons that we cannot actually gain complete information about a system and that, even if we could, we would be unable to perform the calculations necessary to determine the behaviour of the system. The world would really be reversible and 'thermodynamics only a trick for obtaining probable, not certain, results' (Born 1949a 72-3). Born disagrees with this position. He notes that although one might need to agree with it if one did believe that it was 'in principle' (1949a 73) possible to determine the position and velocity of all particles in a system, but he does not think that such a belief could actually be maintained. Brownian motion sets a fundamental limit to the accuracy of our measuring equipment and so we would require 'a spirit who can do things we could not even do with infinitely improved technical means' (Born 1949a 72). Further to this, he thinks that 'the idea of a completely closed system is almost fantastic'.

So we can see that the actual epistemic situation in which we find ourselves is important to Born. The crucial point is this (and it is examined in much more detail in Chapter 5—Born on Determinism): the actual epistemic situation that we find ourselves in is constrained by the laws of nature. We have just seen that Born does not think that it is in fact in principle possible to determine all of the positions and velocities of all of the particles in a system due to Brownian motion and I think that we can take him at face value here. Statistical mechanics, in treating the world statistically, was on the right track. Of course, if we insist on maintaining determinism, then there must be some sort of trick or fudge going on here. However and as we shall examine shortly, there is no need for such fudges in quantum mechanics.

3.2.3 Antecedence in Quantum Mechanics

Born finishes his overview of antecedence in physics with a discussion of quantum mechanics and, in particular, the statistical interpretation of it. Following a detailed derivation, Born discusses the nature of an indeterministic physics. It should be noted that

for Born, the theory really is indeterministic—he disagrees strongly with Einstein and Planck, who argued otherwise (Born 1955 164). He is also happy with this notion, and does not think that we ought to concerned by it. He writes 'can our desire of understanding, our wish to explain things, be satisfied by a theory which is frankly and shamelessly statistical and indeterministic? Can we be content with accepting chance, not cause, as the supreme law of the physical world?' (Born 1949a 101).

His answers to these questions are affirmative, and as follows: With regards to the second, he argues that it is not causation that has been thrown out by quantum mechanics, but merely determinism. Relations of physical dependence still exist (which is how Born defines causality), but those relations are not deterministic and hold between 'probabilities of elementary events, not those single events themselves' (1949a 102). Determinism is something that has, for historical reasons, been bound up with causation. It is not causation itself (Born 1949a 102). On the notion of relations holding between probabilities of events, there is more in Chapter 6 concerning Born's views on probability. On the notion that someday physics will return to determinism, Born says that although it would be foolish to argue that this could never happen, it does not seem likely. He writes 'scanning the history of physics in the way we have done we see fluctuations and vacillations, but hardly a reversion to a more primitive concept' (Born 1949a p109). 'Primitive' here might simply mean something like 'prior' or 'old' but it would certainly fit with the overall argument of *Natural Philosophy of Cause and Chance* if it means something like 'less sophisticated' or 'less comprehensive'. The narrative that Born presents is one of physics gradually abandoning action-at-a-distance and determinism as it starts to respect the principles of contiguity and antecedence. In this way, indeterministic and statistical physics, which has not only led us to more accurate theories but also given us theories that respect antecedence, would be a conceptual advance on the more 'primitive' concept of determinism. Born also cites Von Neumann's proof that a hidden variable (i.e. deterministic) interpretation could not reproduce the predictions of an indeterministic interpretation as further evidence that a return to determinism is unlikely (1949a 109). We

should note here that it is of course the case that John Bell later showed Von Neumann's proof was fallacious (Bell 1964), although Born would not have been aware whilst writing *Natural Philosophy…* in 1949.

Born acknowledges that the behaviour of the wave function violates the antecedence principle— 'there is no distinction between past and future for the spreading of the probability density'— although it does fulfil contiguity (1948 103). He suspects, though that this is not the final state of the theory: 'one has the feeling that these vestiges of classical causality are provisional and will be replaced in a future theory by something more satisfactory' (1948 103). He writes that 'we have the paradoxical situation that observable events obey laws of chance, but that the probability for these events spreads according to laws which are in all essential features of causal laws' (1949a 103).

We do, however, find that other parts of quantum theory respect antecedence, namely quantum statistical mechanics, and that it does so in a more principled way than classical statistical mechanics. Born writes of the classical theory's reconciliation of the indeterminism of the behaviour of bulk systems with the supposedly deterministic behaviour of the individual components of those systems:

> This was achieved by proclaiming a distinction between the true laws which are strictly deterministic and reversible but of no use to us poor mortals with our restricted means of observation and experimentation, and the apparent laws which are the result of our ignorance and obtained by a deliberate act of averaging, a kind of fraud or falsification from the standpoint of determinism (Born 1949a 110).

Why would this be a fraud? Born, as we have seen earlier does not really think that it is— it's simply the epistemic situation we find ourselves in and so we do not have good reason to insist on determinism. If, however, one is a determinist, then the whole situation starts to

look a little strange: We have an inconsistency between the description of the behaviour of particles on a microscopic level as deterministic and time-symmetric, and the description of macroscopic collections of those particles as indeterministic and time-irreversible. For Born, if we are determinists then the irreversibility of the system, and indeed the law that tells us that entropy always increases, seems to have purely epistemic origins—they come directly from the fact that we don't know everything about the system.

However, we need have no such worries about quantum mechanics. It is already statistical and uncertain. Born writes 'Quantum theory can appear with a cleaner conscience. It has no deterministic bias and is statistical throughout. It has accepted partial ignorance on a lower level and need not doctor the final laws' (1949a 110). There is no total information regarding the system, and so there is no conflict between indeterminism on a macroscopic level and determinism on the microscopic level. Quantum kinetic theory is thus time-irreversible on a fundamental level and thus properly respects the principle of antecedence.

4   What is Born after?

In order to account for the status of Born's principles we need to account for the following aspects of them. Firstly, they have an origin that is pre-empirical, or at least predates the systematic investigation of nature. Remember that Born considers his definition of causation itself to be metaphysical (1949a 124). It's attributes—contiguity and antecedence—also seem to have some non-empirical origin. Before defining them, he tells us that it is 'always assumed that cause precedes effect' and that for something to cause an effect in an area of space where it is not located is considered 'repugnant' (i.e. nonsensical) (Born 1949a 8).

Secondly those attributes, at least in the present day, are not simply metaphysical in that they are 'consequences of the actual empirical laws' (Born 1949a 124). What Born means by this is presumably the following: modern physics, which gives us our most accurate descriptions of nature gives a fully contiguous description of causal relations. In the same manner, modern physics is fundamentally statistical, and statistical descriptions are time-asymmetric (remember that although Born acknowledges that parts of quantum mechanics are still deterministic, he doesn't think that this situation will continue). Thus we can say that nature as described by modern physics is contiguous and does ensure time-ordering of causal relations. Hence we can say that the principles of contiguity and antecedence follow from the empirical laws. Now, this seems to present an obvious problem—what about the aspects of modern physics that appear to not only fail to respect these principles but explicitly violate them? These aspects are the so called spooky-action-at-a-distance in quantum mechanics and the manner that general relativity apparently allows for time travel. I will address this problem in a later section, but will hold off from answering it for now beyond saying that there are reasons to think that this isn't too much of a problem for Born's position.

Thirdly, it is heavily implied that, in the form of Born's principles, they have played a guiding role in theory construction and selection in physics by encouraging scientists to seek out explanations that respect them—he writes that 'although I maintain that neither causality itself nor its attributes, which I call contiguity and antecedence, are metaphysical...there is no doubt that these ideas have a strong power over the human mind, and we have evidence enough that they have influenced the development of classical physics. Much effort has been made to reconcile Newton's laws with these postulates' (Born 1949a 17).

4.1 Principles in Physics: A Zoo

What I want to do in this section is to explore exactly what sort of things Born's principles are. Suppose that Born is in fact correct about his principles (I note that I am not in fact arguing that he he is). What would their epistemic and metaphysical status be and what their relationship to physics? In this section I aim to describe a zoo of potential options for principles in physics—what sort of metaphysical and empirical status they could have and what sort of guiding role (if any) they could play in the development of physics. I will also set out what would distinguish them from one another—what sort of characteristics would mark out a set of principles as belonging to some particular metaphysical and heuristic class.

The options that I want to discuss are as follows - Kantian analogies of experience or laws derived directly from them, purely psychological a priori principles, influential but inconclusively confirmable metaphysical systems, heuristics with deep metaphysical backing, empirically discoverable physical laws, and pragmatist ontological principles.

I also want to note that there are at least two sorts of role that Born's principle might play. 1) They can serve as principles that define what it is for a relation to be a causal relation. 2) They serve a role in the development of the sciences. It might well be the case that these two are explained by different options. It should not be assumed at the outset that Born's treatment of his principles is completely coherent—these two roles might well come apart

4.1.1 Kantian Synthetic A Priori Principles

One possibility for the status of Born's principles is that they are meant to play the same role as Kant's analogies of experience or are rules derived from them—in either case they are a priori and necessary. We can examine how this might work by looking first at the *Critique of Pure Reason* and in particular the Analogies of Experience. Kant writes: 'The general principle of the analogies is this: All appearances are, as regards their existence, subject *a priori* to rules determining their relation to one another in time' (Kant 1982 208).

The second analogy is described as the 'Principle of Succession in Time, in accordance with the Law of Causality' (Kant 1982 218) and it is this that is of particular relevance to the discussion of Born. Kant writes that we when we perceive two appearances as following one another, we are connecting two perceptions in time via a synthetic faculty of imagination. Imagination could place two such states in one of two orders, with either state coming first or second. There is, therefore, no empirical way to determine their *objective* relation in time—we are only aware that such imaginative faculties place one perception prior to the other and not that one state *really* precedes the other. In order to have some determine ordering in time, there must be some necessary connection between two such states such that one must precede the other. Thus, Kant argues, we must appeal to a concept drawn from understanding, not from perception: the relation of cause and effect (Kant 1982 219*).*

Kant goes on to argue that it is only possible to experience an event via the assumption of necessary ordering of its parts—otherwise we would merely experience a succession of appearances. As this assumption is part of the grounds of experience itself it is therefore *a priori.* We do not need Newtonian absolute time to order events temporally in a Kantian framework. Indeed, such an explanation is rejected. Kant writes 'For time is not viewed as that wherein experience immediately determines position for every existence. Such determination is not possible inasmuch as absolute time is not an object of perception with which appearances could be confronted' (Kant 1982 236).

If Born's rules of Contiguity and Antecedence are synthetic a priori, how would we expect Born's overview of these principles in the history of science to go? Well we would, as above, expect a rejection of Newtonian Absolute Space and Time as an explanation for the ordering of events (spatially or temporally). These concepts cannot explain such ordering and they are not needed to do so—the analogues of experience do so. Furthermore, we

wouldn't need any physical concepts of time and space to explain such ordering and would reject such explanations as belonging to the noumenal. Additionally, such principles would not be compatible with any physical theory that postulated non-contiguous action-at-a-distance or anti-antecedent causal ordering in time—these theories would be simply unintelligible. If Born's principles are meant to have Kantian status in physics then we could not, for example, have a good theory that did not respect them.

There are two options here for Born's principles. They might themselves be synthetic a priori principles, without which we would be unable to reason physically, or they might be deducible from the synthetic a priori in the manner that Kant thinks Newton's laws of motion are. I think that it is quite clear that Born's principles are not synthetic a priori. This is apparent from his treatment of Newtonian mechanics. Born's criticisms of Newton are that gravitational forces act non-contiguously and that the description of time in the scholium doesn't make the formalism of the theory time-asymmetric. As we've seen, this doesn't indicate that he thinks Newtonian physics to be poor science or nonsensical. If contiguity and antecedence were Kantian principles, then this could not be the case—any theory that did not respect them would simply be nonsensical. Born does reject the Newtonian solution to temporal ordering, but he does not do so because a priori considerations do the job instead. His reason seems to be that, as far as the description of connections between events go, Newton's solution is something like an idle wheel. It should also be pointed out that they are not entirely a priori principles in any case. Although in his introduction Born seems to indicate something like a common sense basis for the principles, which might be taken as an a priori justification of sorts, he concludes that 'all specifications of this dependence [causal] in regard to space and time (contiguity, antecedence) … seem to me not fundamental, but consequences of the actual empirical laws' (1949a 124). This is again unlike Kant—Born argues that the principles follow from the actual, empirical laws of physics, not from a priori rules. It might of course be the case that we have managed to derive a priori rules from an examination of nature—i.e. that they stand alone once we have found them, although the route we have followed to find them is

empirical—but there does not seem to be anything in Born's writings to indicate that he thinks that this is the case.

We might also remind ourselves that Born does not consider himself to be a Kantian. In his autobiography (of which this part was written in the 1940s) he writes 'Kant's teaching has a great attraction for one who is inclined towards rationalism...But it did not work for me. However, it was a slow process which made me acknowledge this failure and led me to a standpoint which might be classified as a kind of empiricism' (Born 1978 93).

So we have three good reasons to say that the Born's principles of contiguity and antecedence are not synthetic a priori. Firstly, they don't regulate theories in the same way: Born thinks that theories can be intelligible and useful without respecting his principles. Secondly, Born's principles are at least partially empirical in nature. Thirdly, Born explicitly rejects Kantian philosophy in favour of an empiricist position.

4.1.2 Chang's Ontological Principles

In two papers, *Taking Realism Beyond Foot-Stamping* (Chang 2001) and *Ontological Principles and the Intelligibility of Epistemic Activities* (Chang 2009b), Chang describes what he terms 'Ontological Principles' at play in science. He illustrates these via way of some historical examples, the primary one being Leibniz's rejection of Descartes' laws of collision. Chang quotes Descartes' second rule of motion:

> If [some body] B were somewhat larger than [some body] C, and if
> they met each other at the same speed, then C would be the only one
> to rebound in the direction from which it came, and henceforth they

would continue their movement in the same direction; B having more
force than C, the former would not be forced to rebound by the latter
(Descartes 1644/1988 10-11)

Leibniz rejected this rule, considering the limiting case in which the size of B approaches C, finally becoming identical with it. In this circumstance, as B's mass decreases it continues to move in the same trajectory and at the same speed its mass becomes identical with C's. Exactly at this point it rebounds. Leibniz argues that this case violates the principle of continuity—a continuous change (the size of the ball) leads to a discontinuous one (the direction of motion after collision when the sizes of the balls becomes the same). The point here that Chang wishes to make is this: Leibniz rejects this law not on empirical grounds (although its falsity can easily be demonstrated by experiment), but on metaphysical ones. For Leibniz, a theory that violated the law of continuity was simply nonsensical and to be rejected as such. Chang argues that this is an example of 'an eminent philosopher-scientist using the lack of intelligibility as a cogent reason for rejecting a major piece of theory' (Chang 2009b 66).

Now, this example is not meant to be a proper example of an ontological principle. Chang rather regards the example of Leibniz as a 'ladder to be kicked away' - something that helps to get up somewhere, but not needed once you've climbed onto something else (2001 8). He argues that principle of continuity is not really an ontological principle; rather it is an ontological opinion. This is because its denial is, contra Leibniz, intelligible (2009b 68). Indeed, in the face of modern physics, it seems to be false—think of the photoelectric effect where a continuous change in frequency leads to a discontinuous change in photon emission or any continuous mathematical function with a maximum and a minimum. It might be a strange world in which the principle of continuity is not in play, but it is not a nonsensical one. Indeed as Chang points out, the world of quantum mechanics might well be one in which the principle of continuity is not fulfilled. Intelligibility in many cases seems to be contingent on the prevailing thoughts of the day. Think, Chang says, of

Einstein's denial of non-deterministic quantum mechanics or those adherents to Cartesian mechanics who insisted that the action at a distance in Newtonian mechanics made that theory unintelligible (2009b 67).

So if the principle of continuity is not an example of a genuine ontological principle, then what would count as one? Chang argues that one such a genuine principle might be what he terms the *principle of single value*, that a physical quantity can have no more than one value in a given instance (2009b 68). Why might this have the status of a principle rather than an opinion? It does not seem to be an epistemic generalization—we cannot go round checking its truth. Indeed, Chang notes, a report of a counterexample would simply seem nonsensical. Nor is it a logical truth—he notes that we can conceive of things that have several values such as the names of persons and the multivalued functions found in mathematics (n.b. multivalued functions are not *technically* functions) (2009b 69).

We might think that it is odd that it is not a logical truth, after all if x = 2 and x = 5, then 2 = 5, which is a falsehood. However, in order for this to be a logical falsehood, we must first assume that it is false that 2 = 5. We must make this assumption because there is nothing in the rules or structures of first-order logic that tells us that two numbers cannot be identical to one another. *We* might know that '2 = 5' is false, but logic does not, and we only know that it is false because of the semantic content external to logic that is attached to Arabic numerical symbols. Arithmetic is a stronger system than first-order logic. If we want to make 2 = 5 a logical falsehood, then first-order logic must be enriched with something like Peano arithmetic. After all, there are plenty arguments of similar structure would not generate a falsehood. Think of the following: Clark Kent = Superman and Superman = Kal El. Hence Clark Kent = Kal El. This is not a falsehood. The point is this: we must *assume* that temperatures are things like real numbers which can only take a single value, and are not things like names, which can take multiple values, i.e., we must assume the principle of single value. We might also point out that, because of the imprecision of natural language, translation from it into a formal language is often

something of an art. In this sense, I do not think that it is unambiguously clear that that to say that some object has temperature A at time T and also has temperature B at time T implies that A = B. After all, if we were to say that Robin has a triagonal planar molecule at time T and also that Robin has an Octahedral molecule at time T, we would not take this to imply that a triagonal planar-shaped molecule = an Octahedral molecule.

On the other hand, we might feel that the principle is in some way necessary. The closest thing to this might be the nature of Kantian synthetic *a priori* principles, although Chang maintains that they are distinct from those of the ontological variety (2009b 69). For one thing, there are exceptions to the principle of single value (names and multivalued functions). There cannot be such exceptions to universal principles.

So where does this necessity come from? From the requirements of testing, says Chang. Ontological principles are paired with particular epistemic activities: without the assumption of a particular ontological principle, certain epistemic activities cannot be performed (2009b 69). If we wish to test, say, a prediction that a liquid will be at a certain temperature at a certain time then, without the principle of single value, it would not be possible for any experiment to refute our theory. If we predict that the liquid will be $15\,^{\circ}\mathrm{C}$ and measure it to be $20\,^{\circ}\mathrm{C}$, then without this principle we must accept the possibility that the temperature is both $20\,^{\circ}\mathrm{C}$ and $15\,^{\circ}\mathrm{C}$. It is only the principle of single value that excludes such a result.

There is, Chang says, a connection between ontological principles and epistemic activities —for a type of epistemic activity (in the above example, testing by overdetermination) there is a corresponding ontological principle (the principle of single value). Chang gives a list of further examples, which I will not go into here (2009b 71). But the point is this—an ontological principle is necessary because without it the epistemic activity paired with it cannot be performed.

So how are Born's principles similar to Chang's and how are they different? They are similar in that they are both at least partially non-empirical—neither Born's nor Chang's principles can be tested in any simple way, although of course Born does think that his principles have at least some empirical status. They are also both logically contingent (i.e. violations are not logical falsehoods)—non-contiguous or non-attendance relations are logically possible. They differ in that Chang's principles are presupposed by certain epistemic activities, for example the principle of single values by measurement, whereas Born's are not. We can see this from Born's treatment of Newtonian mechanics. Assuming a principle giving direction in time is not enough to fulfil the principle of antecedence. For a theory to respect Born's principles, it is the working parts of a theory that must guarantee that there is no violation—simply assuming some ad hoc addition or metaphysical background principle is not enough to fulfil the requirements of contiguity or antecedence. This is crucial to understanding Born—it is and can only be the working parts and formal structure of a theory that can fulfil his principles. However, violations do not render a theory unintelligible—despite the fact that Newton's laws violates his principles, Born does not think that they are unintelligible. Rather they are an important and useful part of science.

The question then arises: why respect the principles at all? There is a clear historical thesis in Born's work—that theories which respect his principles are more accurate and that they are more accurate for the same reason that they respect the principles. Born argues that Continuum mechanics provides a more accurate model of the behaviour of solids by treating them as contiguous bodies. By treating the behaviour of particles as fundamentally non-deterministic, quantum kinetic theory provides more accurate treatment of their behaviour and makes the theory properly antecedent.

Here is where I part ways with Chang, and come back to Born. As I have said, I do not think that Born's principles of causation meet the standards for Changian ontological

principles—for one thing it is hard to see what *necessary* connection they have with a particular epistemic practice. But, as I have also said, I do think that Chang's work might help us understand what Born's principles are supposed to do. It goes something like this: Born's principles make causation intelligible. They are necessary for identifying causal relations between events. If violations of them are allowed by a theory, then it is not possible to use that theory to search for causes, something that for Born is the ultimate goal of physics. It is not possible to use such theories to search for causes because if the principles are violated then it is not possible to distinguish between cause and effect. We can still find correlations and maybe even relations, but not ones that are understandable as being causal. A theory that allows such violations may well be a good theory, and may well be intelligible. Newtonian physics, despite the action-at-a-distance that violates contiguity and despite the time-symmetric character of the equations which describe the motion of bodies, is not unintelligible. We can understand perfectly well what bodies are supposed to *do* and indeed we can test this. What is unintelligible, at least for Born, is the notion of causation in such a system. We can perfectly happily provide descriptions of the behaviour of such systems. What we cannot do is provide causal explanations of such behaviour.

4.1.3 Meyerson's Principles of Identity (via way of Eli Zahar)

Ellie Zahar, in *Einstein's Revolution: A Study in Heuristic* (1989)*,* argues that the principles of identity of philosopher (and chemist) Emil Meyerson (1859-1933) can be used to explain the roles that both mathematics and various metaphysical principles have played in the development of physics.

Meyerson claims that our attempts to explain the world all originate in an inherent mental tendency to deny the diversity and divisibility of the world; we wish to categorise diverse and different entities and phenomena as being the same. It is a priori, being an inherent

mental tendency, although, as Zahar notes (1989 41), which particular parts of reality are explicable via the principle can only be determined a posteriori.

The principle gives rise to two types of explanation—legal and causal. When we explain events legally, we explain in them in terms of laws that apply universally, regardless of when and where some they take place. Zahar uses the example of gravitation—the movement of the moon, the falling of a stone and the passenger thrown forward in a decelerating vehicle are all instantiations of the same law (Zahar 1989 41). Meyerson argues that there is evolutionary benefit to possessing an a priori inclination to explain in such a way—it allows us to predict future events (Zahar 1989 41).

The causal principle explains in terms of conservation of substance over time—there are certain sorts of things whose total quantity does not change over time. It is from this principle that Meyerson thinks conservation laws arise. The mind, he says, hypostasizes certain processes by imbuing them them with substance, the total quantity of which remains constant over time. From here we get principles such as the conservation of mass. Meyerson also thinks that we can explain conservation of momentum in a similar way—by multiplying velocity with mass to define inertia, Descartes turned a ratio into something more concrete—a thing whose total quantity persists over time.

Zahar argues that we can also understand the geometrisation of nature in relativistic physics via this principle (1989 43). The attempts by physicists to construct a geometry that underlies all physical phenomena is an attempt to dissolve the diversity of phenomena, to reduce the number of primitives that are needed to explain them and to render the world more homogeneous. Einstein, for example explained both gravitation and inertia in the same geometric terms. Zahar notes that we can read general relativity's field equation:

$$R_{mn} - 1/2\, g_{mn} R = -kT_{mn}$$

in two ways. Left to right, it tells us that energy defines geometry. It can, however, also be read from right to left as saying the geometry of space defines its energy content. Einstein, Zahar argues, preferred the latter interpretation—that geometry defines energy.

So what would Born's principles of causation look like if they were to play a similar role to Meyerson's? Firstly they would be a priori and so not discoverable, but always in place. We would have to have some kind of innate psychological tendency to search for causal explanations of a certain kind—those in which cause and effect are contiguous and temporally ordered. We would not be forced to explain all relations in such a way— Meyerson's principles are purely mental, after all, and so the world may simply not match up to them in a way which makes such a project possible. In fact the search would be for the *respects* in which nature respects contiguity and antecedence. We would, however, attempt this search as we develop new tools which might make such explanations possible. I don't think that Born's principles really operate in these terms. There's nothing to indicate that antecedence and contiguity are rooted in inherent psychological traits. Given that (as already mentioned) Born intends that the principles have some kind of empirical status, even if he thinks that they have their origin in some psychological tendency, they would not be fully captured by that status. This is not to say that there are not some similarities in the way that Meyerson's and Born's principles operate—neither are necessary in any way and both seem to guide theory choice—but what similar aspects they have do not seem to be those that are at the core of Meyerson's project.

4.1.4 Influential Metaphysics

In his 1958 paper, *Confirmable and Influential Metaphysics,* J.W.N. Watkins describes a conception of metaphysics called which provides an explanation of how some particular system of metaphysics might have some influence on science (in a manner differentiable

from some other system of metaphysics) with respect to such aspects of a theory as methodology, theory construction and theory choice.

Consider, Watkins says, a belief that a castle is haunted. Such a belief is confirmable—perhaps by distant howls or the clanking of chains—although perhaps never conclusively so. We should note that this is a very weak notion of confirmation—it simply consists in the observing of events that are consistent with the belief. It is not, however, refutable. There is no amount of empirical investigation that could disconfirm the presence of otherworldly spirits in the castle. We might compare this with Popper's characterisation of Freudian and Adlerian psychoanalysis in *Conjectures and Refutations* (1962)—everything that happens in the castle is compatible with the belief that it is haunted and nothing will disconfirm that belief. Belief in hauntings will also have an impact upon one's scientific methodology, affecting what types of explanations are sought for certain phenomena. Lastly, Watkins notes, such beliefs will also affect one's actions—one might not stay overnight in the castle, or might always carry a flask of holy water whilst there.

Such metaphysical systems—those which are (inconclusively) confirmable, irrefutable and influential—are what Watkins refers to as *Haunted Universe Doctrines.* Watkins alternatively categorises these as *all and some doctrines*—that is to say, they contain both universal and existential quantifiers. These are statements of the type $\forall x \exists y(P(x) \rightarrow (Q(y) \wedge R(x, y)))$: "for all things of a certain kind, there exists some thing that will have a particular effect on them". Such statements are not conclusively verifiable—the universal quantification makes that impossible, barring an infinite number of observations. Moreover there are no empirical tests that can rule out the existence of the individual whose existence the statement implies. So such statements are both unfalsifiable and unverifiable (conclusively, at least).

Watkins offers a four-level categorisation of statements by their level of 'empirical decidability' (Watkins 1958 345). Level one-statements are of the kind 'there exists some

object in some time and some place. These statements are both falsifiable and verifiable. Level two-statements are universal statements of the kind $\forall x(P(x) \rightarrow Q(x))$: 'all metals expand if heated' (Watkins 1958 345), for example. These statements are falsifiable but not verifiable. Level three-statements are of the kind $\exists x(P(x) \wedge Q(x))$—'there exists some metal which expands when heated'. Such statements are verifiable but not falsifiable. Level four statements are Watkins' 'haunted universe doctrines'.

Watkins gives a number of examples of such level four-statements: determinism, mechanism, a-priori conservation doctrines, field theories and metaphysical ideas connected with psychology. Whilst such doctrines are not empirical (being neither falsifiable not confirmable, except in a loose sense), Watkins argues that they can perform a regulative role in scientific thinking and methodology. This, he says, is evident from the historical record—metaphysical doctrines regarding conservation laws, determinism, and the simplicity of nature have played influential roles in the development of new scientific theories.

One could, Watkins says, argue that such doctrines are not *really* descriptions of the world. Rather, insofar as they have some effect upon the functioning of science, they are merely methodological prescriptions disguised as descriptions (Watkins 1958 355). In this manner, Watkins says, the metaphysical doctrine of determinism would really be something like an instruction to always search for natural laws, rather than the thesis that the world is fundamentally and completely governed by such laws. Although this might be an accurate description of the way in which haunted universe doctrines have an affect upon scientific methodology, Watkins does not think that they can be completely characterised in this way. There is, he says, a logical gap between the two—it is quite possible to follow a methodological prescription but reject the truth of the metaphysical doctrine behind it.

If Born's principles of causation were to be haunted universe doctrines then how would they work? The metaphysical doctrine in question would be something like "for any event

(or whatever) B that depends upon event A there exists some contiguous, time-ordered process linking them". Such a belief would be weakly confirmable and unfalsifiable. It's certainly unfalsifiable. Indeed, Born does not seem to consider the existence of Newtonian mechanics—a successful theory that does not respect his principles—to be a falsifying instance of contiguity and antecedence. It's presumed that there does exist some such contiguous process which will be found at a later date. We can see this fairly explicitly when in Born's discussion of quantum mechanics when he tells us that he suspects that some time-asymmetric theory will replace time-symmetric wavefunctions (1949a 103). Whilst it might well be the case that any confirmation of these principles is of the weak variety, I suspect that Born intends their 'empirical' nature to be a little stronger.

Watkins' notion of confirmability is extremely weak, being as it is of the Popperian variety: all that confirmation really means is something like "consistent with" or "explainable in terms of in some ad hoc manner". Remember, Watkins talks of the haunted castle theory being confirmed by the clanking of chains in the night, and refers to the Freudian who produces much confirming evidence for his wish-fulfilment theory of action (Watkins 1958 354). Furthermore, although Watkins recognises that such principles can play some role in the development of theories (maybe even a positive one), he does not think that they are part of science. For Watkins, they exist in a 'no man's land' that lies between analytic and empirical statements (1959 359). He doesn't think that even the entailment of such principles by physical laws implies any kind of 'high probability' that they are true (1959 364). This doesn't seem quite consistent with Born's intentions. Born wants to say that in the final accounting of modern physics (or at least 1940s physics) the principles of contiguity and antecedence 'follow from' the empirical laws.

 I think that Born would not regard theories which are confirmed only in the loose sense that Watkins means as being theories that 'follow from the empirical laws'. Firstly, as we can see from Born's rejection of the causal character of Newton's mechanics, 'consistent with' is not enough. Born rejects the idea that Newtonian physics respects antecedence via

absolute time because absolute time does not do any work in the theory—it is entirely separable from its dynamics and empirical content. The point is that absolute time is empirically irrelevant to the theory—think of the earlier discussion of van Fraassen's (1980) treatment of absolute space—and so Born does not regard it as confirmed by the theory's success. We might say that, on the model of confirmability that Watkins uses, absolute space and time are confirmed by the success of the theory. It's trivially true that Newtonian mechanics is consistent with the temporal ordering of causal relata. We must remember, though, that Born does *not* think that Newtonian mechanics respects antecedence, and we can reasonably claim that he clearly does not think that mere consistency acts as confirmation. I also think that 'follows from' is indicative of something stronger that than Popperian confirmability—by this, Born means that the theories of modern physics, which are themselves confirmed to high degree (remember, Born's a realist, as will be discussed in Chapter Four) are in fact contiguous and (at least to some extent) time ordered.

Still, Born's principles do seem to be intended to influence physics in a similar manner to Watkins' doctrines—they give an impetus to search for certain kinds of explanation, in this case, causal theories that are contiguous and give time-asymmetric descriptions of events.

4.1.5 Zahar on Metaphysical Principles and Heuristic Superiority

In his paper *Why did Einstein's Programme Supersede Lorentz's?,* Elie Zahar (1973) argues that it was not the case that Einstein's theory was accepted because Lorentz's theory was refuted, nor because Lorentz's theory was ad hoc and Einstein's was not. Rather, the success of Einstein's programme over Lorentz's was due to several heuristics employed by Einstein which led to his research programme being more fruitful. This has some relation to Lakatos's concept of negative and positive heuristics within research programmes. A negative heuristic is what directs solutions to problems for the programme away from its

hard core. Positive heuristics guide theory production in the research programme (Musgrave and Pigden 2016). Zahar further argues that the heuristics employed by Einstein were both metaphysically and methodologically heavyweight; that is they correspond to beliefs that Einstein held about how the world really is and they played an active role in the development of the theory of relativity (1973 224). Unlike Chang's ontological principles, they are not assumed simply for reasons of pragmatism. In this section I will discuss Zahar's views on Einstein's heuristics and on the place of metaphysics within science in general, and will then go on to compare and contrast them with Born's views on the role that causal principles play in science.

Einstein's two heuristics are given by Zahar as follows: '(I) Theories have to fulfil the so-called internal requirements of coherence. *Science should present us with a coherent, unified, harmonious, simple, organically compact picture of the world.*' (Zahar 1973 224). This functions both as metaphysical principle and as a heuristic device for theory construction. Why? Because it tells us that simple theories are to be preferred to complex ones and that simple theories are preferable because the world itself is simple. As Zahar puts it ' Simplicity or coherence…are an index of verisimilitude' (1973 224).

The second heuristic is: (II) There are no accidents in nature and hence observed symmetries should be taken to reflect real symmetries in nature. This generates the heuristic rule: '*replace any theory which does not explain symmetrical observational situations as the manifestations of deeper symmetries—whether or not descriptions of all known facts can be deduced from the theory.*' (Zahar 1973 225). What does this mean? It means that a good theory should unify the explanation of observationally identical phenomena. We expect good theories to do this because we expect that such symmetries are not coincidental—they reflect the ordering of the world. Zahar uses the example of the treatment of gravitational and inertial mass to illustrate this. In Newtonian mechanics inertial and gravitational mass, despite being empirically indistinguishable, are different properties, respectively representing a body's resistance to acceleration and its

susceptibility to a gravitational field. No explanation, unifying or otherwise, is provided by the theory for why these two sorts of mass should be identical for any given body. Einstein's theory of relativity, by contrast, treats all mass as inertial mass thus providing a unified explanation of the observed symmetry (Zahar 1973 227).

Zahar goes on to argue that unification and simplicity principles played an essential role in the development of the theory of relativity. For one thing, Zahar thinks, these principles underpinned Einstein's decision to extend the special theory of relativity to both mechanics and electrodynamics. They contributed essentially to the heuristics which lead the theory to become more fruitful than Lorentz's ether theory and which attracted scientists such as Planck to the programme.

The point that I wish to take from Zahar's work is that methodologically useful heuristics can arise from metaphysical beliefs. These beliefs are not like Chang's Ontological Principles—they are not simply assumed out of necessity and held primarily for reasons of pragmatism. Nor are they 'disposable methodological instruments' (Zahar 2007 227)— mere fads which change with the passing seasons and reasons for use.

We should also note that Zahar does regard metaphysical principles in science as being confirmable in some sense (2007 141). Zahar uses a Lakatosian model of scientific theories. For him, such principles form part of the hard core of a particular research program and can be confirmed (in an indirect way) by the empirical success of that program. In this way, such principles have an identifiable role within science, something that Zahar thinks that Popperian models fail to properly account for (2007 141-2).

Again, Born's principles bear at least some similarity to what Zahar describes here. Given their apparent common-sense basis, we might see them as starting out as as (something

like) metaphysical principles before developing into heuristics that Born at least thinks have a positive effect on theory production. In Born's case the metaphysical beliefs that we start out with would be that a) Causal relations exist as timeless laws of dependence; b) cause must be prior to or simultaneous with effect; c) cause and effect must be spatially connected or connected by intermediate chains of causes. This generates the following heuristics 1) Physics should search for causal relations; 2) Physics should search for descriptions of those relations that are time-asymmetric; 3) Physics should search for descriptions of those relations that involve spatial connection between the relata. The main difference between this and Watkins' account is that Zahar doesn't suggest that the metaphysical beliefs that generate the heuristics are confirmable in the way that Watkins does. I do think that the process that heuristics are arrived at in Zahar's account resembles Born's process. We start with what is explicitly a metaphysical belief that causation exists (Born 1949a 124) and what are at least non-empirical beliefs about how causation applies to individual cases (Born 1949a 8-9) and these lead us to search for causal explanations of a particular type. Eventually, via the success of theories that respect those principles, we find some measure of confirmation for them. For this reason in particular, Born's principles align more closely with Zahar's account that Watkin's. Watkins, being a good Popperian, has little truck with confirmability. Zahar's Lakatosian account allows confirmation of metaphysical principles that are part of the hard core to follow from the empirical success of the overall program.

4.2   What is the Status of the Principles?

Of the above options, I think that Zahar's framework most closely matches what Born intends for his principles. I think that they are clearly not Kantian or pragmatic a priori

principles. I think that it's not entirely clear whether or not the principles have a psychological or metaphysical origin. However, given that Born does discuss psychology in terms of gestalts in *Natural Philosophy of Cause and Chance* (1949a 209) but does not connect this with causation (the discussion is about invariants of observation, of which there is a detailed discussion in Chapter 4), I don't think that we ought to infer that he thinks his principles have a psychological origin in the way that Meyerson's do. We might simply say that they originate as non-empirical beliefs and leave it at that. So I think we ought not to regard them as Meyersonian principles either, even in just their origin. Although Born's principles are not Changian, we might find that the notion of a principle of intelligibility accounts for their non-empirical origin in that they account for what sorts of relations are intelligible as being causal relations.

We might worry that Zahar's position is not all that distinguishable from Watkins', or is perhaps simply a subcategory of it. Formally speaking, this would be correct – Zaharian metaphysical principles are just as unfalsifiable as Watkinsonian ones and they seem to have the same formal structure – for any observable symmetry, there will exist a unifying explanation, for example. It's clear that Born's principles of causation are not straightforwardly empirical. Although he calls them 'empirical' in that he takes them to follow from physical laws, it is clear that there is some inductive leap to metaphysics required, just as the move from a regularity to law of nature does. Are they not then examples of Watkins' level four principles, a causal rather that haunted universe doctrine? I think that we can convincingly argue for a distinction in terms of confirmability and in the role that such principles or doctrines might play in science. Watkins' takes them to be unfalsifiable and thus, as a good Popperian, not part of science – they fall into a 'no man's land' between the realms of the analytic and the empirical (Watkins 1958 359). We should note again here how weak Watkins' notion of 'confirmable' is - as a Popperian he does not believe that anything can be confirmed in any manner beyond mere consistency with

observations. Again, think of Popper's example in Conjectures and Refutations of the psychoanalyst.

Watkins happily grants that such principles can be *useful* but that use does not extend to them being part of science. Neither does their entailment by scientific theories even grant the 'high probability' that they are true (and presumably along with it some epistemic warrant for believing in them) (1958 364). Watkins does not think that they are meaningless or that scientists should be banned from talking of them, but he only grants this for pragmatic considerations, i.e. they have utility for theory production but they do not tell us about the world. Watkins worries that different theories may have incompatible metaphysical principles. He also argues that although such principles do have utility in challenging orthodox positions, it's really only when they they challenge the orthodox that they play a useful role (1958 365). They are a useful tool for theory generation and for challenging orthodox positions, but they are not in any way confirmable and they are not part of science. This seems to be precisely at odds with what Born thinks the contemporary status of his principles is.

Hendry (forthcoming 2018) contrasts this position with that of Lakatos and Zahar. For them, metaphysical principles are a proper part of scientific theories, forming part of the hard core of a research program (Zahar 2007 138). Not only do metaphysical principles inspire research programmes and influence their development but the success of the empirical (i.e. falsifiable) components of those research programmes also acts as confirmation of their metaphysical components. Zahar writes 'the sustained empirical success of an RP [research program] can legitimately be claimed to lend some support – if only an indirect one – to its hard core' (2007 141). Both Watkins and Zahar recognise that metaphysical principles have an influence on science. They differ on whether or not those principles are a proper part of theories and whether or not they are confirmable. For Watkins they are neither a proper part of science nor confirmable in any way beyond the trivial. For Zahar they are. So we can see that Zahar's position is much more in line with Born's. For Born, not only did his principles of causation inspire and guide theory

production, but the success of the contemporary theories of physics that respect his principles act as confirmation of them, i.e. he thinks that they are empirical – precisely what Watkins does not.

Now there are some ways in which Born's and Watkins' positions are similar. Born seems to understand 'metaphysical' to simply refer to that which lies outside the remit of science. Empirical is not quite precisely defined but seems to refer to that which is testable and within the remit of science. Like Watkins, he sees these as two mutually exclusive categories. For Born, such metaphysical principles can be *useful* to science, even if they are not testable – he sees a belief in induction as falling into this camp. Unlike the positivists, Born does not think that we should drop everything that is not either deductive or directly observable. So there is a way in which it does look like Born follows Watkins. We should remember though, that Born is not using a fine-grained distinction between metaphysical and empirical and that although he thinks that his principles are not metaphysical, they plausibly are. The distinction between Born and Watkins, and the similarities between Born and Zahar are then made clear in the way that Born thinks that his principles gain empirical support from the physics that is generated by and respects them.

I think we can also say a few more general things about Born's principles and what they indicate about his philosophy. He clearly does think that his principles are confirmable. As opposed to Kant or Chang, he wants to say that his principles are (at least) continuous with physical laws and commensurable with physical theories. We can also see that he rejects the transcendentalist move to regulative principles prior to physics being indispensable to it. This sort of move is indicative of a naturalist position (Maddy 2001) and so we can reasonably infer that Born holds such a position.

5   Does Physics Actually Respect Contiguity and Antecedence?

Before finishing, it should be noted that here are a number of potential problems with Born's thesis that he does not address. We might think that Born's principles are violated by 'spooky action at a distance' in quantum mechanics—this appears to be explicitly non-contiguous. We might also think that the solutions of the general theory of relativity that allow for closed time like curves and wormholes appear to violate the antecedence principle. Whilst a full discussion of these issues goes beyond the scope of this chapter—it would be a substantial task to settle the question of whether not contemporary physics violates locality or allows time travel—it is worth devoting at least some time to them.

5.1.1 Spooky Action at a Distance

The problem of action at a distance in quantum mechanics has to do with measurement collapsing distant wavefunctions, i.e. EPR correlations. Imagine that two quantum systems have interacted in a way that means some particular quantity, for which the value is known prior to interaction, is conserved after that interaction. Until we make a measurement on one system we don't know the value of that quantity in it because the state of that system exists in a superposition of the various possible quantities. However, because the quantity is a conserved one, as soon as we know the value for one system, we instantly know it for the other. The reason that this looks like action-at-a-distance at a distance in orthodox quantum theory is because it looks like measurement of one system collapses the superposition of the other system, thereby changing the wavefunction that describes it.

It's not actually clear that Born would have been aware of this particular problem when he wrote *Natural Philosophy of Cause and Chance*—he certainly does not address it there. Although the first presentation of this problem was in Einstein, Rosen and Podolsky's *Can*

Quantum-Mechanical Description of Physical Reality Be Considered Complete? (Einstein et al 1935), it's not explicitly clear that Born read it. It's not addressed in the Born-Einstein letters at least. However, shortly after Born wrote *Natural Philosophy…*, Einstein sent him a manuscript of an updated version of the 1935 EPR argument in which he explicitly accuses orthodox quantum theory of violating contiguity (Born 2005 168). Born's response is discussed in detail in Chapter 6—Born on Probility, but the relevant point here is that he does not recognise Einstein's example as a case of action-at-a-distance. Born takes Einstein's example—which is a clear EPR correlation-type thought experiment—to be about acquiring information of some distant system via measurement and not (as Einstein intends) about acting on that system. Born thinks that such acquiring of information is merely down to the two systems being correlated by some common history This, again as I shall argue in Chapter 6, is mostly likely because Born does not really believe in the reality of the wavefunction.

5.1.2 Relativistic Time Travel?

The specific problem that wormholes and closed timelike curves introduce is that they can produce situations in which cause is indistinguishable from effect for an observer *even if there is only one allowed t solution.* This would seem to allow an explicit violation of the principle of antecedence, one that might even be stronger than the allowed violations in Newtonian mechanics. After, all the primary problem that Born thinks Newton's theory raises for antecedence is that it does not explicitly prevent –t solutions, in that its dynamics is time-symmetric. Indeed, the same goes for motion in general relativity.

In the case of timelike separation, in which signals can pass between two points in spacetime, we simply have the situation of Newtonian mechanics—two solutions for t, one going forwards, the other back. The ordering of events is preserved across reference

frames. But consider two events, *a* and *b*, which are spacelike separated. For these events, there will exist reference frames in which *a* occurs prior to *b*, frames in which *a* and *b* occur simultaneously, and frames in which b occurs prior to a. Ordinarily we should have no worries regarding a violation of the principles of causation because, as a condition of spacelike separation, there can be no interaction between them short of superluminal travel.

However consider a different situation—two sets of spacetime coordinates which are spacelike separated but connected by a traversable wormhole. In this situation there can be interaction between these spacelike separated coordinates via the wormhole. Imagine two astronauts, one at each coordinate, playing catch through a wormhole. Consider the case in which the two spacelike separated events are one astronaut throwing the ball and the other astronaut catching it. Because the ordering of events is not preserved across reference frames, there can be no frame-independent fact of the matter regarding which is the cause and which is the effect.

Further worries are introduced if we consider the possibility of closed timelike curves (CTCs), solutions of GR which allow for world lines to curve and interact with themselves. One example of this occurs if traversable wormholes are possible, although there are many other solutions that also allow for CTCs. Think of an asteroid, ambling through space, which collides with an identical asteroid that hurtles out of the mouth of a nearby wormhole. The collision knocks the asteroid into the other mouth of the wormhole such that it collides with itself upon exiting, knocking its earlier counterpart into the mouth of a wormhole… and so on ad infinitum. It is easy to see how this sort of situation can create problems for the cause-effect relationship.

This all raises a few questions for my account. The first question is historical: is this a problem for Born in the context in which he was writing? Ought Born to have explicitly addressed these issues, or is quite reasonable to conclude that he was simply unaware of

them or, if he was, he simply considered them irrelevant? The second question is ahistorical: are traversable wormholes and closed timelike curves physically plausible enough to present a possible threat to Born's wish to read time-ordered causal relations into physics?

5.1.3 Should Born Have Considered Relativistic Time-Travel?

The first thing to consider is when in relation to Born's work the scientific work on wormholes and closed causal loops was published. *Natural Philosophy of Cause and Chance* was published in 1949a and is largely a transcript of a lecture series given in 1948. So when were wormholes first proposed? In their paper *The Particle Problem in the General Theory of Relativity* (1935), Einstein and Rosen attempted to solve the problem of the apparent status of particles in GR as being singularities. They described a solution of GR involving 'sheets' of spacetime connected by bridges and it is this that later became known as an 'Einstein-Rosen Bridge' (a term frequently heard on science fiction shows). However, Einstein and Rosen do not appear to consider their bridge to be literally a bridge: - it is a way of representing the particle without singularities. "The neutral, as well as the electrical, particle is a portion of space connecting the two sheets (bridge)", they write (Einstein and Rosen 1935 77). It is not a traversable pathway between regions of spacetime. I do not know whether or Born read or considered this paper (although given his friendship with Einstein and his interest in relativity, it is certainly not unlikely that he at least looked at it), but regardless of that fact, it seems unlikely to say the least that it would lead him to conclude in the existence of traversable wormholes. Indeed in their 1962 paper on wormholes *Causality and Multiply Connected Space-Time*, Robert Fuller and John Wheeler note that despite the prior existence of solutions of the GR equations allowing for such situations, "it was only recently through the work of Fronsdal and Kruskal that one has come to understand the unusual nature of the topology implied by the Scharzschild solution" (Fuller and Wheeler 1962 920). The papers referred to by Fronsdal (1959) and Kruskal (1960) were not published until the late 50s. It is therefore reasonable to conclude that

Born was unaware of the existence of wormholes when he wrote Natural Philosophy of Cause and Chance.

So what about closed timelike curves? The first well-known paper on the subject seems to be Kurt Gödel's *An Example of a New Type of Cosmological Solutions of Einstein's Field Equations of Gravitation* (1949). This presents a solution of the GR equations in which an entire universe is contained in a closed timelike curve but was only published in 1949, taking it out of the scope of Born's project, or at least gives a good reason for any potential ignorance of it on Born's part. There were, however, earlier solutions allowing for CTCs. Kip Thorne describes William van Stockum's (1937) paper, containing a CTC solution for an infinitely long cylinder of rapidly rotating dust, noting that physicists largely regard it as unphysical due to the unavoidable infinities it involves (Thorne 1992). So even if Born was aware of van Stockum's paper, and there is nothing in particular to indicate that he was, it is reasonable to conclude that he would not have considered it a threat to his view of causation because of the unphysical nature of the solution described.

The next question that I will address is this: regardless of what Born knew of them when he wrote *Natural Philosophy of Cause and Chance*, are traversable wormholes and closed timelike curves physically possible? Kip Thorne argues that although van Stockum's and Gödel's CTCs are unphysical (because the presence of infinities and because the cosmological constant must be non-zero, respectively), this is no reason to conclude that CTCs in general are not physically possible. He notes that the past failures of physicists with regards to determining what will turn out to be unphysical should make us suspicious of such moves. To go into this matter further is outside the scope of this chapter. What we can say is this: the physical possibility of CTCs and traversable wormholes is not so obvious as to provide the basis of a serious objection to Born's views on causation.

I think that we can see that neither action-at-a-distance in quantum mechanics nor relativistic time travel present a particular problem for the historical Born. He doesn't

believe in such action-at-a-distance (again, see Chapter 6, but this is probably because he doesn't believe in the physical reality of the superpositions. It is quite possible that he had not encountered CTC or wormhole solutions at the time of writing *Natural Philosophy…*, but even if he had, those that existed prior to 1948 were clearly unphysical. Indeed, it still remains unclear as to whether or not such things fall within the domain of the physically possible.

6   Conclusion

I've argued in this chapter that we should view Born's principles of causation as being akin to Zahar's account of principles in science. They can't be Kantian or Changian principles because they are not necessary in the way that those principles are—Born is perfectly happy to allow for the existence of good physical theories which violate his principles. We should interpret Born's account of them as follows: they have their origin in non-empirical beliefs about causal relations. These are then applied as heuristics that guide theory choice and production—we are inclined to search for theories that respect these principles. Finally, we find that they have been confirmed, in that the descriptions of nature in the equations of contemporary physics respects the principles of both contiguity and antecedence. We can also see in this an indication that Born holds a naturalist position.

Whilst we might legitimately ask questions about apparent possibilities—spooky-action-at-a-distance and relativistic time-travel—that appear to violate contiguity and antecedence, we find that these are not actually problems for Born. His view of quantum mechanics seems to preclude such action-at-a-distance. Relativistic time-travel post-dates *Natural Philosophy of Cause and Chance* and it is any case unclear as to whether it is physically possible at all.

Chapter 4—Born on Scientific Realism

1   Introduction

This chapter will examine Born's work related to realism and argue that he fulfils the criteria—metaphysical, semantic, epistemic and progress/continuity of reference—for being a scientific realist. I'm going to separate this discussion into two parts—in the first I will examine a number of Born's writings in the 1940s and 50s in which he gives arguments against positivism and advance his own position, which he terms invariant realism—realism about quantities which are invariant under transformation. I'll then give an overview of definitions of realism before arguing that Born meets all of the relevant criteria, as well as responding to an opposing argument of Galavotti's (1995).

Born deals most directly with realism in two papers, 'Physical Reality' (1953) and 'The Concept of Reality in Physics' (1958), in which he offers a criticism of logical positivism (and of historical materialism in the latter paper). 'Physical Reality' was published (in Philosophical Quarterly and reprinted in *Physics in My Generation*) in response to a talk delivered by Herbert Dingle at the meeting of Section A of the British Association in Edinburgh in 1950, 'Philosophy of Physics, 1850-1950' (and published in *Nature* as Dingle 1951). Born was also in attendance at the meeting, giving a paper entitled 'Physics in the Last 50 Years' (it being halfway through the century, anniversaries and reminiscences seemed to be common themes). 'The Concept of Reality in Physics' is a further development of these ideas and a response to the Russian physicist Sergei Suvorov, who translated 'Physical Reality' into Russian and published it in the Soviet Union, along with his own criticism of the Copenhagen school and an argument for materialism.

I'm then going to examine his paper *Symbol and Reality* (1966) which presents a different position on realism that I'll argue is much more structural in nature than the earlier version based on invariance. This is presented separately because it does represent a change or development in Born's thoughts on the topic.

2   Born, Positivism and Invariant Realism

2.1 Dingle

In this section I am going to examine Dingle's (Dingle 1951) argument and Born's (Born 1953) response to it. Dingle argues that modern physics is positivist in nature and that this is reflected in the development of particle physics and relativity. He further argues that the proto-positivism of Arthur Eddington and in particular (a slightly modified version of) the operationalism of P. W. Bridgeman should be adopted.

Dingle argues that the traditional philosophical position embodied in the theories and practices of physics, in so far as there was one, was characterised by a kind of naïve realism—there exists an external world and physics aims to discover its 'contents and laws of behaviour' (Dingle 1951 632). This, he says, arose not from an attempt to study or follow the methods of Newton and Galileo, but simply from common sense, hence naïve realism (1951 631). The existence of the external world is simply assumed a priori. This assumption, he says, was something accepted as being 'apart altogether from the practice of science' (1951 632). The discoveries of science had to be accommodated within this

framework and, until the mid-nineteenth century, this accommodation was entirely unproblematic.

Whewell's *Philosophy of the Inductive Sciences*, he argues, fits quite precisely into this picture. Dingle argues that 'the way in which the ideas are presented does take its shape from the necessity to accommodate the physical science of the time' (1951 632). Whewell was, he says 'unconsciously' (1951 632) guided to attempt to demonstrate a picture of the world that justified the current state of physics. Such a picture, Dingle notes, was easy to construct and to be confident in.

In the next section of his paper, Dingle argues that the history of physics post 1850 represented the 'uprooting' of confidence in this picture of the world via the development of quantum mechanics and relativity (1951 632). He also notes that, apart from these, physicists in the late nineteenth century had conceptual worries with the Newtonian framework, specifically with regards to the conceivability of absolute space and the circularity of the second law of motion. Dingle doesn't elaborate on the latter point further than telling us that there were 'difficulties in framing the second law of motion in a form that did not include a circular argument' (Dingle 1951 p632) but presumably is referring to Mach's *The Science of Mechanics* (1919). The failure of the ether theory, he writes, caused some physicists (he lists 'Kirchoff, Hertz, Mach, Pearson, Poincaré') to doubt whether physics could actually be regarded as the study of the external world. Again, Dingle does not elaborate on which this failure consisted in, but presumably it has to do with the failure of the Michelson-Morley experiments to detect the ether. This, however, had little effect on the practice of physics—in general physics was successful. Why worry about the foundations?

It was the progress of physics itself, Dingle argues, not the conceptual worries of various individuals that killed off this philosophy. There were, as previously noted, two strands to this - the gradual development of particle physics from kinetic theory into quantum theory

and the sudden (as Dingle puts it, at least) development of special relativity. Dingle deals with particle physics first. The kinetic theory, he writes, attempted to explain the behaviour of bodies in terms of particles obeying Newtonian laws. All of our observations ultimately derive from the mechanical behaviour of molecules. It is these molecules that are real. What we observe is but appearance. It was thus the job of physics to discover all that it could about these molecules (Dingle 1951 632-3).

Dingle argues that the development of statistical mechanics represented an introduction of a positivist methodology into physics. This, he thinks, happened in the following way: 'What they [the physicists] were doing was finding the rational relations between their observations, and using the molecules...as useful conceptions to extend the scope of those relations' (1951 633). This does not mean that the physicists considered themselves to be positivists, rather they were 'indifferent' (1951 633) to any tension between their method and their philosophy. Dingle claims that 'the matter...came to a head' (633) with the development of atomic physics. We cannot, he thinks, actually *know* anything about fundamental particles—we can only calculate the probability of them being in a certain position. They do not obey causal laws. They are not even individuable. And yet they are supposedly the fundamental constituents of reality. This, Dingle thinks, cannot be the correct way of thinking about them. He writes 'The pursuit of reality has ended as a greyhound race ends, with the disappearance of the hare underground. And the only intelligible conclusion is that the object of the whole business is not to catch the hare but to run the race' (1951 633). Thus, Dingle thinks, we can only understand physics as the attempt to establish rational relations between observable phenomena, and not as the attempt to describe some 'hidden reality' that lies beneath them.

Dingle next turns his eye to relativity theory. Relativity, Dingle says, teaches us a similar lesson—that physics will be more successful if it concentrates on examining the relations between phenomena and abandons the attempt to describe the "reality" of entities that underlie those phenomena. The objects of Newtonian physics, Dingle says, had precise and

unvarying intrinsic properties—length, mass —and this is not the case for the objects and bodies of relativistic physics (1951 633-4). In physics described by the theory of relativity, such properties depend on the velocity of the body and vary with it. This velocity in turn depends on the reference frame of an observer and can take any value from zero to approaching c. The velocity of the reference frame is simply a choice—Dingle writes that 'Nature has nothing whatsoever to do with the matter' (Dingle 1951 634). Hence, Dingle thinks, we should abandon the idea of objective properties of bodies and instead embrace the idea that we should concentrate on the relations between the phenomena. This is what relativity has done, and this is what has led to its success.

Dingle argues that there are two responses to the situation that particle physics and relativity presents to us. The first is to carry on as usual and take the objective of physics to be the investigation of the real and external world, albeit one that is 'essentially mysterious and even self-contradictory' (Dingle 1951 634). Dingle is clear that he thinks that the conclusions of mysteriousness are inescapable. That the objective of physics is the study of an essentially mysterious external world is a position that he takes to be most unsatisfactory.

The second response is Dingle's preferred one—to take the developments of particle physics and of relativity to indicate that, rather than studying an objective, external world, physics can only concern itself with the establishment of rational relations between measurements.

2.2 'Physical Reality'

Born responded to Dingle in his 1953 *Philosophical Quarterly* paper 'Physical Reality' (Born 1953), republished in the collected volume 'Physics in My Generation' (1956). In it, Born argues against the positivist viewpoint and, in doing so, gives his own views which,

in his words 'cannot be explained better than by way of contrast' (Born 1953 151). Born disagrees both with Dingle's argument that physics does not really deal with a real external world, and with his argument that the practice of physics does not require a concept of reality.

Born thinks that Dingle is advocating a denial of the existence of a 'pre-existing external material world' (Born 1953 152) which ultimately expresses itself as a kind of subjectivism about the world or a 'physical solipsism' (1953 152). Born notes that, although such a position is logically coherent, logical coherence by itself is not much of an argument for anything. For Born, the main argument for Dingle's position is historical, and in particular related to progress. It is the claim that, although perhaps helpful in the past, maintaining a belief in an objective, observable world would be 'detrimental' to scientific progress. If physics cannot be understood in reference to an objective external world, its activities and conclusions should be understood instead in terms of experiences. Born disagrees. His argument has three strands. Firstly he argues that such a positivism is methodologically problematic: physicists do behave as if they are working with a real external world and that it is implausible that they could act otherwise. Secondly he denies that there is a principled distinction to be drawn between the observable and the unobservable. If this is the case then there would seem to be no basis for the metaphysical distinction between them that Born thinks that Dingle is drawing. Thirdly, Born objects to the conclusions Dingle draws from developments in modern physics—that its properties and entities somehow fail to meet the criteria for objective reality. I'm now going to examine in turn the arguments that Born gives for these positions.

2.2.1 Methodology

Born argues that concepts of fields, particles, etc. are, far from being unnecessary and unhelpful crutches that should be gotten rid of, are necessary for the functioning and progress of physics. He asks 'How could an experimentalist work and communicate with his collaborators and his contemporaries without using models composed of particles,

electrons, nucleons, photons, neutrinos, fields and waves, the concepts of which are condemned as irrelevant and futile?' (Born 1953 152). I'm not convinced that this is a fair characterisation of Dingle. Dingle never seems to make an assertion of this kind of strength—he might well respond by pointing out that we are not supposed to ban the discussion of such models, but rather should restrict what we consider real about them to the bare phenomena and how we might order them.

A position such as Dingle's is not, Born acknowledges, held without reason. He explains that 'a certain caution' (1953 52) is needed: the 'naïve' approach to reality that he says was taken by Newtonian systems is 'not satisfactory' (1953 52), and modern physics has demanded a new approach.

Born notes that the word 'reality' is part of ordinary language, and so its meaning is 'ambiguous', in that it can be used in multiple ways just any word can. He notes that there is an element of subjectivity in what is considered as "real" by different individuals and groups—'the realities of a peasant or craftsman, a merchant or banker, a statesman or soldier have certainly little in common' (Born 1953 152), he writes. What is most real to these individuals, he says, is what is most important to them. Born wonders if there is any philosophy that can give some definition of reality that is 'untainted by some such subjective associations' (1953 153). I think that we might actually fairly point out that we can differentiate between 'real' in terms of what is most important and 'real' in terms of what we actually believe exists in some mind-independent manner. What concerns Born here though, is whether it is possible for science to make some such precise definition of reality.

Could science, as Born thinks that Dingle suggests, discard without detriment the concept of reality? Born thinks the answer is no. It could only be discarded by 'men isolated in ivory towers, remote from all actual doing and observing, the type of man who becomes extremely absorbed in pure mathematics, metaphysics or logic' (Born 1953 153). Born

notes that Niels Bohr, who he regards as having 'contribut[ed] more to the philosophy of modern science than anybody else' (1953 153) holds that it is necessary to describe experiments as a naïve realist. Born writes 'it is an essential part of this procedure to distinguish between ideas, projects, theories and formulae on the one side, and the real instruments and gadgets constructed according to those ideas. Here the naïve use of the word real, the simple belief in the real existence of the material apparatus, is imperative' (1953 153). Born says that he does not think that Dingle does forbid applying the term 'real' to our equipment, but does forbid its application to 'atoms, electrons, fields, etc., terms used in the interpretation of observations'. There is, Born thinks, a problem here. The problem is this: how do we draw a line between what is unproblematically real, and what is not? Where, Born asks 'is the border between these two domains'(1953 153)?

2.2.2 Denying the Distinction

Born then asks us to consider a piece of crystal. Such an object clearly and unproblematically falls into the domain of 'crude reality' (1953 153). We can grind that crystal into a powder comprised of a dust whose particles are too small to be seen with an unaided eye. We can, however, observe such particles with an ordinary light microscope. He asks 'are the particles then less real?' (1953 153). We can use an ultra-microscope (a microscope which uses light scattering rather than reflection to observe objects up to a nanometre scale) to observe still-smaller particles. Born tells us that after the ultra-microscope fails us, we can use an electron microscope to observe large molecules. He writes 'Where does that crude reality, in which the experimentalist lives, end, and where does the atomistic world, in which the idea of reality is illusion and anathema, begin? (1953 153). The answer he is gives is (of course) that there is no such distinction to be drawn. He writes 'if we are compelled to attribute reality to the ordinary things of everyday life and materials used in experimenting, we cannot cease to do so for objects observable only with the help of instruments' (1953 153).

This latter argument seems to be a version of the "denying the distinction" argument given against positivism by Grover Maxwell in his paper 'The Ontological Status of Theoretical Entities' (Maxwell 1962 181-192). Maxwell argues similarly for a continuous transition between observable and unobservable. Maxwell's version is more detailed and also incorporates observing large objects with binoculars and spectacles but the point is the same—there is no principled way to draw a distinction in the ontological status of some entity based upon how we observe it. If we cannot do that then the positivist stance of denying the reality of unobservables becomes untenable. Of course there are arguments against this position—for one, it is not necessarily the ontological status of an entity that is relevant, but rather our epistemic warrant to believe in its existence (See van Fraassen 1980, amongst others), but this is beside the point—we are interested primarily in Born's position with regards to realism, not whether he has resolved the realism/antirealism debate.

So this seems to be a clear rejection of the positivism that Dingle advocates. First Born argues that physicists, on the whole, treat 'theoretical' entities as real, and indeed have to, as he thinks that to do otherwise would render scientists unable to communicate ideas with one another. Secondly, despite worries about reality, the physicist must be a naïve realist about her equipment in order to be able to get on with the job of experimenting. Thirdly there is no ontological distinction to be drawn between the equipment and the things it measures—our equipment is part of the ordinary macroscopic world and Born denies that there is any ontological distinction to be drawn between that and the world that can only be viewed through electron microscopes. Therefore we must also treat unobservables as real; or rather we say that things like electrons—things that are only observed indirectly—are still observable. Born adds some nuance to this, and discusses precisely what sort of things we have warrant to believe in (invariants of observation) later on in the paper. Next, however, he turns to Dingle's argument that modern physics does not deal with an objective reality.

2.2.3 Invariant Realism

Born deals first with Dingle's arguments from kinetic theory. He notes that he cannot tell whether or not Dingle thinks that kinetic theory is entirely 'superfluous' (Born 1953 154) or whether he thinks that we should not regard molecules as real, and rather as simply '"counters" or "dummies"'—i.e. simply as place-holder terms with no meaningful content (Born 1953 154). Born argues that Dingle has missed a crucial point in his discussion of kinetic theory—that kinetic theory provides strong evidence for the existence of molecules.

Born notes that kinetic theory gives 'definite properties' to molecules: 'weight, size, shape (degrees of freedom), mutual interaction' (Born 1953 154). He explains that 'a small number of molecular constants determines an unlimited number of phenomenological properties, in virtue of the molecular hypothesis' (1953 154). Therefore, Born thinks, predictions of new phenomenological properties act as confirmation of the existence of molecules. Born gives as examples x-ray scattering by crystals and radioactive phenomena. The latter he thinks give 'striking' evidence for the molecular hypothesis and therefore the positivist position:'to speak of a dummy producing a track in a Wilson chamber' is 'inadequate' (1953 155)—i.e. something that is merely a place-holder or a model cannot produce tracks in a cloud chamber. Something real (a particle) must be doing the job.

Born gives the following argument for this inadequacy. Imagine that you see a gun fired and a man collapse some distance away. Do you know that the bullet found in the man's wound actually traversed the distance from the gun to the man's body? You have not seen it travel and indeed *nobody* could have seen it travel, except in the case that someone had happened to set up a sophisticated photographic system. Born is doubtful that one would be willing to hold that the bullet in flight is merely a theoretical dummy in order to organise the observable phenomena of the shot being fired and the bullet being found in

the wound. Yet he thinks that this is exactly what a positivist attitude implies. He writes 'I only wish to point out that if one denies the existential evidence of an atomic track which *can* be seen, one is committed to denying the existence of a bullet in flight which *cannot* be seen, and of numerous similar things' (Born 1953 155).

Born thinks that the origin of the positivist denial of the reality of things like molecules is due to the concept of reality being understood as 'known in every detail' (Born 1953 155), something which he thinks is at odds with the way it is ordinarily used. He notes that we think of the Romans contemporary to Caesar and the Chinese people contemporary to Confucius as being real, although we have no way of directly observing them. He asks (rhetorically): 'Are these Romans or Chinese of the present or the past only dummies invented by the historians to connect phenomena? Which phenomena? Perhaps the words found in newspapers, in books, or on ancient tombstones?' (Born 1953 155*)*.

So Born clearly thinks that not only is there no distinction between observable and unobservable for the positivist to get to grips with, but that even if there were, Dingle applies such a distinction inconsistently. Born appears to think that it cannot be applied just to the microscopic world, but also to many elements of the macroscopic world—very fast objects in motion, and historical persons. This is a bullet that Born clearly thinks that the positivist would not be willing to bite.

Born next discusses Dingle's argument from the development of the theory of relativity. He quotes the conclusion of Dingle's argument that 'by abandoning all attempts to assign any property at all to matter [because the properties of mass and length become observer-dependent] we can learn more and more about the relations of phenomena'. Born thinks that this is a completely mistaken characterisation of relativity. Indeed, he describes Dingle's position as a 'misrepresentation' of relativity. It has not at all abandoned such attempts. Rather, it has 'refined the method of doing so in order to conform with certain new experiences, such as the famous Michelson-Morley experiment' (Born 1953 156).

Why does Born think Dingle is so mistaken? He writes 'This root of the matter is a very simple logical distinction which seems to be obvious to anybody not biased by a solipsistic metaphysics; namely this: that often a measurable quantity is not a thing, but a property of its relation to other things' (Born 1953 156*)*. To explain this Born gives the example of a shadow cast by a circle of some opaque material on a wall. The shadow will, he says, generally be elliptical. By turning the circle, one can generate an elliptical shadow with an axis length of any value between 'almost' zero and some maximum. He writes that this is an 'exact analogue of the behaviour of length in relativity which in different states of motion may have any value between zero and a maximum' (Born 1953 156). He similarly gives an analogue of mass in relativity by describing the cutting of a long sausage into elliptical slices with one axis varying between some minimum value and ' 'practical' infinity' (Born 1953 156).

The point of these examples is this: by observing the shadows cast on different planes we can determine that the thing casting the shadow is a circle and uniquely determine its radius. This is because the radius is an invariant of transformations from the parallel projection. Similarly, the cross-section with the smallest area is an invariant of the various cross-sections of a sausage. Born thinks that most measurement in physics is concerned with projections, rather than directly with the things being measured.

Born then explains projections, explaining that they are defined in relation to some system of reference. He notes that there many equivalent systems of reference and that for any physical theory there is some rule, called a law of transformation, that connects projections from the same thing across different systems of reference. There are quantities called invariants that hold the same value across all systems of reference and so are independent of the various transformations. Born will later come back to invariants—it is these that he thinks we are epistemically entitled to believe in (Born 1953 157).

For Born, the point about projections with regards to Dingle's argument is that the discovery that mass and length are projections of invariant quantities, rather than being invariants themselves, is nothing new. Indeed, he writes: 'the main advances in the conceptual structure of physics consist in the discovery that some quantity which was regarded as the property of a thing is in fact only the property of a projection' (Born 1953 157), i.e. discovering that something is a projection of some property is not abandoning the objective reality of it, it is simply coming to understand it better.

Born gives the example of the transition from a 'primitive (pre-Newtonian)' concept of gravity to the Newtonian one. Pre-Newtonian gravitation is described by group of transformations for which 'the vertical, the normal to the plane surface of the Earth, is absolutely fixed'. In this system of transformations, gravitational force is an invariant and the weight of a body 'an intrinsic property', i.e. an invariant. In a Newtonian system, space is isotropic and weight dependent on the position of a body (Born 1953 157). In this system, mass is the invariant quantity

Born argues that relativity is simply a continuation of this. He notes that the Galilean transformations of Newtonian physics separate space and time (i.e. there is no connection between spatial coordinates and time) whereas the Lorentz transformations of the theory of relativity connect them. Because of this, Born explains, many quantities that were thought to be invariants in the classical system—'distances in rigid systems, time intervals shown by clocks in different positions, masses of bodies' (Born 1953 157)— turn out not to be, rather they are 'projections, components of invariant quantities not directly accessible' (Born 1953 157). Born tells us that, just as in the case of the shadow and the sausage, these invariants - 'maximum length and minimum mass' (Born 1953 157) - *can* be determined.

Born thinks that this explanation clearly answers the question of whether or not relativistic science has abandoned attempts at objective descriptions of the properties of bodies. Bodies do have definite, invariant quantities—rest mass, proper length and proper time -

but the older quantities of mass and length turn out not to be among them. These, it turns out, are rather properties of the relations between a body a particular reference system. So we have not given up trying to give objective accounts of properties - it is simply the case that what we thought were invariant properties turned out to be projections and that bodies are possessed of a different set of invariant properties (Born 1953 158). Born notes that we might have avoided some confusion by renaming mass and length to something else and reserving the older terms to refer exclusively to invariant properties.

Born next discusses the situation in atomic/quantum physics. He writes of Heisenberg's uncertainty principle 'Is not this vagueness, this impossibility of answering definite questions about position and velocity of a particle, an argument against the reality of particles and altogether of the objective material world?' (Born 1953 158). Born answers that we must reflect on what we mean by a particle with regards experimental evidence. Once more, he thinks that 'these words [particle] signify definite invariants which can be unambiguously constructed by combining a definite number of observations' (Born 1953 158). His point seems to be this: uncertainty certainly exists, but it is a matter of physical law. There does exist some definite total amount of information about a system that can be obtained, including a number of invariant quantities. By performing multiple experiments, we are able to determine them. Thus the world of the particle is not, as Born takes Dingle to think, an unreal and subjective one.

Born explains how the relevant projections work in quantum mechanics. He notes that although we abandon the idea that particles in quantum mechanics follow deterministic laws, instead embracing a theory which can only give us probability distributions, we have not abandoned the idea of a real description of particles. He writes of the probabilistic descriptions of quantum mechanics 'This is of course a decisive change in our attitude to the world. It calls for new ways of describing the physical world, but not the denial of its reality' (Born 1953 159).

He explains this new way of describing the world by reference to an experiment involving refracting a polarised beam of light. First we polarise a light beam by passing it through a Nicol Prism (an optically clear polarising crystal). Then we pass the beam through a crystal which double-refracts it, producing two separate beams of light which are linearly polarised perpendicular to each other. These beams are then projected onto some detector. The intensity of a beam is the square of its amplitude, given in the following way: Let the primary beam have amplitude A. Let the angle between the direction of polarisation of the primary beam and either of the secondary beams be θ. The amplitudes of the secondary beams are then given by $A\cos(\theta)$ or $A\sin(\theta)$. The intensity of either wave is then the square of the amplitude $A\cos^2(\theta)$ or $A\sin^2(\theta)$. Next we reduce the intensity of the primary beam until it is low enough that only individual photons are impacting on the detector. We then count these photons, finding that their average number is given by the square of the amplitude. The point of this is that the intensities of the secondary beams are projections of the amplitude of the primary beam. Born writes 'The prediction made by the theory in regard to the intensities of the emerging beams, or the number of photons in these, has a meaning only in relation to the whole experimental arrangement, the Nicol prism and the crystal' (Born 1953 159).

Born notes that this goes for all quantum phenomena—any measurement is of a projection from some natural phenomena with respect to a system of reference comprised of the experimental apparatus as a whole (Born 1953 159). He discusses Bohr's concept of complementarity with respect to the idea of invariants of observation. Born describes complementarity as follows: 'Bohr has introduced the idea of complementarity to express the fact that the maximum knowledge of a physical entity cannot be obtained from a single observation or a single experimental arrangement, but that different experimental arrangements, mutually exclusive but complementary, are necessary' (Born 1953 160). Born relates this to his earlier example of the shadows cast by disc on a surface. Just as we needed to observe its shadow cast on different planes in order to determine its invariant properties, he tells us, Bohr's principle of complementarity tells us that in order to obtain

maximum knowledge of some entity, we need to make observations of many different projections from it. When we obtain such maximum knowledge, we get information about the invariant properties (Born lists charge, rest mass, spin, etc. (Born 1953 160)) of whichever entity we are studying. He argues that when we have determined these properties, we decide that they are properties of some particle. He writes that 'I maintain that we are justified in regarding these particles as real in a sense not essentially different from the usual meaning of the word' (Born 1953 160)).

Born gives the following argument for his position. He notes that the positivists regard only sense data as real and everything else as conceptual tools for ordering that data. He also makes reference to Henry Marganau's position that there are two layers of reality—that of sense data and that of constructs. Born thinks that both the positivists and Margenau are incorrect in their positions—they ignore 'two essential points of reality' (Born 1953 161). The first is that it is wrong to regard 'crude sense impressions as primary data'. The second is that it is only constructs that are invariant under transformation that have 'the character of a real thing'. Other scientific 'constructs' do not. We should note for fairness here that although this may be an accurate characterisation of Dingle's position (who does seem to be a phenomenalist of some stripe), it is not an accurate characterisation of positivism as a whole—Schlick, for example, characterises positivism as rejecting the debate between realism and idealism (Schlick 1948).

Born writes that as children we develop the ability to 'distinguish and recognize objects' (Born 1953 161). We do not experience the world as a 'kaleidoscopic' sequence of impressions and sensations, but as a sequence of recognisable events and objects in which the identity of entities is preserved despite their changing impressions. Born thinks this is due to the mind picking out the invariant features of the observable world. He describes the situation of walking your dog and it chasing a rabbit. As the dog runs further away from you, you still see it as your dog, not a series of impressions of gradually decreasing size. Born thinks that the Gestalt school of psychology agrees with this situation. Indeed he

thinks that "Gestalt" should be translated not as shape or form, but as "invariant" (Born 1953 162).

Born further notes that the information transmitted to the brain by nerve fibres is nothing like the 'physical stimulus' that causes them. He tells us that the brain decodes these signals 'determining the invariant features in this welter of ever-changing signals' (Born 1953 162).

We cannot, Born thinks, describe scientific activity by assuming that we start with raw sense impressions. He writes 'if we attempted to build a philosophy of science based on the assumption that our raw material is unordered sense impressions, we could not even describe our manipulations and simple instruments' (Born 1953 162). This is, presumably, because we start with the ability to pick out invariant features of the world—it is already partly ordered for us. Born writes that science must start with ordinary concepts and language—the naïve realism he discussed earlier. When we go beyond the directly observable world—using 'magnifying devices, telescopes, microscopes, electro-magnetic amplifiers, etc.' (Born 1953 162)—we cannot use our unconscious power to pick out invariants and must instead use conscious methods 'thinking, mathematics and all its tricks' (Born 1953 162). Using these devices, we are able to pick out the invariant features of the unobservable world, and it is these that we are entitled to treat as real. Born writes 'Thus we apply analysis to construct what is permanent in the flux of phenomena, the invariants. Invariants are the concepts of which science speaks in the same way as ordinary language speaks of 'things', and which it provides with names as if they were ordinary things' (Born 1953 163).

Born acknowledges that entities such as electrons are 'of course' (Born 1953 163) not precisely the same as ordinary, macroscopic objects. They are not, for one thing, always individuable—Born explains that if we knock an electron out of an atom with another electron, there are no circumstances under which we can determine which was the knocked

and which was the knocker (Born 1953 163). Born thinks that they do, however, have enough in common with a macroscopic particle to be described by the same term. Such an extension of a term is, Born thinks, commonplace enough to be unproblematic. He explains how the mathematical term 'number' was extended from purely describing integers to being used for fractions, irrational numbers, transcendental numbers and imaginary numbers. He explains that in physics we are perfectly content to describe infrared and ultra-violet radiation as light, despite the term 'light' initially referring purely to the visible spectrum. In the same way, we call electrons particles even though they do not share all the same properties as grains of dust. Born writes that 'the principle of doing this is always the same' (Born 1953 p163): we identify the invariant features.

Born explains this by discussing the concept of a wave. He notes that we consider waves on a body of water as real, even though they are merely 'a certain shape on the surface'. We do this because a wave is picked out by a set of invariant features—wavelength and frequency. Water waves have these. Born argues that exactly the same holds for light waves in quantum mechanics. That they represent 'only a distribution of probability' is no reason to claim that they are not real. They share the invariant features of any other wave, and thus they are real waves (1953 163).

2.3 'The Concept of Reality in Physics'

In a 1958 talk entitled 'The Concept of Reality in Physics' delivered in Düsseldorf and reprinted in English in the 1962 collection Physics and Politics Born offers a restatement of his position with a few additions. The arguments are largely the same—positivism is mistaken in thinking both that there is some proper distinction between observable and unobservable; we are entitled to believe in invariants of observation—with the addition of a criticism of materialism (in the Marxist sense). Born's earlier paper had been read by Sergei Suvorov, a Russian physicist who suggested that, as Born rejected positivism, he

should embrace materialism. Unfortunately, Suvorov's paper only exists in Russian (Suvorov 1958—Soviet Physics Uhspeki Vol1 Issue 2).

Born disagrees with Suvorov. He writes that Suvorov agrees with Born's position on positivism, but disagrees with him regarding invariance, arguing that it is materialism that solves the problem. Suvorov, Born tells us, thinks that invariant properties are insufficient to identify individual types of entity. Instead we must find all of the 'objective relationships which are specific to that object' (Born 1962 p31). Born discusses two examples of Suvorov's which Suvorov thinks are identifiable only by discovering the objective relationships, and not merely by the invariant properties. The examples that Suvorov gives are the anti-proton and the anti-neutrino. Suvorov thinks that the invariant properties of charge and mass are insufficient to identify the anti-proton. This is because, as Born notes, it can be 'confused' with a negative hydrogen ion as they have the same charge. They can however be distinguished because the anti-proton retains its charge, whereas the hydrogen ion will often lose its, by losing one of its electrons. Therefore we need to know the probability for the hydrogen ion to lose an electron in some particular time in order to distinguish between the ion and the anti-proton. Suvorov calls this a 'specific relationship'. Born's response is that that the ionisation probability of a negative hydrogen ion is just another invariant. He writes 'if anyone wants to use the term "specific relationships" to designate the totality of invariant properties associated with a particle, I do not object—except that this a somewhat vague, nebulous term' (Born 1962 32). Presumably Born considers the probability to be an invariant because it is something that all observers, after a number of tests, will come to agree upon.

Born also offers some specific criticisms of materialism. He quotes Suvorov's 'ominous assertion' that 'Objective laws exist in society which are specific for a given society and independent of human conscience' (Born 1962 33), linking it to historical materialism. Historical materialism, Born thinks is 'a descendent of the physical determinism derived from Newtonian mechanics' (Born 1962 34). In a deterministic system we ought to be able

to predict precisely what will occur in the future. Born thinks that the materialists apply this principle to history and society. Born, however, does not think that the assumptions of measurements of perfect precision that are required for determinism were viable even in classical physics (there is more detailed discussion of this argument in the Chapter 5—Born on Determinism), let alone in quantum mechanics. So Born thinks that historical materialism is derived from a false premise—that of physical determinism—and is hence defunct.

So we can see that 'The Concept of Reality in Physics' (Born 1962) does not add a huge amount to the arguments given in 'Physical Reality' (Born 1953), but we can glean a few things. The first is that, rather unsurprisingly, Born rejects (Marxist) materialism as a philosophy of science. This is certainly consistent with him being a scientific realist, but it doesn't confirm him as one either: we should consider that rejection of materialism is not confined to scientific realists. Also of note is that Born seems to consider the decay probability of a hydrogen ion as an invariant quantity, which implies an objective (as opposed to epistemic) view of at least some probabilities in physics. Born's position on probabilities in physics is discussed in detail in Chapter Six—Born on Probability.

3   What Is It to Be A Realist?

In this section I wish to offer definitions of the positions available in the realism debate so that Born's writings can be compared with them in the hope of finding where his allegiance lies. It must again be stressed that Born's position may not fit precisely (or at all) with the positions that modern philosophy of science allows. This survey is for the purposes of illumination and elucidation. It is not intended to provide a number of differently shaped holes, through one of which Born must be shoved, however inelegantly.

Crudely put, scientific realism is the thesis that our current best scientific theories are at least approximately true. It claims that our theories really do explain the nature of the world and that they really do describe those parts of it that are beyond our ability to perceive directly.

In *The Scientific Image* Van Fraassen (1980) offers what he considers to be minimal and general definition of realism: 'Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true' (1980 8). This, he thinks, should be acceptable to realists of all stripes. Anjan Chakravartty (2009) also offers a minimal definition in *A Metaphysics for Scientific Realism*: commitment to a theory also involves commitment to the content of that theory— the entities, processes and interactions describe by it. There is no universal agreement by realists to anything more than this.

In *Scientific Realism: How Science Tracks Truth*, Stathis Psillos (1999) offers a meatier definition. Realism, he thinks, involves commitments to three theses, one metaphysical, one semantic and one epistemic. The metaphysical thesis is this: The world has a mind-independent natural kind structure. This is required because in order for realism to be true, the world must be a certain way. In particular the entities posited by scientific theories must exist independently of our minds. We cannot even get the epistemic commitments of realism off the ground if this is not the case.

The semantic thesis regards the statements of scientific theories as having truth values; they are either true or false. If a theory is true, then the entities (or whatever) posited in it exist. It is not the case that all a scientific theory *really* refers to is direct observation, i.e. they are intended to refer to the mind-independent world of the metaphysical thesis. Nor is it the case that one need not take there to be an objective reality behind such theories. If scientific theories refer at all, then they refer to reality.

The epistemic thesis is that we are entitled to take the theoretical statements of mature scientific theories as being true (as opposed to false). If our best scientific theory says that there are electrons then we are, ipso facto, justified in believing in electrons. One could still hold the metaphysical and semantic theses but not be a realist because one does not think that we have the epistemic warrant to accept one.

Richard N. Boyd offers a fourfold definition of realism in his paper *On the Current Status of the Issue of Scientific Realism* (1983). His definition includes the metaphysical, semantic and epistemic theses mentioned above and adds the claims that (i) the progress of science is due to theories becoming better approximations of the truth and (ii) that newer science is in some sense continuous with older science. This is needed in order to assuage worries about theory change and the pessimistic meta-induction.

We may also wish to make a distinction between theories about the status of science and the aims of science. Van Fraassen (1980) famously argues that we can successfully construe the aim of science to be empirical adequacy rather than truth. Although it is unlikely that the realist will simply want to stop at claiming the aim of science to be truth, it does seem that this is going to be a condition.

3.1 Is Born A Realist?

So we have four (maybe five) theses which are central to scientific realism

1. There exists a mind-independent world.

2. Semantic reaism: The teoretical terms of scientific theories are intended to refer to this mind-independent world (as opposed to merely referring to sense-data or to readings on meters or suchlike).

3. The progress of science is due to the increasing verisimilitude of scientific theories. We might put it another way: scientific theories gain in explanatory power and empirical accuracy because the descriptions that they provide of a mind-independent reality more accurately describe that mind-independent reality than previous (less empirically accurate) theories.

4. We have the epistemic capacity to confirm our scientific theories and furthermore least some of those scientific theories have in been fact confirmed.

5. The aim of science is truth rather than empirical adequacy.

Not all realists accept all of these things whole-heartedly. In particular there is disagreement over which parts of theories we are entitled to regard as confirmed and about which parts of the world (for example, structures or entities) we can take science as having continuity of reference. There will also be disagreement on how it is that we can condirm our theories and what sort of threshold of success supplies this. But all realists must agree that some manner of confirmation is achievable and has (in some circumstances) been achieved. For this reason, we would not need to see Born having a whole-hearted realist commitment to all the statements of a scientific theory in order for us to consider him a realist of some stripe—an indication that he thinks that we are entitled to believe in parts of our theories and that we have some principled reason for doing so would be sufficient.

3.1.1 The Metaphysical Commitment

I'm now going to look at each of these commitments in turn, with reference to the parts of Born's work (and a few other relevant points). First, we need to ask whether or not Born has the appropriate metaphysical commitment. I think that we can argue here that his

invariant realism does commit him to a mind-independent world. Born doesn't use those exact words, but remember what the goal is—to explain subjective experiences in terms of objective elements of the world and to oppose Dingle's phenomenalism. Born clearly recognises the problem at hand—that we could not be committed to the truth of our theories if we do not think that there is at least some objective component of the world. Invariant realism seems to be a clear commitment to the claim that there are at least some aspects of the world that are mind-independent in that there exist properties that all observers, perhaps after making multiple observations, will come to agree upon. We get a very clear statement of this in Born's paper *Physics and Metaphysics* (Born 1950), one of his various works in which he takes issue with Einstein's philosophy of science. He writes of the view of quantum mechanics in which 'there is no objectively existing world, no sharp distinction between subject and object', saying of it 'There is of course some truth to it, but I do not consider this position to be very fortunate' (Born 1950 105). It's not very fortunate, Born thinks, because we should really understand external world talk (which is at best 'pre-scientific' in his view) as talk about invariants of observation - the gestalt psychology that I've mentioned earlier. Physics is able to identify such invariants on 'a different level of perception' via measuring equipment. Again, we can see clearly a commitment to invariant features as mind-independent.

3.1.2 The Semantic Commitment

We can also make a case for Born's invariant realism adhering to the semantic commitment. Invariant realism makes this commitment because it states that in talking about quantities like charge, mass or spin we are talking about real aspects of the world behind the phenomena and not just, for example, where the needle on the meter of our measuring equipment lies. I also think that Born's commitment to the reasonableness of particle and wave talk discussed earlier indicates a semantic realism about them. Born thinks that it is still perfectly meaningful to talk about electrons being particles in modern physics despite their odd properties in quantum mechanics. In *Physics and Metaphysics* he

writes, 'I think the fact that various observations of electrons give us always the same charge, rest-mass and spin, justifies perfectly speaking of them as real particles' (Born 1950 105). This seems to be a clear commitment to the claim that electron talk refers to objects in the real world. He goes on to criticise the conventionalism espoused by Poincaré (in his youth) and Einstein. Born understands this doctrine as saying that 'all human concepts are free inventions of the mind and conventions between different minds, justifiable only by their usefulness in ordinary experience' (1950 105). This he thinks might be appropriate for the more abstract components of theories but is the wrong way to regard the measurable parts of a theory like this. He supplies the proposal of Schrödinger's to replace the concept of an electron with a 'diffuse cloud of electricity' saying that 'It was soon abandoned since electrons could not be counted. The corpuscular character of the electron is certainly not a convention' (Born 1950 106).

So we can see here that Born clearly has a commitment to reality that goes beyond experience. This commitment is to invariants, but we should be clear that this is not just a commitment to invariant mathematical structures but also to quantities such as charge, rest mass, etc. Furthermore it also appears to contain a commitment to the entities (fundamental particles) that possess such quantities.

3.1.3 The Epistemic Commitment

Can we find a commitment to the concept that we have an epistemic warrant to regard at least some of the content of our scientific theories as true, or that we can know things about an objective, mind-independent world, in Born's work? Something that does indicate that he has such a position is the 'denying the distinction' (see Maxwell 1962) style argument that he gives in *Physical Reality* (Born 1953). We can see in this the epistemic commitment because it commits Born to the claim that observations of the world of electrons via complex measuring equipment is not a fundamentally different activity to observation of the everyday world with our senses, i.e. that there is not an epistemically

significant distinction between the observable and the unobservable. We can also see it in two elements of Born's invariant realism. The first is his claim (for example in Born 1950 105) that observation of the everyday world rests on identification of invariants. The second is in realism about invariant quantities full stop. Born thinks that it is in virtue of our capacity to identify such quantities as invariant under transformation that we are entitled to be realist about them. Thus, we have the capacity to gain knowledge about the external world via identifying and measuring invariant quantities.

Born also employs something very much like an inference to the best explanation or no miracles argument in *Symbol and Reality* (Born 1966). Recall that he writes that the 'coincidence of structures revealed by using different sense organs and communicable from one individual to the other is accidental' is extremely improbable and that we are thus entitled to consider it essentially wrong.

Now it does seem to be the case that Born does not have an epistemic commitment to all aspects of a theory. His commitment is merely to the *invariant* components of such a theory. Hence, Born's realism is of the selective variety, although it does not appear to be either entity or structural realism. There are structural elements to invariant realism (more on this later in this chapter), but Born seems to be primarily committed to invariant *quantities*.

3.1.4 Commitment to Progress/Continuity

It's a little less clear that Born is committed to the increasing verisimilitude of science and to the continuity of its current theories with past ones. I think, though, that we get at least a hint of this in *Natural Philosophy of Cause and Chance* (Born 1949) when Born discusses the possibility of physics returning to a deterministic world view at some time in the future. Here he writes:

> It would be silly and arrogant to deny any possibility of a return to determinism. For no physical theory is final; new experiences may force us to alterations and even reversions. Yet scanning the history of physics in the way that we have done we see fluctuations and vacillations, but hardly a reversion to more primitive concepts. (Born 1949 109)

Now it's true that this is not quite equivalent to a commitment to progress and continuity but I think we might find an implication of it here: Born understands that theories can and do change—no theory is immune to it. Despite this, when theories change they do not revert to 'more primitive concepts'. By 'primitive', Born could just mean 'earlier in time' but he could also mean 'less sophisticated' or 'less comprehensive'. We can find reason to take the latter interpretation in Born's discussion of mass and length in relativity—we find that what we once thought were objective quantities to be mere projections. This I think is at least an indication that Born considers theory-change in physics to be progressive in nature.

I do think that it is fairly clear that Born thinks that physics progresses, rather than merely changes, over time though. This seems implicit in the structure of *Natural Philosophy of Cause and Change*, which tells a story of increasingly sophisticated physical theories increasingly coming to respect Born's principles of contiguity and antecedence as they grow in sophistication (there are details on this in Chapter 3—Born on Causation). We can also see Born's views on this in *Physics in the Last Fifty Years* (Born 1951), a published version of a talk that Born gave to section A of the British Association meeting at Edinburgh in August 1951 (incidentally, this is the meeting at which Dingle delivered the talk that Born responds to in *Physical Reality*). Here, Born gives his own account of the changes in physics from 1901-1951, a period throughout which he had been active as a scholar. The whole thing gives an impression of revolution and progress, but I think this comes across most clearly when Born discusses advances in atomic physics in the early

20th century. He writes 'In fact, atomic research had reached here a point where progress was not possible without a radical change in our fundamental conceptions' (Born 1951 116). The meaning here seems clear—that revolution in physics lead to progress in our understanding.

What about continuity of ideas/concepts? Born certainly understands the problem. He writes in *The Concept of Reality in Physics* with regards to realism about invariants that 'One complication arises in physics, and that is that the magnitudes which appear as invariants in one period of knowledge, and are therefore considered there as representations of "reality", lose their standing in other periods' (Born 1962 28). In the particular case of relativity, where we find that quantities like length and mass are observer dependent. Born writes that 'Distances, time intervals, and masses as defined before are no longer invariants but projections. In their stead are now other invariants called "rest length", "proper time", and "rest mass". If these are used one again obtains symbols which are suitable for describing material realities' (Born 1962 29). Born's solution here seems to be that although we've discovered that some quantities are not in fact invariants, we've also found they are projections of invariants which corresponds to the old properties. So although we find that mass is not an invariant, we do find a new corresponding invariant property, rest mass, of which the old concept of mass is a projection. In this way we still get some continuity across different physical theories.

Born gives a slightly different response to this problem in terms of quantum mechanics, which comes through his ideas about the widening of concepts. Recall his argument that we are entitled to call electrons in quantum mechanics particles (Born 1953 136, 1962 24-5). He thinks that although a quantum-mechanical particle like an electron does not share all of the same characteristics as a classical particle, we are still entitled to refer to both as 'particles'. This is because they both still share enough properties—for Born it is that both are possessed of invariant properties like charge and mass—to say that what we have done is to widen the concept. Such widening of concepts is something that Born thinks is

157

standard practice in mathematics and physics. So I think this again indicates that Born thinks that there is continuity in the history of physics—that 'electron' refers to the same thing in both classical and quantum physics. In this sense I think we can be justified as seeing Born as at least in some cases being sympathetic to continuity of reference for theoretical terms such as mass even though the meaning of the term has changes. These particular cases are where we can justify 'extending the concept' talk. One thing in particular that I think does point to this is Born's (earlier mentioned) claim that it might have been better to have kept the term "mass" referring to the invariant quantity (which we call "rest mass"). In this sense, what is understood by 'mass' still has continuity of reference to the invariant quantity.

3.2 Hold On a Minute…

So I've given an argument that Born fulfils all four criteria needed to commit him to scientific realism but he does say certain things that don't realist. Born repeatedly praises a heuristic technique, attributed by him to Einstein and Heisenberg, that says that we should remove things that are not observable from our physical theories and it's worth saying why this doesn't indicate that he is an anti-realist. He says in *Natural Philosophy of Cause and Chance:*

> Heisenberg justified the rejection of traditional concepts by a general methodological principle: a satisfactory theory should use no quantities which do not correspond to anything observable. The classical frequencies mv(n) and the whole idea of orbits have this doubtful character...Now quantum mechanics itself is not free from unobservable quantities. (The wave-function of Schrödinger, for instance is not observable, only the square of its modulus.) To rid a theory of all traces of such redundant concepts would lead to unbearable clumsiness. I think

though there is much to be said for cleaning a theory in the way recommended by Heisenberg, the success depends entirely on scientific experience, intuition, and tact (Born 1949a 88-89).

He espouses a similar position in *Is Classical Mechanics in Fact Deterministic?* (discussed in detail in Chapter 5—Born on Determinism):

Modern physics has achieved its greatest successes by applying a principle of methodology, that concepts whose application requires distinctions that cannot in principle be observed, are meaningless and should be eliminated. The most striking examples of this are EINSTEIN's foundation of the special and general theories of relativity (of which the first rejects the concept of absolute simultaneity, and the second the distinction between gravity and acceleration as unobservable) and HEISENBERG's foundation of quantum mechanics (by eliminating the unobservable orbital radii and frequencies from Bohr's theory of the atom) (Born 1955 167).

On the face of it, this does sound like an operationalist principle and it is clear that Born agrees with its application in physics. But remember that Born believes in molecules because we can see their tracks in a cloud chamber. He believes in invariant quantities that stand behind the appearances which are their projections, and he thinks that it is perfectly acceptable to talk about particles because they possess such invariant quantities. None of these things can be directly observed. Remember too that Born explicitly does not think that there is a meaningful distinction between 'observable with the senses' and 'observable with equipment'. What Born means (and indeed what he says in the second quotation) is that we should not treat as real things that are unobservable *even in principle*. There is a world of difference between arguing that we should not be realists about things not directly observable and arguing that we should be sceptical about things that are not observable or

measurable even in principle. The first position is indeed anti-realist, but the second is merely a species of weak empiricism or weak operationalism.

Redhead (1980) notes that we can regard some theories as having 'surplus' structure in their mathematical formulations, in that they have components which are uninterpreted in the sense that they play no part in the inferences by which the theory can be tested. We might also think of van Fraassen's discussion of absolute space in Newtonian mechanics (1980): absolute space is to all intents and purposes empirically irrelevant to the theory, in that any particular value that absolute coordinates take is as equally empirically adequate as any other value. Van Fraassen is of course not a realist, but with regards to realism we can say the following: if we have an argument that some particular aspect of a theory cannot be confirmed (or indeed falsified) by any test whatsoever, then it is by no means unreasonable to argue that this aspect should not be the subject of realist commitment. This position would be a selective realism, but we have already seen that Born subscribes to such a position in that he is only committed to invariants. For Born to reject the reality of in principle unobservable components would be entirely consistent with his position on invariants and, frankly, is quite a plausible position for any realist to take full stop.

Still, there is at least one philosopher who thinks otherwise. Galavotti in *Operationism, Probability and Quantum Mechanics* (1995) defends Born against a charge of subjectivism about probability (more on this in Chapter 6—Born on Probability). Part of her defence is based on Born and Heisenberg's rejection of positivism based on crude sense data. She cautions us, though, against taking this to mean that Born is a realist. She writes 'Here the word "realism" should not lead us astray, for Born seems to be as much of an anti-realist as Heisenberg: "...scientific forecasts—Born says—do not refer to 'reality' but to our knowledge of reality"' (Galavotti 1995 107). How can we, if at all, reconcile such a statement with the realism that I've argued Born espouses? First, I'm going to look at the context of the quote Galavotti uses. It's from the paper *In Memory of Einstein*, first published in 1965 and reprinted in a later edition of *Physics in My Generation* (Born

1969). In the section that Galavotti quotes from, Born discusses his disagreements with Einstein over quantum mechanics (again, there is more detail on some of this in Chapter 6 —Born on Probability). He thinks that the root of these disagreements was in fact 'a fundamental difference in our view of nature' (Born 1965 163). Born gives his view as follows:

> What it boils down to is that scientific forecasts do not refer directly to 'reality' but to our knowledge of reality. This means that the so-called 'laws of nature' allow us to draw conclusions from our limited, approximate knowledge at the moment on a future situation which, of course, can also only be approximately described. This is a way of thinking diametrically opposed to Einstein's own, and it is not surprising that he looked upon me as a renegade. Yet I have the feeling that I pursued the path which he showed us in his great days, while he himself stopped at a certain point. The point is the idea that the outer world as it really is, is faithfully and exactly described by science. Seen from this angle, today's theory of matter is indeed a jumble of absurdities, and EINSTEIN from his own point of view was quite right to reject it or, at most, to accept it as only provisional. (Born 1965 163-4)

This certainly does sound an awful lot like an anti-realist position. I think that we are faced with three options here—1) Born *wa*s a scientific realist but by the time he came to write the above in 1965, he had changed his position; 2) Born was always an anti-realist and although much of his work appears to express a position aligned with scientific realism, to think that it does is to misinterpret it; 3) Born was always a scientific realist and although the above appears to express a kind of scientific anti-realism, to think that it does is to misinterpret it; 4) Born does not have a considered, consistent position– he has conflicting intuitions with regards to the matter and has not reconciled them. I think that 1) is certainly possible, but we can make a decent case for 3). Both options 2) and 3) require extra work

to establish what Born really means in the above passage if he is not an anti-realist, and I see little evidence for 2). A decent case for 3) ought to also function as an argument against 4)

How could this statement be compatible with a realist position? One thing to remember is that Born is not a realist about *everything* in a physical theory, merely about invariants. Another is that Born is committed to a statistical explanation of physics. Something else is that although Born regularly praises Heisenberg's position that things that are in principle unmeasurable are not meaningful, he is also committed to realism about indirectly measurable entities like electrons. This is indicative of a weak empiricism, not a strong one —Born means that we ought not to take as real elements of a theory that are *in principle* unmeasurable, not merely unmeasurable in any direct manner. Despite not taking such elements as real, Born does not think that we should purge them from a theory (Born 1949 89), so he clearly does not think they are semantically meaningless. He writes that 'to rid a theory of all traces of such redundant concepts would lead to unbearable clumsiness. I think, though there is much to be said for cleaning a theory in the way recommended by Heisenberg, the success depends entirely on scientific experience, intuition, and tact' (Born 1949 89). So there is clearly some useful role that such concepts can play in a theory, even though we regard them as redundant. I suspect that what Born means is this: there is no sense in ridding a theory entirely of such concepts because they can be useful when we talk about the theory—hence why throwing them all out can lead to clumsiness. Still, such concepts are 'redundant' and so we should not take them as real.

A further note here is that Born takes Einstein to have used this principle in unifying gravitation and acceleration (Born 1955 167). This, I suspect, is what he refers to when he talks about himself 'pursuing' the path that Einstein showed in his early days. The point of all this is that when Born tells us that he does not agree with the view that 'the outer world as it really is, is faithfully and exactly described by science', we can still understand him as being a realist because his realism is a selective one: he thinks that theories contain

elements that we are not justified in being realists about, as well as those that we are. Modern physics contains things like superpositions of the wavefunction (which I'll argue Born doesn't take as being real in Chapter 6—Born on Probability) which are in principle unobservable, as well as various invariant quantities which are.

I think we can say a few things about Born's statement that scientific forecasts refer only to our 'knowledge of reality'. The first is that, as we've already seen, Born does not take any scientific theory as being final. The second is that Born is committed to the uncertainty principle and the fundamental indeterminism of quantum mechanics (1949 104-5). This means that we only ever have incomplete information about some particular state and it means that we can only make predictions within the limits of that information. For Born we are limited in what we take as being real by what we can determine to be invariants of observation and transformation. We also necessarily operate under epistemic restrictions—we cannot know everything due to (at least) the restrictions on measurement imposed by the uncertainty principle. I suspect that this is what Born means by 'knowledge of reality' rather than 'reality'. We might also say that the reference to 'knowledge' refers to the epistemic component of realism, i.e. that scientific forecasts tell us *what we can know* about reality.

So what Born is saying here is this: when he was corresponding with Einstein and arguing about quantum mechanics, Einstein took Born's defence of it to be a defence of the whole theory, laws, superpositions and all. But Born doesn't interpret the whole theory realistically—only the observably invariant components of it. That is what the confusion was about. So we can still say that Born is a realist, it's just that it is clear that he is only a realist about invariants and the entities and structures that possess them.

We might also worry that Born does not apply this principle in a consistent manner – after all a wavefunction does not seem to sit in the same category as electron orbits. A response

to this point is discussed in more detail in Chapter 6 – Born on Probability (section 4.2.2) because the bulk of the discussion regarding Born's anti-realism regarding wavefunctions is there. The conclusion of the argument there is there is a way of reading Born consistently, but even if we do take him to be misapplying the principle in this instance, it does not seem to threaten his realism.

3.2.1 A Lack of Identity

Born acknowledges that electrons are not individuable. Ought we to worry that this is in conflict with the argument that he is a realist? We can cash out this worry in three ways: 1) Is there a general conflict between realism about electrons and the fact that they are not individuable? 2) Is there a specific conflict between Born's invariant realism and realism about particles that are not individuable? 3) Does Born think that things that are not individuable are not real, or at least things about which we ought not to be realists?

3) Might come about in the following way: Something about the inability to individuate an electron might lead to an inability to identify invariant quantities born by it.W.V.O. Quine's famous slogan holds 'no entity without identity'. This rather pithily sums up the position that our ontology should not contain entities for which there are no clear identity criteria (SEP Quine). Quine's target here is primarily abstract objects, but given that particles in quantum mechanics also plausibly lack clear identity criteria might we also have worries about them? We might also think to Leibniz's identity of indiscernables – that no two (or more) entities share all of the same properties.

It is important to consider exactly what is implied by the claim that electrons are not individuable. Lowe (1994) describes the standard example as follows: If some electron *a* is captured by an atom and at some time that atom releases some electron *b*, then there is no

objective fact of the matter as to whether or not *a=b*. What is not implied by quantum mechanics is that electrons are not self identical. There does exist a fact of the matter that some determinate number of electrons persists in the system – the state with one electron is measurably distinct from the state with two electrons – and an electron is always identical with itself (Lowe 1994). Lowe further notes that he considers this - determinate self-identity – to be the 'hallmark' of entityhood. Lowe further argues that there is no contradiction in regarding electrons as (sometimes) vague objects, i.e. that during entanglement the fact that quantum mechanics implies that there is no fact of the matter as to whether or not some *a=b* is not in and of itself a contradiction of the idea that they are individuals.

This is not the be-all-and-end-all of the matter: French and Krause (1995) note that we might find Lowe's position to be unsatisfactory. There is no mere epistemic problem of telling whether or not some electron is *a* or *b*. Quantum mechanics tells us that we cannot in principle do this. They write 'two (or more) bosons or two (or more) fermions in the appropriate entangled state have the same monadic properties and the same relational properties to one another' (French and Krause 1995 20)

So if we are to account for (particles like) electrons as individuals whilst in an entangled state despite their being indistinguishable, then there must be some ontology of relations to allow this. French and Krause note that there does exist such a relation, but accepting it comes with a heavy price These relations will have to be 'strongly non-supervenient'. They are non-supervenient because they cannot be dependent on the monadic properties of the electrons (or any fermion or boson) involved. Now, when we apply this to Lowe's position, we find that electrons are individuals but it is indeterminate whether or not some particular electron of an entangled pair *a* and *b* is *a* or *b*. Therefore the indeterminacy of indeinity of the electrons must arise because of these relations. French and Krause write 'One view might be to say that, given particles are individuals, their identity is perfectly determinate, only, because of the existence of non-supervenient relations, we cannot tell whether electron *a* is identical to *b* or not' (1995 22). Further to this (French and Krause

1995 22) we cannot in principle tell whether or not this is the case. Quantum mechanics does not allow us to get inside an entangled state and find out what is 'really' going on, This, French and Krause note is the 'price' of Lowe's position (1995 22), and it is a fairly heavy one. The alternative, which they take to be more palatable given the ontological cost of Lowe's position, is to argue that electrons (and particles like them) are in fact not individuals at all.

We might also note again here that Lowe regards self-identity as the hallmark of entityhood and takes electrons to uncontroversially possess it. French (2015) argues that Born, amongst others, denies the self-identity of particles in quantum mechanics. He writes: 'Alternatively, but relatedly, non-individuality can be understood in terms of the denial of self-identity. This suggestion can be found most prominently in the philosophical reflections of Born, Schrödinger, Hesse and Post (Born 1943; Schrödinger 1952; Hesse 1963; Post 1963). It is immediately and clearly problematic, however: how can we have objects that are not identical to themselves?' (French 2015). So what exactly does Born say about the individuality of quantum particles. He writes:

> *If now these photons are treated as genuine particles, having an individuality of their own, Planck's law would not be obtained. One has instead to assume that two states which differ only by the exchange of two photons are physically indistinguishable and have statistically to be counted only as one state. In other words, photons have no individuality.* (Born 1943 27-28)

He writes similarly of electrons:

> *States in which two electrons would have the same set of quantum numbers (those of the spin included) do not exist; and, moreover, if two sets of quantum numbers differ only by exchanging those of one electron with those of another*

they represent only one and the same state of the whole atom. Here again we recognise the lack of individuality of electrons. (Born 1943)

So we can see here how, if we take Born's statement about photons not being 'genuine particles, having an individuality of their own' to also apply to fermions, which does not seem unreasonable given how how he phrases the exclusion principle in the above quotation, we might regard him as taking quantum particles in general to lack self-identity. We might also take this to express an antirealism about quantum particles - things that lack an individuality of their own ought not to be considered genuine particles, and thus we ought not to realist about them. Equally, it might also be taken to merely express a view that bosons and fermions ought not to be counted as particles and no more than that – they are simply weird objects that do not correspond to the notion of 'particle'. In this way, we need not take what he says to indicate any kind of general antirealism – it's just a statement about what sort of objects fundamental particles are not. It's also not clear from this that Born is thinking in terms of the formal and metaphysical properties of lack of self-identity here.

The question is then what Born's position on the matter is with regards to his invariant realism, if indeed he has a considered one. In *Physical Reality* (1953). He writes of particles :

The main invariants are called charge, mass (or rather: rest-mass), spin, etc; and in every instance, when we are able to determine these quantities, we decide we have to do with a definite particle. I maintain that we are justified in regarding these particles as real in a sense not essentially different from the usual meaning of the word (Born 1953 160).

He expands on this point in *The Concept of Reality in Physics* (1958), saying

...by making complementary experiments with negative or positive rays, we can obtain sets of invariant magnitudes associated with these phenomena: charges, masses, spins, numbers. This is enough to permit us to talk of electrons or ions as particles of a definite kind, using the previously explained principle of the widening of concepts. These particles are no longer like particles of grain or dust. True, under certain conditions of experiment we can directly see the paths of individual particles; many have seen the atomic tracks observed in the cloud chamber, or traces produced in fine-grained photographic emulsions. On the other hand, these particles have no individuality. For statistical purposes, they must be counted in a different way from the way in which ordinary objects are counted. In large numbers they lose their particle character altogether and produce interference phenomena, which make one think of waves. (Born 1958 30-31)

The 'widening of concepts' is the previously discussed position that it's unproblematic to expand concepts to admit newly discovered features of mathematics or nature, even if they don't share all of the same properties. Now, even if we do grant that the position Born takes in *Experiment and Theory in Physics* (1943) is one in which the self-identity of particles in quantum mechanics is denied and that this denial is in conflict with a realism about those particles, it does not seem that the position expressed here (in Born 1953 and Born 1958) is the same. He fairly clearly takes it that there is nothing about a quantum particle's lack of individuality that means that it is *not* a particle or that it is not real.

There are a couple of options here – one is that there is simply an unconsidered tension in his position. Born realises that electrons are not individuable, and may not even possess self-identity, but does not realise that this introduces a metaphysical problem if we want to take electrons to be entities. The other is that he does realise that electrons are not entities in some sense, but whatever they are, they are also collections of invariant properties and can be referred to as particles and considered real. So maybe the object causing the track in

the cloud-chamber lacks individuality in some important sense and so maybe it is not a 'thing' or an entity. He writes in *Physical Reality* 'a thing may be real though very different from other things we know'. But whatever its ontological category is, for Born an electron is that microscopic collection of invariant properties that causes tracks in cloud chambers. There are a number of things which point to Born considering electrons to be 'real', not least that he explicitly says that they are.

Now, we might make a criticism of this point. French and Krause (1995) in their reply to Lowe (1994) if electrons are not self-identical, then it seems hard to to imagine them to be 'bundles' of properties – how exactly can we tie the properties that make up the bundle together if they are supposed to be held by something that lacks self-identity. Again, we might point out that Born doesn't seem to regard this as a problem for his position, so if there is a tension, it's one that he doesn't recognise.

If electrons do lack self-identity, then I think it might well be hard to see how they can be real entities. So this might well point to some unconsidered tension in Born's position. Precisely why Born does not seem to recognise a tension is not really clear. He may well simply be acting under an experimentalists intuition – we can still observe electrons via tracks in cloud chambers, and we can still count them, although as Born mentions we don't do so in the way that we do for classical particles. He might well be unaware of or uninterested any wider metaphysical or formal implications. As mentioned earlier, there is certainly no indication that he (especially when he is explicitly writing on realism in the '50s) is committed to Quinean and Leibnizian principles.

 It's also worth noting briefly that there are potential roads for realists about particles to take even if they deny the self-identity of a quantum particle This might seem odd – how is it that we can even refer to something that does not have self-identity? Marcus (1993 25) suggests that we might make a distinction between objects and things. All objects can be referred to, but not all objects are 'things' that identity applies to. French (2014) takes this as starting point for a 'philosophically respectable' account of non-individuals in quantum

mechanics in which we are still able to count such objects, albeit in an ordinal, rather than cardinal manner (i.e. first, second, third etc.). There is obviously considerable disagreement over whether such an approach should be accepted and indeed no indication that Born endorses this sort of solution. I would not want to argue that he does, but merely to note that if there is a tension in his account, it's not one which will necessarily tear it apart.

It seems fairly clear from this that Born does not sign up to anything like a 'no entity without identity' principle. He thinks that we have good reason to think that electrons are real because via a complementary set of experiments we can determine the magnitudes of the invariant properties that they bear. It is worth remembering that there are several strands to Born's realism - he runs a 'denying the distinction' style argument against the observable/unobservable dichotomy and an inference to the best explanation style argument regarding particle tracks in cloud chambers as well as the argument that we can have objective knowledge of invariant properties. It's fairly clear that objective knowledge of invariants is meant to be objective knowledge of the microscopic world. Born seems to be aware of the issue at hand, noting that such particles have no 'individuality'. Although this means that they are not like the particles of classical physics, he does not take this as a barrier to them being particles or to them being real. Born doesn't think that anything about their lack of individuality leads to us being unable to identify invariant quantities. So again we can see that invariant quantities are the hallmarks of reality, or at least epistemic warrant for belief, for Born.

One other thing that might be going here is a tension of realist intuitions. One reading of invariant realism is that it is a variety of structural realism (a more detailed consideration of this appears in the next section). One obvious way out of worries about the individuality of particles in quantum mechanics is to interpret them as real, but in a purely structural manner, i.e. not as individuals at all. Now Born does seem to want to talk about 'definite

particles' but acknowledges that they are not individuals, at least in large numbers. He certainly wants to say that we can talk about particles as being real. He is also clearly impressed by cloud chamber experiments and might well have an intuition that they tell us something important about the reality of particles. So one other way we might see Born looking at particles than simply as weird objects is that there is an unacknowledged tension between experimentalist/entity realist intuitions and structuralist inclinations.

Now, this might be a tension in his position, but at the very least, it seems to be a tension that he himself does not recognise, or perhaps is unaware of. I think that it's fair to argue that a realist account of science that contains tensions is still a realist account.

4   Symbol and Reality: Is Born a Structural Realist?

It's clear that Born is a semi-realist of some kind. Given that this realism is focussed on invariants we might reasonably ask if he is actually a structural realist of some kind. Ladyman (2016) indicates that Born's invariant realism is at least related to Ontic Structural Realism. Born is quoted, along with Cassirer and Eddington, and Ladyman goes on to say 'The idea then is that we have various representations of some physical structure which may be transformed or translated into one another, and then we have an invariant state under such transformations which represents the objective state of affairs' (Ladyman 2016). In *Remodelling Structural Realism*, French and Ladyman, also write that 'structuralist element creeps in' when Born relates psychological gestalts to invariants (2003 51).

We should take a look at precisely what structural realism entails. Broadly, it comes in two flavours: epistemic structural realism (ESR) and ontic structural realism (OSR). Both agree that we ought to be realists about only the structural components of scientific theories. What they disagree on is how we should regard the non-structural components (Ladyman 2016). ESR claims (broadly) that we cannot *know* anything about them beyond the relations that hold between them, i.e. it acts as a purely epistemic constraint on realism. OSR makes a metaphysical (ontic) commitment, regarding the ontological status of individuals. Ladyman (2016) notes that a crude statement of OSR is the claim that structure is all there is. OSR is a broader church than this. Ladyman (2016) writes 'On the broadest construal OSR is any form of structural realism based on an ontological or metaphysical thesis that inflates the ontological priority of structure and relations'.

It certainly seems that there is a relation between invariant realism and Ontic Structural Realism. Born certainly seems to want to talk about particles in addition to structures, but we could easily read Born's invariant realism as arguing that invariant properties are ontologically prior to the particles that bear them. We could also take particles to really be something like nodes in a quantum field.. Although it's clearly the mathematical structure of an invariant that defines it as such, for Born they seem to be the markers of properties and entities that possess those features. So Born is a realist about invariants because of their structural features

We can still quite happily interpret Born as being fully committed to realism in a structural manner. He is still committed to a mind-independent external world. He still thinks that there is no proper distinction between observable and unobservable and that 'theoretical language' does not simply just translate to directly observable concepts. We still retain continuity of reference across theories via reference to invariant structures. We still have epistemic warrant to believe in the structural elements of theories as telling us truths about the world and beyond the directly observable.

4.1 'Symbol and Reality'

The above is at least true for Born's writings on the subject in the 1940s and 50s, when he is focussed on invariant realism. I think he does lean more towards something that might be a more elimintive variety of OSR (i.e., one that contains only structures). I've separated the discussion of this paper from the others, because I do think that it represents a somewhat different position to that presented in *Physical Reality*. Here, he does talk explicitly about structure as the hallmark of reality, arguing that it is well-defined structures that are the key to finding out what we can know of the world.

Born starts the paper by again describing naïve realism and discussing an experience which induced in him a kind of scepticism. Born had an older cousin studying philosophy who once asked him how he knew that they both saw the same leaf as the same colour. Born says 'Thus it dawned on me that fundamentally everything is subjective, everything without exception. That was a shock' (Born 1966 144). The problem, as Born sees it, is not to distinguish between subjective and objective, but to move from the subjective to the objective. He notes that he has not found any solution to this problem in philosophy, but he thinks that he has found one in physics (1966 144).

Born gives a brief overview of Kant, noting the distinction that he makes between a priori and a posteriori forms of reasoning. Born also notes that the 'thing in itself' (1966 145), that which lies behind the phenomena, is according to Kant not knowable. This is not particularly satisfactory to Born as he takes it that one must accept that there is such a thing as the thing in itself in order to understand how there can be 'objective statements valid for all individuals' (Born 1966 145) and also declare that it itself is not knowable.

Born then briefly mentions Husserl, whose lectures he attended as an undergraduate, noting that his position 'did not satisfy me' (Born 1966 146). Presumably, he is referring to the course on the philosophy of mathematics that he attended, as Born found the other course of Husserl's too dull to see to completion. According to Born, Husserl's mathematics course offered a phenomenological solution to the problem of the epistemological validity of mathematical axioms (Born 1978 96). He then gives a brief account of logical positivism and materialism, charactering them in much the same terms as he did in his earlier discussions of realism.

He then turns to his argument—how we can obtain objective knowledge via the processes of science. Born starts by returning to the familiar theme that most scientists are naïve realists when it comes to experiments. They are, he says 'content to observe a phenomenon, to measure it and describe it in their characteristic slang' (Born 1966 146). This, Born thinks, is no different to any other craft. Things become different, he tells us, when scientists start to develop theories. They use concepts that transcend those of ordinary experience and ultimately develop complex mathematical formalisms to describe their theories (Born 1966 147). Born asks what is happening when physics does this. He answers 'In physics the mathematical formulae are not an end in themselves, as in pure mathematics, but symbols for some kind of reality which lies beyond the level of everyday experience' (Born 1966 147). Born thinks that the question of how to achieve objective knowledge is connected with this answer.

Born proposes to approach the problem by examining certain methods of thinking in physics. He notes that these are not primarily derived from philosophy, rather they have been developed by scientists when 'the traditional thinking of philosophers has failed when applied to modern physics' (Born 1966 147). Nor are they 'empiricist' in that they have been derived exclusively from experience. Rather, they are 'pure ideas, inventions of great thinkers' that have been extensively tested by experience. Further, they have been

enormously successful, not just in contributing to the understanding of natural phenomena, but also in discovering new ones and to 'human domination over nature' (Born 1966 147).

The first of these methods is 'decidability', which he defines as 'Use a concept only if it is decidable whether it can be applied in a special case, or not' (Born 1966 147). Born describes how this principle was applied by Einstein in the development of relativity. As an observer in sealed chamber cannot decide whether she is accelerating or in a gravitational field, we must unify these concepts (Born 1966 148). The concept which we are unable to apply, and thus ought not use, is that acceleration and gravitational force are distinct things: hence we must unify them. Born also argues that decidability was crucial in the development of quantum mechanics. When it became clear that the planetary model of the atom was inadequate, Heisenberg noticed that it involved fundamentally unobservable concepts and developed a new theory using only those concepts which were empirically decidable (Born 1966 148). Born thinks that we ought to apply the principle of decidability to the problem at hand—how to gain objective knowledge of the world.

Born's next section is titled 'Comparability, Symbols' (Born 1966 149). Here, he discusses his solution to the problem discussed at the start of the paper—how is that we can obtain objective data from sense impressions? Born thinks that we can do this by comparing different sense impressions. He notes that it is not possible to determine whether one person sees the same colour another—to conclusively determine whether the content of a single sense impression is shared. He thinks, however that the situation is different with multiple impressions of the same sense. Consider, he says, two different colours—for these 'there exist decidable, communicable, objectively testable statements' (Born 1966 149). He notes that although it is not possible to determine if on viewing two leaves you share all visual experience with another individual, it is perfectly possible to see if you agree on whether or not they share the same hue. Born notes that this principle of 'objectivication [sic]' is practised 'systematically' in physics (1966 149). He also states that this principle is

the root of speaking, writing and of mathematics and proposes to term all such means of communicating objective statements between individuals 'symbols' (1966 149).

The next method is 'Correspondence, Coordination' (Born 1966 150). This is the discovery or development of corresponding pairs of symbols and sense impressions. Born gives some of examples of this: Pythagoras' discovery that the 'natural' intervals in music correspond to the divisions of a vibrating string; measurement of temperature with a thermometer is the correspondence of the sense perception of heat with what Born refers to as a 'geometrical quantity', the height of the mercury or the position of the needle (Born 1966 151).

Born notes that 'In every field of experience this correspondence of sense impressions with symbols has been established' (Born 1966 151). In ordinary life, he thinks, this is done in a fairly simple manner: 'the words and sentences of a language… corresponding to perceptions, emotions etc. are learned and used without being further analysed' (Born 1966 151). He thinks that science goes much further though, and this leads him onto his next method of thinking: structures.

The mathematical techniques ('symbols') used by the physical sciences reveal 'structures', according to Born. He writes that 'mathematics is just the detection and investigation of structures of thinking which lie hidden in the mathematical symbols' (Born 1966 151). Born writes that the integer line consists of symbols which can be combined and operated on by the axioms of arithmetic. From this, he says, we can reveal a 'vast number of structures' such prime numbers and their properties (1966 151). He further notes that geometry and group theory also reveal and deal with structures.

Born doesn't think that mathematical structures are, in and of themselves, real. Physics, however, uses them by correlating mathematical symbols with observed phenomena. Born

writes that 'when this is done hidden structures are coordinated to phenomena' (Born 1966 152).Those structures are taken by the physicist to be 'the objective reality lying behind the subjective phenomena' (1966 152).

As an example of this, Born gives the gradual movement away from explanations in terms of mechanical models and hidden mechanisms (for example, early atomic theory and the ether explanation of EM forces) to explanations in terms the mathematical structures of theories themselves (Born describes Hertz's treatment of the EM field as an 'entity in its own right (Born 1966 152)). The development of quantum mechanics is, Born thinks, a further example of this: it has moved far away from the need for explanation in terms of mechanical models. He tells us that 'Thus physical research has won a freedom necessary to handle the ever increasing amount of observations and measurements, We try to find the mathematics appropriate to a domain of experience, then we investigate its structure and regard it as representing physical reality, whether it conforms to accustomed things or not' (Born 1966 153).

The last of Born's methods is probability. It's less clear how Born thinks that this lets us arrive at objective knowledge of the world but it seems to be that the death of determinism as physical principle has freed science from a commitment to 'absolute certainty, absolute precision, final truth etc.' (Born 1966 153). These are merely 'phantoms which should be excluded from science'.

So how does this all come together? Born notes that this question of reality, whilst avoidable in everyday life, cannot be 'eluded' easily in science. When we observe with the aid of instrumentation, those observations must be interpreted—they are 'incomprehensible without theory' (Born 1966 154). Those observations are of phenomena which do not correspond to our everyday experience. Born tells us that they 'can only be described with the help of abstract concepts' (1966 154).

He notes that he does not think that we can give a categorical answer to the problem at hand, but we can 'make use of the freedom to consider an extremely improbable statement as wrong' (Born 1966 154). Born argues that the idea that the 'coincidence of structures revealed by using different sense organs and communicable from one individual to the other is accidental' is just such an extremely improbable statement. This, I think, is a version of a no miracles argument - Born is arguing that it would so fantastically improbable that idea the agreement on the structures of science by many scientists using many methods could be accidental is so fantastically improbable that we ought to consider it wrong.

These structures, Born thinks, can be identified with Kant's thing in itself. Born tells us that they are 'pure form, void of all sensual qualities' (1966 155). By this, I think Born means that they are entirely objective, having been stripped of their subjective phenomenal qualities. He notes that of course this does not fit with the 'traditional' definition of the thing in itself (Born 1966 155). Although he agrees that they are perfectly abstract, he does not think, contra Hegel (whom he quotes here) that they are empty, 'from a world beyond' (Born 1966 155). The objects of atomic physics might be perfect abstracta, but they are not from a world beyond, Born thinks, because of the practical use that we can gain from them in their application (1966 155).

Born concludes by arguing that we ought not to move to a completely abstract system of science. We still need to describe our experiments in ordinary language. Indeed, Born writes 'physicists are bound to describe the content of their abstract formulae as far as possible in terms of ordinary language with concepts based on intuition' (Born 1966 156). We should remember, Born tells us, that experience is based on our senses—we cannot and should not abstract away from them entirely. Born writes 'A theoretician who, immersed in his formulae, forgets the phenomena which he wants to explain is no real scientist, physicist or chemist, and if he is estranged by his bocks [sic] from the beauty and variety

of nature I would call him a poor fool' (Born 1966 156). We ought, Born thinks, to preserve a 'sensible' balance between experiment and theory.

We can see that this is a development of Born's position, but it is clearly not the same as the one described in Physical Reality (1955) and Natural Philosophy of Cause and Chance (1949a). He is much less interested in causation, arguing that the concept should be replaced by that of 'coordination'. Invariants appear only as part of the general group of mathematical structures that physics uses to describe reality (Born 1966 152). He does not really talk about the reality of particles in the way he does earlier. It's clearly still a realist position—there exists an objective external world, we refer to that world using mathematical structures and something like a no-miracles argument is employed to argue that we can know about that world—but it seems to be much more structural in emphasis than Born's earlier work.

When he writes that the physicist regards the mathematical structures that are coordinated to the phenomena as the 'objective reality lying behind the subjective phenomena', and when he says that he is not afraid of identifying 'well-defined structures with Kant's 'thing in itself'' (Born 1966 155), it seems clear that he is giving us a kind of realism about structures . The problem at hand is whether or not this he is making epistemic or ontic claims here.

 Given that the task he sets himself is explicitly how to move from subjective sense impressions to knowledge of an objective world outside them, we might suspect that his motivation is epistemic rather than ontological, i.e. he is more interested in what we know than what there is. Perhapshis position is that the only way to objectively coordinate subjective phenomena with explanations is via those explanations having a well-defined mathematical structure. But might he still hold a position that is aligned with OSR? I think that he might. As I have noted earlier, he does not discuss particles in the same realist terms that he does in *Physical Reality* (Born 1953). In fact in *Symbol and Reality* he seems

to take care to avoid doing so. He writes 'the trace of droplets in a Wilson expansion chamber suggests a particle in flight...One cannot give up such interpretations without paralysing intuition which is the source of research and rendering communication between scientists more difficult' (Born 1966 156). This does not seem at all like a resounding defence of the reality of particles. It sounds much more like a defence of using particle talk for primarily pragmatic reasons—it makes thinking about the theory intuitively easier and it aids communication between experimentalists. Given the fairly clear commitment to realism about structure in this paper, I think that we can read *Symbol and Reality* as espousing a version of structural realism that is at least epistemic in nature. It is also clearly compatible with the version of OSR that maintains that 'individual objects are constructs' (Ladyman 2016). In this, objects are heuristic devices only. We might also note that we could at least plausibly read this as a wholesale elimination of of particles as something we count as 'real'.

Born's above mentioned claim that the physicist regards the mathematical structures that are coordinated to the phenomena as the 'objective reality lying behind the subjective phenomena', and when he says that he is not afraid of identifying 'well-defined structures with Kant's 'thing in itself'' (Born 1966 155) *is* very plausibly an ontic rather than merely epistemic claim.

On the other hand, if Born is an ontic structural realist, then might this conflict with some of his other writings? We have seen that Born is generally cautious about reading metaphysics off physics. This is apparent in the criticism that he gives in *Is Classical Mechanics In Fact Deterministic?* (Born 1955) of the idea that the success of Newtonian mechanics implies that the world is deterministic. There, he argues that the fact that the dynamics of Newtonian physics are deterministic is not sufficient reason to conclude that

the world is (there is a detailed examination of his argument in Chapter 5—Born on Determinism). We might also recall his remarks in an appendix to *Natural Philosophy of Cause and Chance* that 'metaphysical systematization means formalization and petrification' (Born 1949a 209). We can also see in his 'expansion of concepts' argument a desire to avoid heavily revisionist readings of the metaphysics of physics—we can still quite intelligibly call particles and waves in quantum mechanics particles and waves even though they do not share all of the properties of their classical analogues. This all might suggest that the strong metaphysical claim of ontic structural realismsis something that is at odds with Born's general approach to the relationship between physics and metaphysics.

Also pertinent to this is Born's argument in *Symbol and Reality* that we should preserve a balance between experiment and theory (1966 157). We can see that Born holds a similar position in his earlier work too. In his short book *Experiment and Theory in Physics* (Born 1943—a slightly expanded version of a talk delivered to the Durham (!) Philosophical Society in May 1943) he argues that it is a mistake (contra Eddington) to think that theory should and does lead experiment. He writes, for example, the theoretical predictions (and eventual experimental discovery) of the positron and the meson are 'not products of pure reason, but the final outcome of a long chain of empirical research' (Born 1943 31). We might take this to indicate something like the view that experiment and theory are separate sources of knowledge—Galison (1988) and Chang (1995) espouse similar positions about the roles of theorising and experimentation in physics.

One simple response here is that by 1966 he has changed his mind, and become more comfortable with drawing such metaphysical conclusions. On the other hand, as we saw in the discussion of the status of Born's principles of causation (See chapter 3), Born isn't really running a fine-grained distinction between empirical and metaphysical. He is happy to consider principles that philosophers would generally take to be metaphysical as empirical because they have inspired successful theories and are consistent with them. If, in *Symbol and Reality*, Born is giving an argument that the success of our theories of

physics is down to their regarding their only structural content as being 'objective reality' (1966 155), then there is a clear and consistent road to an ontic position for him: the success of those theories acts as confirmation of structural realism. He also might simply not be paying attention to the distinction.

Something else that might tell against an OSR interpretation of Born's position is that we have seen how strongly he cleaves to Einstein's and Heisenberg's maxim that elements of physical theories which are in principle unobservable, or are in principle unmeasurably distinguishable from some other concept, ought not to be the subject of realist interpretation. This is all perfectly in line with ESR—we are unable to decide whether or not the components of physical theories that are non-structural are real, and so we are only committed to a realist interpretation of the structural components. The stronger claim of OSR, that only the structural components are real, might be seen by Born as just such an undecidable statement and, as such, not one that we ought to be committed to. However, Born does not give such an argument in *Symbol and Reality* and so we might be cautious about putting one in his mouth. For one thing, it might be a position that he has abandoned by this point in his life. Even if we grant that he still wants to hold fast to this principle, then I think there is another reading consistent with an ontic position: All that the non-redundant components of physical theories tell us about is structure, and so it is meaningless to talk of the existence of anything beyond that. Again, this is consistent with OSR.

So I think that it is fairly clear that Born, in *Symbol and Reality* at least, holds a position that it at least an ESR and is at least compatible with OSR. There are additionally some indications that it is indeed the latter position that he holds.

As for where all of this comes from, if I were to hazard a guess I would point out that, as Ladyman (2006) tells us, invariants and group theory were a major part of the toolbox of early structuralists like Cassirer and Weyl, and Born has read Cassirer on quantum mechanics. He mentions Cassirer's book Determinism and Indeterminism in Modern Physics (1937) in the appendix of Natural Philosophy of Cause and Chance (Born 1949a 208). Born writes that it 'gives an excellent account of the situation, not only in physics but also with regards to the possibility of applications of the new physical ideas to other fields' (1949a 208). The only part of Cassirer's book that Born specifically mentions is unfortunately not about realism, but rather the idea that the indeterminism of quantum mechanics has nothing at all to do with free will, a position of which Born approves. He does mention in passing that Cassirer also sees the importance of multiple complementary perspectives, an idea from which Born might well draw the idea of invariant realism, but doesn't give us anything else to go on. Unfortunately, a detailed examination of Cassirer is outside the scope of this project, although I suspect that there might be something fruitful in it.

5   Conclusion

I've argued in this chapter that Born is a realist about physics. He presents a view in papers in the 1940s and 50s termed 'invariant realism' that argues that we ought to be realists about only invariant properties and the particles that possess them. We might interpret this structurally Furthermore, Born denies that there is an ontologically significant distinction between the observable and the unobservable, and argues that it is not practically possible to work as an experimental physicist whilst being a positivist. Whilst Born does say certain things that might lead us to think that he is an anti-realist, it is in fact the case that he is

merely a weak empiricist who thinks that we shouldn't be realists about the components of our theories that are in principle unobservable, even indirectly so. We ought also to bear in mind when reading him that he does not think that the current theories of physics are final, is committed to the idea that quantum mechanics allows only statistical predictions and is only committed to realism about invariants. He has a later development of this position which more closely resembles a structural realism that is at least epistemic, and may well be ontic. We might also acknowledge that there may be some tension in his view on particles.

Chapter 5—Born on Determinism

1   Introduction

In his 1955 paper *Is Classical Mechanics in Fact Deterministic?* (1955 164-170), Born argues that classical mechanics is not, in fact, deterministic. His argument for this is that the laws of classical mechanics, in combination with the inescapability of experimental error, mean that we are unable to produce precise deterministic predictions of the behaviour of any classical system whatsoever.

The purpose of the paper appears to be to oppose those physicists such as Einstein who still persisted in arguing, contra Born's own view of quantum mechanics, that physics could still be deterministic and do so (according to Born at least) for philosophical reasons regarding the unsatisfactoriness of indeterministic physics. Born wishes to argue firstly that the idea of determinism as the only conceptually satisfactory position arises largely from the success of an apparently deterministic classical mechanics; and secondly that classical mechanics is not in fact committed to determinism. Hence, given the conjunction of these two positions, there is no reasonable conceptual worry regarding the indeterminism of quantum mechanics.

I'm going to give a detailed examination of Born's argument which rests upon the claim that measurements of absolute precision are not possible in classical worlds and hence *any* classical system is in actual fact unstable. I will note that this argument has similarities to those found Stone (1989) and Suppes (1993). Stone (1989) argues that this means that we should drop absolute predictability as a necessary requirement for Laplacean determinism. Suppes (1993) argues that this means that we should be transcendentalists about determinism as we are rendered unable to distinguish between a deterministic but chaotic (i.e., unstable in Born's terms) world and one that is fundamentally stochastic. Born's

argument resembles Suppes'(1993), although he does not engage in any transcendentalism. Born cleaves strongly to his interpretation of Heisenberg's principle that distinctions in principle unmeasurable are not physically meaningful (Heisenberg 1925 261-2). From this principle, he argues that our inability to distinguish between a world that has absolutely precise but unreachable values (for quantities such as position and velocity) and one in which such absolutely precise values do not exist means that we cannot conclude that classical mechanics describes a deterministic world.

2   Born's Argument

2.1 No Return to Classical Determinism

I'm now going to examine Born's argument in detail and try to pull out some details regarding how he understands determinism and probability. Born describes determinism in classical physics thus: 'The laws of classical mechanics, and through them the laws of classical physics as a whole, are so constructed that, if the variables in a closed system are given at some initial point in time, they can be calculated for any other instant—in principle, at least...' (Born 1955 164). One should not here conclude that Born is identifying determinism (as he does in *Natural Philosophy of Cause and Chance* (1949a 8)) with predictability rather than simply with a single possible future or a unique trajectory in phase space. He tells us that modern quantum physics has abandoned such determinism and only deals in statistical descriptions of systems and that most physicists have accepted this due it's correspondence with experiment.

Despite the success of quantum mechanics, there were physicists who did not accept indeterminism—Born lists Planck, Einstein, Schrödinger and De Broglie. Born does not think that they deny the experimental accuracy of quantum mechanics but rather that 'their

rejection is in every case founded on the assertion that the usual interpretation of the quantum formulae is obscure and philosophically unsatisfactory' (Born 1955 165). Why is this interpretation considered philosophically unsatisfactory? Because it is not deterministic.

Born thinks that such a commitment to determinism comes purely from the huge success of the Newtonian program. It does not seem to exist prior to the scientific revolution—he writes that 'the religious tenets of fate and predestination relate not to the processes of Nature, but to Man, and are certainly fundamentally different from the mechanical determinism which we are here to consider' (Born 1955 165). That sort of mechanical determinism, we are told, is 'inconceivable' (Born 1955 65) without the great success of Newtonian celestial mechanics.

2.2   Problems of Scale

Having defined determinism and given arguments that those who reject the indeterministic interpretation of quantum mechanics do so only because of the success of deterministic classical mechanics, Born proceeds to give his argument for classical mechanics not being deterministic after all, or rather against the idea that 'classical mechanics in fact permits prediction in all circumstances' (Born 1955 165).

He starts out by suggesting that some doubts should be sown when we consider that we are attempting to apply a rule, determinism, derived from celestial processes to atomic ones, particularly when we consider the relative timescales involved. He notes that the age of the universe is something like 10^9 orbital periods of the Earth, whereas 'the number of periods in the ground state of the hydrogen atom is of the order of 10^{16} per second'. If we consider each of these situations in units appropriate to their timescales we see that 'the stellar universe is short-lived, and the atomic universe extremely long-lived' - i.e. there are far more units of action in the atomic universe than there are in the stellar one. We might think

here of how we use the concept of 'dog years' to compare the ages of dogs with their longer-lived human owners. In terms of hydrogen atoms and planets, we define year as 'orbital period'. In this sense, the world of atomic physics is much longer-lived than the world of planetary mechanics, i.e. for a hydrogen atom, more 'years' pass in a second than do for a planet over timescale as long as the age of the universe. Born thinks that this should induce some doubt in the universal validity of determinism—we ought not to draw conclusions from the short-lived universe and apply them also to the long-lived one. Born doesn't elaborate on this argument here, so I think it should just be taken at face value—we ought to worry about whether or not we have the epistemic warrant to draw conclusions about an unfamiliar domain of the universe from our experience of the familiar one. In any case, he follows this up with a different and stronger argument.

2.3 Absolute Predicability

Born next considers the state in the kinetic theory of gases. He writes of it 'It is usually asserted in this theory that the result [of some prediction or calculation] is in principle determinate, and that the introduction of statistical considerations is necessitated only by our ignorance of a *large number* of molecules. I have long thought that first part of this assertion to be extremely suspect' (Born 1955 165).

Born asks us to consider a simple a case—a spherical particle, bouncing around between other fixed particles. (He likens the situation to a three-dimensional bagatelle. We might compare it to pinball). In a situation like this any arbitrarily small change in the initial velocity of the moving particle will at some time later result in very large changes in its path as it bounces around—eventually, Born points out, some particle that was hit will be missed and vice versa. It doesn't matter by how much we reduce the initial change in the particle's velocity, the path will eventually be significantly changed (earlier on for larger deviations, later on for smaller ones). If, Born says, we want the system to be deterministic (i.e. allow precise predictions of the path of the moving particle) for all times, we must

disallow *any* variation in the initial velocity of the particle, no matter how small. This, Born thinks is not a physically meaningful idea (a point on which he elaborates later in the article) and hence 'systems of this kind are in in fact indeterminate' (Born 1955 166). What we can say here is this: there exists some level of precision of the specification of the initial conditions of a system below which we are unable to distinguish between a predictable and an unpredictable system.

In the next section of the paper, Born attempts to justify his assertion that such systems are in fact indeterminate, or at least that they should be considered so for scientific purposes. His argument runs as follows: Consider the distinction between dynamically stable and dynamically unstable systems. A system is stable in just the case that some small change $\Delta x_0 \, \Delta v_0$ in the initial coordinates and velocities of the system will result in a small change in the final state of the system. If this is not the case, the system is unstable. If a system is not stable, it presumably will not be deterministic in Born's sense of the word, i.e. being able to fully specify a later state of the system from an earlier one. The problem for determinism as Born sees it is that no system is actually deterministic as long as there is *any* non-zero value for $\Delta x_0 \, \Delta v_0$. He illustrates this with the following example: Consider another simple system—a particle with no forces acting upon it moves frictionlessly along some straight line and is elastically reflected by some wall at the end of the line (at distance *l*). The coordinate at which we find the particle will then always be in the interval $0 < x < l$ for the initial state $x_0 \, v_0$. Because no forces are acting on the particle the velocity will remain constant but the deviation in *x* will increase with time as follows $\Delta x = \Delta x_0 + t\Delta v_0$. Thus the deviation in x will eventually reach any arbitrarily large value. What this means, as Born points out, is that if we want to maintain the determinacy (predictability) of a system there can be no deviation whatsoever in the initial state of the system - $\Delta x_0 \, \Delta v_0$ must be zero.

This means that we must be able to measure the initial conditions of some system with absolute precision in order for it to be deterministic. Born, however, not only thinks that we cannot do this, but to talk of such absolute precision is meaningless. He writes

'Statements like 'A quantity x has a completely definite value' (expressed by a real number and represented by a point in the mathematical continuum) seem to me to have no physical meaning' (Born 1955 167). Why does he think that such statements are meaningless? Because he takes seriously the principle that if some physical condition cannot be in principle observed or observably distinguished from some other condition, then it is meaningless. He gives examples of the successful application of this principle: Einstein's dissolution of the distinction between gravity and acceleration in general relativity, and Heisenberg's elimination of planetary-like orbits from the model of the atom. We should, he thinks, apply this principle to the current problem. When we do we come up with the following: 'a statement like $x = \pi$ cm would have a physical meaning only if one could distinguish between it and $x = \pi_n$ cm for every n, where n is the approximation of π by the first n decimals' (1955 167). Given that there is no upper limit on the value of n, it seems clear that there is no way that such a statement can be meaningful.

This doesn't mean that we need to banish real numbers from physics—we just replace precise values with the probability of some value lying within some interval. Born notes that in classical physics this system was only used for modelling large numbers of particles, but the model just presented shows that it must be used for all classical systems, even those consisting merely of a single particle. The initial probability density for a single particle system, representing something close to precise knowledge of the initial conditions will, we are told, eventually become the microcanonical distribution for that system (i.e. representing a distribution of the particle over all energy-accessible states of that system).

Born then describes the treatment of the same model in quantum mechanics: We start with some initial state for which there is an uncertainty in the initial position Δx_0 described by some wave packet, and an uncertainty in initial velocity Δv_0 given by the uncertainty relation as $\Delta x_0 \Delta v_0 > \hbar/2m$ (where m is the mass of the particle—note Born follows his initial example in giving the initial state in terms of velocity and not momentum). If the mass is large, Δx_0 and Δv_0 are small and the formulae describing the system will be a close approximation of their classical counterparts. As with the classical system, there will

be some time at which the uncertainty will become large enough that the system can only be described statistically and will be described by stationary waves, which Born describes as 'the analogue of the classical canonical distribution' (Born 1955 169).

Born concludes from all this that the primary distinction between classical and quantum mechanics is not that one is deterministic and the other statistical, but 'other features' (1955 169), primarily the quantum mechanical treatment of the probability density as the square of the probability amplitude of a system (i.e. the square of either a vector in a Hilbert space or of the complex number given by the Schrödinger equation)—this gives rise to probability interference making it 'impossible to apply without modification the idea of an "object" to the mass particles [sic] of physics', the ultimate consequences of which, he tells us, are beyond the scope of the present paper.

So, Born argues that classical mechanics is not in fact deterministic, but statistical—the treatment of probability in it will be different from that in quantum mechanics, but both theories are still fundamentally statistical ones. Crucial to Born's argument is acceptance of the principle that one ought to consider distinctions and quantities which are in principle unmeasurable as being meaningless. It would seem that we have to accept this principle as more than a mere heuristic—Born wishes to conclude from it that because we are unable to measure the initial conditions of classical systems (or indeed quantum ones) with absolute precision, it is meaningless to talk of those systems having absolutely precise initial conditions: we are unable to distinguish between arbitrarily small differences between one initial state and another, and thus must consider such a distinction meaningless. We are unable to tell via measurements if a classical system has infinitely precise initial conditions and thus we must take the concept to lack physical meaning.

There is also a brief precursor of this argument in the section of *Natural Philosophy of Cause and Chance* on statistical mechanics. In this he argues that absolute precision of measurement in classical systems cannot be achieved due to Brownian motion. Born writes of experimental accuracy: 'Before Einstein's work [on Brownian motion] it was assumed

that progress in this direction was limited only by experimental technique. Now it because obvious that this was not so' (Born 1949a 64). Why was this the case? Because as we make the needles and indicators of our measuring equipment smaller, they become increasingly affected by Brownian motion. Born also notes that the phenomena of noise in electronic equipment produces a similar limit of accuracy.

2.4   The Structure of the Argument

So does Born's argument here work? Let's examine its structure :

P1 Einstein, De Broglie, etc. oppose QM because they do not want to accept a theory that is not deterministic.

P2 The only reason to prefer a deterministic over an indeterministic theory (given that QM is experimentally successful) is because classical mechanics was extremely successful and deterministic.

But:

P3 Classical Mechanics is not deterministic.

[Because: **P1** A theory is deterministic IFF precise predictions of the state of some system can be made for any point in time, i.e. that the initial state of the system determines all other states of the system.

P2 For any classical system, any non-zero uncertainty in initial conditions will lead to there being some critical time at which precise predictions of the state of the system cannot be made.

P3 For any classical system there will always be some non-zero uncertainty in initial conditions.

Therefore

C Classical mechanics is not deterministic]

Therefore

C There is no good reason to oppose QM on the grounds that is is indeterministic.

P1 seems reasonable—it does appear to the be the case that the root of many physicists' disagreement with QM was to do with it's indeterministic status. P2 is plausible, but seems a little weak without serious supporting historical work, which Born doesn't provide. He does seem correct to assert that mechanical determinism has its origins in the success of Newtonian mechanics. So this part of the argument is certainly not watertight, but it's not unreasonable either. Certainly, it would seem to be the case that a good argument for classical mechanics not being deterministic would undercut at least some of the motivation to pursue a deterministic physics.

3   Determinism and Predictability.

So we need to look at the argument for premise 3—that classical mechanics is not deterministic. Firstly, we should investigate how Born defines determinism and compare that to accepted definitions of determinism given by other philosophers—for this I am going to use John Earman's *A Primer on Determinism* (Earman 1986). Earman formulates a definition that attempts (successfully) to avoid binding determinism up with predictability, and thus mixing one's ontology with one's epistemology. He cautions 'The history of philosophy is littered with examples where ontology and epistemology have been stirred together into a confusing brew' (Earman 1986 7). His point is that determinism concerns whether or not earlier dynamic states uniquely determine later ones,

rather than whether or not, given the dynamics and the initial state, we can accurately predict later states.

Earman defines determinism as follows: take the set of all physically possible worlds, i.e. the set of possible worlds which have the same laws of physics as ours. Determinism is true of the world if it is the case that if this world and some other share all the same physical properties at some time, then they do so for all times. So this is a modal definition for determinism about worlds. It's clearly independent from predictability. This definition says nothing about our ability to know about the world and make predictions from that knowledge. It therefore applies to worlds in which we are unable to make absolute predictions about, for either principled or practical reasons. Principled reasons might be something like a hidden variable theory. Practical reasons might be to do with difficulties in making precise measurements and/or performing the relevant calculations.

We might also usefully make a distinction between determinism about worlds and determinism about theories. A theory might be deterministic even if the world is not. We can even apply much the same definition—a theory is deterministic if, given the same data set for time t, it will always produce the same predictions for all later times. So we might say that the dynamics of classical mechanics mean that it is a deterministic theory even if (a la Born) we are unable to make the precise measurements that we would need in order to ensure that it is stable or (a la Earman) we worry about the possibility of space invaders (Smith 148).

3.1 Space Invaders and Norton's Dome

It's worth saying a little about two cases in which determinism and classical mechanics come apart: space invaders (Earman 1986 36-37) and Norton's dome (Norton 2008). In the space invaders case, a system of four colliding particles can become unbounded in finite time, i.e. their position on some axis goes off to infinity in finite time. Because classical

mechanics is time-symmetric, we can reverse this solution to describe particles coming in to some system from infinity over a finite time (Earman 1986 36). The problem for determinism here is that no state of the system can be considered closed because of the possibility of intrusions from spatial infinity (Earman 1986 34). We might consider imposing boundary conditions at infinity to prevent such intrusions, but this only serves to prevent 'space invaders' by fiat (Earman 1986 38).

Norton's dome is as follows: consider a ball sitting atop of a dome in a gravitational field. The dome is radially symmetric and described by:

$$h = (2/3\,g)\,r^{3/2}$$

Where h is the vertical distance between the apex of the dome, g the constant of acceleration in the gravitational field the dome rests in and r the radial distance coordinate (Norton 2008 787). The ball is modelled as a point mass. At some time, the ball may spontaneously roll down the surface of time. It also may not. Whether or not the ball rolls down the dome or, if it does, at precisely what time, is not specified by the laws of classical mechanics (Norton 2008 788). This is indeterministic because the dynamics do not tell us that the initial state of the system is followed by a single possible series of later states.

Born's argument does not of course concern either of these scenarios. The point of bringing them up is to demonstrate that there can be classical dynamical worlds that are not deterministic, i.e. the laws of Newtonian physics fail to ensure that the world is deterministic.

3.2   Stone on Deterministic Chaos

A number of other philosophers also advocate the separation of determinism from predictability (Stone 1989, Bishop 2003, Rummens and Cuypers 2010, for example). Having separated determinism from predictability, we need to examine how these two concepts come apart. One area where they do which is particularly relevant to the discussion of Born is deterministic chaos, in which systems are deterministic but not predictable.

Stone (1989 123) discusses what he calls 'Scientific Determinism' in the light of this. Scientific determinism is drawn from Laplace, and involves two claims: 1) all deterministic systems are predictable, and 2) all systems are deterministic (Stone 1989 123). From this it follows that 3) all systems are predictable (Stone 1989 123). Of course, we know from quantum mechanics that it is least far from clear that 2) is the case, and so the inference to 3) does not go through. Stone cautions us against concluding that it is *only* because of quantum mechanics that not all systems are predictable (1989 124). In fact, he thinks that *both* claims 1) and 2) fail under what he calls 'deterministic chaos' (1989 124).

Stone also raises what is essentially the same problem as Born for the predictability of classical mechanics but does not deem it a problem for determinism. He notes that it is true that our measurements always contain errors and is doubtful that there exists such a thing as an error-free measurement. He also raises the same problem that Born does with regards to the continuity of numbers. Stone writes 'for example, in any calculation in which I must use the value of π, I must use an approximation because I cannot write down the entire string of digits of π…The problem is not one of measurement but one of representation, and hence it is not just a practical problem but a problem in principle' (Stone 1989 125). It is a problem in principle because there will always be some non-zero error in our

specification of the initial conditions and consequently there will always be some non-zero error in our calculations. He thinks, though, that this does not mean that we are unable to distinguish deterministic from indeterministic systems. This is because for a deterministic system 'the accuracy of a state description is infinitely refinable' (Stone 1989 125). For an indeterministic system like quantum mechanics, this is not the case because such systems contain some built-in limit on accuracy. What Stone means is that if the system is deterministic then, at least in principle, for any given measurement we can make another with a higher degree of accuracy. We can't do that with indeterministic systems. Stone then offers his own definition of determinism as follows:

(a) there exists an algorithm which relates a state of the system at any given time to a state at any other time and the algorithm is not probabilistic;

(b) the system is such that a given state is always followed by the same history of state transitions;

(c) any state of the system can be described with an arbitrarily small (nonzero) error. (Stone 1989 125)

(a) ensures that if a system is deterministic then there are some laws or some mathematical formalism that enables prediction. (b) ensures something like Earman's definition—if some world is in some particular state, then that state will always be followed by the same set of states. (c) distinguishes a deterministic system in which absolute precision of measurement is not possible from an indeterministic system in which there exists some non-arbitrary upper bound on the accuracy of measurement.

Stone thinks that this definition of determinism pulls away from predictability, i.e. it is the case that not every system that meets these conditions will be predictable, because of the existence of chaotic systems (1989 127). This is because in chaotic systems our mapping algorithm (i.e. the dynamics of the system) *magnifies* initial errors to a very large degree. As Ornstein and Weiss put it, 'for chaotic systems there is a very strong sensitivity to initial conditions, in the sense that arbitrarily small changes in the starting point eventually

produce large changes in the trajectory' (1991 14). The trajectory referred to here is a trajectory in phase space, so for chaotic systems the trajectories of objects that start very close to one another in phase space can diverge dramatically. This divergence is exponential in nature (Rickles at al 2007 934).

All of this comes into play with respect to the separation of determinism from predictability because, as we've seen from both Stone and Born, even deterministic systems will always contain non-zero errors in initial conditions. Because in chaotic systems even paths that start very close to one another in phase space have trajectories that diverge quickly and dramatically, those non-zero errors multiply exponentially. This means that even a deterministic system will become unpredictable very quickly if it is chaotic, because even a slight variance in the position in phase space that our initial conditions give will lead to wildly divergent outcomes. Stone points out that this is essentially inescapable. Although we can always insist on a degree of accuracy of measurement that will give us accuracy of prediction over some particular length or timescale, we cannot insist on this generally—there will always be a length or timescale for which our predictions will not be accurate (Stone 1989 127). Stone writes 'for any input there will always be some distance [temporal or spatial] over which error will be sufficiently amplified such that all accuracy is effectively lost. Thus in a strong sense chaotic systems are not predictable even though they are deterministic' (Stone 1989 127).

To define predictability, Stone adds the following condition to those for determinism: '(d) any state of the system can be described with arbitrarily small (nonzero) error from any other state of the system' (Stone 1989 128). What the existence of chaotic systems demonstrates is that (d) does not follow from the earlier conditions (a), (b) and (c), and so determinism and predictability cannot be identified with one another (Stone 1989 127).

3.3 Does Born Think That Predictability and Determinism Are Identical?

So it seems quite clear that we have reasons beyond very general worries about confusion between ontology and epistemology to think that determinism and predictability are not the same thing. Born did not have access to arguments deriving from chaos theory when he wrote *Is Classical Mechanics in Fact Deterministic?* in 1955—the first formulation of the problem is generally accepted as Edward Lorenz's paper *Deterministic Nonperiodic Flow* (1963), which discussed extreme sensitivity to initial conditions in weather systems (Bishop 2017). So the question is, does he identify determinism with predictability? And does his argument rest on an identification of determinism with predictability?

So how does he define determinism? Born describes an aspect of classical mechanics thus: 'The laws of classical mechanics … are so constructed that, if the variables in a closed system are given at some point in time, they can be calculated for any other instant in time' and refers to that description as 'this deterministic idea' (Born 1955 164). We might note that this bears a strong resemblance to Stone's (1989 125) condition (a) for determinism. A page later, when he begins his argument that classical mechanics is not deterministic he does so with the following question: 'But is it the case that classical mechanics in fact permits prediction in all circumstances?' (Born 1955 165). So it certainly looks like Born is running with a definition of determinism that is rooted in predictability.

This is not overly surprising. According to Earman, this is precisely how determinism is usually defined—both Laplace (1902) and Popper (1982) define it in this manner, and although Russell (1912) offers something different, Earman does not think that it works, hence Earman offering his own definition (Earman 1986 10-12). It ought not to shock us therefore that Born also defines determinism in terms of prediction. We should also note that Born doesn't explicitly offer this up as a considered definition of determinism (and

indeed never in the paper writes anything akin to 'I define determinism thus...), merely describing it as 'this deterministic idea'.

Two questions then arise: 1) Is Born aware that there may be some ontological matter going on behind the epistemic problem of predictability that is important to determinism? And 2) If not, is Born's argument only compatible with a definition of determinism based on predictability, or will work with a definition that separates the two concepts like Earman's or Stone's? There is, I think a decent but not watertight case to be made for 1). A primary problem for the idea of predictability as the defining feature of determinism is that it is perfectly conceivable to have some system in which although there is only ever one possible trajectory in phase space for any given event or set of events, that path is not predictable because the information required to predict it is, in some principled way, not accessible to us. It is clear that Born is both aware of this concept and that he does not count such a situation as non-deterministic because he counts de Broglie and his hidden-variable theory as falling under the category of those who are unsatisfied with quantum mechanics because of its indeterminism. He describes de Broglie's approach thus: 'they reject waves and seek a re-interpretation of quantum mechanics, in which everything is in principle determinate, and an uncertainty in prediction arises only by the presence of concealed and unobservable parameters' (Born 1955 165).

Later in the paper, when concluding his discussion on instability in classical systems, Born writes:

> If we wish to retain the assertion that in this system the initial state determines every other state, we are compelled to demand absolutely exact values of x_0, v_0 and to prohibit any deviation Δx_0 Δv_0. We could then speak of 'weak determinacy' as opposed to the 'strong' case where all motions are dynamically stable (Born 1955 167).

It's clear what 'strong' determinacy refers to in this case—a classical setup which is entirely stable and hence entirely predictable. It's not immediately obvious what Born means by 'weak' determinacy but presumably it refers to the situation that Born has just finished describing—that in which all classical motion is, after some critical time, unstable and thus unpredictable. In that case, we might ask, why refer to it as some species of determinacy at all if the notion is to be entirely grounded in predictability? I think that we might reasonably infer from this that 'weak determinacy' refers to systems which, in more precise terms, are deterministic but not predictable, and that therefore Born understands that predictability is not all there is to determinism—i.e. that it's about how the universe is and not just what we can know about it, even though he does not offer a definition in these terms.

3.4 Is Born's Argument Against Determinism or Predictability?

Next we need to look at Born's argument for his claim that we cannot achieve precise measurement of initial conditions in classical mechanics. Something to consider here is that in order to achieve precise measurements we do not merely need precision down to some specified minimum level. If, as Born suggests we need to express values with 'a real number and represented by a point in the mathematical continuum' we need *infinite* precision—there exists infinite space between any two points on the real number line. Without infinite precision there will be some uncertainty in the initial conditions, i.e. some ambiguity between two points on the real number line, and therefore there will be some critical time at which the motions in the system will become unpredictable. Born writes 'A statement like x = π cm would have physical meaning [more on the meaning part shortly] only if one could distinguish between it and π_n cm for every n, where π_n is the

approximation of π by the first *n* decimals' (Born 1955 167). It is of course impossible to do this: no matter how accurately we can perform some measurement we can always take n to another decimal place. This is not simply impossible in practice. It is impossible in principle because no matter how fine-grained we make our measuring instruments, they will still not be able to make an infinitely precise measurement.

We might at this point note that we can supplement Born's argument with Stone's point about chaos. Because we can refine the precision of classical measurements as much as we want, the critical timescales over which the systems that Born describes become unstable can also end up arbitrarily large. We might thus wonder how unpredictable classical systems actually are in practice. Noting that the existence of chaotic systems means that this critical time may be much smaller might assuage this worry.

Stone (1989), as discussed above, acknowledges this point but does not think that the necessary non-zero error on measurements in classical mechanics implies indeterminism of any kind. This is because of the distinction that he makes between deterministic and indeterministic systems: in indeterministic systems there is a defined upper bound on accuracy, whereas in deterministic ones there is not.

If Born's argument was merely about predictability, he could stop here. He's shown fairly conclusively that, over a sufficiently long time scale, any classical system will start to behave unpredictably. If we supplement his argument with the existence of chaotic systems, his point becomes very clear indeed. But Born does not just want to argue that we cannot take measurements with sufficient precision to avoid instability, he wants to argue that such talk of quantities having definite values is meaningless.

Born here applies a principle that he attributes to Einstein and Heisenberg: 'that concepts whose application requires distinctions that cannot in principle be observed are meaningless and must be eliminated' (1955 167). More precisely, Born attributes Einstein's elimination of the concepts of absolute simultaneity in special relativity and of

the distinction between acceleration and gravitational force in general relativity, along with Heisenberg's elimination of the orbital radii of electrons from the model of the atom. It's not quite clear however that Einstein did actually apply such a principle.

In his 1973 paper *Why did Einstein's Programme Supersede Lorentz's?* Elie Zahar attributes Einstein's unification of gravitation and acceleration to the following principle: II) 'All observationally revealed symmetries in nature signify fundamental symmetries at the ontological level—there are no accidents in nature. Hence the heuristic rule: *replace any theory which does not explain symmetrical observational situations as the manifestations of deeper symmetries—whether or not descriptions of all known facts can be deduced from the theory*' (1973 225). What does this mean? It means that a good theory should unify the explanations of observationally identical phenomena. We expect good theories to do this because we expect that such symmetries are not coincidental: they reflect the ordering of the world. Now, this does not seem to be the quite same thing as Born's principle, but it might lead us to the same conclusion - if there exists an observational symmetry between two different physical conditions, then the distinction between those conditions is meaningless. By declaring that distinction meaningless we are forced to invoke some underlying principle in order to explain this, although I'm not quite sure that Born has the same demand for explanation that Einstein does.

It might be slightly better put in the terms that Zahar uses in *Einstein's Revolutions: A Study in Heuristic*—that this principle is tantamount to saying that there are no accidents in nature (Zahar 1989 90). So in Born's terms we ought to consider the concept of precise physical values meaningless because for any given value, there will be some other from which it is observably indistinguishable. In Einstein's terms (by way of Zahar) we might say that we ought not to consider it a mere accident that we cannot achieve in principle precise measurements of physical values—it should be considered a reflection of how the world is.

 What we can take from this is that, according to Born, we are not simply in the position of having to regard classical systems as unstable; we are in the position of having to regard the concept of a system having the precise initial conditions that would render it stable as meaningless. This is because in classical mechanics a deterministic world is observationally indistinguishable from an indeterministic one, due to our principled inability to perform the absolutely precise measurements that would allow us to describe a stable system. Hence, Born thinks, classical mechanics is not deterministic. It's important to note here one thing that Born is *not* arguing: He is not making any claim that the dynamics of classical physics are indeterministic—if a system has precise initial conditions then it will behave deterministically. Classical mechanics as an abstract theory is still deterministic on Earman's definition. If we start with precise values then any worlds that share all of those values at one time will share all of those values at all times. The same is true for Stone's definition.

The problem is that we lack the capacity to determine whether or not classical mechanics applies to the world not just in practice but in principle. The world as we find it does not allow us to conclude that such precise conditions exist. Hence we are mistaken to take as a conclusion from the success of classical celestial mechanics that classical physics ever in fact described a deterministic world.

We can also look at this in terms of Stone's argument. Because of non-zero errors in our measurements, some apparently deterministic systems will behave unpredictably. They are, however, still deterministic because those errors can be reduced to an arbitrarily small value, even though this still fails to make the system predictable—this is reflected in his condition (c) (Stone1989 125). If we apply Born's principle here we run into the following problem: regardless of what our theory says, no measurements that we can carry out can ever tell us whether or not (c) is true. This is because there is always some indeterministic upper bound on accuracy that is not measurably distinct from an arbitrary non zero error in measurement. There are also, one should note, no measurements that one could carry out in order to determine whether or not (b) is the case. So again, Born would I think say, the

problem is not that the theory that classical mechanics supplies is indeterministic, it is that we have no way of distinguishing between a deterministic world and an indeterministic one.

To pull this back to a distinction between determinism about theories and determinism about worlds, what Born's argument seems to say is that the success of classical mechanics as a deterministic theory does not enable us to conclude that the world itself is deterministic. It can be perfectly true that the dynamics of classical mechanics are deterministic. What we can't do is conclude from those dynamics that the world itself is deterministic. This is because we are unable to make any measurable distinction between on one hand quantities that are precise but just beyond whatever level of accuracy we happen to be using; and on the other hand quantities that are inherently ill-defined/imprecise/indeterminate. This leads to classical mechanics being, on the appropriate timescale, a statistical theory in a way that is indistinguishable from a genuinely indeterministic one (as opposed to a statistical theory that is actually deterministic). Chaos in a system just lessens this timescale.

Suppes (1993) makes a similar, but not identical argument to this. Suppes argues that determinism falls into the category of the transcendental. From Ornstein and Weiss (1991) he draws the following theorem: 'There are processes which can equally well be analysed as deterministic systems of classical mechanics or as indeterministic semi-Markov processes, no matter how many observations are made' (Suppes 1993 254). This, Suppes thinks, puts both determinism and indeterminism into the category of positions that cannot be refuted by empirical evidence. Hence they are transcendental, in that they are beyond the reach of empirical evidence to decide (Suppes 1993 256). We should also note that Suppes does not think that we can convincingly claim that quantum mechanics shows that the world is fundamentally indeterministic (1993 252-3).

Earman (2007) is not quite convinced by Suppes' (1993) argument. He writes 'There are two competing hypotheses to explain observed macro-stochasticity: it is due to micro-

determinism plus sensitive dependence on initial conditions vs. it is due to irreducible micro-stochasticity' (Earman 2007 1391). His scepticism regarding Suppes claim that determinism is in the category of the transcendental arises from thinking that not enough work has been done to show how the second hypothesis (irreducible micro-stochasticity) could actually be the case. Even if that had been done, Earman thinks that Suppes would still have to show that the choice between the two hypotheses is genuinely underdetermined by any possible evidence (Earman 2007 1391).

Born's position seems to be very much in line with Suppes'. That we can treat classical mechanics statistically is entirely his point. Although he doesn't make any kind of explicitly transcendental claims, his argument about what the success of classical mechanics can tell us about the world depends on a closely related point. However, the work of arguing that we, without further argument, we can't assume classical mechanics (or at least the world as described by classical mechanics) to be deterministic is done by the principle which claims that it isn't meaningful to talk about precise values in classical systems. Earman is obviously discussing chaotic systems in his response to Suppes, but I think Born could simply respond by again leaning on his principle to say that the world doesn't supply the precise values needed to show that micro-determinism is the case and so we cannot conclude that classical systems are deterministic.

We ought to note here how much heavy lifting is being done by the principle that allows us to claim that talk of precise values isn't meaningful (although not in a semantic sense—see Chapter 4:Born on Realism – statements attributing infinitely precise values to physical quantities are not meant to be meaningless in the sense that they are gibberish/ungrammatical, but rather in the sense that they are supposed to play no meaningful role in the physical interpretation of a theory and we cannot make inferences from them in that sense). If we don't agree with that principle or with how Born applies it, then we are going to be left with something like Stone's (1989) position that classical mechanics is deterministic but not predictable. We should note though that Born's principle might not be unreasonable. We might be able to understand what's going on via

Redhead's notion of 'surplus' structure. Redhead (1980 149) argues that we can regard some theories as having 'surplus' structure in their mathematical formulations, in that they have components which are uninterpreted in the sense that they play no part in the inferences by which the theory can be tested. The surplus or excess structure here would be the considering a continuous variable to actually, perfectly and precisely represent the position of a particle to a greater degree of accuracy than any conceivable experiment could supply, rather than simply treating the representation of physical quantities by real number variables as being part of the formalism. Such surplus structure isn't, for Born, physically meaningful. The determinists then have made the mistake of interpreting part of the wider mathematical structure that Newtonian dynamics is embedded in (continuous real number variables) physically, i.e. infinitely sharp real number variables ought not to be considered part of the physical structure of Newtonian dynamics, merely part of the mathematical one.

What *kind* of things physical quantities are isn't something that Newtonian dynamics can tell us because in order to tell if they are infinitely sharp we would have to perform measurements to a greater degree of accuracy than any conceivable experiment could supply, i.e. we cannot actually make any testable inferences from this notion. The assumption that they are this sort of value is surplus to the empirical content of the theory because this is not something that a system of dynamical laws can tell us about – it's something that has been carried over from the mathematical model that the theory uses.

4   A Bit of Reconstruction

I'm now going to completely take off my historian hat (which has already become a bit askew in this chapter) and offer a reconstruction of Born's argument in terms of

determinism alone. The first thing to say is this: the real number line is part of our *representation* of reality. We can have no direct empirical reason to think that the quantities of physics are *actually* real number-valued. So, to assume that they are is to make a metaphysical assumption. It can't be an empirical claim.

By making metaphysical assumptions like this, we introduce *excess structure* into the theory. There is nothing forcing us to interpret physical quantities as being real-number valued: we can quite happily *represent* them with real numbers without presuming that they *are* real numbers. Excess structure in this manner is non-empirical content of a theory that we introduce by marrying a physical model with a mathematical structure (Redhead 1980 149). In this case, the physical model is Newtonian dynamics and the excess structure is the use of precise continuous variables rather than say, ranges of approximation, to perfectly represent the position of some object. The mathematical structure contains real numbers to represent continuous variables and this has been 'carried along' with the theory and been taken to *really* represent physical values in it, as opposed to merely being part of the theory's formalism.

What Born's principle tells us to do is to is to cut out such excess structure from the physical interpretation of the theory because it doesn't play any empirical role. This is because, for any given level of accuracy of measurement there are empirically equivalent deterministic and indeterministic interpretations of our system. This follows from the fact that there exists an underdetermination between some quantity in our initial conditions being fuzzy and that quantity being precise but unmeasurable. The former is an indeterministic interpretation and the latter is what we might call an ignorance interpretation—quantities are infinitely sharp, but we don't know exactly what they are. So we have an underdetermination between a system which is indeterministic and unpredictable and a system which is deterministic and unpredictable. This is a genuine

underdetermination because the level of unpredictability is the same for both interpretations—it is set by whatever level of error exists in our measurements.

Now, if quantities are stochastic, then the world must also be. A world with stochastic quantities will fail to meet Earman's definition of determinism: two such worlds that share all physical properties at one time will not necessarily share all physical properties at all time. It doesn't follow from that, however, that our dynamical laws themselves must also be stochastic. It is perfectly possible for those laws to be such that, if quantities are precise, predictions made by them will also be precise. Indeed, this is the situation for the laws of classical mechanics. In this sense, such a set of laws would not be wrong—they would produce precise or statistical predictions depending on what sort of quantities we put into them. Therefore a set of dynamical laws that gives us precise predictions fails to tell us whether or not the world is deterministic. Hence the reason to worry about the indeterminism of quantum mechanics—that classical mechanics was highly successful and deterministic—vanishes. We were never able to conclude from a deterministic set of dynamical laws that the world itself is deterministic, even if those laws were correct.

5   Conclusion

So to summarise, Born's argument is follows. We are mistaken to conclude that classical mechanics shows that determinism is true of the world. This is because there is a necessary non-zero error on all measurements taken in classical systems (to say nothing of quantum ones), and this means that for any classical system there exists some critical time after which the system must be treated statistically. Born thinks that we cannot simply stipulate that there do actually exist precise values even if we cannot reach them (he calls this 'weak determinism') because there are no measurements that could possibly tell if this is the case. This, we should note, is independent of whether or not a 'deterministic' mathematical

formalism exists for classical mechanics: without precise values we are forced to use statistical methods. Hence, he thinks, we ought not to conclude that the world of classical mechanics is a deterministic one. Stone (1989) and Suppes (1993) discuss arguments that bear a similarity to Born's, although theirs concern chaotic systems. By looking at these arguments it is made clear how much Born's argument relies on the aforementioned principle—without it, the most that he can show is that classical mechanics is deterministic but not predictable. A reconstruction of Born's argument in terms of underdetermination has also been offered.

Chapter 6—Born on Probability

1   Introduction

This chapter will examine Born's interpretation of probability in quantum mechanics. He is not explicit about it, so we have to try to tease out his thoughts from his writings concerning probability. As with many of the other questions that I ask throughout this thesis, I am not trying to argue that he holds the correct interpretation of probability in quantum mechanics, if indeed there is one; nor am I trying to argue that there is a best interpretation, and so he must hold it. Once again, the categories that Born may fit into were not necessarily available to him. What I am trying to find out is which interpretation makes fits his position the best. More particularly, the questions to be asked are whether he holds any view on probability at all, and if so, is that view objective or epistemic? Which of the subcategories for these positions does that view fit into? What I am going to conclude is that he certainly has an objective interpretation of probability and most likely considers probabilities in quantum mechanics to be something like propensities.

Probability and statistical physics is a topic to which Born devotes considerable attention. His work on quantum mechanics in the 1920s was focussed on statistical quantum mechanics. In his paper *On the Quantum Mechanics of Collisions* (Born 1926a), Born introduced the statistical interpretation of the wavefunction, that one could interpret $\left| \Psi^2 \right|$ as giving the probability of observing some particular solution to the superposition of the wavefunction. The statistical approach was developed in later papers with Jordan,

Schrödinger and Heisenberg, finding further grounding in Heisenberg's Uncertainty Principle. Born and Heisenberg also presented the statistical approach in a joint paper at the 1927 Solvay Conference.

Before taking a detailed look at Born's views on probability, it is worth first examining the different ways in which the notion of probability can be interpreted. In other words, what can we mean when we say that there exists a probability P of event E occurring? There are a number of different interpretations of probability which I'm going to discuss—The classical interpretation, the frequency interpretation, the subjective interpretation and the propensity interpretation. It should also be noted that one can hold a pluralist view of probability. One could, for example, hold that probabilities in the everyday world are best described by the subjective theory and that probabilities relating to parts of fundamental physics, such as radioactive decay, are best described as propensities.

These theories can be understood on an axis of objective vs. epistemic interpretations. An objective theory of probability holds that probabilities are in some important sense given to us by the world and there exist objectively correct and incorrect assignments of probability for the outcome of any particular event. Within this category are frequency interpretations, which identify probabilities with the frequency with which a particular outcome to some event occurs over a long run of such events; and propensity interpretations, which explain probabilities as being physical properties of systems that give rise to frequencies. An epistemic theory of probability holds that probability is fundamentally a feature of our knowledge, or indeed our ignorance, about the world. Into this category fall classical/logical interpretations and subjective interpretations. The classical theory (in the Laplacean mould) regards probability as arising from unavoidable epistemic fallibilities in a deterministic world; the logical interpretation arises from this and treats probability as degree of rational belief; and finally the subjective interpretation identifies probabilities as entirely internal to some agent as degree of belief in some outcome full stop.

Much of Born's non-scientific work throughout his career is devoted to the topic. Statistical physics and its development is central to his argument in *Natural Philosophy of Cause and Chance* (1949a)*,* and the collection *Physics in My Generation* (1956) contains three papers that explicitly address the subject.

In this chapter I am going to survey the standard theories of probability, and argue that it is implausible to view Born as having a classical or subjective view of probability. This is because they are incompatible with the objective view that Born takes of probabilities in quantum mechanics. Hence, if he holds any view at all, it must be a frequency or propensity view. Next I will examine some of Born's work in quantum mechanics, particularly *On the Quantum Mechanic of Collisions* (Born 1926a). I will then give an exegesis of Cartwright's (1987) paper on Born, in which she argues that Born's work on quantum mechanics indicates that he holds a propensity view of probability. I'll use that as a springboard to examine some of Born's later writings on probability, in particular an exchange of letters between him and Einstein on the topic, arguing that these indicate that the best fit to his position is a long-run propensity interpretation. Finally, I will look at Cartwright's (1987) claim that Born seems to hold one of the modal interpretations of quantum mechanics and argue that although his views do seem to be consistent with van Fraassen's initial (1991) formulation, there does not exist enough information to claim that Born's views align with any of the contemporary programs for modal interpretations.

2   Epistemic Interpretations of Probability

2.1 Epistemic Probability: Laplace and Keynes

2.1.1 Laplace and the Classical Theory of Probability

The classical theory takes probability to be epistemic, coming simply from our ignorance of the world. Pierre Simon Laplace expresses this thesis in his *Philosophical Essay on Probabilities* (1814). Laplace describes conceiving of a vast intelligence. He writes:

> We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. (Laplace, 1814, 4)

So for such a super-intelligence, there would be no such thing as chance. There was *really* only one face upon which a flipped coin could land, only one result that a thrown die could give. It is just that we are ignorant of whatever set of laws and conditions determines the outcome and are probably unable to perform the calculations which would precisely predict it. The super intelligence is not ignorant of these considerations, and so for them there would be no need for a theory of probability. We can see that the classical theory, at least in its Laplacean conception, is bound up with the ontological determinism present within Newtonian mechanics. A rejection of determinism, it would seem, would entail a rejection of Laplace's view of probability or at best adopting a pluralist view of some kind.

So how does the classical theory actually work? In a nutshell, when we are ignorant of the outcome of some particular event, we assign a probability by giving an equal weighting for every outcome of the same kind in such a way that the sum of those probabilities is one—

in effect we divide equally the certainty that one out of the range of possible outcomes will occur between the number of possible outcomes.

Formally speaking, this works in the following way: the probability of some outcome A occurring is given by *m* the number of possible outcomes that give us *A*, (this can be more than one—think of the probability of drawing a card of a particular suit from a complete deck, rather than just the probability of drawing the ace of spades) divided by the total number of possible outcomes *n*. The probability of A is then

$$P(A)=m/n$$

So for the probability of drawing a spade, *m* will be 13, the total number of spades in a deck and *n* will be 52, the total number of cards in the deck. Thus P(drawing a spade) = 13/52 = ¼.

It does not seem to be quite clear how Laplace's theory deals with cases which are *not* equiprobable. Gillies (2010 18) writes that there does not seem to be a way for the classical theory to deal with a biased coin. Although he notes that Laplace does at one point discuss calculations for a biased coin, considering a case in which there are particular non-equal probabilities for heads and tails, Gillies takes this to imply that there is some objective measure of probability used to determine how biased the coin is and so thinks that this contradicts Laplace's epistemic interpretation of probability (Gillies 2010 18). Galavotti disagrees with Gillies, writing that 'this is not so much a case of unknown chances, but rather of chances known to be unequal' (Galavotti 2005 61). She notes that the process of determining such unknown chances is the starting point for the frequentism of those such as Von Mises. This doesn't mean that there is any circularity in Laplace's understanding of probability. Galavotti understands Laplace as basing his interpretation of probability partly on our ignorance and partly on our knowledge (2005 60). She writes 'once probability is taken as epistemic, it stands on a different ground from the possibility of events' (Galavotti 2005 61).

Something will turn on this dispute with regards to Born. If the classical theory is really only able to deal with situations in which all outcomes are equally likely, then it seems unlikely that Born holds a classical view of probability, particularly in quantum mechanics, for the primary reason that this is simply not always how probabilities are treated in quantum mechanics. Born's statistical interpretation calculates probabilities in quantum mechanics from the square of the modulus of the wavefunction. When we want to calculate the odds of an electron being deflected at some particular angle from an atom, he does not take the range of energetically possible deflections and divide one by their number, assigning each possible outcome an equal probability. Instead he models the electron as an incoming plane wave and performs a perturbation theory calculation to give us a superposition of each of the solutions. The square of the modulus of the wavefunction for each particular solution gives us a transition probability for the electron to transition from its eigenstate before collision to some eigenstate afterwards. This is simply not a classical model. This is to say nothing of the interference of probabilities in the two-slit experiment, something else that Born is well-aware of. We should note here that it is of course the case that statistical physics does often use this method to determine initial probabilities—this point is discussed in more detail further on.

It's not clear that Gillies' interpretation of Laplace is the correct one, so we need more than this to argue that Born would not accept the Laplace interpretation of probability. We might note that there seems to be a determinist motivation for the classical theory and Born does not support a determinist interpretation of quantum mechanics. We can see this clearly in his (1926a) collisions paper when he writes that he is 'inclined to give up determinism in the world of atoms', and indeed in later work such as *Is Classical Mechanics In Fact Deterministic?*, (Born 1955 and see also Chapter 6—Born on Determinism) in which he argues that we are not in fact entitled to interpret classical mechanics as telling us that the world is deterministic

I think a deciding point is that it seems fairly clear that Born does not think that probabilities are epistemic in nature. He asks, for example, in *Natural Philosophy of Cause and Chance* 'how could we rely on probability predictions if by this notion we do not refer to something real and objective?' (Born 1949a 106). We can also see in Born's arguments against those who think that physics ought return to determinism, either via a new (non-quantum mechanical) theory or via hidden variables, (Born 1955, see also Chapter 5—Born on Determinism) a belief that quantum mechanics is fundamentally probabilistic. Again, this seems to be indicative that Born does not have an epistemic view of probability

2.1.2 The Logical Turn

In the early part of the 20th century John Maynard Keynes developed a theory of probability that interprets it as partial logical entailment (Gillies 2000 Chapter 3). Gillies explains that Keyne's thinking runs as follows: We know that there is no deductive entailment from prior observations to later ones. The fact that the sun rose yesterday does not deductively entail that it will rise tomorrow, nor does the observation of white swans on the Thames entail that swans observed on the Severn will also be white. Such matters are instances of induction, rather than deduction. Can we, Keynes asks, understand such things in a formal way as instances of a partial deductive relation? (Gillies 2000 30).

So Keynes aims to give a logical account of the relation between some set of prior knowledge and the likelihood of some outcome given that knowledge, that account being based on some relation of partial entailment. Gillies notes that Keynes also frames this as degree of rational belief in some outcome as well as degree of partial entailment. If we have some set of propositions as our premises and some set of propositions as our conclusion, then if knowledge of our premise set justifies some particular rational degree of belief in the conclusion set then we can identify that degree of belief with the probability relation between the two sets (Gillies 2000 31).

Keynes' theory has some peculiar elements, namely that he divides probabilities into two sorts: those to which numerical values can be attached and those to which they cannot. We can only assign numerical values to those situations in which all outcomes are equally likely. In other situations we cannot assign numerical values. This doesn't mean that we cannot give something like relative weightings for outcomes in those cases; it does mean that we cannot put numbers to those weightings (Gillies 2000 34).

In order to make the distinction between when we can and cannot use equiprobable distributions, and so when we can and cannot assign numerical values to probabilities, Keynes employs what he calls The Principle of Indifference. This states essentially that when we have no prior reason to prefer one outcome to another, we should assign all outcomes an equal probability. We should note that that there are several paradoxes that arise from application of this principle (Weatherford 1982 55-66), but I'm not going to discuss them because I don't that there is anything to suggest that Born accepts or rejects a logical understanding of probability on the merits of those paradoxes, or that he is even aware of them.

It is worth noting, however, that the principle of indifference appears to have successfully been used in science and in particular statistical physics and that we might take this to give us reason to think that it is true (Jaynes 1973, Hájek 2012). What else are we doing when we assume a micro-canonical distribution but applying the principle of indifference? The argument runs that as we have been successful in applying the principle of indifference in developing multiple successful theories of statistical physics—Maxwell-Boltzmann, Fermi-Dirac and Bose-Einstein—we ought to consider the principle as valid. Gillies criticises this position on the grounds that it confuses a (undoubtedly successful) heuristic with a logical principle. The point is that whilst this may be a good argument for the principle itself, it cannot serve as an argument for an a priori interpretation of probability: we can only determine the utility of the principle of indifference in constructing

empirically adequate systems of physical statistics a posteriori. Hájek (2012) raises a similar problem, that different statistics apply to different types of particle—Fermi-Dirac statistics to fermions like the electron and Bose-Einstein statistics to Bosons—none of which can be determined a priori.

Interestingly in *Einstein's Statistical Theories* Born (1949b) says something like this. When discussing arguing that Einstein was involved in the foundation of wave mechanics, he writes 'I cannot see how the BOSE-EINSTEIN counting of equally probable cases can be justified without the conceptions of quantum mechanics' (Born 1949b 89). He goes on to explain that even in cases in which particles are indistinguishable, such particles can still be distributed 'between two boxes' in a number of different ways. Key for Born in the Bose-Einstein case is the use of a symmetric wavefunction to describe the system. He explains that in the case that the wave-function is 'skew' (i.e. non-symmetrical) we end up with Fermi-Dirac statistics (Born 1949b 89).

So I actually think that Born might well be sympathetic to the line of argument that we should consider the principle of indifference valid because it has been used successfully in physics. It's a similar argument to ones that he has made about his principles of contiguity and antecedence, and the heuristics used by Heisenberg and Einstein to eliminate unobservable distinctions (See chapters 3 and 5 on causation and determinism). The problem here is that the principle of indifference, even if considered valid by Born for these reasons, would not be an a priori principle, but rather would be an empirical or metaphysical one. We get a very clear statement that this is indeed Born's position in *Physical Aspects of Quantum Mechanics* (1927). He writes when discussing the ergodic hypothesis 'it thus seems that the justification of the choice of equally probable cases by dividing the phase space into cells can be only be derived *a posteriori* from its success in explaining the observed phenomena' (Born 1927 6).

Furthermore, I don't think that Born would follow Keynes in thinking that any time we cannot apply the principle of indifference we cannot give numerical values for probabilities, largely for the same reason that I argued he would not hold the Laplacean-classical view of probability—it's not going to be compatible with how quantum mechanics treats probability in all cases. In addition we have the already-mentioned indication that Born thinks about probability as objective, rather than epistemic.

2.2 The Subjective Interpretation

The subjective interpretation was developed independently in the 1920s and 30s by Frank Ramsey and Bruno de Finetti. Gillies (2000) notes that although Ramsey developed his version in the 1920s, prior to de Finetti's publications in the 30s, Ramsey's work was not published until 1931, after de Finetti's initial publications. Thus there is justification for the claim that their work was genuinely developed separately. Unsurprisingly given its name, the subjective theory holds that probabilities are not objective things. Rather, the probability of an event occurring is the degree of belief that an individual has that that event will occur. Note that this means that there exists no one probability for any particular outcome. There is merely a series of probabilities for different individuals. The question then, is how can this degree of belief for an individual be measured? Both Ramsey and de Finetti conclude that betting, or more particularly the odds that individual is willing to place on a bet for a particular outcome, can provide this measure.

This is done in the following way: consider a situation in which a person A is called upon to place a bet upon an event E. In order to do this A must choose a number q, referred to as the *betting quotient*. Another person, B chooses the stake S. A pays B qS for the promise that B will pay out S if E occurs. Importantly, S can be positive or negative, reflecting whether or not A's bet is for or against the occurrence of E, but the sign of S is chosen by B and not known to A. The degree of A's belief in E can now be measured by *q*.

But we are not done yet. Now we must show that such measure of degree of belief leads to a set of rules that can be used to construct the probability calculus. This is done through the introduction of the notion of coherence. This is defined as follows: A set of betting quotients chosen by A $q_1...q_n$ for events $E_1...E_n$ is coherent if and only if B cannot choose stakes $S_1...S_n$ such that B will win whatever happens. The situation that B always wins is referred to as a Dutch book (Hájek 2012).

2.2.1 The Axioms of Probability

It can then be proved that a set of betting quotients is coherent if an only if they satisfy the axioms of probability as stated below.

$$0 \leqslant P(E) \leqslant 1$$

For any event E and the probability for some certain event *P(Ω) = 1*.

If $E_1...E_n$ are events which are exclusive and exhaustive then

$$P(E_1)+...+P(E_n)=1$$

For any two events *E, F,*

$$P(E \wedge F)=P(E|F)P(F)$$

Where $P(E \wedge F)$ is the probability of both P and F occurring and $P(E|F)$ is the probability of event *E* given event *F*.

So it can be shown that a Dutch book cannot be formed IFF an individual's degrees of belief satisfy the probability calculus. In this respect it would seem that the subjective theory is a success (Hájek 2012). However there is an obvious objection at this point. Whilst it seems reasonable to claim that many probabilities are subjective, or at least contain a substantial component that is subjective, there are surely some probabilities that are truly objective—think of the result of the throw of an unbiased die or the chance for an

atom to decay. So how does the subjectivist deal with the apparent existence of objective probabilities? There, are, as Gillies (2000) notes, two solutions. The first is to simply accept, as Ramsey does, that there are two acceptable notions of probability, each applicable in different circumstances. The second approach, which de Finetti takes, is to insist that even apparently objective probabilities are in fact subjective and can be calculated as such. De Finetti does this by introducing the concept of exchangeability.

So what is it precisely? It is the condition under the assignment of a probability value for some particular result amongst a set of outcomes is dependent only on the number of times that result occurs and so is independent of the ordering of those outcomes. More formally, we might say that a sequence of variables is exchangeable IFF the probability for some outcome of that sequence is invariant to the random permutation of those variables. Consider how this works when throwing a die. The probabilities assigned to some particular outcome are said to be exchangeable IFF the same probability of the die coming up six is assigned no matter where the sixes occur amongst the other results of one, two, three, four and five in any set of outcomes for which the same number of sixes is observed (Gillies 2000 71). Probabilities will not be exchangeable when the ordering of events is important to the assignment of a probability value for some result.

Observers with differing prior probabilities for some apparently objective event can come to agree, via Bayesian updating, on the same prior probability. For example, in the case of throwing a die, bettors might initially offer different probabilities based upon whether or not they think that the die is balanced. The more times the die is thrown, the more closely the probability assignments from different individuals will converge as the results either give an even distribution of faces or some faces occur significantly more frequently. As Gillies points out (2000 70), although this might look like an objective probability assignment to those who cleave to such theories of probability, De Finetti thinks that such a concept is meaningless. Gillies writes 'All that is happening is that, in the light of

evidence, different individuals are coming to agree on their subjective probabilities' (Gillies 2000 70). Exchangeability is the condition that allows this to happen.

What exchangeability is supposed to do is to replace the notion of independence that occurs in objective theories of probability. We are not supposed to need to make any assumptions about the independent nature of the results outside of what we observe of them. We can reduce assumptions of probabilistic independence to that of exchangeability, thus reducing apparently objective probabilities to subjective ones (Gillies 2000 75). It's important to note that although independence and exchangeability are closely related, they are not identical. Although it is true that all independent sequences are also exchangeable, the converse is not the case. For example, sampling without replacement, where one selects individuals from a population at random with the exception that no one is chosen more than once is exchangeable, but it is not independent. Imagine the simple example of drawing three coloured balls from a bag which contains five white balls and five black balls. The probability of drawing a white ball on the first draw will be:

$$P(W_1)=5/10=1/2$$

The probability of drawing a white ball on the second drawn will be:

$$P(W_2)=(W_2|W_1)+(W_2|B_1)=(5/10)(4/9)+(5/10)(5/9)=1/2$$

The probability of drawing a white ball on the third draw will be:

$$P(W_3)=(W_3|(W_2|W_1))+(W_3|(B_2|W_1))+(W_3|(W_2|B_1))+(W_3|(B_2|B_1))=$$

$$(5/10)(5/9)(3/8)+(5/10)(5/9)(5/8)+(5/10)(5/9)(5/8)+(5/10)(4/9)(5/8)=1/2$$

Hence as the probability of drawing a white ball on either of the three draws is equal, the sequence is exchangeable. However it is not independent. If the sequence was independent, then:

$$P(W_1)=P(W_2|W_1)$$

i.e. the probability of drawing a white ball on the second drawn after having drawn a white ball on the first drawn is equal the probability of drawing a white ball on the first draw. However in this example,

$$P(W_2|W_1)=4/9 \text{ and } P(W_1)=5/10=1/2$$

As ½ ≠ 4/9, we can say the sequence is not independent.

2.2.2 Is Born A Subjectivist?

So, might Born be a something like a subjectivist about probability? How is it that could we tell? I'd suggest that if we were to characterise Born as subjectivist, we wouldn't necessarily need to find evidence that he thinks of probabilities in terms of rational betting quotients. He doesn't need to have the same formulation of subjectivism as de Finetti. It would be enough to show that he thinks of probability in terms of subjective, not objective assignments.

In a 1973 paper entitled *Einstein: Originality and Intuition*, de Finetti discusses Einstein's views on probability, substantially mediated by several papers in Born's Physics in My Generation. In it, de Finetti argues that Born's and Einstein's views on probability are in line with his subjective interpretation. In this section, I'm going to argue that he's mistaken. The various claims that de Finetti makes about Born, that he is strongly empiricist, that the subjective interpretation is in line with the way that Born thinks about probability don't stack up when we consider Born's wider views.

De Finetti argues that both Einstein and Born are empiricists, citing in support Einstein's favourable views on Mach, and some remarks that Born makes on observables. Why does de Finetti want to argue that Born and Einstein are empiricists? Because the subjective theory itself is empiricist with regards to probability. For the subjective theorist, there is

nothing going on underneath the betting quotients that people assign to outcomes. At the very least, if there is, then it's got nothing to with the things we call probabilities.

I don't want to engage with de Finetti on Einstein (although Eli Zahar would I think disagree with de Finetti here—see Zahar 1973 and 1989 for example), but I do want to disagree with regards to Born. He cites the following passage from Born 'the world of physical objects lies outside the realm of the senses and of observation, which only border on it' (de Finetti 1978 124) as evidence. According to De Finetti, we could not know a region simply from observing its edge: 'we thus find ourselves in the impossible situation of one who would claim to describe a whole unexplored region whilst knowing only its contour' (de Finetti 1978 124). The problem is that Born does not think this. Consider the next clause in the quotation from Born '…; and it is difficult to illuminate the interior of an extensive region from its boundaries' (1928 25). He then proceeds to discuss some of these difficulties in the context of quantum mechanics. Now, there is a world of difference between a difficulty and an impossibility and these difficulties are not ones which Born thinks are insurmountable, as I've argued in some detail on in the Chapter 4—Born on Realism.

This is not the only arrow in de Finetti's quiver. He quotes Born with regards to micro-coordinates in classical statistical mechanics '"of course it is not forbidden to believe in the existence of these co-ordinates; but they will only be of physical significance when methods have been devised for their physical observation"' (de Finetti 1978 124). De Finetti thinks that this is a clarifying quotation with regards to the 'fatuous disquisition as to whether a given thing 'exists' or 'does not exist'' (de Finetti 1978 p124). Given that de Finetti thinks that Born agrees with him, it would seem that he interprets this quote as indicating that, for Born, the question of whether or not something exists is meaningless—all that matters is what is measurable.

The context of the quotation is Born's paper *Physical Aspects of Quantum Mechanics* (Born 1927). Born wishes to point out that the epistemic situation in quantum mechanics is actually not so different from that in classical mechanics. In classical mechanics one introduces micro-coordinates for the positions of the particles in the system and then averages over them to give a statistical result for the whole system. Born's point is that as far as the experimentalist is concerned, the classical micro-coordinates might as well not exist—they are only introduced 'to keep the individual phenomena at least theoretically determinate' (Born 1927 8). Averaging over the system destroys the initial coordinates and they are at no point practically measurable. Quantum kinetic theory achieves the same result without ever introducing precise micro-coordinates—they don't exist in the theory or in the results. Born's point is this: in both classical and quantum mechanics we calculate the statistical behaviour of ensembles. In quantum mechanics, there are no precise trajectories because the position and momentum operators don't commute. Despite in classical mechanics it always being assumed that there were precise trajectories, Born does not think that there was any good reason to do so.

Now, this might mean the following: Born thinks that the question of the reality of quantities which are not in principle measurable and play no role in theoretical calculations is meaningless (although not in a semantic sense), or at the least irrelevant. It doesn't indicate, at least on its own, a kind of operationalism or logical empiricism. The micro-coordinates that Born refers to are not simply impossible to measure indirectly or independently of other parts of the theory. Rather, they play no role in the theory for any understanding of the terms "observable' or 'measurable". The micro-coordinates are for all intents and purposes hidden variables. Denial of hidden variables is not an indication of an operationalist stance. It is merely consistent with one.

It is possible that de Finetti might want to argue for something weaker than the claim that Born is a subjective theorist. It could be that Born's position is consistent with the

subjective interpretation and that de Finetti thinks that Born should be a subjective theorist, given the theory's other advantages. So what does de Finetti think that the core of the subjective theory is? It is this: probabilities do not exist as entities in their own right. The probability of an event only exists 'as an estimation of hope or fear or at any rate a degree of expectation of its coming to pass' (de Finetti 1978 126).

The problem here for de Finetti here is that, as Galavotti points out in her 1995 paper, *Operationism, Probability and Quantum Mechanics*, although the subjective theory is operationalist and Born certainly says things that make it sound like he's an operationist (see his repeated emphasis on the heuristic principle that Heisenberg used to eliminate quantum orbits because of their unobservability, and his argument that classical mechanics is not determinist based on what looks a very strong commitment to that principle), de Finetti is also a positivist, and Born is not.

For the operationalist, theoretical concepts can only be defined in terms of observable concepts. Now it might seem that Born makes noises in this direction, but we must understand that by "observable", Born does not mean 'observable with the senses'. He is quite happy (see Chapter 4—Born on Realism) to regard microscopic things or even fields as observable and denies the sharp distinction that the positivist wants to make between observable and unobservable. Born might be an empiricist of some type (he regards the term as merely meaning that theories should correspond to observable results, something that he thinks is 'trivial'), but not of the Machian variety, and this is precisely the position that De Finetti occupies. Galavotti notes that De Finetti describes himself as a Machian, and thus 'probability is for him nothing other than the sensation of the individual (Galavotti 1995 114). As Galavotti points out and I have gone into some detail on in the realism chapter, this is a position that Born rejects explicitly.

I suppose that one could attempt a reconstruction of the subjective theory that did not rely on some form of positivism but, as I will argue later in this chapter, I consider it far more likely that Born holds some form of propensity view. Think of his talk in *Einstein's Statistical Theories* of the importance of 'fundamental' laws of probability. De Finetti seems to take this to mean that Born wishes to base all of physics on probability rules. In many ways, De Finetti is right—Born argues that we should replace the deterministic laws of classical mechanics with a statistical reformulation in *Is Classical Mechanics Deterministic?* (Born 1955—see Chapter 5—Born on Determinism for a detailed account of his argument). This does not mean, however, that Born wishes to replace the certainty of classical laws with subjective probabilistic ones. It could just as easily be a realist commitment to objective, although probabilistic, laws of nature. In this light we can interpret his commitment to fundamental probability laws as a commitment that to the idea that the probabilities themselves are features of the physical world. In this way we certainly have a kind of propensity theory.

De Finetti does though think that there is an alignment between Born's treatment of quantum mechanics and his notion of exchangeability. He writes 'the notion of "mixture of pure cases" coincides with the one derived from my definition of 'exchangeability', a notion that (apart from its relevance for applications) serves to express in an intrinsic and exact manner the case which normal usage describes as one "of independence with constant but known probability"' (De Finetti 1978 p126). He then goes on to elaborate on the notion of exchangeability vs independence, writing 'To take a trivial example: if I extract poll balls and them return them to a ballot box, the composition of the contents of which is unknown, the extractions *would be* independent if I knew the percentage of black and white balls; if I *do not know it* each extraction is *informative*, increases the probability of compositions richer in balls of that colour, and will all told almost certainly tend towards an asymptotic value corresponding to the observed frequency and coinciding with that of the true percentage' De Finetti 1978 p126).

So the idea is this: we don't know that the ball extractions are independent because we don't know anything about the composition of the ball-box system. However, such a system is exchangeable, so the order in which we pull balls out of the box doesn't matter. Hence every ball-pulling is equally informative and contributes equally to generating the "correct" probability distribution for the balls. This is no doubt a decent argument for the utility of exchangeability as a concept or formal definition. The problem with it as an argument for subjectivism is that it is not at all clear that only subjectivists about probability can use exchangeability. It seems that we can quite happily define it as a formal property of some (but not all) independent sequences and leave it at that.

He then goes on to quote Born again, saying: 'In Born's words (*ibid*) "each new observation annihilates the former distribution of probability and substitutes another one; thus we have the phenomenon of the 'reduction of probabilities' that I have already mentioned and to which Einstein took exception"' (De Finetti 1976 p126-7, quoting Born 1965 163). De Finetti does not deign to furnish this quote with further explanation, presumably considering in conclusive.

So I think that de Finetti must be attempting to draw a direct analogy between the mixture of pure cases, exchangeability, and the reduction of probabilities in quantum mechanics. At least, he doesn't give us anything else to go on, so it seems fair to presume that his argument rests on some supposed similarity of them. First off, the reduction of probabilities that Born refers to is this: that a wave-function is changed into a different one on measurement. Earlier in the article that de Finetti is quoting from, Born writes of the reduction 'a state represented as a wave-function in configuration space (more generally: a vector in the Hilbert space) is turned into another one by experimental interference' (Born 1965 162). We can see that the idea in quantum mechanics of each measurement 'destroying' the previous probability distribution (i.e. the collapse or projection postulate) and replacing it with a new one coincides with exchangeability in the following way—

each measurement is equally informative with regards to the system, just as is the case if exchangeability is applicable. A mixed case in quantum mechanics is an ensemble of wave-functions, or vectors in Hilbert space. It is, importantly, not the same thing as a superposition of those wavefunctions (Rae 2005 277). The pure state is what some system ends up in after measurement, i.e. when properties of that state are known definitely. A measurement is also *not* a transition from a mixed state to a pure one. In fact, as Rae tells us: 'In the context of ensembles, therefore, collapse corresponds to a transition from a pure state to a mixture'. It's not clear how applicable the subjective notion of exchangeability is here: we might already have to have some objective knowledge of the system before we can know that it is in a mixed state, rather than a pure one. (see Rae 2005 277). So it seems inconclusive that Born is aligned with de Finetti on this basis, especially if we consider that he already disagrees with him on operationalism. I also want to note that I'm not arguing that de Finetti is wrong (or right, for that matter) to say subjectivist view of probability might make some sense of quantum mechanics, just that he doesn't give us much to suggest that Born holds such a view.

I also think that even if de Finetti is correct about exchangeability being appropriate in this context (and he says so little that it's hard to tell exactly how he thinks it applies) I'm not convinced that the notion of exchangeability is *uniquely* tied to a subjective interpretation. All it means is that in some sequences, ordering does not matter.

Galavotti, on the other hand, does think that de Finetti may be correct about this point. She explains that exchangeability works if we start from the presumption of subjectivity, rather than objectivity. This may well be correct, but given that there's good evidence that Born has an objective view of probability, I don't think that it cuts against my argument. She goes on to say 'Going back to the analogy of a mixed state, this is indeed striking with respect to such authors as Born and Heisenberg, especially in view of Heisenberg's claim —recalled above—to the effect that there are two components of a probability function in

quantum mechanics: a subjectivist one and an objectivist one' (Galavotti 1995 131). On this matter I would say this: Galavotti specifically refers to a point of Heisenberg's here, not of Born's.

One criticism of my argument is this: It may well be the case that Born is not an operationalist about theoretical concepts in general, but he could be an operationalist about probability. Perhaps he could reject de Finetti's positivism but still think that probabilities can only be understood in terms of their observable frequencies. But I don't think so, as I shall argue next.

3   Objective Interpretations of Probability

3.1   The Frequency Theory

The frequency theory of probability was developed in the 20th century by Richard von Mises and Hans Reichenbach. Both Gillies (2000 88) and Galavotti (2005 71-81) note that it had precursors in the 19th century. The frequency theory takes probability to be a mathematical science, the subject of which is 'mass phenomena and repetitive events' (von Mises 1957 vii). Gillies notes the contrast to the subjective theory—rather than considering probabilities assigned by single individuals to single events, the frequency theory takes probabilities to be measurable, objective quantities, defined by the behaviour of systems consisting of a very large number of parts or by a single event repeated a very large number of times.

Crucial to von Mises' frequentism is the notion of a collective. He does not think that it makes sense to talk of probabilities with regard to individuals. We can only talk about them with reference to collectives, which he defines as follows: 'a sequence of uniform

events or processes which differ by certain observable attributes, say colours, numbers or anything else' (von Mises 1957 12). So a collective can be a group of anything that we want—the crucial thing is that there exists an observable common characteristic amongst all of its members.

Von Mises developed the theory from two 'empirical' laws of probability. He describes the first in the following way:

> It is essential for the theory of probability that experience has shown that the game of dice, as in all the other mass phenomena which we have mentioned, the relative frequencies of certain attributes become more and more stable as the number of observations is increased. (Von Mises 1928)

Gillies terms this the '*Law of Stability of Statistical Frequencies*' (Gillies 2000 92). So as the number of observations of a particular process tends towards a very large number the frequency of a particular outcome tends towards a stable quantity from which a probability for that outcome can be derived.

Von Mises' second empirical law concerns the failure of gambling systems. He explains it in the following way:

> The Authors of all such systems have all, sooner or later, had the sad experience of finding out that no system is able to improve their chances of winning in the long run, i.e., to affect the relative frequencies with which the different colours or numbers appear in

> a sequence selected from the total sequence of the game. (Von
> Mises 1957 25)

So the relative frequencies found in long runs of a particular game remain the same even for a smaller subsequence of the whole game. Gillies terms this the *Law of Excluded Gambling Systems*.

From these empirical laws Von Mises produces the limiting frequency definition of probability. Gillies states this as follows:

> Axiom of Convergence
>
> Let A be an arbitrary attribute of a collective **C**, then $\lim_{n \to \infty} m(A)/n$ where m is the number of times a result is obtained in n goes of a sequence.

We can know make the following definition:

> The probability of A in C[PAC] is defined to be $\lim_{n \to \infty} m(A)/n$

This is the limiting frequency definition of probability. So on the frequency theory we can now take the meaning of the probability of a particular outcome event to be the limiting frequency with which that outcome occurs as the number of events tends to infinity. In this way, probabilities are conditional not upon the beliefs of an individual as in the subjective theory, but on the attributes of a particular collective. One important consequence of this definition is that we are only allowed to introduce probabilities for situations which belong to a genuine empirical collective, i.e. ones which form part of a set of mass events or long sequence so that the number of those events is very large. We cannot postulate

probabilities for events which are repeated only a few times and we certainly cannot postulate probabilities for single events (Gillies 2000 118).

So could Born hold a frequency theory of probability? We have seen that he holds an objective view of probability and so fulfils at least one necessary condition to be a frequentist. But does he identify probabilities with frequencies? Well, he *might*. Two comments form the Born-Einstein Letters (2005) suggest something of this sort:

> To say that ψ describes the 'state' of one single system is just a figure of speech, just as one might say in everyday life: 'My life expectation (at 67) is 4.3 years'. This too is a statement made about one single system, but does not make sense empirically. For what is really meant is, of course, that you take all individuals of 67 and count the percentage of those who life for a certain length of time. This has always been my concept of how to interpret $|\psi|^2$. (letter sent in 1950 in Born 2005 182).

> Einstein admits that one can regard the 'probabilistic' quantum theory as final if one assumes that the ψ-function relates to the ensemble and not to an individual case. This has always been my assumption as well, and I consider the frequent repetition of an experiment as the realization of an ensemble. This coincides exactly with the actual procedure of the experimental physicists, who obtain their data in the atomic and sub-atomic area by accumulating data from similar measurements. (Commentary on a letter of Einstein's sent in 1953, Born 2005 206).

This certainly looks like Born thinks of probabilities as being frequencies. It's also the case that he seems to have read at least some of Von Mises' work. He refers to von Mises' book *Probability, Statistics and Truth* (von Mises 1957) in a footnote of *On the Meaning of*

Physical Theories (Born 1928). He writes in *Einstein's Statistical Theories* 'I have seen no definite statement of his about the question 'What is Probability?' [unfortunately Born also neglects to give us his answer to this question, which would have saved a lot of work]; nor has he taken part in the discussions going on about VON MISES' definition and other such endeavours. I suppose he would have dismissed them as metaphysical speculations, or even joked about them' (Born 1949b 90). So Born might be alluding to Von Mises' frequencies in his letters to Einstein. On the other hand, we might ask why, if Born has read and agrees with Von Mises, why he does not say so here? I think though, that if there is anything that I have learned about Born it is that it he is rarely that explicit.

I think that it is far from conclusive from these points as to whether or not Born is a frequentist. Undoubtedly he holds that probabilities are objective and that the probabilities that quantum mechanics gives us are observed as frequencies. However we should remember, as I will discuss in detail in the following section, that we can also understand frequencies as arising from propensities. Given that this is the case, in order to fully commit Born to frequency interpretation, we would have to show that he thinks probabilities are observed frequencies and *nothing else* and I don't think that we can. I think that it is also the case that a lot of what would appear to commit Born to a frequency interpretation—a belief in objective probabilities and a belief that probabilities ought not to be understood in the single case—also count as evidence of a long-run propensity interpretation.

3.2 The Propensity Theory

The propensity interpretation is another objective interpretation of probability, defining probabilities as some kind of property of an object or system or event which gives rise to a range of outcomes. We might think of them as being dispositional properties of a system to produce some particular frequency of outcomes. To distinguish this from a frequency interpretation we might say that whereas frequentism identifies probabilities with

frequencies, a propensity interpretation seeks to *explain* frequencies as having arisen from some physical characteristic, the propensity, of a given system. They seem to have their origin in some work of Popper's that sought to account for probabilities in quantum mechanics (Von Plato 1994 15). Hájek (2012) and Galvotti (2005) trace the idea back to Peirce.

We might wish to distinguish, as Gillies does and Hájek recommends, between single-case and long-run propensity theories. As we have seen above, one feature of the frequency theory is that it does not describe probabilities for single events, only for limiting frequencies of collectives. There are in quantum mechanics, and indeed otherwise, situations that look very much like single event probabilities—consider the decay of a single particle. Single-case propensity theories attempt to explain this by taking propensities to be tendencies for some individual outcome. Single-case probabilities might seem to be a somewhat odd idea—Hájek notes that it is 'prima-facie' clear that they cannot obey the probability calculus (Hájek 2012). We might thus question whether or not it makes sense to talk of such things as probabilities at all.

Gillies (2010 119) notes that there is a major problem for objective theories that wish to admit single-case probabilities, *the reference class problem*. The problem is this—if we want to assign a probability to a single case, we need to decide what reference class that case belongs to. Gillies uses the example of trying to assign a probability to the survival of a forty year old man to his forty-first birthday. The probability will depend on whether we assign him to the class of all forty year old men, or forty-year old men living in a particular country. Still further, we might ask whether or not he belongs to the class of forty-year old men who smoke, who have hazardous jobs, who do regular exercise, who do regular exercise but that exercise is mountain climbing and so on. The problem is how to objectively assign our subject to the correct reference class. Galavotti (2005 117) also notes that we might also want to involve further considerations like there being relevant qualitative but not quantitative data. She considers the reference class problem to be inescapable. Gillies considers some solutions, in particular that we might always assign

our subject to the narrowest reference class for which there exists sufficient statistical data but notes that there might still be subjective qualitative considerations such as personal knowledge of the subject (2010 121).

In any case, single-case theories are not the only sort of propensity theory. There are also long-run propensity theories. Gillies defines them as follows: 'A long-run propensity theory is one in which the propensities are associated with repeatable conditions, and are regarded as propensities to produce, in a long series of repetitions of these conditions, frequencies which are approximately equal to the probabilities' (Gillies 2010 126). I think that we can understand this in the following way: if we understand propensities as dispositional properties in general, we can understand a long-run propensity as a dispositional property that is associated with some particular repeatable condition, for example the throwing of a balanced die in a windless environment. We might have a different propensities associated with the same object for different conditions. We can make the differentiation of these propensities as fine-grained as the differences in conditions that affect the frequencies generated by their repetition.

Interpreting probabilities with repeatable conditions rather than collectives actually sounds almost exactly in line with how Born talks about frequencies—recall how he writes that 'This has always been my assumption as well, and I consider the frequent repetition of an experiment as the realization of an ensemble' (Born 2005) in a letter to Einstein.

So propensities interpret probabilities as being rooted in some physical or dispositional property of a system that is carried with it throughout different situations. If we are to look for this interpretation of probability in Born, we would look for statements which identify probabilities with objective physical properties of (quantum) systems. I think that this is all that is (minimally) required to define Born as a propensity theorist about probabilities. Given Born's remarks criticising the interpretation of probabilities in the single-case, I

think if we can find evidence that he holds a propensity interpretation of probability, we can safely say that it is a *long-run* propensity interpretation. I think that it is clear that Born does hold an objective view of probability. In order to argue that this view is a propensity theory, as opposed to a frequency theory, I'm next going to examine some of Born's writings on quantum mechanics.

We might also briefly consider a pluralist interpretation of probability. We could for example, argue for a subjective interpretation in all cases apart from those few in fundamental physics, such as radioactive decay, for which a subjective interpretation seems implausible, and in those cases argue for a propensity interpretation. With regards to Born, he does not really say anything about probability outside of physics, so there is no evidence to indicate that he holds such a view. On the other hand, since he says nothing on it, there is no reason to think that he did not hold such a view. I'll therefore say no more about it.

4   Quantum Mechanics and Probability

The importance of probabilistic and statistical physics is a theme that runs through Born's career from at least his work on quantum mechanics onwards. Probabilities underpin his arguments for how one can derive causal direction (antecedence in his terminology) from physics (Born 1949a) and let us not forget that it was he who first introduced probabilities into quantum mechanics proper by proposing a statistical interpretation of the wavefunction.

In Born's 1926 paper *On The Quantum Mechanics of Collisions* (Born 1926a), he showed how to determine the result of an interaction between a free particle and an atom via means

of a perturbation theory calculation. Born interpreted the resulting superposition of solutions probabilistically: This is the statistical interpretation of the wave function.

At this point in time there existed two different versions of quantum mechanics - the matrix mechanics developed in Göttingen by Heisenberg, Born and Jordan; and the theory of wave mechanics developed by Erwin Schrödinger. Although they had been proven to be formally equivalent when Born wrote his collisions paper, it was not yet clear what the physical meaning of this equivalence was (Rosenfeld 1971) . Schrödinger's theory was something of an attempt to return to the classical concepts thrown aside by Heisenberg. Born tried and failed to solve the problem using matrix mechanics, but succeeded with Schrödinger's formulation of the theory. Born notes that 'exactly for this reason I might regard it as the deepest formulation of the quantum laws' (Born 1926a).

Born's use of wave mechanics was in itself was something of a controversial move. Although it had recently been proved that wave and matrix mechanics were equivalent theories (Schrödinger 1926) it was not clear how exactly one was able to switch between them. It was also the case that there was a social division within the scientific community between the practitioners of the different formulations of quantum mechanics. There was little academic exchange between them. In fact wave and matrix mechanics papers were not even published in the same journals. Matrix papers went to *Zeitschrift fur Physik* and wave papers to *Annalen der Physik*. Zeitschrift was associated with more liberal ideas, compared to Annalen's conservative background (Kragh 1999). The solution that Born presented in his collision paper, which solved a problem using the wave functions of wave mechanics and interpreted the solution in terms of the particles and transition probabilities of matrix mechanics, was therefore quite unprecedented.

Born explains in the paper that collision processes in quantum mechanics are not easy to calculate. It is not possible to have a simple classical-style solution in which one simply

picks out the states in the interacting systems that one is interested in and calculates how one is influenced by the other. When the systems of the atom and the particle interact, all states become coupled in a way that is too complex to allow the simple plucking out of relevant information. However, we also know that there is a point after the collision at which the electron has reached sufficient distance from the atom for the interaction of the two systems (and hence the coupling of their states) will have decreased to the point where the atom has a definite state and the electron a definite rectilinear motion (Born 1926a 23). The question is, how do we formulate mathematically the behaviour of these particles?

Born's solution is as follows. We treat the electron as an incoming plane wave that scatters off an atom. Although the waves produce a complex interaction when collision occurs, a solution should be calculable when the electron goes off to infinity (1926a 53).

So one has to solve the Schrödinger equation for a system of an atom and an electron with the boundary condition that 'the solution in a preselected direction of electron space goes over asymptotically into a plane wave with exactly this direction of propagation' (Born 1926a 53) This boundary condition represents the arriving electron coming in from infinity to collide with the atom. In such a system the behaviour of the scattered wave at infinity will give us a description of the motion of the electron after the collision.

We let $\Psi_1^0(q_k)\,\Psi_2^0(q_k)$ be the eigenfunctions of the atom prior to the collision, making the assumption that we have a discrete spectrum.

Prior to the collision we model the eigenfunctions of the electron as a continuous manifold of plane waves.

$$\sin(2\pi/\lambda)(\alpha x + \beta y + \gamma z + \delta)$$

From de Broglie we can give the energy of the electron as

$$\tau = h^2/(2\mu\lambda^2)$$

The state of the electron arriving from the +z direction prior to the collision can be thus given by the eigenfunction

$$\psi^0_{n,t}(q_k, z) = \psi^0_{(q_k)} \sin(2\pi z/\lambda)$$

Born defines the potential energy of the collision as $V(x, y, z; q_k)$ By applying a first order perturbation (which Born does not give in the first paper, but provides a more mathematically comprehensive description of in a second (Born 1926b) the scattered wave resulting from the perturbation as z→∞ is given as

$$\psi^1_{(n,t)}(x, y, z; q_k) = \sum_m \iint_{\alpha x, \beta y, \gamma z > 0} d\omega \, \phi_{n_\tau, m}(\alpha, \beta, \gamma) \sin k_{n_\tau, m}(\alpha x + \beta y + \gamma z + \delta) \psi^0_m(q_k)$$

In which dω is an element of the solid angle in the direction of the unit vector, the elements of which are α,β,γ and Ψ$_{n\tau m}$(α,β,γ)is the wave function in which the information regarding the position of the electron is given. This is what was later called the differential cross section (Jammer 1966 284).

Born concludes from this calculation that, if one wishes to understand this result in terms of particles 'only one interpretation is possible. The amplitude (or rather the *square of the amplitude*, as was added in proof after 'more careful consideration' (Born 1926a) of the wave function gives the probability that the incident electron is thrown out in the angle indicated by the coefficients α, β or γ.

So Born has produced a solution to the problem of collisions in quantum mechanics. But it is not a definite solution. As he puts it 'One gets no answer to the question "what is the state after the collision", but only to the question, "how probable is a specified outcome of the collision"' (Born 1926a 24). We cannot say precisely what the result of such an interaction is, but merely say how likely it is that a particular thing should occur. Born

thinks that this sounds a death knell for determinism in quantum mechanics. 'I myself', he writes 'am inclined to give up determinism in the world of atoms.' (Born 1926a 54)

So, the relevant question here is what, if anything, this tells us about what Born actually thinks probabilities represent. Although Von Mises' first paper on the frequency interpretation was published in 1919, there appears to be no evidence that Born was aware of it at this point in time (or at the very least gives any thought to with regards to this paper) and given that the subjective theory was not published until the 1930s and the propensity theory until the 50s it would be wrong to ascribe to Born any intention of interpreting quantum probabilities in line with a particular probability theory. However it is quite legitimate to ask whether or not his work on collision processes implies or expresses a particular view of probability and whether or not such a view is in line with one of the probability theories.

We can try to get some idea of this by looking at Born's argument for and interpretation of his results, and seeing if they go beyond the minimum interpretation required and if there are alternate interpretations available.

Born's argument can be summarised as follows. We want to solve the problem of collision processes between a free particle and an atom. Matrix mechanics (at this stage at least) is suitable for solving periodic processes but unsuitable for solving asymptotic ones, such as collision. However using wave mechanics, we can regard such collision processes as being analogous to the diffraction of an incoming plane wave and so produce a solution. Born then interprets the wave intensity at a particular angle as giving the probability density for the position of a particle and not, as Schrödinger does, as giving the density of a distribution of matter (Rosenfeld 1971 50). This allows Born to preserve the notion of a particle albeit at the cost of the notion of a determinate trajectory for a particle. Importantly, it also provides a physically meaningful link between wave and matrix

mechanics—as Rosenfeld puts it 'it established a complete harmony between the statistical meaning of the wave intensity and the statistical character of the rules of quantum algebra for the calculation of transition probabilities' (Rosenfeld 1971 51).

So, is Born saying anything more here than is minimally required to solve collision problems? Well, he does not *need* to give the particle interpretation. He could just leave it at the solution of the scattered wave from which the wave intensity at a particular angle can be derived. However Born's particle interpretation is only unnecessary if one assumes that there are no particles for there to be an interpretation about. Otherwise it is simply the answer to the question of what the solution says about particle behaviour in collisions. So it is not clear that this says anything about Born's views on probability; rather it tells about Born's commitment to particles and to the physical significance of the mathematical equivalence between matrix and wave mechanics.

In 1927 Born published a paper in Nature entitled *Physical Aspects of Quantum Mechanics*. The paper was an extension of one given to Section A (Mathematics and Physics) of the British Association in August 1926. In it Born made 'An attempt to understand the physical significance of the quantum theoretical formulae.' via considering how quantum mechanics answers the question of 'the course of phenomena' in a system when the equilibrium of that system is disturbed (Born 1927). This, he says, is the question with which classical mechanics deals almost exclusively and is the one which quantum mechanics mostly ignores. Quantum mechanics, of course, answers that question statistically. But, as Born points out in a theme that runs through *Natural Philosophy of Cause and Chance* (Born 1949a), so can classical mechanics. Born notes that 'it always made use of certain statistical considerations. As a matter of fact, the occurrence of probabilities was justified by the fact that the initial state was never exactly known; so long as this was the case, statistical methods might be, more or less provisionally, adopted' (Born 1927 7).

First Born considers how classical physics deals with probabilities. Derivations of probabilities in classical mechanics, Born says, start if possible with the assumption that we can consider certain cases equally possible. An initial distribution is produced from this assumption. It is then attempted to prove that the final distribution—that which is to be predicted and observed—is independent of the initial distribution. He says something very interesting about the assumption of an initial equiprobable distribution—that it is only justifiable in an a posteriori way from the success of the assumption in explaining some observable phenomenon (Born 1927 7). My point here is to emphasise that Born does not think that the principle of equally probable distributions can be an a priori one.

Born notes that the situation is similar for all cases in which statistical theories are used in physics. Consider, he says, the case of a collision between an atom and an electron described using the rules of classical mechanics. We know that the collision will be elastic and that the electron will lose no energy in (at least) the following case: when the kinetic energy of the electron is less than the first excitation potential of the atom. Once we know this, we can try to calculate the direction of deflection for our electron. On the classical model such collisions are causally determined: if one knew the position and velocity of all the electrons in the atom and that of the incoming electron then one could calculate precisely the angle of deflection prior to the collision. However, the physicist cannot practically obtain all of this information and so we instead have to use average positions and velocities and in order to obtain these averages we once again must make the assumption of an initial distribution of equally probable states. He writes that the fact that this assumption must be made is 'usually forgotten' (Born 1927 8) (Born gives a more detailed examination of the implications of precise information being unobtainable in a later paper—for a discussion see Chapter 5 on determinism). Born says that we do this by describing the initial path of the incoming electron using a coordinate system of 'angle variables and phases, and by treating equal phase intervals as equally probable' (1927 8). But this assumption is just that— an assumption made so that the averages are calculable. For Born, the only justification for it can be through the success of the technique (1927 8).

Born notes that there is something peculiar about this set-up: practically speaking, the microscopic coordinates 'do not exist' (Born 1927 8). They are introduced purely 'to keep the individual phenomena at least theoretically determinate' (Born 1927 8). The only thing that is observed is the number of particles deflected in a particular direction. The part of the path in which the collision occurs is not observable. BUT, Born says, we can gain knowledge about how the collision behaves from the observed statistical distribution. Born gives the example of how Rutherford could prove that Coulomb's law held for the interaction between α particles and an atomic nucleus from the statistics of the dispersion of the α particles after collision (Born 1927 8). The microscopic coordinates, which in this case describe the distance of the nucleus from the original path of the alpha particle, are eliminated in the final formula of the distribution by averaging over all of their values.

In this way, we can actually analyse mechanical concepts like force statistically. Born writes that 'these considerations lead us to the idea that we could replace the Newtonian definition of force by a statistical one...The magnitude of a force, classically measured by the acceleration of a particle, would here be measured by the inhomogeneity of an assembly of particles' (Born 1927 9). The problem for Born here is that we now have two definitions of force that need to be unified and the problem for the statistical theory is the necessity of assuming an initially equiprobable distribution, which for the aforementioned reasons Born finds problematic.

Born's point here is that the situation in quantum mechanics, epistemically speaking, in terms that the experimentalist can measure, is not so different from the situation in classical mechanics. What makes the quantum formulation distinct is that whereas classical physics introduces microphysical coordinates only to eliminate them from the final result by averaging over all of their values, quantum mechanics achieves its results without ever introducing them at all. This is an early statement of an idea that Born discusses again in

Natural Philosophy of Cause and Chance, that in some significant respects, quantum mechanics is more satisfying than classical mechanics despite, or perhaps even because of, its banishment of determinism from the microscopic. He goes on to say:

> The quantum theoretical description of the system contains certain declarations about the energy, the momenta, the angular momenta of the system; but it does not answer, or at least only answers in the limiting case of classical mechanics, the question of where a certain particle is at a given time. In this respect quantum theory is in agreement with the experimentalists, for whom microscopic coordinates are also out of reach, and who therefore only count instances and indulge in statistics. This suggests that quantum mechanics similarly only answers properly-put statistical questions and says nothing about the course of individual phenomena. It would then be a singular fusion of mechanics and statistics. (Born 1927 9)

So this would actually seem to be quite in line with the frequency theory. Although we may talk about 'the electron' in the formalism of the theory this is not physically meaningful because the theory never actually deals with single events. However, this by no means uniquely ties Born to a frequency interpretation—it is perfectly compatible with long-run propensity theories as long-run propensity theories also reject the validity of single-case probabilities.

Einstein's Statistical Theories (Born 1949b) was published as part of the *Library of* Living Philosophers volume on Albert Einstein, *Albert Einstein: Philosopher Scientist.* In it, Born tries to present a coherent account of Einstein's views on statistics and to demonstrate (in a friendly way) that it was his work on probabilities that laid the foundations for the indeterminism in quantum mechanics, presumably much to the annoyance of the great

man. We should note that Born is not the only person who thinks this: Martin J. Klein (1979) argues similarly that Einstein's work in the 1900s laid the ground for the development of quantum mechanics in the 1920s. Born starts with Einstein's early contributions to classical statistical mechanics in 1905. Born regards the 'fundamental' step that Einstein made in his Brownian motion paper to be showing that the statistical character of molecular motion has observable consequences. He notes that 'these investigations of EINSTEIN have done more than any other work to convince physicists of…the fundamental part of probability in natural laws' (Born 1949b 82). With regards to quantum mechanics Born similarly notes that 'This statistical reasoning is very characteristic of EINSTEIN, and produces the impression that for him the laws of probability are central and more important by far than any other law' (Born 1949b 83).

So what would indicate Born holding a particular interpretation of probability? He is certainly not giving an account of probability in general. What I'm interested is whether or not Born has a particular understanding of probabilities in quantum mechanics. So are there particular hallmarks of the various interpretations of probability that would indicate that Born's thoughts are in line with it.

It seems quite clear that Born regards quantum mechanics, and by extension physics, as being fundamentally indeterministic. He writes in his 1926 collisions paper that 'I myself am inclined to give up determinism in the world of atoms' (Born 1926a p54). We can see it again his discussion of determinism in classical mechanics, in which he rails against a return to such a deterministic world view (Born 1953 and see also Chapter 5). We can also find this view in *Natural Philosophy of Cause and Chance* (Born 1949a). He writes that although it would be 'silly and arrogant to deny any possibility of a return to determinism' as any theory will undergo change and modification due to encountering new problems he would 'never expect that these difficulties could be resolved by a return to classical concepts [i.e. determinism]' (Born 1949a 108-9). He also refers to Von Neumann's (as it turns out erroneous—see Bell 1966) proof that hidden variable theories are inadmissible

into quantum mechanics as 'a more concrete contribution' to the question of whether or not physics could return to determinism. The implication of Von Neumann's proof was that quantum mechanics as it was was not consistent with hidden variables—one would have to replace the theory with something completely different. Von Neumann concludes after giving his proof that 'd is therefore not a question of a reinterpretation of quantum mechanics, - the present system of quantum mechanics would have to be objectively false, in order that another description of the elementary processes than the statistical one be possible' (von Neumann 1932 325) Of course, it turns out that Von Neumann was incorrect about this as John Bell states 'On the Einstein-Podolsky-Rosen Paradox' (Bell 1964) and proves in (Bell 1966) but Born would not have been aware of Bell's result in 1948. Born concludes by noting that he thinks that the 'indeterministic foundations [of quantum mechanics] will be permanent' (Born 1949a 109).

This fairly strongly suggests that Born has an objective view of probability. If one believes that the physical world is fundamentally indeterministic and that probabilistic calculations and predictions can be made, it is hard to see how one could think otherwise. This point is backed up by some remarks of Born's in the 'Metaphysical Conclusions' of *Natural Philosophy…*. In this section Born notes that 'even an exact science like physics is based on fundamental beliefs' (Born 1949a 123). These are 'fundamental assumptions which cannot be further reduced but have to be accepted by an act of faith' (Born 1949a 123). These are things that Born thinks we must simply accept. Amongst them is 'the belief that the predictions of statistical calculations are more than an exercise of the brain, that they can be trusted in the real world' (Born 1949a 124). Again this leads towards an objective view of probability.

I think that we have a clear argument that Born holds probabilities to be objective—he is committed to the fundamental indeterminism of quantum mechanics and believes that statistical predictions are not just in the mind. Hence, he must have some kind of propensity or frequency interpretation. We've already seen that Born rejects probabilities

in the single case, so if he holds a propensity interpretation, in must of the long run variety. What then is the difference between a frequency theory and a long-run propensity theory? A frequency theory takes probabilities to be properties of collectives. Long-run propensity theories regards probabilities as properties associated with repeatable conditions that to give rise to frequencies.

These two can be distinguished by where they locate the probability. In a frequency theory, the probability simply is the frequency of an event, or the limiting frequency of the event over an infinite run. The two are identified. This is why Gillies thinks that (2000 100-101), even in the infinite group version, the frequency theory is still operationalist—it defines a theoretical concept (probability) in terms of an observable one (frequency). Indeed, von Mises writes in *Probability Statistics and Truth* 'a quantitative probability concept must be defined in terms of potentially unlimited sequences of observations or experiments. The relative frequency of the repetition is the 'measure' of probability, just as length of a column of mercury is the 'measure' of temperature' (1957 vi). Now there, is a question about whether or not a long-run frequency is *really* observable, but with regards to Born I think we can let this go—he is not at all motivated by operationalism. In the long-run propensity theory, the probabilities are properties of states that give rise to those frequencies.

I'm now going to give an argument by Cartwright (1987) that Born holds a propensity view of probabilities in quantum mechanics.

4.1 Cartwright on Born

We've seen in the preceding section that Born clearly holds the indeterminism of quantum mechanics to be fundamental. This, on its own, is not enough to indicate that Born believes in propensities. Belief that quantum mechanics is fundamentally statistical probably

indicates a belief in objective, rather than epistemic, probabilities but it could just as easily indicate a propensity for a frequency interpretation on Born's part. This is because Born might simply hold the laws of physics to be fundamentally probabilistic in nature. If we are to argue that Born believes in something like propensities, we need to argue that he thinks of probabilities as being properties of systems, even if they are properties to give rise to frequencies.

In her 1987 paper, *Max Born and the Reality of Quantum Probabilities*, Nancy Cartwright argues that Born holds a propensity view of quantum probabilities that is rooted in his realism regarding particles and his rejection of the reduction of the wave packet. Cartwright notes what Born says of quantum mechanics in his (1926a) 'We free forces of their classical duty of determining directly the motion of particles and allow them instead to determine the probabilities of states'. This might simply be a manner of speaking—a way of saying that the trajectory of the particles is determined by probabilistic, and not deterministic laws. Indeed, Cartwright says that this was her initial thought (Cartwright 1987 409). But it could also indicate something else—that the probabilities themselves are real things—properties of the system that could be acted on by forces. It is this propensity interpretation that Cartwright argues for. She writes 'For Born, *probabilities are real in a special way*' (Cartwright 1987 410).

Here I'm going to give an overview of Cartwright's argument, which is both plausible and is not contradicted by Born's other work. The thrust of her argument is that that Born is not a realist about the superposition of the wavefunction but is a realist about probabilities in quantum mechanics. Hence he does not believe in the collapse of such superpositions due to measurement. Instead, Born believes in particles and ontologically significant state probabilities. I'm going to further suggest that its conclusion can be strengthened by examining an exchange of letters between Born and Einstein.

Consider, she says, Born's views on the superposition of the wavefunction as given in his two papers on the quantum mechanics of collision processes (*Zur Quantenmechanik der Stossvorgange* and *Quantenmechanik Der Stossvorgange* (Born 1926a and 1927a, respectively). Crucial to this view, Cartwright argues, is that Born solves problems in terms of particles—for him, these represent the reality of the situation, rather than superpositions. She draws three relevant points from Born's 1926 papers (Cartwright 410-11). The first is that although Born uses Schrödinger's wave mechanics to solve the problem of collisions in quantum mechanics, he interprets the solution in terms of matrix mechanics. The second is that Born understands both the initial set-up and the solution to the problem in terms of particles, and that this is crucial to the probabilistic interpretation of the wavefunction that he gives. The third is that Born uses a perturbation theory to construct a quantum-mechanical theory of collisions.

For Cartwright the importance of the first point is that the probabilities that Born interprets $|\psi|^2$ as giving are transition probabilities. Born does not have to interpret them in this way. Cartwright points out that an alternative would be to view them as simply giving distributions of the values of the dynamical variables, as in classical statistical mechanics. We know that he views them like this because their status as transition probabilities between different quantum states is made clear in Born's Nobel prize lecture. In this lecture he states this explicitly: 'Instead of describing the motion by giving a co-ordinate as a function of time, $x(t)$,an array of transition amplitudes x_{mn} should be determined' (Born 1954 259).

Importantly, Cartwright thinks, the probabilities to which Born refers are transition probabilities, i.e. the probability for the system to move from one state to another. Not only are they transition probabilities, but they are the probabilities for the transition from one eigenstate to another, not for the transition of a superposition to an eigenfunction. Why is this? Because Born thinks that the eigenfunctions that represent the physical states of the

system, but superpositions do not. We should note that this implies that Born rejects the standard formulation of quantum mechanics in which if A and B are states of the system, then so is any linear combination of them. i.e. he rejects the principle of superposition. Cartwright here quotes *Physical Aspects of Quantum Mechanics* to make this point clear:

> Every state of the system corresponds to a particular characteristic solution, an *Eigenfunktion*, of the differential equation; for example, the normal state of the function ψ_1, the next state ψ_2 etc. For simplicity we assume that the system was originally in the normal state; after the occurrence of an elementary process the solution has been transformed into one of the form $\psi = c_1\psi_1 + c_2\psi_2 + c_3\psi_3....$, which represents a superposition of a number of *eigenfunctions* with definite amplitudes c_1, c_2, c_3 ...Then the squares of these amplitudes c_1^2, c_2^2..., give the probability that after the jump the system is in the 1,2,3, state. Thus c_1^2 is the probability that in spite of the perturbation the system remains in the normal state, c_2^2 the probability that is has jumped to the second and so on (Born 1927 10).

This makes it fairly clear that does Born does indeed consider transitions as being between eigenstates. Cartwright also notes that Born only refers to the superposition as a 'solution'. She does not think that he takes it to be 'a representation of a real physical state'. (Cartwright 1987 413).

She quotes Born again, this time from *The Adiabatic Principle in Quantum Mechanics* '...It also will not do to speak of the simultaneous existence of more states of one does not want to give up the natural significant of Wilson cloud chamber streaks and related occurrences as the passing of corpuscles' (Born 1926c 169). This, for Cartwright, is important. She tells us: 'Born's view about superpositions is intimately connected with his

knowledge that there are particles, and that we observe their trajectories' (Cartwright 1987 413).

Cartwright does, though, point out a potential problem with this view, drawing on Max Jammer: if he does not think that superpositions are real, how does Born account for things like the two-slit experiment, in which interference between wavefunctions of electrons affects their distribution post-diffraction? Jammer takes the interference of wavefunctions to imply that the wavefunction must be real and not simply some mathematical artefact or a mere reflection of our ignorance. But how can we understand the result of the two-slit experiment if superpositions are not real things? How does Born account for the fact that, as Cartwright puts it 'the superposition is required to predict the correct probabilistic behaviour of the system'? (Cartwright 1987 414).

She explains it thus: 'There is a state that is the actual physical state of the system (recall that this is not a classical state with well-defined rules, but is itself a quantum state obeying the uncertainty rule) and there is a different state that describes the probabilistic behaviour of the system' (Cartwright 1987 414). Given that Born believes that superpositions are not physically real, he must take the probabilities that govern the behaviour of the system to be real instead. Cartwright also notes that Born's position seems be aligned with the modal interpretation of quantum mechanics (Cartwright 1987 414).

She thinks that Born's view can be expressed as follows 'particles not only have their physical quantum state, but they also have physically real propensities over and above this state' (Cartwright 1987 414). This is cashed out in two important ways. The first is is that 'Two systems in the same eigenstates may nevertheless be correctly represented by different Schrödinger superpositions. That means that systems can be in identical states and subject to identical forces and yet have different probabilistic behaviour' (Cartwright 1987 414-5). The implication of this is is that the propensity is a *separate* property of the

system from that represented by the quantum state. The second point is that as the Schrödinger equation evolves in time, so do the probabilities that it gives (Cartwright 1987 415). As Born does not believe in superpositions and hence cannot believe in their collapse either, he must also hold that it is the probabilities themselves that evolve in time as the system is subject to forces—this is precisely what he says when he talks about forces determining the probability of states. It is not simply a metaphor. Cartwright puts it thus: 'The forces act directly on the *probabilities* at one time to give probabilities at other times; they do not evolve the probabilities by acting on the actual state of the system' (Cartwright 1987 p415).

So this is Cartwright's argument for Born having a propensity interpretation of probabilities in quantum mechanics: Born talks about things like forces acting on probabilities, and probabilities acting upon one another. We might write this of as just a manner of speaking—what he *really* means is how wavefunctions are acted upon and interfere. But Born does not actually believe that superpositions are real. He only thinks that particles are. For this reason, we ought to take what he says at face value— probabilities are real.

It's crucial to all of this that Born does not believe in the reality of the superposition of the wavefunction. It's an apparent disbelief in the wavefunction that indicates Born thinks of probabilities as properties of the quantum system itself, rather than simply thinking that quantum mechanics obeys probabilistic laws. What I want to do next is to look at some other evidence for this position, particularly with regards to the work in the 1940s and 1950s that is the primary focus of this thesis. I then want to examine a few points from *Natural Philosophy of Cause and Chance* (Born 1949a) and argue that they give extra support for this position. We also ought to examine Cartwright's claim that Born's view has commonalities with model interpretations of quantum mechanics. This seems quite plausible and gives us a bit more of a framework for his position.

4.2 Born, Einstein and the Reality of the Wavefunction

Something that gives more support to the claim that Born really does not believe in the superposition of the wavefunction (and I think also helps to resolve some potential problems for his ideas about contiguity, the principle that cause and effect must be spatially connected - See Chapter 3—Born on Causation) is an exchange of letters between Einstein and him in April and May of 1948 concerning 'spooky action at a distance'. These are collected as numbers 88 and 89 in the Born-Einstein Letters (Born 2005 pp165-173).

Einstein wrote a letter to Born in April 1948 in which he argues that a non-hidden variables interpretation of quantum mechanics must violate a principle concerning the independence of objects. He describes this as: 'The following idea characterises the relative independence of objects far apart in space (A and B): external influence on A has no direct influence on B; this is known as the principle of contiguity' (Einstein in Born 2005 p 168). By 'far apart in space' I think we are entitled to presume that Einstein means 'space-like' separated. Einstein thinks that the standard interpretation of quantum mechanics violates this principle. Thus we are forced either to accept a violation of the contiguity principle and retain our current interpretation of quantum mechanics or to reject the violation and accept a hidden variables interpretation. Einstein takes this dilemma to indicate that quantum mechanics, as usually understood at least, is incomplete.

It's interesting to note that Einstein had at this point read Born's *Natural Philosophy of Cause and Chance*—Born had sent him a copy of the manuscript in 1948 which Einstein had (affectionately, I take it) appended with some 'caustic' comments (Born 2005 159). Thus the fact that Einstein claims that quantum mechanics violates 'the principle of contiguity' might not unreasonably be taken to be a direct appeal to Born's own philosophical sensibilities.

First Einstein sets out two possible interpretations of what is represented, in a physically real sense, by a particle described by a 'spatially restricted ψ-function (completely described - in [the wavefunction, presumably] the sense of quantum mechanics). According to this, the particle possesses neither a sharply defined momentum nor a sharply defined position' (Einstein in Born 2005 166). These two interpretations are (a) that the particle does have a definite position and momentum even if these cannot both be measured, and consequently the wavefunction is not a complete description of the physical state of the system; and (b) that the particle really does not have a definite position and momentum, and as a consequence of this 'two ψ-functions which differ in more than trivialities always describe two different real situations' (Einstein in Born 2005 19).

Einstein asks us to consider the following situation: Take a system S_{12} which consists of two parts, S_1 and S_2. They may have interacted in the past, but are no longer doing so. The system is completely described (in the 'quantum mechanical sense' as Einstein puts it) by the wavefunction Ψ_{12} of coordinates q_1, … and q_2, … of the two parts. At some time t the two part-systems are separated (Einstein does not specify, but I suspect we are meant to take this be space-like separation) in space in such a way that Ψ_{12} only differs from 0 (presumably the initial state) when q_1,… belong only to some part of space R_1 and q_2, …. belong to some part of space R_2 that is separated from R_1. The individual wavefunctions for the two parts are then unknown and indeed, Einstein tells us 'do not exist at all' (Einstein in Born 2005 168). We can, however determine the wavefunction of one part-system from a complete (as far as is allowed, at least) measurement of the other. 'Instead of the original Ψ_{12} of $S_{12,}$ one thus obtains the Ψ-function Ψ_2 of the part-system S_2.', Einstein writes. So we have a wavefunction (Ψ_{12}) consisting of the sum of two components, Ψ_1 and Ψ_2. Initially, the two part-systems are represented not by individual wavefunctions, only by an entangled state and so we can gain no information about either. However, by measuring one component of the system, we can gain information about the wavefunction of the other.

Einstein's criticism concerns how measurement works in the interpretation of quantum mechanics that he is arguing against. He writes 'if S_1 consists of a single particle, then we have the choice of measuring either its position or its momentum components. The resulting Ψ_2 depends on this choice, so that different kinds of (statistical) predictions regarding measurements to be carried out on S_2 are obtained, according to the choice of measurement carried out on S_1' (Einstein in Born 2005 169). Crucially this means that in this interpretation we end up with a different wavefunction for S_2 depending on what measurement we carry out in S_1, and thus if one accepts interpretation (b) (which Einstein thinks that most physicists do) a 'different real situation' depending on how we measure S_1.

This, Einstein thinks, is not a problem from the perspective of quantum mechanics by itself. Because a different measurement on S_2 creates a different wavefunction and hence a different real physical state 'the necessity of having to attach two or more different ψ-functions ψ_2 $\underline{\psi_2}$ ... to one and the same system S_2 cannot arise' (Einstein in Born 2005 p169).

It is, however, a problem from the perspective of those who wish to respect the both the principle of contiguity and quantum mechanics. This is because a measurement on the part-system S_1 ought to only affect the region of space it is in, R_1 because the principle of contiguity guarantees the 'independent existence of the real state of affairs existing in two separate parts of space R_1 and R_2' (Einstein in Born 2005 169). Such a measurement ought not have any physical effect on S_2 which is all the way over in R_2. Presumably, although Einstein does not state it, the two part-systems are meant to be space-like separated in order to guarantee that there can be no local interaction between them. He writes 'It follows that every statement about S_2 which we arrive at as a complete measurement of S_1 has to be valid for the system S_2, even if no measurement whatsoever is carried out on S_1.' Einstein does not precisely say why but it is implicit—because it is what is required if we want to account for both contiguity (i.e. that a measurement of S_1 cannot physically affect

S$_2$) and for the interpretation of quantum mechanics that gives us different wavefunctions for S$_2$ as the result of different measurements being carried out on S$_1$.

This leads to bizarre and unacceptable consequences for those that value the principle of contiguity: 'all statements which can be deduced from the settlement of Ψ_2 or $\underline{\Psi}_2$ [the different wavefunctions that we end up with as a result of taking different measurements of S$_2$] must simultaneously be valid for S$_2$' (Einstein in Born 2005 169)—i.e. multiple different physically real wavefunctions and their solutions (not just a superposition) must all apply at the same time to a single particle. This can't be the case and so Einstein thinks that, rather than rejecting the principle of contiguity, we ought to reject the standard interpretation of quantum mechanics (b) that leads us to it in favour of a hidden variables approach (a). He admits, however, that most physicists are likely to do the reverse and reject contiguity (which he acknowledges is nowhere enshrined in the principles of quantum mechanics) as the price retaining interpretation (b).

OK, so far so Einstein. What's interesting here is how Born responds to this problem. I'm now going to examine that response, point out some oddities in it and propose that one explanation of them is that Born's interpretation of quantum mechanics does not treat superpositions as real.

Born's reply to Einstein's argument starts with what is intended to be an illustrative example: Consider a beam of light which passes through a double refracting crystal and splits into two beams. We can measure the direction of polarisation for one of the beams and from this deduce the direction for the other. Born writes 'In this way one has been able to make a statement about a system in a certain part of space as a result of a measurement carried out on a system in another part of space' (Born 2005 171). We can do this, Born explains, because we know that the split beams had a single origin and can thus said to be coherent. This example is meant to be analogous to Einstein's 'abstract' one—Born writes that it 'shows that such things happen within the framework of ordinary optics. All

quantum mechanics has done is to generalise it'. What strikes me as odd about the idea that this example is analogous to Einstein's is that it is about *information*, not effect. Nothing in the classical optics model suggests that we in any way change beam two by measuring beam one. We simply gain some information about it. We don't affect it any way. This is not the problem that Einstein presents with quantum mechanics. Einstein's problem arises from the fact that when we have a system of two parts in superposition, carrying out measurements on one part of the system affects the wavefunction of the other; and because, in the interpretation of quantum mechanics that he is criticising, different wavefunctions represent physically real differences, such measurement physically affects the other part of the system.

Born next critically examines Einstein's 'axiom of the independence of of spatially separated objects A and B'. He doesn't find it to be as convincing as Einstein 'makes it out to be' because it leaves out the idea of coherence—that things far apart with a common origin are not necessarily independent. This is simply a fact as far as Born is concerned: 'I believe that this cannot be denied and simply has to be accepted' (Born 2005 171). But what does Born understand here by independence? He frames Einstein's position in the following way:

> 'You say: the methods of quantum mechanics enable one to determine ψ_1 from S_2 from ψ_{12} provided a complete measurement, in the quantum mechanical sense, of the spatial system S_1 exists as well. You evidently assume that ψ_{12} is already known. Therefore a measurement in S_1 does not really give any information about events occurring in far distant S_2, but only in association with the information about ψ_{12} that is, with the help of additional earlier measurements. In the optical example, we have the information that both partial beams are produced from the same crystal' (Born 2005 171)

Note that again Born does not talk about distant action, but only about information. In his framing we do not *affect* S_2 by measuring S_1. We only gain information about it. He again refers to the optical example, seemingly as an analogy, with reference to the information we have about that system. It genuinely looks as if Born is missing the point of Einstein's argument. The difference seems to be this: Einstein interprets the act of determining the wavefunction ψ_2 of S_2 by measuring S_1 as *physically acting upon* S_2 by measuring S_1. Born seems to interpret this act as merely giving us *information* about S_2. This, he thinks, is unproblematic in this case because S_1 and S_2 have a common origin.

He then goes on to argue that Einstein's example is 'too abstract' to be of much use and that in any actual example of finding information out about some system by measuring another involves those systems not actually being independent—i.e. that those systems have interacted in the past and we have information about that interaction. He does offer some sympathy to Einstein's position a paragraph later, writing

> 'But I feel that I am not expressing my opinion as lucidly as I would like to do. Basically I am coming back again to the fact of coherence, which cannot be denied. But as the usefulness of mechanical analogues cannot be denied either, one must be content with a formalism which covers both. I am therefore inclined to make use of the formalism, even even to 'believe' in it in a certain sense, until something better turns up' (Born 2005 172)

He makes reference to having 'expounded' on this in his 'Oxford lectures'. It's not explicit here, but the 'Oxford' lectures probably refers to *Natural Philosophy of Cause and Chance* (Born 1949a), which in its initial form was the 1948 Waynflete lecture series, delivered at Magdalen College. The 'formalism' I think refers to superpositions of the wavefunction—they are waves, and so coherent and can be interpreted in terms of particles (as Born did in his 1926 paper) giving us a mechanical analogue. Note the scare quotes around 'believe' -

if this implies a realism about the wavefunction then it does not seem to be a very committed one. Perhaps it indicates what Arthur Fine might refer to as 'realism without the table-thumping' (Fine 1996). It is a useful place-holder until something better appears.

I think that a passage from *Natural Philosophy of Cause and Chance* makes Born's stance a bit clearer. Born speaks highly a number of times of Heisenberg (and indeed Einstein's) principle that theory should not contain things which are in principle unobservable (note again that this is not really an indication of anti-realism—Born means not observable in way at all, not merely not directly observable. It is indicative of a species of weak empiricism, but I think nothing more). Here he talks about it with regard to the wavefunction:

> Now quantum mechanics itself is not free from unobservable quantities. (The wave-function of Schrödinger, for instance, is not observable, only the square of its modulus.) To rid a theory of all traces of such redundant concepts would lead to unbearable clumsiness. I think though there is much to be said for cleaning a theory in the way recommended by Heisenberg, the success depends entirely on scientific experience, intuition, and tact (Born 1948 89).

So Born thinks that the wavefunction is an unobservable quantity. It's a 'redundant' concept, but not one we necessarily want to cleanse the theory of because it serves a useful purpose. Still, it doesn't have the character of physical reality and I think this goes some way in explaining Born's response to Einstein. We might think it odd that Born only considers the square of the modulus of the wave-function to be observable if we think that phase of the wave-function might be observable via interference effects. I think that we can explain this in the following way—for Born the phase of the wave-function is still part of the mathematical formalism. Although it's true that the phase of a wave can affect the

observed intensity (i.e. the square of the modulus of the wave-function) via interference with other waves, it's only the intensity that we actually observe, via measuring the probability of finding a particle in a particular volume of space, at least in the quantum-mechanical sense. We can see this in *Natural Philosophy of Cause and Chance*, (again) where he writes, in reference to the wave-function, 'One must remember that only $\phi\phi^* = |\phi|^2$ has a physical meaning (as a probability)' (Born 1949a 102).

4.2.1 Why Does Born Appear to Misinterpret Einstein?

Born, as we have seen, seems to miss the point of Einstein's argument in his initial response. He does it again in the later commentary, appended to the letter written when it was published. There he characterises Einstein's principle of independence (which he describes as the 'root of the disagreement between Einstein and me' (Born 2005 173)) as saying that 'an observation of the state of affairs at B cannot tell us anything about the state of affairs at A' (2005 173). Again, Born talks about gaining information—the state of B 'cannot tell' us anything about A. He doesn't talk about the problem of distant action—the idea that an observation of B cannot physically alter A. In the commentary, he is mystified as to why Einstein did not accept his argument. Born sees it as a simple matter of (Optical-style) coherence that cannot be denied and seems to presume that any objections Einstein had would be on the grounds that coherence only applies to optics.

So why is this? One option, which shouldn't be discounted, is that Born simply misunderstands or misreads Einstein and there is nothing else to it. But, given Cartwright's argument, and also what Born writes in *Natural Philosophy…,* I think we that we might suggest that the reason that Born's response appears to misunderstand Einstein is because Born does not really believe in the physical reality of the wavefunction. Einstein worries that quantum mechanics forces us to conclude that a measurement on system A can physically affect a different system B that is spatially separated from it by affecting B's

wavefunction. Because Born doesn't believe in the physical reality of wavefunctions this isn't a worry for him—one cannot affect something that isn't real—so he translates it into a problem about information, not about action at a distance. It's Born's disbelief in wavefunctions that leads him to think that the optical beam splitting analogy that he supplies really is analogous with the situation that Einstein describes. It's worth noting that this isn't necessarily a satisfying solution to the problem that Einstein raises, but suggesting that Born doesn't believe in wavefunctions goes some way to explaining why he responds to Einstein in the way that he does. It can still be the case that Born misunderstands Einstein's point, but that misunderstanding is motivated by how Born thinks about quantum mechanics.

So can we connect Born's writings in this period - the exchange of letters between him and Einstein and *Natural Philosophy of Cause and Chance*—with a propensity view of probability? I think we can do so by looking at what he writes on invariant realism. Invariant realism (discussed in more detail in chapter 4—Born on Realism) is the thesis that we ought to be realists regarding the characteristics of our physical theories which are invariant under transformation (for instance, relative velocity under Galiean transformation and rest mass, proper length and proper time under Lorentz transformation). When writing about the issue of realism with regards to whether or not one should consider the 'wave' part of wave-particle duality as real, Born writes 'I personally like to regard a probability wave, even in 3N-dimensional space, as a real thing, certainly as more than a tool for mathematical calculations' (Born 1949a 106). Why is this? Because it has invariant features: 'it predicts the results of counting experiments, and we expect to find the same average numbers, the same mean deviations, etc., if we actually perform the experiment many times under the same experimental conditions'. Now there are a few things to note here. The first is that Born specifically refers to a *probability* wave as opposed to a wavefunction. The second is that, although Born does not explicitly say this, it is clear that the invariant features of said waves are not the wavefunction itself or the superposition, but the probability distributions. He goes on to say 'Quite generally, how could we rely on

probability predictions if by this notion we do not refer to something real and objective?' (1949a 106).

We might try to explain Born's position in the following way. Suppose you have a system in an eigenstate of an operator $\hat{A}$. That system must be in a superposition of eigenstates of some none-commuting operator $\hat{B}$. $\hat{A}$ and $\hat{B}$ might be components of spin in a mutually orthogonal direction, such as $\hat{S}_x | \hat{S}_y | \hat{S}_z$. Born doesn't want to interpret this superposition in a realist manner qua superposition of $\hat{B}$. He only wants to interpret it as an eigenfunction. This is what explains the probabilistic behaviour of, say, electrons in an eigenstate of $\hat{S}_z$ when we measure the spin in the y- or z- direction.

Born also connects probabilities in quantum mechanics with causation. He asks, when discussing indeterministic physics, 'Can we be content with accepting chance, not cause, as the supreme law of the physical world?' (Born 1948 101). His reply is that to ask such a question is a misunderstanding—it is not causation that has been eliminated from physics, only determinism. He goes on to say 'Causality in my definition is the postulate that one physical situation depends on the other, and causal research means the discovery of such dependence. This is still true in quantum mechanics, though the objects of observation for which a dependence is claimed are different: they are the probabilities of elementary events, not those single events themselves' (1949a 102). What I think Born is saying here is that the objects of causal dependence in quantum mechanics are the probabilities for events, i.e. that probabilities causally depend on each other. The question then, if we take Born at face value, is what sort of things are probabilities such that they can be the objects (and subjects) of causal dependence?

This, I think, all combined gives us good reason to think that Born believes in the quantum probabilities of a system as a real physical feature of that system. Since they are also

properties that determine the statistical behaviour of a system, it is safe to say that they are propensities.

We can also use this to motivate the argument for the propensity interpretation in another way. Born might be making an argument in for the reality of the probability waves in virtue of the fact that they possess invariant properties, in the same way that he makes an argument for the reality of particles. He writes 'We believe in the 'existence' of the electron because it has a definite mass *m* and a definite spin *s*' (Born 1949a 104). Born here is trying to motivate belief in the existence of electrons from the fact that they possess certain invariant properties. Now, I don't think that he is making precisely the same argument for probability waves—part of the point of the electron argument is all electrons have the same mass and spin—but even if he is trying argue that 'probability waves' are real because they have invariant properties (in a slightly different way from electrons) we still must acknowledge that if this is indeed the case, then Born must treat the quantum probability as being a property that is, in virtue of the fact that it is invariant, importantly like such properties as charge, mass and spin. i.e., they are *real* properties and hence something like propensities.

One of the standard criticisms of a propensity account of probability is to do with probabilities in a single case, so it's worth examining this and asking if Born thinks that quantum mechanics deals with single-case propensities or whether he thinks they are physical properties that produce frequencies. I think that it's actually quite clear from a comment of Born's that he does not think that quantum mechanics deals with probabilities in the single case. In *The Born-Einstein Letters* he writes in a comment added to a letter in September 1950 'To say that ψ describes the 'state' of one single system us just a figure of speech, just as one might say in everyday life: 'My life expectation (at 67) is 4.3 years'. This too is a statement made about one single system, but does not make sense empirically. For what is really meant is, of course, that you take all individuals of 67 and count the

percentage of those who live for a certain length of time. This has always been my concept of how to interpret $|\psi|^2$'(Born 2005 182). He says again in a commentary on a letter that Einstein had written to him in December 1953 'Einstein admits that one can regard the 'probabilistic' quantum theory as final if one assumes that the ψ-function relates to the ensemble and not to an individual case. This has always been my assumption as well, and I consider the frequent repetition of an experiment as the realization of an ensemble. This coincides exactly with the actual procedure of the experimental physicists, who obtain their data in the atomic and sub-atomic area by accumulating data from similar measurements' (Born 2005 206).

From these passages we get a quite explicit statement that Born does not think that probabilities in quantum mechanics deal with single cases and instead deal with frequencies or repeated conditions.

To sum up the argument, Born has a propensity interpretation because he talks about probabilities in quantum mechanics as though they are physical properties of systems—they are the sorts of things that forces act on and are the objects of causal dependence. We can be confident that this sort of talk is not just a shorthand for a belief that it is merely the laws of physics that are statistical because Born does not believe in the reality of the superposition of the wavefunction in quantum mechanics—it is the probabilities themselves that are physical properties of the system which will give rise to the same distribution of outcomes under the repetition of the same experimental condition. We can also be confident that he holds a long-run propensity interpretation, as opposed to the single case variety: Born explicitly rejects probabilities in the single case.

4.2.2   A Potential Inconsistency

We might worry that I have introduced a problem for my claim that Born is a realist here. Is there some tension in Born being a realist about electrons in virtue of their possessing invariant quantities, but not about superpositions? I've suggested in this section that his non-realism about superpositions arises because he takes them to fall into the category as electron orbits, that of the unobservable even in principle (i.e. it is the theory that tells us they are not observable, not merely some technological limitation), and holds strongly to Heisenberg's maxim that such things have no place in physics. There might be a worry that this implies an inconsistency on the part of Born. After all, if we can infer the existence of an electron from our identification of a bundle of invariant properties, then surely we can do the same for a superposition, inferring its existence from the identification of a particular probability distribution, something that I've argued Born takes to be an invariant property. There are two options here. One is that there is indeed an inconsistency in Born's position. The other is that Born is not a realist about superpositions, but for reasons other than them being unobservable even in principle.

Let's examine what we know: Born is committed to the existence of invariant properties and quantities. He is committed to the existence of electrons as bundles of invariant properties, although not necessarily as *mere* bundles of properties. He is committed to realism about the probability density given by superpositions, as I've argued above, viewing them as invariants of experiment. If he is already committed to the reality of the probabilities, then the superposition itself adds nothing more to our ability to predict and explain the evolution of a quantum system. The probability amplitudes exhaust what is observable about a superposition. If he is a realist about the probabilities themselves, then Born is free to write off the rest of the superposition is merely part of the formalism of the theory, as I've argued he appears to do in the above discussion of his letters with Einstein.

On the other hand, as noted above, Born does seem to be fairly explicit about regarding superpositions as unobservable even in principle. We might, however, be able to use the same argument to explain why Born takes this position: for him, probabilities and 'probability waves' are real, but there is nothing observable about superpositions over and

above these probabilities. Now it might be that Born is misapplying this principle, i.e. he really means the former option even if he implies it's the latter, or he takes it do a job that it can't but even in this case this doesn't seem to imply an anti-realism on his part.

5   Does Born Have a Modal Interpretation of Quantum Mechanics?

We ought to try and make sense of what Born thinks is real in quantum mechanics. I've argued that he does not think that superpositions are real, but does think that $|\Psi|^2$ probabilities are. One thing to quickly ask is whether or not a propensity interpretation of probabilities in quantum mechanics is a realist one.

Redhead (1987) thinks that it is. He notes that some have argued that long-run propensities are not in fact properties of physical systems, but rather of microsystems combined with repeatable conditions. Because of this it is claimed that a propensity interpretation is not a realist one as the relevant microsystems do not possess the relevant properties outside of experimental testing, i.e. the repeatable conditions (Redhead 1987 48-49). Redhead disagrees with this view for two reasons. The first is that although it is indeed the cases that propensities are only manifested in the context of the repeated experimental, this is no reason not to regard them as dispositional properties that are always possessed by the microsystems, even if they are not always being manifested (1987 49). We don't, in general, regard dispositional properties as antithetical to realism. Propensities ought to be no different in this regard. The second is that although we might label the repeatable conditions as experimental set-ups, they are not dependent on human minds performing them or observing them in anyway. Such set-ups can quite happily exist and the propensities quite happily manifest without any human interactions whatsoever (Redhead 1987 49).

As previously mentioned, Cartwright suggests that Born's interpretation of quantum mechanics belongs to the class known as model interpretations of quantum mechanics and I now want to examine this in some more detail.

5.1   What is a Modal Interpretation of Quantum Mechanics?

In *Quantum Mechanics: An Empiricist View,* van Fraassen (1992) discusses a particular conceptual problem for quantum mechanics: how can we reconcile the fact that quantum mechanics is indeterministic with the deterministic evolution of an isolated quantum system? Between measurements, a quantum system evolves deterministically in accordance with the time dependent Schrödinger equation. Immediately after a measurement is made on the system, the wavefunction will be described by the eigenfunction corresponding to the eigenvalue given by the measurement (Rae 2005 67). The probability of obtaining some particular result is given by the square of the modulus of the wavefunction. This is is known as the projection or collapse postulate—we say that the eigenfunction is *projected* onto the system or that the wavefunction *collapses* into the measured state.

Van Fraassen suggests that there are three (broad) solutions to this problem (Van Fraassen 1992 273). The first is Von Neumann's, in which isolated systems do not develop deterministically. The second says that quantum mechanics is not in fact deterministic. The third option accepts both the indeterministic and deterministic elements of the theory - this is the modal interpretation.

Van Fraassen splits the concept of a state into two—the value state and the dynamic state:

Value State: fully specified by stating which observables have values
and what they are.

Dynamic state: fully specified by stating how the system will develop
if isolated, and how if acted upon in any definite, given fashion. (Van
Fraassen 1992 275)

Lombardi and Dieks (2017) argue that modal interpretations have their origins in attempts
to solve problems arising from the projection postulate. There are a number of different
varieties, but they all have the following five attributes in common: (a) The formalism of
quantum mechanics is the same as that of the standard version except that the project
postulate is excluded. (b) It takes quantum mechanical systems to always possess a number
of definite values—in this way it is 'realist' in a semantic sense. (c) Quantum mechanics is
taken to be fundamental in that it is not taken to be in some way an incomplete theory or
underlaid by a more fundamental theory. (d) The dynamical state of the system completely
specifies what possible properties a system may have and what the probabilities of those
properties are. (e) Measurement is an ordinary physical interaction—it does not collapse
the wavefunction—the dynamical state is *always* described by the evolution of the time-
dependent Schrödinger equation (Lombardi and Dieks 2017).

Cartwright certainly thinks that Born separates the quantum mechanical state into value-
and dynamical- parts. She notes that Born's views have much in common with modal
interpretations such as van Fraassen's and goes on to say that 'there is a state that is the
actual physical state of the system (recall that this is not a classical state with well-defined
values, but is itself a quantum state obeying the uncertainty relations); and there is a
different state that describes the probabilistic behaviour of the system' (Cartwright 1987
414). This would account for attributes (b) and (d). The question is, does Born subscribe
to all five attributes?

(a) Does Born reject the projection postulate? There aren't many specific discussions of quantum mechanical measurement in Born's writings, but there are a few mentions. It's worth stating that in 1926, the projection postulate had not really been formulated—its precise origin lies in the work of Dirac and of von Neumann in the 1930s (Myrvold 2017). We would hence not expect Born to be thinking of quantum mechanics in terms of the projection postulate in the 1920s. We should therefore at least consider that it would be a consistent possibility for Born to have both held something like a modal view when he developed his work on collisions, as Cartwright argues, and to have rejected a model view when quantum mechanics was axiomatised later on.

Unfortunately, I have found little discussion of this in his later work. The closest that I can find is a mention of what Born calls the 'reduction of probabilities': a state represented as a wave function in configuration space (more generally: a vector in Hilbert space) is turned into another one by experimental interference' in his paper *In Memory of Einstein* (Born 1965 162) in reference to his disagreements with Einstein. Born says that Einstein wrote to him in December 1947 (according to the Born-Einstein letters it was March of that year) in which he says that he cannot believe in quantum mechanics because of 'spooky action at a distance' (Einstein in Born 2005 155). Born writes that 'What the [sic] was alluding to was presumably the situations arising from the interference of probability amplitudes …; and what is usually called "reduction of probabilities": a state represented as a wave function in configuration space (more generally: a vector in Hilbert space) is turned into another one by experimental interference' (Born 1965 162).

He does mention Von Neumann's work on quantum mechanics in *Natural Philosophy of Cause and Chance*, writing 'He puts the theory on an axiomatic basis by deriving it from a few postulates of a very plausible and general nature, about the properties of of 'expectation values' (averages) and their representation by mathematical symbols' (Born 1949a 109). Born doesn't comment on Von Neumann's work further except to say that it proves (unknown to Born erroneously) that quantum mechanics cannot be simply modified

to include hidden variables: any such addition would have be in terms of a new theory. The point here is that the projection postulate is part of Von Neumann's formulation of quantum mechanics and there is no indication here that Born rejects it. Indeed he states that Von Neumann's axioms are all 'plausible'. It could be the case that he considers Von Neumann's work to be of formal but not physical significance, but again it is at best ambiguous as to whether he rejects the projection postulate in the way that modern modal interpretations do. I think we can also take this to indicate that Born does think that quantum mechanics is fundamental (see also Chapter 4—Born on Realism).

> Now, it is true to say, I think, that Born subscribes to a position that is at least similar to (e) in that he does not take measurement to be some special interaction. He says a number of times that measurement in quantum mechanics is not a fundamentally different activity from that in classical mechanics. In *Natural Philosophy of Cause and Chance* he writes that 'one cannot even make a measurement without interfering with the state of the system. In classical physics it is supposed that we have to do with an objective and always observable situation; the process of measuring is assumed to have no influence on the object of observation. I have, however, drawn your attention to the point that even in classical physics this postulate is practically never fulfilled because of the Brownian motion which affects the instruments. We are therefore quite prepared to find the assumption of 'harmless' observations is impossible' (Born 1949a 99).

As to whether or not he thinks that measurement collapses the wave-function, I think the answer is probably this—we have ample evidence that Born does not believe in the reality of superpositions and it would obviously follow from this that there is no collapse either. There can be no collapse if nothing exists to collapse. As to whether or not this entirely

fulfils (e), I think that it is again little ambiguous with regards to whether or not Born thinks that the dynamical state is always described by the evolution of the wave-function. Born, I think is probably not a realist with regards to   superpositions, but he perhaps could still hold that it accurately describes a system as part of the formalism without interpreting it as physically real.

In general, I think that it is unclear as to whether or not Born holds something that aligns with the one of the full-blown contemporary programmes for modal interpretations of quantum mechanics. I don't think that this is a problem. One thing that philosophers and historians who engage in this kind of project—that of trying to interpret the positions of historical scientists in terms of contemporary categories—must bear in mind when doing so is that we are not always going to be able to put the objects of our study into some particular intellectual box, whether due to lack of evidence or to the fact that they simply were not thinking about things in a way that allows us to categorise their positions easily.

However we may still be able to attribute a view to Born which is modal in the same sense that van Fraassen's view is modal, even if it is at best ambiguous as to whether or not his view is consistent with the contemporary programme for modal interpretations of quantum mechanics. Van Fraassen's view is modal because the dynamic state tells us what is merely possible (Lombardi and Dieks 2017). The way that Born seems to think about probabilities in quantum mechanics gives us something similar.

5.1.1   Realism and Modal Interpretations

We should pause for a moment to address the following concern: I've attributed (something like) a modal interpretation of quantum mechanics to Born, who I've also argued is a realist. As noted previously, modals interpretations of quantum mechanics

originate with Van Fraassen, a constructive empiricist. Might we worry that there is some conflict or tension in attributing a position developed by a well-known anti-realist to someone who I am arguing is a realist? Am I attributing an anti-realist interpretation of quantum mechanics to someone that I elsewhere argue is a realist? There are two responses to this point. The first is that, in proposing the modal interpretation of quantum mechanics, Van Fraassen is not attempting to provide an interpretation of quantum of quantum mechanics that is uniquely constructive empiricist in nature, and therefore hostile to realism. The second is that modal interpretations in general are realist in a semantic sense, and that that it is not clear from the evidence that Born holds a view akin to one of the full-blown contemporary realist programs is not reason in and of itself to take him to be an anti-realist or for holding such an interpretation to entail some significant inconsistency in his views.

The first response is made as follows: Van Fraassen's Modal Interpretation is obviously intended to be compatible with constructive empiricism, but it does not follow from this that it is incompatible with any realist view of science. Constructive empiricism is agnostic, rather than atheistic, towards the question of the reality of scientific theories. In *Quantum Mechanics An Empiricist View* (1991) Van Fraassen makes an explicit point that the analysis and interpretation of scientific theories is separate matter from questions regarding whether or not we ought to be realists about those theories. i.e. the question of interpretation - how the world could be the way a theory says it is is a question regarding what the content of a theory *is*, not whether or not that content is true or false or whether or not we have some mandate to believe in it. He notes that asking questions regarding what a theory is like does not presuppose realism (Fraassen 1991 3). By the same token, he writes 'When we come to a specific theory the question: *how could the world be the way a theory says it is*? concerns the content alone. This is the foundational question *par excellence*, and it makes equal sense to the realist and empiricist alike' (van Fraassen 1991 4).

The point is this: When Van Fraassen proposes the modal interpretation of quantum mechanics he is in no way trying to provide an anti-realist interpretation of quantum mechanics. He is trying to solve a conceptual problem with regard to the content of quantum mechanics, and the realist ought to be able to get on board with the proposed solution as much as the constructive empiricist.

With regards to the second point, we might note that the SEP points out that all modal interpretations agree on the following point (amongst others) 'The interpretation should be "realist" in the precise sense that it assumes that quantum systems always possess a number of definite properties, which may change with time. It should be noted, however, that this semantic realism is compatible with agnosticism or van Fraassen's brand of empiricism (van Fraassen 1991, Bueno 2014), and does not presuppose epistemological realism' (Lombardi and Dieks 2017). i.e., modal interpretations of quantum mechanics are both compatible with realism and with constructive empiricism. Constructive empiricists and realists will obviously differ with regards to their epistemic commitments, but the interpretation is neutral on this ground. It is, however, uncontroversially realist in a semantic sense. We might further note that modal interpretations are compatible with propensity interpretations of probability, although not every modal interpretation is also necessarily a propensity interpretation (Lombardi and Dieks 2017).

So it seems that there is little worry that there is some inconsistency in attributing to Born both realism and an interpretation of quantum mechanics that is continuous with one developed by Van Fraassen.

6   Conclusion

I've argued in this chapter that Born holds a propensity view of probabilities in quantum mechanics. It seems unlikely that Born holds anything like a classical, logical or subjective view because it seems to quite explicit that he takes quantum mechanics to be fundamentally indeterministic, and the probabilities that it involves to be objective in nature. Furthermore, as I've argued earlier in this thesis (see Chapter 4—Born on Realism), Born is not a positivist and for this reason his views are misaligned with de Finetti's.

It might be possible that Born is a frequentist—he certainly holds probabilities in quantum mechanics to be objective and does not seem to think that the theory deals with single cases. However, I've argued that it is more plausible to view him as holding some kind of long-run propensity theory in which he views probabilities in quantum mechanics as physical properties of a system that give rise to frequencies under repeatable conditions. This is because Born talks about probabilities as being the sorts of things that can be acted on by forces, are the objects and subjects of relations of causal dependence, and are invariants of observation, which puts them into a class of property about which Born thinks we ought to be realists. We've got good reason to take Born at face value when he says this kind of thing because he believes in particles, does not believe in superpositions, and so we have good reason to think that he does not *just* think that the laws of physics are statistical. It might well be the case that thinks that the laws of physics are statistical but in addition to that he thinks that probabilities in quantum mechanics are properties carried by quantum systems that give rise to something like frequencies. This kind of view might indicate that Born holds some variety of modal interpretation about quantum mechanics, but I've argued that this is unclear, at least with regards to the contemporary programme. Still, I think it is most likely that Born holds a long-run propensity view of quantum mechanical probabilities.

Conclusion

In this thesis I have examined the philosophy of Max Born. I've argued that he holds that there exist principles regarding causal relations that have guided the development of physics and have, in its modern formulation, been confirmed as empiricalan empirically-supported status; that he is a selective realist, initially with regards to invariant properties and, later on, an epistemic structural realist; that he has produced an argument, compatible with modern philosophical definitions of determinism, that we were never entitled to conclude from the success of classical mechanics that the world was deterministic; and that Born holds an objective interpretation of probabilities in quantum mechanics which, due to his strong belief in the physical reality of quantum-mechanical probabilities and his apparent disbelief in the superposition of the wave-function, is most likely a long-run propensity theory. I have also I have also used a presentist methodology to examine Born's work in science and philosophy. The purpose of this was to add to our understanding of Born in a way that a purely contextual account could not by looking at how his views aligned with positions in contemporary philosophy.

In Chapter 1, I started out by giving a defence of presentist historiography of science. I gave a survey of the various worries that historians have with regards to presentism—Whiggism, the assumption of a linearly progressive course of history;, triumphalism, the writing off of unsuccessful theories as a worthy object of study in the history of science in favour the successful; and the assumption of identifiable causal links across different historical contexts—and argued that we need not think that presentism must necessarily fall prey to them. Indeed, they can be largely avoided by being careful. I have also given my own analysis of a disagreement in the history of alchemy between William R Newman and Ursula, showing that a number of problems that presentism is accused of having are

not unique to it. Neither Newman nor Klein are presentists but each accuse the other of historiography that is problematic in ways associated with presentism. Finally, I gave a comparative examination of three biographies of Newton, drawing out some problems to be avoided in scientific biography.

In Chapter 2, I gave a biographical overview of Born's life. This part of the thesis is largely scholarly in nature and serves to inform the reader about Born's life, and to try to set his work in context. It concentrates in particular on his education andand on his scientific work.

In Chapter 3, I gave an detailed examination of Born's book *Natural Philosophy of Cause and Chance* (Born 1949a). I gave a detailed overview of Born's argument in it, that the development of physics has been guided by two principles regarding causal relations: that cause and effect must be spatially connected, and that cause must be prior to, or at least simultaneous with, effect. This was followed by a survey of the various sorts of statuses that principles in science can have. I concluded that they have a status similar to Zahar's metaphysically serious heuristic principles, although Born regards his as being empirically confirmed in a way that I think Zahar's would not be, although some confirmation is available to a Zaharian principle due to its place in the hard core of a research programme.. I concluded by arguing that Born had good reasons to not to not worry about those elements of modern physics—spooky action at a distance and relativistic time travel—that might appear to cause problems for his view, or at least that these issues do not provide insurmountable reasons to reject his views. The presentist methodology in this instance was deployed by using Zahar's work to try and understand what metaphysical status Born's principles of causation had. There was also a need to use physics after Born to evaluate the issued due to relativistic time travel.

In Chapter 4, I examined Born's position on scientific realism. I gave an account of how he deals with the topic by arguing against positivism and for a position called invariant realism. I followed this with an overview of definitions of realism, settling on a fourfold one: a metaphysical commitment, a semantic commitment, an epistemic commitment, and a commitment to progress and continuity of reference. I argued that Born's invariant realism, which argues that we ought to be realists about only those physical quantities that are invariant under transformation and the entities that bear those quantities, meets all of these criteria and is a variety of selective realism, albeit one that is not free of tensions particularly with regards to the individuality of particles in quantum mechanics. I offered a rebuttal to Galavotti's (2005) claim that Born is not a realist. I have also argued that Born's position changes later on into ais fairly clearly a variety of epistemic, but not ontic, structural realism. Again, the presentist methodology has been used here by drawing on contemporary notions and varieties of scientific realism in order to explore Born's views.

In Chapter 5, I dealt with Born's argument that we ought not to have considered the success of classical mechanics ought never to have been taken as to have been good reason to hold that the world is deterministic. I gave an examination and explication of his argument—that due to the inevitability of experimental error precise predictions are not achievable for any classical system, and hence that we are therefore unable to find any measurable distinction between a deterministic and an indeterministic system. The presentist methodology was used here to discuss Born's position in relation to Earman's more precise definition of determinism, which draws a distinction not made explicitly in Born's day between determinism and predictability. Drawing on more modern work on deterministic chaos, I gave a reconstruction of Born's argument that is compatible with contemporary philosophical definitions of determinism. I have shown that Born's argument can be revised to apply to determinism as it has more recently been formulated, for instance by Earman.

In Chapter 6, I examined Born's position with regards to the interpretation of probability. I gave a survey of the standard interpretations—classical/logical, subjective, frequency and propensity—along with looking at what sort of evidence would indicate that Born held one of them. By examining Born's scientific and philosophical writings that connect with probability, it was shown that Born clearly holds an objective interpretation. Drawing on an argument of Cartwright's and adding my own analysis of an exchange of letters between Born and Einstein, I argued that Born believes quantum-mechanical probabilities to be real but rejects a realist interpretation superpositions. Consequently, he holds a long-run propensity interpretation of probabilities in quantum mechanics. I concluded by examining Cartwright's argument that Born holds one of the modal interpretations of quantum mechanics, arguing that, although there is insufficient evidence to support him holding one of the full-blown contemporary programs, it does appear that his position is at least continuous with van Fraassen's original version. The presentist methodology was used here in drawing on contemporary philosophy of quantum mechanics (modal interpretations) to explain Born's position, which is distinct from the standard Copenhagen one. It was also used in the assessment of what sort of interpretation of probability Born holds – the propensity interpretation was not explicitly formulated when he was writing, although his views clearly correspond to it.

These chapters largely stand on their own, but I would like to highlight a common thread running through them: that Born cleaves strongly to (his version of) the maxim of Einstein and Heisenberg that elements of a theory which are unobservable even in principle, or are indistinguishable via measurement from some other element of theory, ought not to be the subject of realist physical interpretation, although it is not quite clear that he is always consistent in his application of it.

This thesis does not represent the totality of work that could be done on Born. It has not dealt in any detail with the work he did on crystal lattice dynamics prior to becoming

involved in the development of quantum mechanics. As mentioned towards the end of Chapter 4, I suspect that there may be some links between Born and the philosophy of Ernst Cassirer. An examination of Born's thoughts with respect to Cassirer's may be fruitful. Another, more substantial project would be a fully integrated intellectual biography of him—none currently exists in the literature.

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