ABSTRACT

Title of Document:	QUANTUM MECHANICS AND QUANTUM INFORMATION THEORY
	Wesley William Van Camp, Doctor of Philosophy, 2009
Directed By:	Distinguished University Professor and Chair of the Committee for Philosophy and the Sciences, Jeffrey Bub, Department of Philosophy

The principle aim of this dissertation is to investigate the philosophical application of quantum information theory to interpretational issues regarding the theory of quantum mechanics. Recently, quantum information theory has emerged as a potential source for such an interpretation. The main question with which this dissertation will be concerned is whether or not an information-theoretic interpretation can serve as a conceptually acceptable interpretation of quantum mechanics. It will be argued that some of the more obvious approaches – that quantum information theory shows us that ultimately the world is made of information, and quantum Bayesianism – fail as philosophical interpretations of quantum mechanics. However, the information-theoretic approach of Clifton, Bub, and Halvorson introduces Einstein's distinction between *principle theories* and *constructive theories*, arguing that quantum mechanics is best understood as an information-theoretic principle theory. While I argue that this particular approach fails, it does offer a viable new philosophical role for information theory. Specifically, an investigation of interpretationally successful principle theories such as Newtonian mechanics, special relativity, and general relativity, shows that the particular principles employed are necessary as constitutive elements of a framework which partially defines the basic explanatory concepts of space, time, and motion. Without such constitutive principles as preconditions for empirical meaning, scientific progress is hampered. It is argued that the philosophical issues in quantum mechanics stem from an analogous conceptual crisis. On the basis of this comparison, the best strategy for resolving these problems is to apply a similar sort of conceptual analysis to quantum mechanics so as to provide an appropriate set of constitutive principles clarifying the conceptual issues at stake. It is further argued that quantum information theory is ideally placed as a novel conceptual framework from which to conduct this analysis.

QUANTUM MECHANICS AND QUANTUM INFORMATION THEORY

By

Wesley William Van Camp

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2009

Preface

To a very large extent, this manuscript mirrors the course of my actual thinking on these matters. When I first arrived in graduate school I was not familiar with the subject of information theory, let alone quantum information theory. Therefore, not knowing the subject my dissertation, this seemed an intriguing area of research – new, exiting, promising. Quantum information theory seemed to me to be philosophically promising because much of its successes draw from those aspects of quantum mechanics which had been puzzled over by physicists and philosophers since day one. It did not question these oddities; it embraced them, and got results. It was exiting because the approach in the foundations of physics, seemed to make the claim that physics, at least quantum physics, was "about information". I did not know what this meant, I am not sure I do still, but it is enticing. It lends itself to at least two obvious interpretations. One, classical mechanics is about particles, waves, and motion, and this seemed to have real ontological significance. Classical mechanics is about describing the things out there in the world. So if quantum mechanics was about information, then by analogy, information must have some sort of ontological significance. Rather than a world made up of particles and waves, it is a world made up of information. Very sci-fi. Two, if quantum mechanics is about information, then it is about our knowledge. So the theory of quantum mechanics tells us simply about what we know and that some of our most fundamental physics is inherently reflexive in some way.

Neither of these approaches felt particularly satisfactory to me. The first hardly coherent, the second just a refrain on instrumental interpretations of quantum mechanics. It does not bring new justification for being an instrumentalist, nor does it make it any more realist. So I spent some time analyzing why these approaches are not promising. But then the question becomes, what good does quantum information do us regarding the fundamentals? Is it simply an alternate mathematical structure which does not shed any new light on interpretational issues?

Here, Clifton, Bub, and Halvorson (CBH, 2003) provide a third way, though it was only discussed briefly. The idea was that information-theoretic principles could be provided from which the general features of quantum mechanics could be derived. This, in turn, could mean that there is a *principle theory* of quantum mechanics, just as there is for relativity theory. But for me, this also raises questions: is, in fact, a principle theory, *ipso facto*, interpretationally preferable, and if so, why, and why does this now make quantum information theory important (there could be principle theories without quantum information-theoretic principles)? This is where most of my investigation lies. What makes a principle theory valuable? And does that apply in the case of quantum mechanics? And is quantum information theory the right approach?

I was skeptical that in virtue being a principle theory, there was automatically, so to speak, some interpretational groundwork done or swept aside. Einstein did formulate relativity theory as a principle theory, but he also took thermodynamics to be a paradigm example of a principle theory. The final verdict is perhaps not yet in, but it seems to be far from obvious that thermodynamics is a more fundamental theory than statistical mechanics, which many take to underlie thermodynamic phenomena. The lesson is that being a principle theory is not, in itself, enough.

After trips to more distant territory such as Kant and logical positivism, I was better able to understand the role principle theories play in foundational physics and be more precise about what qualities make them foundational via historical examples. There are times and instances where principle theories can play this role, indeed, must play this role. The next question then is: is now such a time and is quantum mechanics such an instance? I conclude that yes, it is. So is quantum information theory the place to get such principle from? Perhaps. There is nothing *prima facie* intrinsically special about quantum information theory; however, it is perhaps the only place which is in fact offering plausible constitutive principles. Other avenues in quantum mechanics, I contend, unless drastically reformulated, cannot play the role which is necessary. So I see myself as pursuing the quantum information-theoretic approach to see how it could work. In the end, I think that it can if applied in the right manner. In particular, the CBH approach hit on something important regarding quantum mechanics and the potential for approaching it as a principle theory using information theory. But more work needs to be done.

Dedication

For mom.

Acknowledgements

First of all I would like to acknowledge the role played by my dissertation advisor Jeff Bub, having first sparked my interest in quantum information theory and for all of the help with the document and clearing up some of my own confusions. I would also like to thank all of the members of my committee Allen Stairs, Mathias Frisch, Michael Silberstein, and Don Perlis.

I would also like to acknowledge and express my gratitude for the support over the years from my family – my father, my brother, and sister, and my mother. And of course I need to thank my wife Debbie for her love, encouragement, and partnership in this journey.

Table of Contents

Chapter 1: Introduction

The principle aim of this dissertation is to investigate the philosophical application of quantum information theory to interpretational issues regarding the theory of quantum mechanics. Recently, quantum information theory has emerged as a potential source for such an interpretation. The main question with which this dissertation will be concerned is whether or not an information-theoretic interpretation can serve as a conceptually acceptable interpretation of quantum mechanics.

Since its formalization in the 1920's, quantum mechanics has been resistant to any sort of universally accepted "interpretation". One need only look to the current philosophical literature on quantum mechanics to verify this. Moreover, such an interpretation has seemed necessary due to the particular nature of quantum mechanics and its results, which seem to contradict both classical physical theories and commonsense physical experience. As a result, throughout the years, many interpretations of quantum mechanics have been formulated, all of which try to make sense of these quantum puzzles in various ways. Part of the project is to analyze what counts as a successful interpretation of a physical theory.

Apart from quantum mechanics, the 20th century saw the advent of another successful theory, the theory of relativity. While this theory brings with it startling results from the standpoint of previous physical theories, it is generally acknowledged that it does not necessitate the kind of further interpretation for which quantum mechanics begs. Quantum mechanics is at least as successful as relativity theory in terms of making accurate predictions about the physical world to which it applies. This begs for an answer to the question: what is essentially different about the theory of quantum mechanics which makes it so difficult to interpret? Is it just that the world as revealed to us by quantum mechanics is so fundamentally different from experience and classical theory that it is beyond understanding in the usual ways? Or is there a structural or conceptual difference in the type of interpretation offered by relativity theory which separates it from those which have been offered for quantum mechanics?

One possible approach to answering these questions has arisen from the field of quantum information theory. Within recent decades, the field of quantum information theory has blossomed, with an array of researchers exploring the possibilities of this newly tapped resource. Among the areas that have seen fruitful research are quantum cryptology, quantum computation, and quantum information theory. Essentially, the promise of this field comes from the fact that it recognizes that quantum mechanics has several interesting features that can be exploited in the real world with remarkable results, such as quantum teleportation, the possibility of exponential increases in the speed of certain computations, and new communication protocols. Until the 1990s, these features of quantum mechanics had not generally been of central focus for practicing physicists; only an outside literature among philosophical circles paid them much attention, usually as problems to be solved, as opposed to features to be exploited. Now, it appears that quantum information theory might be able to offer new insight into an interpretation of quantum mechanics.

For all the interest quantum information theory is currently generating, it still remains a fledgling field. The theories that do exist are still relatively new and it would seem that many have yet to be discovered. Nowhere is it more evident just how fresh this approach to quantum theory is than in the research into the foundational issues of quantum mechanics. Here, there is a new push into the perennial problems of quantum mechanics from the standpoint of quantum information theory. Advances in the understanding of quantum information tantalize with the promise of providing insight into what it is to be a quantum theory. However, approaches to the philosophical issues in quantum mechanics from this perspective are largely disparate and less than cohesive. Just what might it mean to provide an information-theoretic interpretive approach to quantum theory, or any physical theory for that matter?

The application of the concept of information to physical theories or to the physical world can be made quite specific and technical in one sense, but on the other hand, it remains an enigmatic concept. In much of the literature on quantum information theory, and generally on information in physics such as thermodynamics, there is a tendency to link information and knowledge. The ease with which the term "information" can be anthropomorphized accounts for much of its appeal when dealing with fundamental issues in physics. As is the case with many of the foundational issues in physics, more abstract and complicated theories are interpreted according to principles or concepts that are easier to intuit. From a philosophical perspective, this is done because these more basic concepts offer a better understanding of the phenomena than more abstract theories. The concept of information might be in the position of offering this kind of understanding for a quantum theory long in need of such interpretational clarity. At first blush, the concept of information may seem to be promising. However, a second look makes it apparent that matters are not so simple.

The central question of this dissertation is “Why quantum information theory?” Philosophical interpretations of quantum mechanics through quantum information theory have gained traction in recent years. But does this provide any genuinely new insight into the interpretational issues quantum mechanics presents? There are several types of approaches which utilize information theory to answer philosophical questions regarding quantum mechanics. Answering the question above requires determining whether or not any of these approaches is successful. I argue that none have been, and some approaches appear unlikely to bear fruit. Does this mean that quantum information theory is an interesting theoretical diversion, but that it cannot play any interesting philosophical role?

After exploring various approaches, I find that quantum information theory does offer a promising framework for resolving the standard philosophical problems posed by quantum mechanics. This dissertation will argue that, upon analysis, fundamental physical theories (e.g. space-time) play the role that they do because they define the conceptual framework of empirical meaning. The establishment of an appropriate conceptual framework is necessary, and the type of analysis which precedes it generally arises out of crisis, when there are fundamentally conflicting concepts that require resolution. There is substantial reason to think that quantum mechanics is in such a state. What a foundational constitutive theory does is provide the coherent structure, through conceptual analysis, which defines an explanatory framework. The potential benefit which information-theoretic approaches hold over other interpretations is analogous to trying to develop general relativity with the geometric structure of Minkowski spacetime and other non-Euclidean geometric models available, as opposed to trying to develop it from Lorentzian mechanics. Information theory opens up a broader framework in terms of the concepts available to the theorist, and it provides a new way to analyze the structure of quantum mechanics as it stands.

The next chapter introduces the notion of information theory, delineating what philosophical work it is suited to do, and what it is not. This chapter also outlines some basic information-theoretic approaches to interpreting quantum mechanics that have been proposed. The view that information is somehow the fundamental "stuff" of the universe is considered and dismissed as incoherent. This chapter also considers quantum Bayesianism, as represented in the work of Fuchs, who argues that the quantum state is a measure of subjective belief. Instead of describing some aspect of the world, the quantum state represents our degrees of belief regarding the outcomes of measurements. I find that, for foundational issues in quantum mechanics, this approach fails in virtue of its focus on the subjective nature of quantum information theory. Essentially this approach results in a purely instrumental interpretation of quantum mechanics.

Chapter 3 takes a detailed look at another information-theoretic approach to quantum mechanics, originally proposed by Clifton, Bub, and Halvorson (2003). The authors purport to have found three information-theoretic principles which can be shown to be equivalent to the general features of a quantum theory. The interpretational significance of this approach is found to reside in the discovery of a *principle theory* version of quantum mechanics analogous to the principle theory approach of relativity theory. However, in this particular case, it is found to be unsatisfactory. Despite the theoretical utility and interest in their derivation, there is no reason to suppose that the information-theoretic principles are any more fundamental than the quantum mechanical physics to which they are equivalent. Accepting these principles requires accepting that measuring instruments must ultimately remain "black boxes," leading again to instrumentalism.

This chapter also discusses the distinction between *principle theories* and *constructive theories*, as suggested by Einstein. The distinction is evaluated according to the function of these types of theories. The best way to understand this distinction differs from what other authors have said in that it is based primarily on the explanatory roles fulfilled by each type of theory. It is concluded that principle theories act functionally as framework theories, providing explanation for laws via unification. The explanatory role played by constructive theories is to provide causal-mechanical explanation.

Tracing the analysis of philosophers from Kant to the logical positivists to recent works by DiSalle, Chapter 4 argues that certain types of theories – e.g. those involving space and time – require an added dimension of conceptual analysis. These theories supply the necessary framework for all the physics that takes place within their scope by establishing the meaning of the empirical structure itself. When revolutions occur in the physics of space and time, there is a pattern, from Galileo to

Newton to Einstein, of conceptual analysis which addresses inconsistencies and contradictions within current conceptual schemes, brought on by empirical discovery. Appropriate constitutive principles are developed such that they can redefine structural concepts for the framework of physics by reconciling previously incompatible frameworks. Principle theories necessarily act as our most foundational physics by establishing the basis for meaningful physics.

Chapter 5 returns to an information-theoretic interpretation of quantum mechanics with this new conceptual background in place. Bub and Pitowsky (2007) take the information-theoretic principle of *no cloning* and use it to develop an interpretation of quantum mechanics which shifts from a *dynamics*-based theory to the *kinematic* framework of Hilbert space, analogous to the shift in special relativity to the kinematic framework of Minkowski spacetime away from the dynamic theory of Lorentz. This is presented as a realist interpretation of quantum mechanics just as special relativity is a realist theory of spacetime. I argue that it is not the kinematic nature of special relativity which makes it a realist theory. Realism is not the central question. Instead we must look to the principle of no cloning to determine if it is constitutive in the right way regarding concepts of measurement and realism. There is no compelling case to be made that it is.

Chapter 6 applies what has been argued in the preceding chapters by speculating about where a resolution to the interpretational difficulties of quantum mechanics might lie and that quantum information theory is best placed to make such solutions possible. The standard philosophical concerns with quantum mechanics all involve the notions of measurement and causality. There is also a tension between quantum mechanics and the causal structure of relativity theory. Quantum mechanics needs to be subject to conceptual analysis, and it needs to play the role of being a constitutive theory – that is, a theory not simply of empirical generalization, but one which defines the concepts which establish a causal structure and meaningful measurement within the physical world. This may very well require conceptual resolution with relativity theory. Progress in quantum information theory broadens the conceptual space as did advances in geometry prior to the founding of relativity theory, thereby presenting new conceptual frameworks in which to unite previously incompatible conceptual schemes.

Chapter 2: Quantum Information

2.1. Introduction

As outlined in the introduction, the purpose of this dissertation is to investigate the role that the concept of quantum information theory might or might not be capable of playing in the philosophical foundations of quantum mechanics. The relatively recent advancements in quantum theory and its novel approach to investigating and, more importantly, utilizing various aspects of quantum phenomena have also sparked the interest of philosophers, for whom this new development potentially offers grounds for new insight into the intractable interpretational issues traditionally surrounding quantum mechanics.

In this chapter, I want, first, to establish more clearly what it is that we are talking about when we refer to information theory and quantum information theory. I also want to outline some of the various approaches that have applied some notion of quantum information theory in attempts to answer foundational philosophical questions regarding quantum mechanics. There are, I think, two ways of interpreting the role of information theory in quantum mechanics which suggest themselves immediately, and which have been pursued in various ways, sometimes in concert. Both follow from the general idea that 'physics is about information." But what this means in unclear. The first interpretation of this claim is that the application of information theory to quantum mechanics shows us that the world is made up of something even more fundamental than matter and energy, particles and waves, and that basic constituent is information. That is, this is a reductionist picture, where the basic stuff of the universe is information and so physics is ultimately about this stuff and its characteristics. The other way to see physics as being about information, is not to view information as some kind of stuff or basic constituent, but to see physics as about our state of knowledge or belief, stemming from the link between knowledge and information. Quantum information theory is the final vindication for those according to whom quantum mechanics was never a theory about the world, but one about our information, or knowledge, regarding it. At the end of the day however, both of these are motivated by a misunderstanding about the concept of information and they are untenable positions.

In this chapter we will see that information theory as applied to physics and quantum mechanics involves a very technical notion of information. In fact, this technical concept of information has only a partial overlap with the other concept of information in everyday use, which necessarily involves knowledge, language users and meanings. None of this is a part of the technical concept of information at play in information theory. This technical notion of information is an abstract noun due to the fact that it can only be understood as a *type* of thing, as opposed to a *token* which instantiates that type. Information is a statistical property of some information source. As such, it is a category mistake to think of information as somehow being the basic stuff of the universe. Upon analysis, it seems that this approach borders on incoherent. We shall also see that a prominent information-theoretic approach, that of the subjective quantum Bayesian, fails as a full blown interpretative stance since for all intents and purposes this approach is backed into an instrumentalist outlook.

2.2. There's Information, and then there's Information

In undertaking our investigations, it is necessary to make more precise what is meant by information and quantum information. Much of the excitement surrounding the field from a philosophical perspective stems from a vague idea that by bringing the concept of "information" into the realm of physics we can discover something fundamental about the world and our knowledge of it. This is especially the case in the age of the rise of the dominance of information throughout (information super highways, the power of information, information technology, etc.). But a lot of the talk is vague and enigmatic despite its promise. John Bell lists among the words that should be kept out of the formulation of quantum mechanics "information". He asks, "*Information? Whose* information? Information about *what?*" (Bell J. S., 1990, p. 34) This line of questioning pinpoints the fluidity of this term and highlights its various connotations, including the odd juxtaposition of a term, which usually implies that someone has information about something, with using it in a scientific context. We ought to heed Bell's warning and proceed with caution. What is information? What is the proper use of the concept in quantum mechanics?

Briefly, the most common definition of the technical concept of information, Shannon information, has to do with quantifying the amount of information in a communication channel. The communication channel consists of a source, a receiver, and the channel between them. Nielsen and Chuang (2000) characterize an information source as a set of probabilities p_j, $j = 1, 2, \ldots, d$. The source emits strings of letters j, each with a certain probability p_j. For example, the set j might be the letters of the English alphabet, and p_j corresponds to the frequency of the use of each letter in standard English. However, it is important to note that j need not be letters from any human 'language'. All that it means is that j is a discrete set of outputs from the source. A message produced by the source then is some sequence of letters j of length N. For messages with very large N, the message can be compressed to

$$H(p_j) = -\sum_j p_j \log(p_j)$$

bits of information (Nielsen & Chuang, 2000, p. 52), where a 'bit' (short for 'binary digit') is used to refer to the basic unit of classical information in terms of Shannon entropy, and to an elementary two-state classical system considered as representing the possible outputs of an elementary classical information source labeled 0 or 1. H is the measure of Shannon information, or the source probability distribution in terms of its compressibility, the Shannon entropy.

The analogous measure corresponding to quantum information is the von Neumann entropy. The von Neumann entropy is the measure of the compressibility of a quantum information source in terms of quantum bits, or qubits. Qubits are two-state quantum systems which may be labeled $|0\rangle$ and $|1\rangle$. Whereas, a bit may only be in states 0 or 1, a qubit can, in general, be in a superposition of its basis states; $|\psi\rangle = \alpha|0\rangle + \beta|1\rangle$. The von Neumann entropy for some state is defined as

$$S(\rho) \equiv -\text{tr}(\rho \log \rho) = -\sum_x \lambda_x \log \lambda_x,$$

where ρ is the density operator and λ_x are the eigenvalues of ρ (Nielsen & Chuang, 2000, p. 510).

In a particularly clear and sober analysis of the concept and use of "information" and "quantum information", Timpson insists on the crucial distinction between the technical concept of information used in information theory and defined as Shannon information, and information in the everyday sense. Others have also insisted that this distinction be maintained, including Shannon in the introduction to his seminal paper “The Mathematical Theory of Communication”, where he says,

> The fundamental problem of communication is that of reproducing at one point either exactly or approximately a message selected at another point. Frequently the messages have *meaning*; that is they refer to or are correlated according to some system with certain physical or conceptual entities. These semantic aspects of communication are irrelevant to the engineering problem. (Shannon, 1948, p. 379)

There is a critical distinction to be made between the technical notion of information, as used in quantum information theory, and our everyday sense of information. Timpson emphasizes and addresses this distinction in a number of places (2004; 2005; 2006). With careful philosophical taxonomy, Timpson is able to show that these two concepts are distinct and that it is also not feasible to argue from one to the other.

Any philosophical work that is to be done regarding quantum information theory must first be very clear on the distinction between the everyday concept of information and the technical sense of information. Timpson (2004) argues as follows. The everyday concept of information is based on the more primitive idea of that which is provided when one is *informed*. To inform is to bring someone to know something. Furthermore, “Concerning information we can distinguish between possessing information, which is to have knowledge; acquiring information, which is to gain knowledge; and containing information, which is sometimes the same as containing knowledge” (Timpson, 2004, p. 5). The important distinction is between *possessing* information and *containing* information. To have knowledge, or possess information, is for the knower to have an ability with regard to that information. An ability is a power or a disposition, which only persons have. To contain information, however, is to be in a certain state, and this is categorically distinct from possessing information or knowing, which is an ability. This state is either providing or being able to provide knowledge. That something can be in such a state – that of possessing information about something – comes from the fact that it contains information *propositionally*. In other words, containing information about something requires that the sentences or symbols in which it is expressed carry meaning. Such symbols only possess meaning because of their place in a framework of language and language users (Timpson, 2004, pp. 6-7). "[T]he concept of knowledge is functioning prior to the concept of containing information: as I have said, the concept of information is to be explained in terms of the provision of knowledge" (Timpson, 2004, p. 7). Therefore, the everyday concept of information is necessarily linked with language and knowledge, and, therefore, a knowing subject.

Any statement of fact is a candidate for being a *piece* of knowledge. Timpson makes a further differentiation between 1) a statement or proposition, 2) a sentence type, and 3) a sentence token. Starting at the bottom, a sentence token is a particular instance of a spoken or written sentence type, instantiated in the sound waves or ink patterns of which it is composed. The sentence type can be repeated, instantiated by more tokens of that type. Writing "Today is a holiday," and then saying "Today is a holiday," is to have produced two token sentences of the same type. This sentence type also expresses a proposition; that is, it carries meaning to a competent user of the language. A proposition is distinct from a sentence type, since a given proposition may be expressed with a different sentence type – in another language for example – but carry the same meaning.

A sentence token is the kind of thing that exists in a particular time and space. It is a concrete thing. Sentence types and propositions, however, do not exist in any time and space. They are abstract things, not part of the material world. Timpson continues to argue that the abstractness of types comes from the fact that they are properties of a given kind. The object, in this case the token, which has the *property* of being a certain type, will be a concrete thing, but the properties which it has are abstract things. Thus, the information, in the everyday sense, which is expressed in a proposition, is an abstract noun. Again, a proposition has meaning only in the context of language and language users.

For the technical notion of Shannon information, the everyday notion of information, having to do with meaning and knowing, is irrelevant, as is the notion of sentence type, or that a particular token is instantiated. All that matters is the particular pattern output by the source. This pattern is a type, of which there can be different tokens. Successful communication involves outputting another pattern of this type at the other end of the communication channel. Shannon information characterizes not individual messages, but the source of the messages. As a measure of the quantity of information, it represents the maximum amount a message produced by a particular source can be compressed without losing the reproducibility of the message at the receiver.

Though Shannon information is primarily concerned with being a measure of the quantity of information of a source, one can derivatively express information per letter associated with a message with *N* letters where *N* is large. A *piece* of information can also be defined derivatively.

> [I]nformation is what it is the aim of a communication protocol to transmit: information (in the technical sense) is what is produced by an information source that is required to be reproduced if the transmission is to be counted a success. (Timpson, 2004, p. 21)

This necessitates characterizing information sources, what they produce, and what counts as success. For the communication to be a success, it must at least be possible to reproduce a token of the type emitted by the source at the end of the protocol. The piece of information is the sequence type, since to identify the sequence produced by the source, we refer not to the token, but to the type. Successful transmission of the type means to produce a token at the source, and then reproduce a token of that same type at the output. The sequence type or probabilistic structure of the output has no bearing on questions of meaning and knowledge.

At face value, this technical definition of information has very little to do with the everyday concept of information discussed above. Shannon information does not give the irreducible meaning of the messages. Meaning is irrelevant.

> …*information* must not be confused with meaning. In fact, two messages, one of which is heavily loaded with meaning and the other which is pure nonsense, can be exactly equivalent, from the present viewpoint, as regards information. (Weaver, 1963, p. 8)

Timpson introduces the label information_t to discuss the technical concept of information and to distinguish it clearly from the everyday sense. The suggestion seems to be that much of the excitement and confusion over the development of quantum information theory stems from an unfortunate selection of language. If it had been called "Shannon statistical compressibility", as opposed to "Shannon information" dangerous conflations would never have arisen. This is not to say there is no relation, but instead of beginning there implicitly with no *prima facie* warrant, the connection would need to be explicitly demonstrated. For more on attempts that have failed to make the connection see Timpson (2004).

Both types of information end up being abstract nouns, but for entirely different reasons. As we saw, everyday pieces of information, true propositions, are abstracta. Shannon information, as a measure of the compressibility of a source is an abstract noun, not concrete. And pieces of information, as sequence types, are also abstract. Information in both senses is abstract, but since the notions are separate, the basis for this judgment likewise differs.

This exposition of Timpson's serves two purposes. First, it emphasizes the independence of two uses of the term "information". Second, it establishes the ontological status regarding both concepts of information as being types, and therefore, as abstract nouns, analogous to concepts such as number, which as an abstract concept implies that there is no place in the material world that one finds a number or a piece of information. What one may find in the world is a token which instantiates some piece of information. Both of these points will be exceedingly useful in determining the proper role for information theory in the foundations of quantum mechanics.

2.3. “All things physical are information-theoretic in origin.”

Some views on what makes the quantum information-theoretic approach foundationally interesting is that it supports a claim that information has some sort of ontological significance, either that it is the fundamental “stuff” of the universe or that the universe is best viewed as a massive quantum computer. If this sort of claim could be made, then quantum mechanics could indeed be reduced to a simpler and more basic set of ingredients, information. Though perhaps this seems a radical view, it would not be an entirely new proposal. Various authors have suggested that information is physical and that the world is basically made of information. For a variety of views see (Wheeler, 1990; Landauer, 1991; Lloyd, 2006). For example, Wheeler says,

> It from bit. Otherwise put, every 'it' – every particle, every field of force, even the space-time continuum itself – derives its function, its meaning, its very existence entirely – even if in some contexts indirectly – from the apparatus-elicited answers to yes-or-no questions, binary choices, bits. 'It from bit' symbolizes the idea that every item of the physical world has at bottom – a very deep bottom, in most instances – an immaterial source and explanation; that which we call reality arises in the last analysis from the posing of yes-no questions and the registering of equipment-evoked responses; in short, that all things physical are information-theoretic in origin and that this is a participatory universe. (Wheeler, 1990, p. 5)

Landauer, as another example, argues that “information is physical” and speculates that the laws of physics as algorithms for calculation (Landauer, 1996), and that the “laws of physics are, in turn, limited by the range of information processing available” (Landauer, 1991, p. 29). It has been argued that the formal resemblance between Shannon information, or entropy, and thermodynamic entropy suggests that there is a non-trivial link between them (see Leff and Rex 2003), perhaps pointing to something deep about the world. The problem, of course, is to specify what would it mean to say that information is somehow a fundamental constituent of our world?

First of all, it is again important to keep the everyday sense of information out of the picture. The question is whether information in the technical sense is the kind of thing that can be the fundamental stuff. Would this mean revising our concepts of things and of properties? What then is this manifestation of a physical world?

As Timpson (2004) has noted, the question of determining the meaning of a view like this is presented with a dilemma. On the one hand, if what is meant is that information, in the technical sense – itself a physically defined quantity – can only exist if it is instantiated in some physical manner, then the position is fairly trivial and, from a philosophical standpoint, uninteresting. Landauer can certainly be read in this way. However, as shown above, the actual occurrence of some information type means that it must occur in some physical token of that type, so it should be no surprise that all actual instances of information must be realized in a physical representation.

On the other hand, if what is meant is that the world consists of some basic stuff, and that stuff in information, then employing Timpson's analysis, it is immediately apparent that a category mistake is implicated in claims regarding the type of thing that information is. Information, in the technical sense, is merely a statistical measure of compressibility. A 'piece' of information is a sequence *type*. And it is instantiated in a physical token. Information is an abstract noun. To say that the world is made of information is analogous to the claim that the world is ultimately numbers. This is simply a confusion about what kind of thing a number is.

I will not here pass any final judgment on this proposition, but simply maintain that if it is to be carried forward, there is much that needs to be worked out, and it is not immediately obvious that it is not a non-starter. Such a view must recognize the category mistake and must then somehow argue around it, that, nevertheless, an abstract noun can somehow come to be the fundamental stuff of the universe. An analogously strange argument would need to be made to claim, for example, that the world is made out of numbers, or of relations. I cannot rule such an argument out as in principle impossible, but it would require a very strange ontology indeed, and seems at present to be incoherent.

2.4. Quantum Bayesianism

2.4.1. The View

Another recent trend in the philosophy of quantum mechanics hearkens back to interpretations offered by some of its original founders, particularly Bohr, though, with an information-theoretic spin. Since the days when the formalism of quantum mechanics was conceived, the peculiarities of the theory have led theorists to abandon a realist interpretation of quantum physics. Issues such as not being able to simultaneously ascertain the exact values of variables such as position and momentum, where classically this is in principle possible, or the fundamentally probabilistic predictions of the theory and related measurement problem, convinced many theorists that quantum mechanics is at best a purely instrumentalist theory. Quantum mechanics, with its overwhelming success as a predictive theory, was merely an accurate tool for such predictions, but it offered no access to some objective reality about the world. Niels Bohr phrased it thusly:

> There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how Nature is. Physics concerns what we can say about Nature. (Bohr as paraphrased by Aage Petersen, 1963, p. 12)

Additionally, realistic theories of the same physical phenomena appeared blocked. Einstein, Podolsky, and Rosen (1935), argued that the assumption that quantum mechanics was complete was incompatible with commonsense assumptions about reality. These assumptions are *separability* – that physical properties in one region are completely determined there regardless of other systems – and *locality*, which is the condition that there are no instantaneous influences across a spatial region. Together, these amount to an assumption of the possibility of a common-cause explanation. Bell (1964) was the first to show conclusively that a theory, which includes the conditions of separability and locality, could not arrive at the predictions of quantum mechanics, which corresponded with experimental results. Quantum mechanics, then, describes phenomena which simply cannot be explained in any purely standard causal manner. Einstein's issue is that physics must aspire to more than this. It must be able to describe a world where such explanations are in principle possible. But quantum mechanics does not allow this.

Spurred by recent advances in quantum information theory, some authors have suggested that quantum mechanics is simply about our own state of belief regarding the quantum world. Quantum information theory introduces a formalism for this approach to an interpretation of quantum theory. At first glance, it might also seem to offer a simple and intuitive structure on which to base the more abstract mathematical structure of quantum mechanics, thus providing a better understanding of the theory. I wish to show that despite aspirations to the contrary the subjectivist approach is essentially a sophisticated descendent of Copenhagen-style instrumentalism. Moreover, embracing the formalism of quantum information theory does not lend any support to motivate or justify this view. The basis for adopting the subjectivist standpoint come from the standard issues regarding quantum mechanics and is not specific to quantum information theory. Any temptation to think otherwise would only arise from not being careful about keeping the everyday concept of information, with its connotations regarding knowledge and mental states, distinct from the technical concept of information, which has nothing to do with claims of subjectivism.

Though the details differ, the basic inspirational spark for these views appears similar. For example, Zeilinger makes the following claims:

> The most fundamental viewpoint here is that the quantum is a consequence of what can be said about the world. Since what can be said has to be expressed in propositions and since the most elementary statement is a single proposition, quantization follows if the most elementary system represents just a single proposition…
>
> It is evident that one of the immediate consequences is that in physics we cannot talk about reality independent of what can be said about reality. Likewise it does not make sense to reduce the task of physics to just making subjective statements, because any statements about the physical world must ultimately be subject to experiment. Therefore, while in a classical worldview, reality is a primary concept prior to and independent of observation with all its properties, in the emerging view of quantum mechanics the notions of reality and of information are on an equal footing. One implies the other and neither one is sufficient to obtain a complete understanding of the world. (Zeilinger, 1999, p. 642)

Fuchs, one of the main forces behind what we might call subjective quantum Bayesianism says that it was actually Einstein who led him to the position that quantum mechanics is about subjective information or belief (2002). He quotes an excerpt from a letter of Einstein's to Michele Besso:

> What relation is there between the 'state' ('quantum state') described by a function ψ and a real deterministic situation (that we call the 'real state')? Does the quantum state characterize completely (1) or only incompletely (2) a real state? …
>
> I reject (1) because it obliges us to admit that there is a rigid connection between parts of the system separated from each other in space in an arbitrary way (instantaneous action at a distance, which doesn't diminish when the distance increases). Here is the demonstration: [The EPR argument]
>
> If one considers the method of the present quantum theory as being in principle definitive, that amounts to renouncing a complete description of real states. One could justify this renunciation if one assumes that there is no law for real states i.e., that their description would be useless. Otherwise said, that would mean: *laws don't apply to things, but only to what observation teaches us about them*. (The laws that relate to the temporal succession of this partial knowledge are however entirely deterministic.)
>
> Now, I can't accept that. I think that the statistical character of the present theory is simply conditioned by the choice of an incomplete description. (Letter from Einstein to Besso 1952; quoted in Fuchs 2002, p. 10. Italics are mine.)

Fuchs takes the results of no-go theorems such as Bell's as establishing that completing quantum mechanics in the ways philosophically required by Einstein is impossible. As far as it goes, Einstein's argument is taken to be airtight except that Einstein's refusal to accept such incompleteness in a physical theory does not logically follow from his argument that quantum mechanics is incomplete. Therefore, the quantum state *is* information. Fuchs says, "The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information" (Fuchs, 2002, p. 11). Instead of describing some aspect of the world, the quantum state represents our degrees of belief regarding the outcomes of measurements.

Fuchs argues that an accurate formal account of how quantum information works is by using non-commutative Bayesian probability theory. The best way to understand probabilities is from a Bayesian perspective and this is especially true of quantum probabilities. Quantum mechanics is the formal tool for arriving at the appropriate degree of belief for the outcomes of measurements. Once a measurement has been made, the collapse in the wavefunction is nothing mysterious in the world; it is just the updating of one's previous beliefs about the quantum system. The solution to the measurement problem and EPR-style paradoxes comes by showing these are not a problem when looked at from this purely subjective perspective. Nothing weird happens upon measurement; one just gains some new information, and so one's belief state changes. There is no unexplained collapse, no spooky action at a distance. Quantum states are not probability assignments to states of the world. As for any Bayesian account of probability, there is no right or wrong state of belief about the probability of an event, just one's subjective beliefs. When new information is available, one updates one's subjective beliefs on the basis of the information and Bayesian conditionalization rules.

Timpson (2007) argues that quantum Bayesianism is not an instrumentalist position. The basis for this claim is that the Bayesian approach has pretenses at ultimately reaching a very realist position. It is, for example, Fuchs' goal in building a robust subjective quantum theory in order to be able to strip all of the subjective aspect of quantum mechanics away so that some fundamental core which can answer the question about what makes it the case that the quantum world can only be represented as a subjective degree of belief.

> The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about something to do with that system… The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—miniscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself. (Fuchs, 2002, pp. 5-6)

That is, this program is presented as a realist one. As Timpson notes, yes, the quantum Bayesian is an instrumentalist about the quantum state, however, not an instrumentalist about quantum mechanics *toute court*. Timpson ultimately rejects the quantum Bayesian position on other grounds. However, what I hope will become clear is that these grounds all stem from the same source, and it is that there is a fundamental tension between holding that our best and only possible theory of the quantum world is and must be about our subjective degrees of belief, i.e. it must be instrumentalist, but that we can nevertheless "get at" some picture of the real quantum structure of the world. It is consistent to hold that our theories are purely instrumental and that there are facts which are true of the world. Where the problem comes in is to assert that what the theory provides is a way to update our purely subjective probability assignments. In doing so, it is left unexplained why we should use this method as a reliable way to make predictions. So while the ultimate aim of the quantum Bayesian to find a realist kernel of quantum theory, it is nevertheless backed into an instrumentalist position on the basis of it opening assumptions.

2.4.2. Critique

Timpson poses three problems for the quantum Bayesian picture. These are the problem of explanatory deficit, that the quantum Bayesian is committed to a Moore's paradox-style problem, and that the means employed by the quantum Bayesian are not appropriate to their ends. The first of these problems is that the quantum Bayesian has trouble with providing scientific explanation. Timpson holds that for a non-instrumentalist theory, some level of explanation is a requirement. And while quantum Bayesianism is purportedly not instrumentalist, it is hard to give an account by which it can provide adequate explanation for phenomena which we generally take quantum mechanics to explain. That is, quantum Bayesianism seems to suffer from an explanation deficit. The Bayesian can explain how agents arrive at their beliefs about some event taking place, but what a theory is supposed to do is explain why the event takes place.

For example, we want quantum mechanics to be able to explain why some solid bodies conduct electricity and others do not. For quantum Bayesianism, the quantum state does not tell us anything at all about the quantum properties of objects. All that the quantum state is is a tracking device regarding our subjective degrees of belief about what we expect the system to do. There is no fact of the matter, regarding the microstructure of solid bodies, which explains their various conductive properties. The same story applies to any number of phenomena which we would naturally want and take quantum mechanics to explain. If the quantum state can say nothing about the microstructure, quantum mechanics can only be a predictive tool, not an explanatory device, without some further story about explanation which differs from the standard conception.

The second problem Timpson is concerned with is what he calls quantum Bayesianism's analogue to Moore's paradox. The apparent paradox here is in making a statement such as, "It is raining, but I believe it is not raining." Timpson's concern is that the quantum Bayesian position is committed to a similar type of assertion, what he calls the quantum Bayesian Moore's paradox or QBMP: "I am certain that *p* ([e.g.]that the outcome will be spin-up in the *z*-direction) but *it* is not certain that *p*" (Timpson, 2007, p. 35). That is, it is often the case that a quantum state will be in a pure state with respect to some observable. In this case, the subjective probability that a measurement of that observable performed on the system will return a given value will be one. That is, the result is believed to be certain to occur given the measurement. However, even in the case of a pure quantum state, the subjective quantum Bayesian view is committed to the idea that the quantum state is still purely subjective. There is no objective fact of the matter which determines the measurement outcome, even in the case of pure states. Therefore, there seems to be some sort of tension here between the expectation that an outcome will occur with certainly, and the claim that there is no fact of the matter before hand.

The Bayesian response is that nothing surprising is going on here if we look at it from the right perspective. A story about some underlying structure will not make it any less surprising for the agent whose subjective beliefs are that the quantum system is in a pure state. Whatever Bayesian process occurred for the agent, which led to that assignment, entirely explains the expectation that a particular result will occur with certainty. What needs to be distinguished is the difference between an agent being certain of something, and "*It* being certain" that something is the case. The first is a cognitive state. If I (an agent) do not consider it possible that *x*, then I do not consider it to be possible that not *x*. This is logically independent from the claim that "It is certain that *x*," which is about facts in the world. Therefore, it is not a logical contradiction to say, "I am certain that *x*, but it is not certain that *x*." Likewise, it is possible for the Bayesian to contend that it is perfectly acceptable to give subjective probability assignments of certainty for the agent, while maintaining that *it* is not certain that the outcome will obtain.

Timpson replies to this defense by allowing that we can admit that providing any underlying facts will not alter the subjective belief set of the agent who is certain, thereby disconnecting the certainty of the agent from certainty of things. However, the problem is greater than this. It is not simply the disengagement of a different notion of certainty which needs to be addressed, but the further tension that the Bayesian agent must be certain of a particular measurement outcome, and yet actively *deny* that there are any relevant facts which determine it. "Isn't the agent simply convicting themselves [*sic*] as irrational?" (Timpson, 2007, p. 36) Moore's paradox, presents a puzzle for an individual case where the certainty of an agent conflicts with what seems ought to be the case regarding what is in fact certain. The quantum Bayesian is committed to a much more general and systematic adoption of this sort of position.

The third difficulty Timpson brings up is what he calls the *means/end objection*. The basic problem is that for the quantum Bayesian there is a fundamental disconnect between the ends which they seek and appropriate means to reach them. According to Timpson, there are two distinct ends for the Bayesian view: "one of finding out how the world is; the other the pragmatic business of coping with the world" (Timpson, 2007, p. 37). For the latter of these ends, fundamentally, this means using quantum subjective Bayesian updating, on the basis of the data we have available, to make predictions about future outcomes. However, there is a gap between our means, subjective updating using data from experiments, and the end of being able to make successful predictions. If there are only subjective probabilities, then there seems to be no reason to expect the data to help lead to better predictions. The Bayesian view blocks the possibility of there being any good reasons to expect our subjective beliefs to match outcomes.

To this I might add my own means/end objection, though of a different nature. This has to do with the first end Timpson notes, the quantum Bayesian goal of discovering some fundamental quantum truth, and the means of reaching it, the subjective Bayesian approach. The concern is that the strategy of "picking off all the terms… that can be interpreted as subjective information" to discover the "raw distillate left behind" (Fuchs, 2002, p. 6), depends on the subjective approach being justified independently. If it is not, and it is employed to diffuse standard puzzles such as the measurement problem, then stripping away quantum axioms which can be reformulated in subjective terms, such as the quantum state, may very likely strip away all aspects of the theory which make it a quantum theory. With nothing left but a subjective theory, we are left in an instrumentalist position. It is not clear that stripping out what can be put in subjective terms is not stripping out what might be important from a "raw distillate." This is a concern which may of course be answered by the still open program of the Bayesian approach, but it is not obviously clear that the strategy for finding some deep quantum structure this way is not a non-starter. If the task it to answer the question "why the quantum?" then relabeling and removing quantum aspects of the theory, such as entanglement, as subjective may amount to plastering over the issue as opposed to analyzing it.

2.4.3. Walks Like, Swims Like, Quacks Like

All of the difficulties Timpson discusses are symptomatic of the underlying problem, and that is that quantum Bayesianism, by adopting as its most central tenet a subjective interpretation of the quantum state, is unable to get beyond an instrumentalist theory. Timpson argues that the quantum Bayesian position is not in fact instrumentalist, due to its realist ambitions. However, this may simply be splitting hairs, when, at the end of the day, the problems suffered by quantum Bayesianism are those suffered by instrumentalism and when no clear realist picture is available.

The three problems Timpson describes are all problems which arise not just for quantum Bayesianism, but are also central objections to instrumentalism. The first is that we want more from our theories than to be merely successful tools for calculating predictions. We want them to explain phenomena. By maintaining that the quantum state is just a subjective probability, we can say very little about the quantum level which would explain things we want quantum mechanics to explain. Likewise an instrumentalist interpretation of a theory gives very little to explain phenomena which the theory covers. It is not good enough to account for the agent's expectations. Instrumental theories also give us a means to develop rational expectations about future events. What neither does is explain why the events themselves come about.

As far as the problem of Moore's paradox is concerned, the instrumentalist does not really fall directly into it since the instrumentalist need not make any claims that any theory provides the agent with certainty except regarding empirical outcomes. There is a similar, though weaker tension nevertheless. The instrumentalist maintains some level of trust in the predictions a theory generates, or why go with a theory at all, yet at the same time denies there is any reason behind this past and expected future predictive success related to the real world. As we shall see, the Moore's paradox problem for the quantum Bayesian is tied both to the problem of explanation and Timpson's means/end objection, which are central concerns for an instrumental stance.

The means/end objection, which Timpson says is related to the problem of Moore's paradox, is also a problem for the instrumentalist. Theories are predictive tools. We may say that we have evidence that they work as predictors on the basis of experience with using the theory. That is, the justification for their use comes from their predictive success. It may seem then that the means/end objection is just a problem for the subjectivist and not for the instrumentalist, who can defend the view on the basis of empirical success, whereas the subjectivist does not even have that. However, Boyd (1973; 1984) makes an argument against instrumentalism very similar to the one which Timpson makes against Bayesianism. On the standard instrumentalist account of scientific theories, the content of a scientific theory makes no assertoric claims which are true or false of the world. That is simply not their job. The only elements of a theory which are assertoric are its empirical predictions, and these can be tested via direct observation. Boyd argues that an instrumentalist conception of scientific theories fails to account for the instrumental reliability of theories however. Instrumentalism does accept this reliability, since this is just the statement that we can trust the empirical predictions made by the theory (and without this, science is doing nothing for us). However, Boyd argues, the predictive reliability of theory-dependent judgments can only be explained if, to some extent, they are approximately true. Making empirical observations always requires theory-dependent judgments.

The problem of explanation seems to be creeping in for the instrumentalist as for the subjectivist. The explanatory gap problem is that an instrumental or subjective theory can not explain phenomena, but merely predict them. The means/end objection is simply the redirection of the explanatory gap problem towards the application of the theory itself. For Timpson, it applies to the quantum Bayesian, because, for the Bayesian, there is no *reason* to look at the data and use it for updating our beliefs and think that it should help us “cope with the world”. According to Boyd’s, and similar arguments, any observation is theory-laden. Any test of the predictions it makes must include a principle such as the following: "a proposed theory *T* must be experimentally tested under situations representative of those in which, in the light of collateral information, it is most likely that *T* will fail, if it's going to fail at all" (Boyd, 1973, p. 10). However, such a principle is reliable only insofar as it is explained according to a realistic interpretation of the collateral theories. For the quantum Bayesian and the instrumentalist, there is no explanatory account that would justify how the desired ends (being able to make predictions) should be met by the means (data gathering) one uses to do so. It is here that the explanatory gap argument is particularly forceful.

At the end of the day, the problems for instrumentalism and subjectivism are more or less the same (in some respects, the Bayesian may have more trouble answering the problems). The most central problem for instrumentalism can be summed up with Putnam's claim on behalf of the opposing view that "[r]ealism is the only philosophy that does not make the success of science a miracle" (Putnam, 1975, p. 73). That is, from a realist perspective, quantum Bayesianism and Copenhagen-style instrumentalism are on a par and suffer from the same issues by committing to a view of the theory which specifically and consciously does not make claims about any fact of the matter, and which nevertheless guides what we ought to expect to observe. Of course, the quantum Bayesian position offers a promissory note of some deeper realism. One may hold out hope of such a program finding success, but it is not yet clear how it will come about or if it can.

It should be noted that it may very well be compatible with an instrumentalist approach to scientific theories in general, or to a particular theory, that it consistently maintain that there is some fact of the matter regarding reality entirely independent of our theories. The claim is that our theory tells us nothing about such a reality. This is an epistemological claim. As Leplin notes, more recent accounts of instrumentalism, responding to problems with formulating entirely non-assertoric accounts of instrumentalism, allow that theoretical claims can be true or false in virtue of the way the world is (2000, pp. , 394). But this is only metaphysical realism; the truth or falsity of a theoretical claim can have no bearing on its scientific utility. Truth or falsity is impossible to verify and irrelevant in any case. Instrumentalism as an epistemological stance breaks any connection between the predictive success of a theory and the possibility of its success as an approximately true description of the world. If quantum Bayesianism can do no more than this, any aspirations to metaphysical realism are empty. That is, without a clear means of filling in its metaphysical hopes, quantum Bayesianism seems to fall into this account of instrumentalism.

The point is that while Timpson allows that quantum Bayesianism is not an instrumentalist approach, since it ultimately aims at discovering something real beyond what is merely subjective, the criticisms which can be strongly levied against the Bayesian views are just those brought against the instrumentalist approach. If one demands that this is not an instrumental approach on the basis of its aspirations, this is compatible with what I have to say since the issue primarily rests on a choice of terminology and categorization. At the end of the day, however, the view fails to deliver for just those reasons that a realist insists on rejecting the instrumentalist position.

A central point of my argument here is that the subjective quantum Bayesian approach to quantum mechanics, such as Fuchs', needs to beware of violating Timpson's warnings that information theory is not about knowledge or knowing subjects. It is not about belief either. If we seriously heed this warning, it is clear that the argument that quantum mechanics is subjective (i.e. it is a theory about our knowledge) relies completely on the same crucial points of a very old debate in the philosophy of science about whether the task of science is to develop theories that are about an objectively real world, or whether this is a metaphysical goal beyond the reach of our epistemological capacities, often on the basis of the logical underdetermination of theories given the data. This leads to the idea that science is about developing theories as instruments that merely help us make accurate predictions. They have nothing to say about reality. Of course, the situation gets more complicated when it comes to quantum physics, because here a clear-cut realist theory actually seems to be fundamentally blocked by limitations such as Bell's inequality. Nevertheless, the claim that quantum mechanics is purely instrumental is an epistemological claim, which is a conceptually separate issue from quantum information theory. Quantum information theory may appear, at first sight, to be the ideal way of formalizing a quantum mechanics that is only about our beliefs regarding the world. That is, I fear there is a temptation to slide from new developments in quantum information theory, to claims that this shows that quantum mechanics has always been about information, and that it is only about what we can say about the world and not the world itself. However, this is a slide from a purely technical concept of information to information in the everyday sense, which is then linked to a subjective state of belief. But this is an illicit confusion. As such, that the term "information" is used in a purely technical sense should not be regarded as in any way backing up the instrumentalist claim that quantum physics is only about our state of belief. Any philosophical hay that might be made from quantum information theory must be due purely to its technical formalism, and not that it is more closely related to any subjective account of the foundations of quantum mechanics.

It follows that any arguments for the subjective quantum Bayesian approach will be based on standard instrumentalist arguments. Information theory does not enter the picture. Likewise, any arguments against a subjective quantum Bayesianism views, such as Fuchs', are, therefore, based on broader realist arguments against instrumentalist interpretations of quantum mechanics. At the forefront of these, is the feeling that the very success of a theory that is about the updating of our states of belief ought itself to be explained, and therefore justified, if we are to have good reasons for using the theory. Otherwise, its success seems completely mysterious. Another argument is simply that physics must be about an objective world and offer approximate explanations of it or it is not clear what we are doing in science.

In any case, my argument here is not against instrumentalism, though I do not endorse it. I take such an argument to be ultimately fruitless and based primarily on conviction rather than argument and evidence. The conclusion of this section is that the subjective quantum Bayesian approach to quantum mechanics, which uses quantum information theory, must be wary of equivocation regarding what is contained in the notion of information in the technical sense and that in the common usage sense. The fact that, from a technical standpoint, we can apply information theory to quantum mechanics does not give us any reason to think that quantum mechanics is about our states of belief. It follows that, despite claims to the contrary, quantum information theory does not resolve any foundational issues in quantum mechanics when it is used to develop a subjective interpretation of what quantum theory is about.

Chapter 3: Principle Theories and Constructive Theories

3.1. Introduction

We have now looked at some information-theoretic approaches to interpretational issues facing the theory of quantum mechanics. It has been shown that these approaches either hinge on a category mistake about the kind of thing information is in its technical sense (or everyday sense), or amount to a retelling of the instrumentalist story regarding quantum mechanics, but in subjective Bayesian terms. Whatever merits there may be in employing the language of information theory in discussing quantum mechanics – and there may be interesting ones, both theoretically and practically speaking – the approaches discussed in Chapter 2 do not suffice to answer the traditional philosophical concerns with quantum mechanics.

However, there may be another way in which the concepts of quantum information theory may be utilized for philosophical gain. In a careful use of the technical notion of quantum information, Clifton, Bub, and Halvorson (CBH 2003) propose to have found three information-theoretic principles that can be shown to be equivalent to that which they take to be the general features of a quantum theory. In their formulation, CBH are careful to maintain that the notion of information used here is restricted to the technical version. The philosophical work, it is argued, is done by formulating quantum mechanics, using straightforward information-theoretic constraints, in a manner analogous to the way Einstein used simple empirical principles in the formulation of special relativity.

This chapter will investigate the CBH approach, the ideas explored in the original paper, as well as considerations undertaken by Bub in subsequent papers (2004a; 2004b) articulating the program further. The question is whether this information-theoretic approach is philosophically fruitful. In particular, does it meet its interpretational goals regarding the philosophical issues of quantum physics? A preliminary task is to determine what these interpretational goals are. As an interpretational program, there are three possible routes by which CBH might offer novel insight. By the end of this chapter we will also be able to offer some idea of what it means to ask for and to give an interpretation of a theory. What we will find, broadly speaking, is that a demand for interpretation is a demand for explanation.

The first way of reading CBH is as a fairly straightforward argument establishing the view that quantum mechanics is best interpreted as an instrumentalist theory, where the measuring instruments are to be treated as opaque black-boxes, and that information-theoretic language is best suited to formulating this approach. This approach should be discounted for a number of reasons. First, an instrumental interpretation is not much of an interpretation. Second, the role played by information-theoretic principles appears unnecessary from a philosophical perspective. The call for instrumentalism can be made whether couched in information-theoretic language or in the standard mechanical formulation, but as CBH has shown, the two are formally equivalent. So nothing extra is gained which establishes this conclusion by switching to an information-theoretic stance.

The second possible reading is not particularly viable. A realist spin might be to propose that we should not think of physics as about particles and waves, but information as the basic stuff and that CBH gives us the means to do so. This type of approach has already been dismissed as suffering from a category mistake regarding the concept of information. It does not seem to be the aim of CBH to put forward such a claim, and as we have seen this is not a promising approach. We will not discuss it further.

Finally, in this chapter we will investigate the role played by principle theories. Perhaps, because CBH is a principle theory, as opposed to a constructive theory such as the standard mechanical formulation of quantum mechanics, CBH can offer some alternative interpretational solutions. To investigate this further, we will take a much closer look at the distinction between *principle theories* and *constructive theories* made by Einstein in describing his theory of relativity. What we will discover is that the interpretational role of both kinds of theories derives from the kinds of scientific explanation they provide. Constructive theories are fundamentally explanatory. Their source of explanatory power comes from the ability to offer causal-mechanical explanations of phenomena, a type of scientific explanation advocated by Salmon (1989).

Principle theories are also explanatory. The preliminary conclusion of this chapter is that the primary function of a principle theory is tied to the explanatory role it plays through unification. The standard account of explanation as unification is that advanced by Friedman (1974) and Kitcher (1989). While principle theories may often offer this kind of explanation by unification, it turns out that a better understanding of the role of principle theories for Einstein and for CBH can be articulated. In Chapter 4, it will be argued that in these instances, the role played by principle theories is a constitutive one. It is this role that is of primary importance for understanding principle theories such as special relativity, and as I will argue, for quantum mechanics. It will also be left to Chapter 4 to argue that the constitutive nature of a foundational principle theory has an explanatory function essential for establishing the explanatory framework for empirical theories by way of resolving conceptual inconsistencies. This occurs at a different level than the unification of phenomena described by Kitcher, and it functions as a precondition for this type of unification.

With this explication of the roles of principle and constructive theories, we can see why a successful constructive theory of quantum mechanics has been so notoriously difficult to find, since a standard causal-mechanical explanation of certain phenomena is blocked by Bell-type no-go theorems. Reformulating quantum mechanics as a principle theory, in itself, does not provide an interpretation of these difficulties or make them go away. If a principle theory approach is to succeed interpretationally, it must establish the possibility of unification which gives a principle theory explanatory merit. It is argued in this chapter that the CBH approach does not provide unification in the standard Friedman/Kitcher sense beyond that which is already given by quantum mechanics. As such, though it is presented as a principle theory, in this particular sense, CBH does not offer any further explanation for quantum phenomena. Therefore it does no interpretational work either. In Chapter 4, we will consider the conceptual work that can be done by a principle theory and the interpretational role they can play as constitutive theories, as well as the success of CBH considered in this light. Having done the preliminary work in this chapter of establishing the explanatory role of principle theories as providing explanation by unification, we will be able to see how the success of theories such as special relativity stems from the constitutive nature of their principles. If CBH is going to provide an interpretation of quantum mechanics, it is this aspect of principle theories, which is discussed in the next chapter, that must be at work.

3.2. Informational Constraints as Principles of Quantum Mechanics

Clifton, Bub, and Halvorson's paper, "Characterizing Quantum Theory in terms of Information-Theoretic Constraints" (CBH, 2003) shows that the fundamental elements of a quantum theory can be deduced from three information-theoretic principles. This technical result motivates the claim that quantum mechanics can be viewed as a *principle theory*. The primary inspiration for thinking that quantum mechanics should be viewed as a principle theory comes from a direct analogy with Einstein's insight into his own theories of relativity, combined with their apparent lack of need for interpretation as perceived by the physics and philosophical communities. The fact that quantum mechanics can be derived from a relatively small set of principles, which are in fact a set of information-theoretic constraints, is itself a strongly motivating breakthrough. In the most complete version of this program, Bub (2006) advances this line of thought, arguing that from a foundational perspective, "quantum mechanics is *a theory about the representation and manipulation of information* constrained by the possibilities and impossibilities of information-transfer in our world, rather than a theory about the ways in which nonclassical waves and particles move" (Bub, 2006, p. 95).

The CBH result, with Halvorson's (2004) addendum, shows that quantum mechanics can be successfully represented as a set of constraints, or principles, on the transfer, manipulation, and representation of information. It is at least implicitly argued that a physical theory which is based on a small set of principles offers an interpretational advantage over existing interpretations of quantum mechanics. One argument for this seems to rely on the interpretational success of the special and general theories of relativity as seen through the lens of Einstein's own statements regarding these theories as principle theories. Specifically, regarding quantum mechanics and the CBH approach, Bub (2004a; 2004b) argues that the information-theoretic approach is the only viable option for the foundations of quantum theory.

The CBH approach develops a theory of quantum mechanics from simple and conceptually appealing principles. For them, information-theoretic principles fit the bill the best. CBH begin with a structural framework broad enough to include all of the various physical theories that are available to modern science. The mathematical framework used is the abstract C*-algebra. This framework captures various classes of theories including classical and quantum ones. The strategy is to come up with restrictions on this abstract structure, which divide it into those theories that are classical in nature and those that are quantum mechanical. The first task of the authors is to find physical characteristics that are definitive of a general quantum theory. These are:

- that the algebras of observables pertaining to distinct physical systems must commute, usually called microcausality or *kinematic independence*;
- that any individual system's algebra of observables must be nonabelian, i.e., *non-commutative*;

- that the physical world must be *nonlocal*, in that spacelike separated systems must at least sometimes occupy entangled states. (CBH 2003, 4)

The authors claim that there are three information-theoretic principles that are entailed by these characteristics and which likewise entail them. These equivalent information-theoretic principles are:

- the impossibility of superluminal information transfer between two physical systems by performing measurements on one of them;
- the impossibility of perfectly broadcasting the information contained in an unknown physical state; and
- the impossibility of unconditionally secure bit commitment. (CBH 2003, 3)

The CBH argument shows that a classical theory is equivalent to an abelian C*-algebra. This means that a quantum theory must be non-abelian, or non-commutative. Furthermore, non-commutivity mathematically entails non-local entanglement, but only mathematically. The possibility of broadcasting is shown to imply an abelian framework, and the reverse, that commutivity entails the possibility of broadcasting, is also shown. Therefore, non-commutivity entails the impossibility of broadcasting information. The impossibility of superluminal information transfer is shown to be equivalent to kinematic independence. Finally, no bit commitment is shown to guarantee the existence of non-local entanglement. In this paper, CBH are only able to motivate the entailment of no bit commitment from non-local entanglement, but this entailment is proved in Halverson (2004). Thus, CBH have shown that their three information-theoretic principles are equivalent to their characterization of quantum mechanics.

The question is now, so what does this mean? There is no doubt that this is a very interesting result. CBH take it to show that quantum mechanics can be represented as a principle theory, specifically, one that postulates “that we live in a world in which there are certain constraints on the acquisition, representation, and communication of information” (CBH 2003, 3). As we shall proceed to show, a principle theory can potentially offer what Einstein calls security of the foundations, but can this formulation do any philosophical work for us?

If we are dealing with these information-theoretic principles using only the technical concept of information, then how do we decide whether this formulation is more philosophically successful than the traditional mechanical formulation of quantum theory that deals with particles and waves? In a follow-up article to the original CBH, paper Bub (2004a) addresses this question. He argues that if we assume that these three information-theoretic constraints hold in our world then we are faced with the measurement problem. This is a problem involving the linear dynamics of quantum theory, which describe the interaction of a quantum system with a measuring instrument such that they result in a superposition of states. The eigenvalue-eigenstate rule then holds that only a state in the eigenstate of a particular property actually has that property. Thus, it is possible to have an object and measuring instrument in an entangled state such that the measuring instrument has no definite property corresponding to a particular pointer reading. This does not accord with experience however. The measurement problem involves working out how it is that we experience what we do given the quantum description. One route to developing a solution is to fiddle with the dynamics. The “orthodox” interpretation of quantum mechanics postulates a “collapse”, such that the quantum state collapses into one of the eigenstates upon measurement. Such a solution, however, provides no physical account of this collapse, essentially leaving the instrument out of the physical description, leaving it a black box through which we access the observed features of the world. A GRW-type collapse theory (Ghirardi, Rimini, & Weber, 1986) also alters the dynamics of quantum mechanics by postulating that there is a small probability that the wavefunction of a particle will collapse spontaneously. Bub (2004a) argues that this alteration of quantum theory would violate the principle of no unconditionally secure bit commitment by allowing cheating due to the destruction of entanglement from spontaneous collapse.

Alternatively, one might propose solutions given by no-collapse theories such as Bohm's or Everettian many-worlds interpretations. Bub (2004a) argues that according to the Bub-Clifton Theorem (Bub & Clifton, 1996) no-collapse theories should be regarded as theories required to offer a mechanical explanation for the behavior of the measuring instruments, and so must give an explanation of the informational constraints that have been assumed to exist. On the one hand, such theories may provide a mechanical account of measurement results and violate the information-theoretic principles. But then such theories are not quantum mechanical in the sense defined by CBH. If a theory is quantum mechanical in this sense, then it must be empirically equivalent to orthodox quantum mechanics. Therefore, Bub argues, such theories, by their very nature, cannot provide empirical evidence for their mechanical account of measurement devices beyond that of orthodox quantum mechanics. Therefore, there are in principle no empirical grounds for accepting a no-collapse theory over standard quantum mechanics. All of these theories are explanatorily equivalent. The rational epistemological stance to take at this point is to suspend judgment on these mechanical theories. This means that measuring instruments remain black boxes in the theory. As such, the quantum theory amounts to a theory about the representation and manipulation of information. Thus the three theses of Bub's paper:

1) *A quantum theory is best understood as a theory about the possibilities and impossibilities of information transfer, as opposed to a theory about the mechanics of nonclassical waves or particles.* (By 'information' here I mean information in the physical sense, measured classically by the Shannon entropy or, in a quantum world, by the von Neumann entropy.)
2) *Given the information-theoretic constraints, any mechanical theory of quantum phenomena that includes an account of the measuring instruments that reveal these phenomena must be empirically equivalent to a quantum theory.*
3) *Assuming the information-theoretic constraints are in fact satisfied in our world, no mechanical theory of quantum phenomena that includes an account of measurement interactions can be acceptable, and the appropriate aim of physics at the fundamental level then becomes the representation and manipulation of information.* (Bub, 2004a)

This argument is basically an application of Occam's razor. When we are presented with a set of underdetermined theories, we are not warranted in postulating extra metaphysical assumptions that are in principle untestable. In this case, mechanical formulations of quantum theory are necessarily underdetermined in this way. As a result, we cannot explain the way instruments interact with quantum systems to give us the empirical results that we see macroscopically. This means that instruments remain black boxes at some level. According to Bub, this is equivalent to saying that quantum theory is a theory about information. Measuring instruments, depending on where one draws the line, at some level, must be viewed as information sources.

<u>3.3. Principle and Constructive Theories</u>

3.3.1. Introduction

We have been looking at this paper of Bub's to determine whether this information-theoretic formulation of quantum theory is somehow more philosophically successful than the standard mechanical formulation of quantum mechanics. As pointed out in the introduction to this chapter, Bub's paper lends itself to three possible readings in terms of the interpretational work of which the CBH approach is capable. The first is that it is an argument that quantum mechanics ought to be interpreted instrumentally, with the preferred fundamental structure reduced to an information-theoretic framework. Second, it is trying to carve out a position in which information has some kind of special ontology, and so we are better off thinking of the world as information, rather than as particles and waves. If these readings are rejected – as we argued in Chapter 2 they should be – the information-theoretic principle theory must purport to offer some other interpretive stance, in virtue of being a principle theory, to which the standard mechanical formulation does not have access.

This third interpretive possibility will require a bit more investigation occupying the remainder of this chapter and the next. This approach trades on the distinction between *principle* and *constructive* theories, and the framing of a principle theory approach to quantum mechanics. First, however, it is important to understand that though quantum mechanics *can* be represented as a principle theory does not imply that this representation provides a more successful interpretational basis for quantum mechanics. For CBH, there is a great deal riding on the role of principle theories, versus that of constructive theories. We need to explore how this can be cashed out.

In this section, we explore the distinction between principle theories and constructive theories. We will discuss how this distinction has been understood by others. In particular, Flores argues that a better way to understand this distinction is in functional terms as opposed to the ontology which supports it. He revises the distinction somewhat into what he calls *framework* and *interaction* theories (Flores, 1999). I emphasize that the particular function of central importance to this revision is the explanatory function of the theory, either as a unificationist explanation or a causal-mechanical explanation. Both unificationist and causal-mechanical explanation are valid and scientifically valuable. As such, both types of theories, principle and constructive, can be considered to have important functions in science, with different, but equally strong roles to play. This chapter concludes with the preliminary claim that the distinction between principle theories and constructive theories is best understood as depending on explanatory requirements.

It will be argued, however, that this is not the whole story. Marking the distinction between constructive and principle theories merely as a distinction between causal-mechanical and Friedman/Kitcher-style unificationist explanation, respectively, misses important nuances regarding the role played by an important class of principle theories. A principle theory can provide explanation by unification in the Friedman/Kitcher sense, and we will see that CBH does not succeed in providing unification in this sense of explanation. However, this is not the only sense in which a principle theory can do philosophical work. Principle theories can function at different levels in terms of establishing the possibility of explanation by unification. In Chapter 4, we discuss in more detail this aspect of the function of principle theories, specifically in foundational theories such as Newton's or Einstein's. There it will be argued that the best way to understand this distinction in relation to its invocation by CBH is according to the constitutive characteristic that principle theories can have. Principle theories can be constitutive of empirical meaning and thereby act as preconditions for the explanatory framework itself. We will discuss the success of the CBH approach as a constitutive principle theory in the next chapter. As it will turn out, this latter sense offers a better understanding of Einstein's introduction of the notion of principle theory as it applies to theories such as special relativity as well as for CBH.

3.3.2. Laying Some Groundwork

Central to the CBH argument is the distinction between *principle theories* and *constructive theories* – a distinction raised by Einstein (1954b) regarding his own theories of special relativity and general relativity. The CBH program explicitly compares itself with that of Einstein, who formulated his special theory of relativity from the two principles that 1) physics in any inertial frame is the same and that 2) the speed of light is constant for all observers. Regarding this theory, Einstein invokes a distinction between two conceptually distinct types of theories – principle theories and constructive theories – in "What is the Theory of Relativity", saying,

> We can distinguish various kinds of theories in physics. Most of them are constructive. They attempt to build up a picture of the more complex phenomena out of the materials of a relativity simple formal scheme from which they start out… When we say we

> have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question.
>
> Along with this most important class of theories there exists a second, which I will call ‘principle-theories.’ These employ the analytic, not synthetic, method. The elements which form their basis and starting-point are not hypothetically constructed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy…
>
> The advantages of the constructive theory are completeness, adaptability, and clearness, those of the principle theory are logical perfection and security of the foundations. (Einstein, 1954b, p. 228)

For Einstein, paradigmatic examples of these contrasting types of physical theories are represented in the kinetic theory of gases, which is a solidly constructive theory, and thermodynamics, which is a principle theory. The kinetic theory of gases is a theory that begins with, or makes primary, the physical, molecular constituents and their interactions. It is from these constituents, and the physical properties of these bodies, that the more general theory is built up or constituted. In contrast to this, thermodynamics does not depend on there being any such constituents; rather, thermodynamics begins with a small set of principles – namely the zeroth through third laws of thermodynamics, including the second law of thermodynamics, one formulation of which is the impossibility of constructing a perpetual motion machine. From these broad constraining principles, which are supposed to apply in all physical situations, one can then deduce all aspects of thermodynamic phenomena. Further examples of principle theories are Einstein’s special theory of relativity and general relativity. Indeed, it is Einstein’s aim in “What is the Theory of Relativity” to explain them as such. For the special theory of relativity, the conditions that the laws of physics are the same in all inertial frames, and that the speed of light is constant, are principles which allow the deduction of the consequences of relativity theory. This is a fundamentally different type of theorizing from Lorentzian mechanics, special relativity's predecessor and challenger. Lorentz's theory represents a distinctly constructive theory, depending as it does on the contraction of physical bodies moving through the medium of the aether to explain the same phenomena as special relativity. In this approach, the phenomena are explained by hypothesizing mechanical interactions, which describe in a causal manner that which is observed.

Einstein's principle theory won the day; Lorentz's constructive theory lost. One may reasonably ask, what is the connection between the type of theory that was presented and which theory was accepted. That is, what is the relationship between the type of theory and its success, particularly for a fundamental theory for physics? On the one hand, one could perhaps argue that it was Einstein's use of principles which allowed for his success and that this shows that principle theories were better suited to meet the problem in this case along with many other fundamental theories such as thermodynamics and Newtonian mechanics. As Einstein notes, however, constructive theories are not without substantial merit, and as we shall see, he may have even preferred them. If one is looking for completeness and a greater level of understanding, then constructive theories are better suited to this purpose, as can be seen in the kinetic theory of gases.

On the other hand, principle theories have their own strengths. One of these is the security of their foundations. The very general principles are generalizations extrapolated from empirical conditions which have been found to hold universally. They are then elevated to the status of postulates. In making such principles postulates of the theory, they function logically as more than very strong empirical generalizations. They become principles whose truth is basically no longer in question, and which can only fall should the theory as a whole collapse. The foundational security Einstein talks about is this analytic formal structure founded on essentially irrrevisable principles. It is this foundational security of being a principle theory which appears to best characterize Einstein's motivation behind using principle theories to resolve the fundamental conceptual tensions between classical electrodynamics and mechanics in the case of special relativity, and the conceptual tensions underlying the Newtonian notion of gravity in the case of general relativity.

CBH take their cue from this prospective insight into theory building, and appeal to the distinction made by Einstein between constructive theories and principle theories and apply it to their reformulation of quantum mechanics as a set of information-theoretical principles. Bub (2004b), appealing to Einstein's distinction, argues on two separate grounds that the principle theory approach is not only justified, but preferred and perhaps even necessary for there to be any foundational grounding for quantum mechanics that is philosophically satisfying. The first of these arguments is the more explicit, and it is just that previous interpretations of quantum mechanics fail, based on Bub's argument from underdetermination. Therefore, the only rational recourse we have available to us is the CBH position, a claim about information-theoretic constraints that hold in our world.

The second argument is less explicit, as it seems to rely on an appeal to the authority and the success of Einstein's methodology. The reason that I think that this argument is implicit in the CBH approach is that the above argument is merely a negative argument. It shows essentially that all we are left with is the information-theoretic principles on which to formulate quantum mechanics. However, this does not in itself offer an interpretation of quantum mechanics beyond the claim that a measuring instrument is a black-box. The worry is that this remains an inherently instrumentalist approach. Simply eliminating the feasibility of other approaches does not in itself show that what is being offered is a better interpretation of quantum mechanics. But it is clear that the CBH view is after more than this. Something about being a principle theory needs to be doing some work here. However, what this is is not spelled out. Hence the implicit appeal to the success of Einstein's methodology here. Let us see if this positive argument can be elaborated.

Einstein's distinction, at least for those theories he discusses (thermodynamics, the kinetic theory of gases, relativity theory), is quite a plausible distinction to make. That each type of theory exemplifies its respective strengths is likewise convincing. Moreover, by arguing that special and general relativity are principle theories, Einstein bolsters the idea that principle theories are particularly well suited to give foundational security to a physical theory. The theories of special and general relativity are both powerful fundamental physical theories. Historically, they have been viewed as self-contained from an interpretational standpoint and as exemplifying a type of theorizing which solves philosophical issues rather than creating a host of new problems. The same can not be said to have been the case with quantum mechanics. The more prominent among such interpretations might arguably be seen as more constructive theories. The Bohmian hidden variable approach explicitly postulates definite particle positions guided by the wavefunction to explain quantum phenomena. GRW collapse theories also postulate a mechanism, the stochastic collapse of a real wavefunction, as an account of quantum phenomena and classical characteristics of macro-systems.

In the Everettian many-worlds interpretation of quantum mechanics, there is no collapse of the wavefunction. The wavefunction of the universe plays an ontological role, as it is taken to be a complete and real description of the universe. On this interpretation, a number of things require explanation. The first is the appearance of collapse, or more accurately on this view, the splitting of the world into non-interfering branches. The second is an explanation of the decomposition into the preferred basis that we observe. The third is the Born rule and the appearance of quantum probabilities in a universe where all measurement outcomes actually occur. In recent formulations[1], the explanation for all of these employs the mechanism of decoherence involving the large number of degrees of freedom of particles making up the composite macro-system of the object being measured, the measuring instrument, and the environment. The many-worlds interpretation of quantum mechanics takes the formalism of quantum mechanics at face value, without adding any additional structure, as a complete theory, and instead depends on the ontological role of the wavefunction. However, to explain some important features of experience, and so to

[1] This view is sometimes called the "Oxford" version of many-worlds. See (Deutsch, 1999; Saunders, 1995; 1998; Wallace, 2002; 2003; 2006)

succeed as an interpretation, the current view is that this approach must explicitly appeal to the causal role of decoherence.

Within the professions of physics or philosophy, there is not the general perception that relativity theory requires some further interpretation to justify or make sense of it. To a large extent – more for philosophers than physicists perhaps – there is, however, an ongoing effort to provide such an interpretation for quantum mechanics, or to explain why no such interpretation is required.[2] Therefore, it does seem reasonable to think that taking a different tack in the area of quantum mechanics, from various versions of what might be seen as more constructive theories to a new principle theory based approach, is a smart and innovative strategic move. To sum up, we have reason to reject standard interpretations of quantum mechanics and to look at the CBH principle theory approach on two distinct fronts. First, as Bub and others have argued[3], other approaches to interpreting quantum mechanics, which now may be viewed as more constructive approaches as compared with a information-theoretic principle theory approach, ultimately seem to fail as generally accepted interpretations, and we have reasons to think that they might never succeed. At any rate, for various reasons, none of these interpretations has gained wide acceptance. Second, as suggested by Einstein, principle theories can be an alternative model for success in developing fundamental theories.

---

[2] For some evidence of this see a compilation of some professional meetings in this area by Fuchs (2002, p. 2)

[3] Bub's argument from underdetermination presented above makes this argument on behalf of the CBH approach. Other broad claims that this is the case have been made by Fuchs (2002, pp. 1-3), whose claim is based on historical evidence that for over 75 years of trying no consensus has been reached. For a related statement regarding this failure see van Fraassen (1989, p. 110).

So, the question is now do these arguments justify the CBH approach in particular? That is, if one can in fact formulate a set of information-theoretic principles from which the general features of quantum mechanics can be derived from a technical standpoint, is this something which illuminates or otherwise addresses the traditional problems associated with quantum mechanics since its inception? Part of the task of the rest of this chapter and the next is to specify what it might mean to answer these questions and to answer them. It needs to be spelled out what positive argument might be given for the interpretive role CBH plays beyond an instrumentalist conclusion. In the rest of this chapter, we will continue to outline the distinction between principle and constructive theories. We will also explore the interpretational value of principle theories from the standpoint of their ability to provide explanatory unification in the sense advocated by Friedman and Kitcher. We then need to consider whether or not the CBH approach can provide this. It is not clear that it does. As we will see in Chapter 4, this is not the only way to view the interpretational role of principle theories. It will have to wait until the next chapter to provide a detailed analysis of how this constitutive conceptual resolution works in principle theories such as special relativity.

3.3.3. Einstein's Use of the Distinction

Klein (1967) argues that Einstein, in formulating his special theory of relativity, relied heavily on his understanding of the theory of thermodynamics and used it as a model of good theory building, among other things. As Klein argues, the primary reason for this was that thermodynamics was essentially different from other contemporary theories in terms of its basic structure. Where most theories of the day were constructive theories, thermodynamics represented a prime example of a principle theory. Einstein says about thermodynamics,

> A theory is the more impressive the greater the simplicity of its premises is, the more different kinds of things it relates, and the more extended its area of applicability. Therefore the deep impression that classical thermodynamics made upon me. It is the only physical theory of universal content concerning which I am convinced that, within the framework of applicability of its basic concepts, it will never be overthrown. (Einstein, 1949a, p. 32)

Since thermodynamics does not depend on any particular causal-mechanical model or hypothetical constituents, Einstein was sure of its security and of its ability to guide him in further investigations. Historically, this guidance occurred at two levels. First, the firm grounding of thermodynamic principles quite literally guided Einstein's early work in the area of thermodynamics by offering virtually unquestionable axioms, delineating further avenues of research. Second, the model of thermodynamics as a principle theory served as a philosophical guide, influencing Einstein's ideas about how to develop physical theories in general, and in developing relativity in particular.

According to Klein, a number of concerns guided Einstein in searching after this type of theory. One was that the current state of physics presented sets of contradictions and incongruent structures, particularly between Newtonian mechanics and Maxwellian electrodynamics. Newton's laws hold for any inertial reference frame, but the form of Maxwell's equations hold in only one frame, the frame in which the aether is at rest. For Einstein something needed to be done to resolve these difficulties. As was the case for thermodynamics, the principle theory approach could offer similarly secure foundations to guide his search for a theory which would address these concerns.

Furthermore, in various places, including the passage above, Einstein indicates a driving preference for a unified foundation for physics. Constructive theories must always begin with the underlying constituents, often still hypothetical in emerging scientific inquiry. Sometimes, later developments in science verify the existence of these hypothetical entities on a realist interpretation, sometimes not. The originally hypothetical elements of atomic and molecular theory have since been verified. However, constructive theories, such as Descartes' vortex theory of planetary motion or the caloric theory of heat, eventually proved false.[4] Principle theories, on the other hand, arising from very general empirical claims – such as the impossibility of building a perpetual motion machine – can remain independent of any particular physical picture. Therefore, a principle theory, according to Klein, "could serve Einstein as an absolutely sure guide in dealing with the otherwise inexplicable difficulties of the physics of 1900" (Klein, 1967, p. 510). This notion led Einstein to construct his theories of relativity around this model of a principle theory, thus guaranteeing their foundational security.

Thus far, all we can say regarding the success of the principle theory approach is that they have this "security of the foundations" stemming from the elevation of empirical generalizations to the position of postulates of the theory. That is, when

---

[4] One could argue that a constructive theory could offer equally secure foundations by postulating some basic ontology. Doing so, however, breaks down one of the central characteristics of the distinction. Elevating some ontological element to this status puts it on par with being a fundamental principle, thereby making the theory analytic in nature. As we shall see in the next section, there is an epistemological dimension to this distinction as well. A constructive theory, properly understood, is based on hypothetical constituents.

certain conjectures or empirical generalizations are made postulates, by default they are made secure by that very process, removing such principles from empirical testing and establishing them as axioms whose consequences are developed into the theory. Of course this is not a sufficient reason for the success of principle theories such as special relativity. As presented by Klein, this is merely a *methodological* sort of security, based on the strictly formal conditions of these theories. That is, as a method for scientific discovery, this approach lends itself to success, and, no doubt, for scientific investigations such exploration is valuable. But from the standpoint of providing philosophical interpretations of physical theory, more needs to be said. The philosophical question is not whether or not beginning with strong principles is a successful methodology for scientific advancement, but whether or not such principles can justifiably anchor our most fundamental theories and why. This is the question we must continue to explore.

On later accounts of the development of special relativity, perhaps suggested by Einstein himself, Einstein's principle theory approach to special relativity has been contrasted with the other current theory at the time, that of Lorentz, whose pre-relativistic theory has been portrayed by Einstein and others as being a constructive theory of the same phenomena. Therefore, the argument seemed to go, it was not as good, primarily because Lorentz's theory was not as foundationally secure. Lorentz' theory explained the apparent inconsistencies between Newtonian mechanics and Maxwellian electrodynamics with a set of transformations for Maxwell's equations for different frames relative to the aether. Lorentz accounted for the absence of experimental evidence of the aether from experiments such those by Michelson and

Morley (1887) by hypothesizing that the measuring instruments were contracting as they moved through the aether, thus compensating for the null result. Einstein accused Lorentzian dynamics of being an *ad hoc* theory, attempting to fit this single experimental result into the theory. This treatment of Lorentz turns out to be unfair. We will return to this particular debate in Chapter 5 where a more careful analysis of the contrast between Einstein's and Lorentz's theories will be undertaken. We can say, however, that a constructive theory might open itself up to being *ad hoc* in a way that a principle theory is less prone to according to its nature. The postulation, however well evidenced, of specific mechanisms to explain phenomena, when those mechanisms themselves cannot yet – or in this case in principle – be empirically verified, leaves room for accusations of *ad hoc* theory building. This was a motivating factor for Mach's distrust of unobservable entities in general and against atomism specifically. For him, such purely theoretical constructive objects can at best be of instrumental value.

But there seems to linger an unanswered, perhaps un-posed question, which is at issue here. What exactly is a physical theory supposed to do for us? And are different roles played by different types of theories? Without addressing this question, it is hard to answer the questions we are asking. These are about whether a principle theory or a constructive theory might be somehow better, or more able to fulfill an interpretational role in some sense, and what sense this might be. Should we conclude that because special relativity is a principle theory, it is better or more fundamental than Lorentz's? Why? After all, why should such a causal-mechanical explanation, such as that offered by Lorentz, not be more interpretationally adequate than one, such a special relativity, which requires the entire restructuring of space and time itself? The next section will develop the principle/constructive distinction, and, in the process, uncover the role that explanation plays in the motivations behind developing both principle theories and constructive theories. This will help us understand the roles these types of theories play in physics.

3.3.4. Framework vs. Interaction

One way to approach this issue is to look into the roles which constructive theories and principle theories play in physics as a whole. Much of what follows comes from ideas proposed by Flores (1999; 2005). Flores first notes that there is an even higher level distinction to be made, also attributed to Einstein (1954a). This distinction is between *theoretical physics* and *phenomenological physics*. This distinction is important to us because it gives us some insight into Einstein's conception of the ultimate point, or functional goals, of scientific theorizing.

> The aim of science is, on the one hand, a comprehension, as *complete* as possible, of the connection between the sense experiences in their totality, and on the other hand, the accomplishment of this aim *by the use of a minimum of primary concepts and relations*. (Seeking, as far as possible, logical unity in the world picture, i.e., paucity in logical elements.) (Einstein, 1954a, p. 293)

The category of theories operating as phenomenological physics lies, for the most part, at the purely descriptive level. Phenomenological theories are represented clearly in Kepler's law describing the period of motion of bodies relative to the sun, or in the ideal gas law, $PV = nRT$. These theories accurately describe precise mathematical relationships, but they are not embedded within a broader framework from which those relationships may be derived, or within which more consequences may be derived, or understood. Physical theories such as these arise, more or less, directly from the empirical technique of simple induction. As a result, phenomenological physics “has to give up, to a large extent, unity in the foundation” (Einstein, 1954a, p. 302). Phenomenological physics is a purely descriptive enterprise, closely tied to the phenomena themselves.

‘Theoretical physics’ goes beyond merely ‘phenomenological physics’ by locating the theories in larger deductive structures, more removed from the direct empirical data. This structure permits greater sets of relations between various phenomena and theories, hence providing greater unity. For Einstein, theoretical physics progresses beyond a purely descriptive theory of phenomenological relationships and adds unity by going up a “layer”, as it were, embedding more diverse phenomena in a larger theoretical structure. Indeed, this is ultimately the aim of science for Einstein. The progress of science supports this view that there is a greater role for theoretical physics to play. Theories beyond the purely descriptive are more powerful, in the sense that they can offer simpler theories with a larger domain of predictive power.

Within theoretical physics, for Einstein, as we have said, there are two types of theories, principle theories and constructive theories. Flores describes three grounds on which Einstein justifies his distinction (1999, pp. , 125-7). First, there is an ontological difference. This ontological distinction is cashed out by noting that constructive theories are designed to answer the realist question “What is real?” with the *entities* that they postulate. That is, constructive theories are realistic about the existence of entities, or they are concerned with what Flores calls *entity realism*.

Principle theories cannot offer this type of realism nor are they meant to. Instead, principle theories are concerned with *nomological realism*. Principle theories are concerned with establishing which scientific principles are true in our world.[5]

The second basis for the distinction between the constructive theory approach and the principle approach is their differing epistemological basis. Flores maintains that, for Einstein, principle theories begin with empirically discovered general principles, and then, as is the case for special relativity, "we raise this conjecture [, that the laws of electrodynamics hold in all reference frames,]… to the status of a postulate" (Einstein, 1905a, p. 38). The empirical generalizations are used in theory building as axioms for deriving the body of the theory. This is a theoretical move, but its central epistemological aspect is that it begins with empirical claims. On the other hand, we arrive at constructive theories by hypothesizing the existence of the entities in question, or by way of free creation, in order to explain some other phenomena. When a constructive theory is successful, it is because the existence of those entities is confirmed later experimentally.[6]

---

[5] Note that both theories which could be classified as constructive or as principle theories may be viewed in a purely instrumental manner. However, I am not considering this approach for two reasons. First, I take it that the CBH approach is not meant to be a purely instrumental interpretation of quantum mechanics. More importantly, however, I think that any discussion regarding the foundational significance of different types of theories based on this distinction from an instrumental position would not be particularly meaningful. On this view, there may be a structural difference between constructive and principle theories, however, any foundational difference would not rest on the distinction but merely on the theory's empirical success and criteria such as simplicity and elegance (and quantum mechanics might require no further reinterpretation). I take it that if the distinction is to be at all relevant it is due some aspect of realism gained from the constructive approach.

[6] I think this particular epistemological distinction is somewhat tenuous. At the very least, in theory, it seems that a principle theory could come about through free creation and not be based on any particular empirical generalizations. The process of coming to know or "discover" a theory is notoriously difficult to capture in a simple, all encapsulating formula and I will not address it in any detail since this particular distinction is not central to what follows.

Finally, Flores discusses a third way in which types of theories can differ. This is in terms of the conceptual roles they play, or their function. For Einstein, principle theories function as universal constraints on any further application of theory under those principles. Starting as they do with general empirical principles made into postulates, principle theories set the general conceptual and mathematical constraints imposed by the theory for any physical description falling under it. This is not the case with constructive theories. They are the theories whose elements must satisfy those conditions set by the overarching principle theory covering it. Constructive theories are, of necessity, developed under conceptual constraints, delimiting what is simply off limits. If such a constructive theory meets with difficulty, then, methodologically, we first attempt to modify it rather than the structural constraints imposed on it from above. It is only in times of deep theoretical crisis that such a radical move is made. In this way, the two types of theories play distinct functional roles in science.

According to Flores, for Einstein the ontological dimension of this distinction is primary and the other dimensions are only derivative of that difference.[7] The epistemological distinction is a direct result of the difference between the focus on entity realism and nomological realism. The law-like regularities of principle theories, according to Einstein's view, are empirically discovered and are never the result of free creation. The hypothetical elements of constructive theories are never empirically discovered. Likewise, the functional roles that the types of theories play follow from their ontological status.

---

[7] I am not certain Flores' portrayal of Einstein's thoughts on this issue are entirely complete. As we shall see in the next chapter, Einstein was well aware of the functional role of principle theories and his philosophy reflects considerable thought about the constitutive nature of this role.

Flores resists the idea that the ontological distinction is primary, and instead argues that we should emphasize the functional aspect of the distinction between principle theories and constructive theories. Central to this shift is Flores' argument that there is no clear ontological distinction that applies to all theories. In some cases it is unclear where the fundamental starting point for a theory is; is it a principle or an underlying entity? Flores provides an example of such a theory in Newton's universal law of gravitation. This theory cannot be classified without problem as a principle theory since it operates within the structure of Newton's laws of motion, but it is not entirely derived from these without appeal to the phenomena. As such it cannot be a theory of principles because a set of principles is not its starting place; rather, it is partially a consequence of another principle theory. However, the universal law of gravitation cannot be considered to be a theory based on underlying entities – even if these were broadly construed to include gravitational forces – because no such entities *explain* the law. Quite the contrary, the universal law of gravitation is the description of this force. This means that the universal law of gravitation does not fall neatly into either the category of being a principle theory or a constructive theory on the basis of its ontological foundations.

Instead, Flores proposes that we must focus on the functional roles that theories play. In order to more clearly define the distinction Einstein raises, Flores slightly revises it, calling the "upper-level" theories "framework theories", in lieu of calling them "principle theories", because it is the role of these theories to provide the overarching framework by imposing constraints or restrictions on other theories. "The main elements of these 'upper-level' theories are general physical principles (typically expressed as 'laws') and definitions of physical terms which are expected to be applicable in the analysis of any physical system." (Flores, 1999, p. 126) The theories that these upper-level theories constrain Flores calls "interaction theories", since they typically involve the interactions of various, more elementary constituents (though not necessarily mechanical ones), thus recalling the original definition of a constructive theory. Interaction theories "describe specific physical processes *within* the constraints imposed by the principles (or one of the consequences) of a framework theory" (p. 129). The distinctions between *constructive* versus *principle* and *framework* versus *interactionist* do not follow a strict one to one mapping. Principle theories and framework theories are coextensive, but, although all constructive theories are interactive theories, not all interactive theories are constructive. According to Flores, this permits the classification of theories such as Newton's universal law of gravitation into the category of interaction theory even if they are not constructive due to their functional role rather than their ontological basis.

This revised distinction then sheds light on the nature of scientific theorizing in general. What is it a theory is supposed to be doing? A great deal of literature has been written on the nature of scientific *explanation* and the emphasis on function highlights the important role which scientific theories play in providing explanation. Two of the more influential philosophical viewpoints regarding explanation now appear ready made to fit in with this new distinction. The Friedman/Kitcher program (Friedman, 1974; Kitcher, 1989) has long been to give an account of scientific explanation, or how we explain a law, by arguing that laws are explained through the unification of different phenomena. Friedman argues for this approach with the basic idea that "A world with fewer independent phenomena is, other things equal, more comprehensible than one with more" (1974, p. 15). Kitcher is more precise saying, "Science advances our understanding of nature by showing us how to derive descriptions of many phenomena, using the same [argument] patterns of derivation again and again, and, in demonstrating this, it teaches us how to reduce the number of types of facts we have to accept as ultimate (or brute)" (1989, p. 432). The more descriptions able to be derived from an argument pattern, the better an explanation it is all things being equal. As Flores notes, this is essentially explanation from the top down, explaining by unifying phenomena within an upper-level theoretical structure.

Contrast this with the bottom-up view most prominently expounded by Salmon (1984; 1989). Salmon's position is that scientific explanation essentially stems from the ability to provide a causal-mechanical basis behind physical phenomena. A law is explained by detailing the causal mechanisms which make it hold. Like Friedman and Kitcher, Salmon also links this type of explanation to a notion of *understanding*, saying that there are "intellectual benefits that scientific explanation can confer upon us, namely… knowledge of how things in the world work, that is, of the mechanisms (often hidden) that produce the phenomena we want to understand" (1993, p. 15).

Flores' revised distinction among types of scientific theories can now shed light on this other debate going on in the philosophy of science, that of explanation. The different types of theories – interaction theories and framework theories – are in fact theories which center around and exploit different types of scientific explanation.

If different types of scientific explanation are exhibited by different types of scientific theories, it may not be possible to rectify the unificationist and causal-mechanical approaches to explanation or settle on one definitive model of scientific explanation. However, one can orient them with their respective type of theory and perhaps reach the conclusion that both are equally valid in their place. Instead of competing theories on how scientific explanation works, they can be seen as complimentary aims, both with their own merits, but which serve different underlying roles in the scientific process. When we ask for scientific explanation, perhaps there are two conceptually distinct kinds of things one might be asking for, although both are tied to the notion of increasing our sense of understanding about the world. Different types of theories reflect this.

The analysis of Flores' is insightful. When we go back and look at Einstein's distinction between constructive theories and principle theories, we can see some degree of ambivalence towards their value on his part. In some statements it appears that Einstein prefers the constructive theory approach on the grounds that it provides us with a deeper understanding. On the other hand, sometimes it seems that the logical certainty provided by principle theories is the true aim of our scientific endeavors. It seems that Einstein might actually agree with Flores that the functional roles of principle theories and constructive theories are both equally valid, though conceptually distinct.

3.3.5. Theoretical Pluralism

One way of getting at this question is to look more deeply into Einstein's motivations for ultimately taking the principle theory route and his commitment to sticking with it. On a deeper analysis, it turns out that Einstein was *not* wedded to principle theories. This fact, and the reasons behind it, will shed further light on the role that a principle theory approach can play in fundamental physical theories.

By way of background, this distinction between types of physical theories along the lines of constructive theories and principle theories had been made prior to Einstein. Something close to this distinction was noted, interestingly, by Einstein's contemporary, Lorentz. This connection is the subject of investigation for Frisch (2005). Lorentz had already proposed a distinction between types of theories by 1900. One type of theory begins by postulating "general principles" (1900, p. 335)[8] or "general laws" (p. 336) which express "generalized experiences" (p. 337). There are, however, also theories which postulate a "mechanism of the appearances" (p. 336). Examples of the first type of theory include the second law of thermodynamics and conservation of energy, while examples of mechanism theories include the kinetic theory of gases.

Clearly, Lorentz's distinction resembles greatly the distinction between principle and constructive theories made by Einstein, and it predates Einstein's distinction which was discussed at length in 1919 in "What Is the Theory of Relativity". Moreover, if we look at Lorentz's classifications using the language suggested by Flores, we can see that there is also a similar distinction between the functional roles played by either type of theory. Principle theories act as constraints, guiding further theorizing. Lorentz says, "'only when there is absolutely no other way out to be found' scientists will 'dare to diverge from the generalized experiences' embodied in principle-theories" (Frisch 2005, 668 quoting Lorentz 1900, 337). The

[8] All translations from the German by Frisch.

overarching structure imposed by the principles is subject to revision only in extreme circumstances, providing the rules under which other theories operate. This does not mean, however, that such constraints are ultimately not revisable, but that, barring direct challenges that cannot be overcome, principle theories structure the parameters of scientific discourse. The role of principle theories as unrevisable frameworks connects with the idea that principle theories offer security of the foundations. We also get a hint of a better way to understand the meaning of this security. The security is derivative of the constitutive characteristics of framework theories. This will be taken up in Chapter 4.

The prevailing view, both historically, and to this day, is that Lorentz preferred the mechanism approach to scientific theorizing. Just as Lorentz and Einstein offered competing theories of what is now considered relativistic phenomena, this view contends that they also held competing visions of what an ideal physical theory ought to be like. Einstein was able to formulate the special theory of relativity because he embraced the principle theory approach over the mechanistic one of Lorentz. The view that Lorentz was guided by his predisposition towards mechanism theories is supported by the historical resistance Lorentz had towards Einstein's special theory of relativity in favor of his own far more mechanistic theory.

However, Frisch makes a compelling case that the philosophical views of Lorentz and Einstein in this regard are in fact much closer than is generally thought. In the first place, Lorentz thought that both principle theories and mechanism theories had a valuable role to play in science and he did not fail to recognize the benefits of a scientific theory which uses the principle approach. In a view similar to that of

Einstein, who argues that principle theories offer more foundational security, for Lorentz, principle theories offer strong empirical generalizations covering a broad domain of physical phenomena. Whereas, mechanism theories will, nearly universally, employ some element of hypothesizing, the foundations of a principle theory are particularly strong, since they rest on already established empirical generalizations, and thus can serve as guiding conditions for empirical discovery.

Mechanism theories, in lieu of this security, offer the possibility of greater understanding, by postulating the underlying processes which explain scientific phenomena. For Lorentz, a principle theory can say "nothing or only very little about the mechanisms of the appearances, [thus] lead us to desirable results, but will not show us much during the trip" (1900, p. 355). What is interesting for our purposes is that Einstein was also highly attuned to this advantage which mechanism – or constructive theories as he referred to them – can offer, as well as to the deficiencies of principle theories. Principle theories, based as they are on empirical generalizations, do offer security in their foundations, as they are less likely to be overturned. But in terms of explanatory advantage, just as Lorentz thought that mechanism theories provide understanding in ways that principle theories cannot, Einstein also recognized that, "When we say that we have succeeded in understanding a group of natural processes, we invariable mean that a constructive theory has been found which covers the processes in question" (1954b, p. 228). Understanding is highly valued as an aim in scientific theorizing. It should be recognized that 'understanding' is itself an unclear term. However, it seems to be doing work here on a rather intuitive, commonsense level, and that is that if we have a causal-mechanical story of how phenomena come about we can understand it in something like the way we understand how a billiard ball gets from one end of the table to the other.

To further demonstrate this fissure between constructive theories and principle theories in Einstein's approach to scientific theorizing, it is worth noting that Einstein informs us that he first pursued a constructive approach to resolving the difficulties presented by the conflict between Newtonian mechanics and electrodynamics prior to the formation of special relativity.

> By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more desperately I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results. (Einstein, 1949a, p. 53)

This failure to find a constructive theory led to his eventual principle theory based approach to developing special relativity. Klein (1967) argues that from the very earliest times in Einstein's career he was guided by the example of thermodynamics as a principle theory. This may very well be the case, but it does not lessen the importance Einstein attached to constructive theories. Indeed, as discussed above, Klein's history portrays the thermodynamic model of theorizing as a model for *procedure* and *methodology* and not for the *justification* of foundational theories. Principle theories serve as guides, or constraints, in theory development by setting parameters in the form of universal laws. For this purpose, principle theories are ideal, offering a firm foundation due to their generality and security of logical foundations. Lorentz also seems to see the benefits of the principle theory based approach in terms of a strategy for success, as opposed to offering a justification for foundational theories (see Frisch 2005, 669). Einstein at times also appears to take this view as well. For example, Klein says,

> even in his very early work Einstein was not content to take thermodynamics only on its own terms, so to speak – to take it as a given, closed system. As a "theory of principle" it had to be intelligible from a more basic point of view. In other words, Einstein also concerned himself with statistical mechanics as a way of providing that deeper understanding of the laws of thermodynamics. (Klein, 1967, p. 510)

Having the constraining principles or laws of thermodynamics at his disposal, and being sure of their solidity, allowed Einstein a restricted and guided search for those underlying constructive elements of statistical mechanics, including his work on Brownian motion. But from this passage, it seems that for Einstein it was at this mechanistic level that real understanding and explanation of the phenomena, including justification for the laws of thermodynamics themselves, was to be found.

With this new perspective on Einstein and Lorentz, and their views on theory construction, it becomes less clear what we are supposed to say regarding the status of principle theories as fundamental theories. It had looked as though perhaps principle theories were the appropriate model for doing fundamental physics, with the path forged by Einstein himself as a primary example. However, it appears that he did not necessarily favor principle theories when theorizing. None of the views at which we have been looking has resolved the question concerning any interpretational factors that necessarily motivate a principle theory approach over a constructive approach, or visa versa.

Flores advocates slightly altering the distinction based on the functional role which different types of theories play, particularly when it comes to explanation, into framework theories and interaction theories. This distinction differentiates types of theorizing done in physics, but it does not favor one over the other, as Flores specifically allows that there are different roles, satisfied differently, which are involved in scientific theorizing. Lorentz, as Frisch notes, was explicitly a theoretical pluralist, saying that it is a matter of personal preference and not a matter of which type of theory is objectively superior or more fundamental to scientific enquiry (Frisch, 2005, pp. 669-670). Even Einstein seems, at best, to have been ambivalent. One the one hand, he seems to favor the constructive approach and its clear advantage in providing realistic, causal-mechanical explanation. Yet his greatest contributions to modern physics, special relativity and general relativity, are proudly offered as principle theories along the lines of thermodynamics, which Einstein touts as a highly successful and paradigmatic model of a principle theory.

3.3.6. The Role of Explanation

The question seems to come down to explanatory preferences and what type of explanation our physical theories are supposed to be offering, and even notions of what it means to "understand" some phenomena. However, it seems that, through our analysis, very important, and what we may call fundamental theories, in physics are sometimes principle theories and sometimes constructive theories. Compared side by side specific examples demonstrate this. The kinetic theory of gases is the more fundamental theory, meaning that it, by and large, is taken to explain the principle theory, thermodynamics. Whereas, special relativity, a principle theory, has been adopted over the more constructivist theory of Lorentz. The preceding discussion shows us that there are at least two distinct kinds of physical theories which differ in their content and the function they play in our scientific endeavors, in terms of the explanatory structure they can offer. And this makes sense. Explanation seems to be about this somewhat vague notion of understanding. Understanding is, to a large extent, a psychological pressure to feel as though something is “understood”. According to this view it is reasonable, and indeed expected, that there may be multiple meanings of explanation all united by the idea of increasing understanding, which as a psychological state is, by its nature, imprecise. Thus theoretical pluralism is also to be expected.

Flores argues that the distinction Einstein defined as that between principle and constructive theories emphasizing their ontological difference is better understood as a distinction between framework and interactionist theories on the basis of their functional role. Recall the distinction noted earlier, between *phenomenological physics* and *theoretical physics*. The inevitable progression of science from the phenomenological level to the more abstract theoretical level is rooted in the fundamental drive to unify the disparate phenomena under fewer and fewer basic elements. We should now be able to see that there are two ways this can occur, and these ways are tracked by the distinction between constructive and principle theories. The first way is that the unification comes about with the postulation of a causal-mechanical mechanism explaining the phenomena corresponding to Flores’s notion of an interactionist theory.[9] The second way is that

[9] The phenomenological/theoretical distinction is not hard and fast, but one of degree. Any principle theory must be relatively theoretical. Constructive theories, it would seem, need not be. So constructive theories need not be unifying, though many are. To be clear, it is not in virtue of the unification that constructive theories are explanatory, but their causal-mechanical basis.

the unification provides explanation via theoretical unification, corresponding with Flores's notion of a framework theory.

Theoretical physics seeks to unify. Within theoretical physics there are two differentiable means by which this takes place. One is through causal unification, the other is by theoretical unification. As Salmon notes (1989, pp. 182-3), this second type of explanation is essentially different from the first. It is of a kind that does not actually transcend descriptive knowledge. This type of explanation increases understanding by organizing and systematizing knowledge. The central characteristic essential to causal explanation is that it goes beyond purely descriptive knowledge to the mechanisms and processes behind the nature of things. These two distinct types of explanation, however, are not incompatible. In Chapter 4, we will return to analyze more closely the nature of theoretical unification and further differentiate between unification in the sense of Friedman/Kitcher and a more fundamental notion of conceptual resolution which is necessary for the possibility of such explanation.

Flores argues that the distinction between types of explanation is derivative of the framework/interaction distinction. However, it is clear from the preceding discussion that the basis for the distinction we are talking about rests on the explanatory motivations behind the theory. Flores notes that there are three dimensions to this distinction – ontological, epistemological, and functional. When we focus on the upper-level principle, or framework, theories, their functional role comes to the fore. It is their ability to unify the theoretical structure by uniting diverse phenomena under a single description or by defining its operational framework. This is why the principle theory approach in Newton's mechanics or special relativity is so explanatorily successful. However, when we focus on bottom-up constructive, or interactionist, theories, it is their powers of causal-mechanical explanation that makes them attractive. This in turn depends on their ontological aspect, their realism about causal-mechanical entities. Therefore, Einstein is wrong to think that it is the ontological characteristics of theories on which the distinction is made. Likewise, the way Flores's characterizes the functional aspect of the distinction glosses over the central role played by explanatory characteristics. The problem is that if the framework/interaction theory distinction is prior to the explanatory role, then it becomes unclear what particular function it is that interaction theories are supposed to play. A framework theory defines the framework, but an interaction theory, on this dimension, is defined as a non-framework theory, a theory which is constrained by some upper-level framework. But this fails to capture the importance of its ontological basis. Being constrained does not imply any causal mechanical basis. But this seems fundamental to how Einstein cashes out his notion of a constructive theory. The dimension of most importance which I propose is also a functional one, but the function is explicitly an explanatory one. The explanatory dimension to this distinction is primary. This captures the theoretical unification of framework theories as well as the causal-mechanical role of constructive theories. I will continue to use the terminology of Einstein, *constructive* and *principle* theories, but with this explanatory dimension in mind.

One of the difficulties with this distinction, it that while it is quite useful, and can do a lot of work for us, it is not necessarily exhaustive no matter which dimension of the distinction one focuses on. But this seems to be a problem regardless of dimension along which the distinction is made. It can still be a useful distinction though it may be better to see it as defining opposite ends of a spectrum, within which there are theories whose explanatory roles can overlap and function as hybrids. Nevertheless, we should not lose sight of the centrality of the types of explanation behind the distinction.

At this point, it is necessary to fully clarify the notion of explanation by unification. The most prominent view of unification as scientific explanation is along the lines of the Friedman/Kitcher program. On this view, something is a better explanation if it is able to unify a wider range of phenomena. Many principle theories participate in this kind of explanation. Newton's and Einstein's certainly do. For instance, Newton's laws of motion, together with the universal law of gravitation, unify celestial and terrestrial phenomena from planetary orbits, to the tides, to the behavior of objects on earth. As I will conclude, CBH does not appear to unify in this manner. However, this type of unification is not the only, nor the most fruitful, way to understand the role of principle theories as applied by Einstein or by CBH. It will be argued that in these cases, the primary function of the principle theories is to establish principles which are constitutive of the very framework of some set of physical concepts. This constitutive role is adequately fulfilled only if the principles successfully establish a coherent conceptual framework. As such, theories such as Newton's and Einstein's, play a fundamental explanatory role by establishing the explanatory framework itself. Though this argument must be left for Chapter 4, the conclusion here is that the distinction between principle and constructive theories is best understood as based on their explanatory roles. With this understanding in place we will be able to see the centrality of unification for the principle theory approach. This in turn will help us to understand the role of constitutive theories in establishing that explanatory framework.

We can now perhaps say something useful about the nebulous idea of interpretation. Often is seems that there is a call for an 'interpretation' of a theory because at least one of two aspects of the given theory remain unclear. The first is how that theory fits into a broader understanding of physics. How do we make sense of this theory given other things we know? Why is it the case that this theory holds? The second issue which begs for interpretation is to explain how the world is such that the predictions made according to the formalism of the theory turn out the way that they do. What is the world like if the theory is true? That is, an interpretation, in this case, is supposed to allow the theory to explain the phenomena it covers. Both calls for interpretation are calls for explanation. The first is a demand that the theory be explained, providing an external explanation for the theory itself. For example, why is quantum mechanics or special relativity true of the world? What explains the principles or properties of the entities it postulates? The second is a demand that the theory itself be explanatory. How does the theory explain the phenomena it covers? For example, how does quantum mechanics explain quantum phenomena?

A theory cannot be expected to provide its own interpretation in the first sense. One must go outside of the theory to explain it. Although such interpretations are sometimes called for, they will require a broader theory in which to embed the theory we wish to have interpreted. The broader theory explaining it will be its interpretation. However, we can ask for an interpretation of a theory that allows the theory to explain. This interpretation often comes built into the theory itself. In this case, the theory requires no further interpretation. Here we may look at the kinetic theory of gases for example. In fact, any constructive theory, offering as it does a causal-mechanical explanatory structure, generally requires little if any further interpretation. We will also see how a principle theory, such as special relativity, can supply its own interpretation later in Chapter 4, thereby not requiring any further interpretation to be applied to it.

Quantum mechanics has nearly always been viewed as requiring further interpretation. Historically, resolution has been sought in both senses of interpretation. Certain phenomena covered by quantum mechanics have generally been viewed as inadequately explained, thus quantum mechanics requires interpretation of the second kind. Problems such as the measurement problem and the EPR phenomena are expressions of the need for interpretation. It is the demand that some account be given for how quantum mechanics can explain these phenomena. However, interpreting quantum mechanics in the first manner may also be fruitful. That is, if quantum mechanics can itself be explained, perhaps this explanation might help to explain quantum phenomena as well, thereby offering a suitable interpretation. Different 'interpretations' of quantum mechanics can be characterized variously along this division. The point is, the demand for an 'interpretation' of a scientific theory, and in particular quantum mechanics can often be seen as the demand for an explanation of some kind or an account which shows that such an explanation is not called for. The need for an interpretation in such cases stems from the need to know the reason why and the apparent inability of the theory itself to provide it. Interpretation is not explanation, but often the two issues appear to go hand in hand.

3.4. Some Words on Constructive Quantum Mechanics

Before returning to the CBH approach using information-theoretic principles to create a principle theory of quantum mechanics, I would first like to say a few things regarding the role of constructive theories in quantum mechanics, having explicated the explanatory role intended by such theories. This discussion pertains to constructive theories in quantum mechanics in general, and potential constructive theories using quantum information theory as a basis. Why would such an approach be appealing? For the same reasons which influenced both Lorentz and Einstein: understandability and explanatory power. A constructive theory provides mechanisms which in turn provide explanation by way of providing causal-mechanical understanding.

Again, turning to the insight of Einstein for clues, we find that his very early work, leading ultimately to the formulation of quantum theory, was fraught with the difficulty of fitting puzzling new phenomena into a constructive framework. As Klein's paper demonstrates, the theory of thermodynamics was heavily influential in Einstein's thinking. In the case of quantum mechanics, eventually, Einstein turned to the model of thermodynamics for inspiration, frustrated by the impossibility of discovering any constructive theory which reconciled the problems between electrodynamics and the kinetic-molecular theory, such as the problem of blackbody radiation and the photoelectric effect. Eventually, of course, quantum mechanics developed out of these difficulties. Of interest to us is the fact that Einstein was first driven to find an acceptable constructive theory for the burgeoning quantum theory, as was also the case for his work ultimately leading to the formulation of special relativity. Einstein's modus operandi, when presented with incompatible sets of results, was to first turn to an underlying mechanism which could explain them, thus extending our understanding of them. It was only when such goals were thwarted, that Einstein turned to the strategy of using principle theories.

By the time quantum mechanics had been developed, Einstein famously had serious qualms with the theory. Most notably, Einstein faced off with Niels Bohr on the adequacy or completeness of quantum mechanics. Einstein's most famous objection to quantum mechanics came in the form of a thought experiment presented in the EPR paper (Einstein, Podolsky, & Rosen, 1935). It might be claimed that Einstein's powerful objection here was in fact a way of articulating his frustration that quantum mechanics fundamentally ruled out a constructive formulation in its most rudimentary sense. The incompleteness Einstein was worried about, stemming from the basic assumptions of separability and locality, was an incomplete causal-mechanical explanation for the correlations involved.

Klein seems to take a different perspective on the point I am making. According to him, Einstein, in the EPR paper, is again using reasoning based on the thermodynamic model, that is, reasoning according to constraining principles (Klein, 1967, p. 516). While it is true that the basis of Einstein's argument relies on his principles of separability and locality, I would argue that these are not principles of a particular theory, but rather they are meta-theoretic epistemological, or even metaphysical, principles, which for Einstein constrain the very possibilities of doing physics. That is, a principle theory, such as thermodynamics or special relativity employs empirical generalizations as principles. In this case, the principles involved are at a different level entirely. Specifically, if these principles are violated, then it is inherently impossible to provide a constructive theory. This is distasteful to Einstein because it precludes the possibility of ever obtaining any understanding of the physical world. And for Einstein, physics is about providing that deeper understanding.

As a matter of fact, Bell's later analysis (1964) of the problem clearly illustrates the impossibility of such a straightforward constructive theory of quantum mechanics. The assumptions behind the Bell inequality show that quantum mechanics rules out the possibility of there being any common-cause explanation. As such, any constructive theory of quantum mechanics, in its standard sense of providing causal-mechanical explanation, seems to be in principle ruled out. At the very least, it is no straightforward task to show how to go about designing or envisioning a constructive theory of quantum mechanics.

In the case of quantum mechanics, a successful, straightforward, constructive theory is not obviously available. One might argue that other approaches or interpretations do attempt this. It has been suggested that Bohm's approach, which maintains a causal framework and is constructed from quantum particles and waves, does provide a constructive theory and that had history been different there would be no interpretational qualms surrounding quantum mechanics for that very reason (Cushing, 1998). It seems like this might have the appropriate elements of a constructive theory, and proponents of Bohmian mechanics certainly seem to claim that it has the standard advantages of a constructive theory: causal-mechanical explanation and understanding. However, as all issues in quantum mechanics seem to encounter, there are roadblocks for this view as well. If I am right and the interpretive value in a constructive theory stems from its causal-mechanical explanatory basis, then it will not be a successful interpretation as a constructive theory unless its causal-mechanical explanatory role is fulfilled. But by gaining determinism, by Bell's theorem, Bohmian mechanics must be nonlocal. For Bohmian mechanics, any change in the environment results in the instantaneous change of the quantum potential (Cushing, 1998). GRW collapse theories must also incorporate nonlocal factors. As such, these theories violate the assumption of locality behind the EPR problem and an assumption part of the concept of common-cause. As such, the standard notion of causal explanation is violated by these theories. The proposed ontology of Bohm and GRW collapse theories both require non-locality[10]. Therefore, they cannot function as constructive arguments unless what it is to be constitutive is reinterpreted. Therein lies the root of the fundamental disagreements between various interpretive schools. Constructive interpretations are attempted, but they are not unequivocally constructive in the traditional sense. The many-worlds interpretation,

---

[10] For a discussion on Bohmian mechanics, GRW collapse theories, and non-locality see Maudlin (2008) . Maudlin says, "While in Bohmian mechanics, non-locality is achieved by the way the wave function choreographs particle behavior, so that what one particle does may depend on how a distant particle is treated, in the GRW theory non-locality is achieved through the collapse of the wave function itself. It is this which, in the non-relativistic theory, is instantaneous and insensitive to the spatial separation between particles. Interacting with a particle at one end of the universe can cause a collapse that alters the physical state of an entangled particle at the other end" (2008, p. 166). For the 'the mass density ontology' version of collapse theories, the mass density of a particle spread out in space (e.g. in a two-slit experiment) will undergo a spontaneous collapse, instantaneously localizing the mass density. Maudlin also discusses a recent relativistic formulation of GRW theory by Tumulka (2006). Albert and Galchen (2009) point out that this formulation introduces a new type of non-locality: temporal non-locality. The relativistic formulation does not remove non-locality; it makes it compatible with relativity. There may still be spacelike separated events where the distribution of events at *A* depends on events at *B*. But on the relativistic model, the direction of dependence is not fixed (Tumulka, 2006, p. 9).

as a constructive theory based upon a particular ontological structure, does not straightforwardly fail to be a constructive theory. It is however not unproblematic. One challenge is against the expansive ontology of postulating the existence of perhaps infinitely many 'worlds' and histories in addition to the one we experience. Another is the derivation of quantum probabilities on a theory where all possible outcomes actually occur with certainty. As an explanation, the interpretation is arguably both ontologically over indulgent and insufficient.

Bub also objects to Bohm's and other interpretations. Bub argues that it is not rational to accept this or other mechanical interpretations of quantum mechanics because of the underdetermination of any interpretation equivalent to standard quantum mechanics discussed above. As an explanation, any modal interpretation such as Bohm's that is empirically equivalent to standard quantum mechanics fails by being inherently hypothetical. Interpretations such as this can not provide a reason, or an explanation, for quantum phenomena, since the hypothetical mechanism in principle has no empirical cash value beyond the predictions of quantum mechanics. Bub and others[11] show us that there is no consensus among philosophers, and that furthermore, as of yet, there appears to be no principled way to chose between the various interpretations available to us – e.g. wavefunction collapse, hidden-variables, or an Everettian world structure – except on the basis of some predilection or preference for certain epistemological or metaphysical principles. But insisting on one set of such principles means that others must be dropped. This suggests that there can be no principled reason to choose a *particular* constructive approach that does not contain some element of arbitrariness based on metaphysical leanings one way or

[11] See n. 2

another. It is difficult to consider these successful constructive theories for the two reasons that, for some, the mechanisms are inherently underdetermined, and that any one of them must give up some part of the standard realist views of causal mechanism. This explains the lack of any convergence in the field. The interpretive work that must be done is not in coming up with a constructive theory and thereby explaining puzzling quantum phenomena. It must be in explaining why the interpretation counts as explanatory at all given that it must give up some key aspect of the traditional understanding of causal-mechanical explanation.

3.5. Conclusions

In this chapter, we have been able to demonstrate more precisely what role a theory, either as a principle theory or as a constructive theory, plays in physics. Principle theories offer explanation through unification. Constructive theories are best understood as fulfilling the role of providing causal-mechanical explanation. The lesson that we can take from this with respect to quantum mechanics is valuable. By framing the issue in terms of this distinction, we can see why broadly "constructive" approaches to quantum mechanics, such as Bohm, GRW, or Everett, have remained unsatisfactory interpretations of quantum mechanics to many, since they have failed to provide a straightforward causal explanation for certain phenomena; indeed quantum mechanics might seem to prohibit such an explanation. This is a failure of the basic strength of a constructive theory. Hence the apparent failure of such attempts has led thinkers such as Bohr to embrace the instrumentalist perspective. It is not my goal to conclusively argue that none of the other interpretations of quantum mechanics are unviable. The literature is replete with such arguments, and I need not repeat them here. The point is that these interpretations have by and large been constructive spins on quantum mechanics. There has been as of yet no consensus on such interpretations. And this lack of consensus can be explained because the goal of developing a constructive theory has not been met without giving up some other aspect of a traditional constructive theory.

So why not develop a principle theory? If there is an interpretational aim for CBH it seems it must stem from such a motivation. It does not offer a realist version of quantum mechanics as about information as the basic stuff. And an instrumentalist interpretation both violates the spirit of the CBH approach, and it fails to justify the philosophical use of the information- theoretic language. What we have seen is that one motivation behind taking a principle theory approach might be that it can provide explanation in the Friedman/Kitcher sense of unification. That is, perhaps an interpretation based on a principle theory approach can take advantage of explanatory virtues not available to a constructive theory approach to quantum mechanics. Without significant further work, CBH does not seem to provide any additional aspect of unification in this sense. CBH offers principles which are presented as having formal equivalence to some general quantum properties of theories. Formal equivalence is, strictly speaking, not unification of any apparent sort. Nothing more has been shown to be incorporated into an information-theoretic reformulation of quantum mechanics than quantum mechanics itself. It is hard to see how it could offer more unification than quantum mechanics already does. As such, the CBH principle theory-based approach does not provide any explanatory benefit of the Friedman/Kitcher variety without further analysis. If the more constructive (or mechanical) account which it is supposed to be replacing fails to be explanatorily satisfactory and is thus instrumentalist, so is this approach, if the work it is supposed to be doing is this sort of unification.

Having established the explanatory basis for the principle/constructive theory distinction and the unificationist role played by principle theories, in the next chapter, we will continue with the analysis of principle theories. In particular, it will be shown that there is another way to understand the role of a principle theory, particularly when it comes to high-level theories such as Newtonian mechanics and Einstein's theory of relativity. Specifically, principle theories such as these play a constitutive role in physics. As we shall see, this is a more illuminating way to understand the role of principle theories in certain cases, and the information-theoretic approach to quantum mechanics needs to be looked at in this light. After the analysis of the constitutive role of principle theories and the conceptual role they play, we will be in a better position to evaluate the success of the CBH approach and say something about the role for an information-theoretic principle theory in interpreting quantum mechanics. If CBH is to provide a successful interpretation of quantum mechanics, it is in this sense of principle theory that it will do so, just as this is the sense of principle theory behind the success of theories such as special relativity.

Chapter 4: Constitutive Principles

4.1. Introduction

Having now clarified, to some extent, the distinction made by Einstein between principle theories and constructive theories, I wish to take a closer look at a different and more foundational role principle theories can play. Does this approach hold any hope of illuminating the foundational issues which have plagued quantum mechanics since its inception?

As indicated in the previous chapter, I do think there is an important role which principle theories can and do play, especially for what we might call foundational physical theories. This will be cashed out more in this chapter, but broadly speaking a foundational theory has large scope, with many theories falling under it and not falling under many theories itself. A foundational theory is a theory which provides the basic conceptual framework under which other theories may function. As such, the foundational nature of such a theory comes from the functional role of being a framework, or principle theory. In particular, principle theories are of foundational significance when they are constitutive of the framework in which other theories can operate. The clearest examples for such theories come from space-time physics such as Newton's laws of motion and relativity theory.

In this chapter, I hope to show, through the analysis of historical theory revision and the evolution of ideas in the philosophy of science, that the uppermost-level theories, such as theories of space and time, must be principle theories. Those principles must provide the conceptual framework which establishes the meaning of empirical observation and measurability within the framework. The epistemological character of such principles is that they serve a particular *a priori* role of being necessary prior to observation, but that their choice is, to some extent, conventional. However, there are good philosophical reasons, given particular moments in the progress of science, for selecting which principles ought to be put in place. This choice is guided by the careful conceptual analysis of the existing framework, which is at the time unviable due to fundamental conceptual inconsistencies in the theory. This requires careful revision of concepts to generate a new theoretical framework in which meaningful empirical claims can be made.

An adequate principle theory, i.e. one that lays out principles which do allow us to deduce the basic structure of broad physical theories, such as CBH does for quantum mechanics, does not necessarily offer a better interpretation of a theory by virtue of being a principle theory. This can be seen even in Einstein's take on his favorite example of a principle theory, thermodynamics. While Einstein is entirely convinced of the security of the laws of thermodynamics, based as they are on strongly evidenced empirical generalizations, it is nevertheless not the most fundamental theory regarding its covered phenomena. In this particular instance, the kinetic theory of gases offers a better interpretation of the phenomena from a fundamental explanatory standpoint. As we saw in Chapter 3, an interpretation for a theory is often seen as required when the theory appears to require external explanation, or when the theory appears not to be intrinsically explanatory. In this case, the causal-mechanical, or constructive theory of the kinetic theory of gases has this explanatory power, both explaining the laws of thermodynamics and showing how they explain. In this case, the direction of explanation comes not from the principle theory, but the constructive theory.

However, one source of explanatory power, and one which a principle theory might naturally be seen to offer, is by providing unificationist explanation in the sense of Friedman and Kitcher. CBH, as a principle theory, does not appear to unify in the way necessary for claiming interpretational advantages regarding quantum mechanics on this ground. I do not claim that this cannot be done, but only that it is not clear how it might be accomplished, and that being a principle theory is not, in and of itself, sufficient. Moreover, it does not seem to be the case that it is the aim of the CBH approach to provide unification of this kind.

Special relativity and general relativity are undeniably clear examples of principle theories. They are also currently and historically viewed as highly successful theories both in their predictive power and as foundational theories which are generally not thought to require further interpretation. Indeed, their emergence onto the theoretical scene propelled a newfound interest in the philosophical foundations of science itself. It is no coincidence that there was both a resurgence of Kantianism around this time and also the development of the logical positivist movement in the philosophy of science. Both took the success of Einstein’s theories as significant developments representing scientific and philosophical theorizing at its best.

What sparked this flurry of philosophizing was not only the raw success of these theories, but also the scope of what they covered: the structure of space and time itself. For over two hundred years, the physics of Newton had reigned supreme, and Einstein brought about the thorough overturning of this longstanding theory. Moreover, theorizing about space and time is, in many respects, uniquely different from other physical domains which are addressed in physical theories. Knowledge of the characteristics of space and time has very much to do with accomplishing any other physics at all. For Newton, space and time constitute the arena in which physical events take place. Therefore, our theories and conceptions of space and time are inherently foundational for the general body of physical knowledge.

As we shall learn, such theories must be framework or principle theories, given that their role is to establish such a framework for the rest of physics. A principle theory provides explanation by unification. However, in the class of theories at which we will be looking, the principle theories work at the conceptual level by establishing the necessary preconditions for explanation. We find in both Newton and Einstein, conceptual analysis revealing that current theory is conceptually inconsistent. The work which they do provides necessary conceptual resolution by establishing principles constitutive of the meaning of empirical terms. The constitutive work done by these theories establishes the explanatory and interpretational framework itself.

Einstein's description of principle theories is better understood in this light. Likewise, it is a much more productive understanding of the principle theory approach of CBH. We will investigate the question of whether CBH meets the standards of being a constitutive principle theory which, through conceptual analysis, establishes a coherent explanatory framework for quantum mechanics, leading into

Chapter 5, where we discuss a more comprehensive version of this program as advanced by Bub and Pitowsky (2007).

4.2. Historical Development

4.2.1. The Kantian Origin of the Constitutive Role of Principles in Science

More than a century before the emergence of relativity theory, following the Newtonian revolution in physics, itself a revolution of our knowledge of the fundamental framework for doing physics, Kant was also concerned with the foundational issues of physics and scientific knowledge.

Kant argues that the intuition of space and time "is nothing but the mere form of sensibility, which precedes the actual appearance of the objects, since in fact it makes them possible" (Kant, 2001, p. 284). The concepts of space and time are concepts of the *form* of experience and not of the *matter* of experience. That is to say, space and time are "formal conditions of our sensibility" (p. 284). For Kant, the formal structure of these conditions on experience was crystallized in the work of Newton and specifically took the form of Euclidean space and time. The epistemological nature of concepts such as space and time is that they are *synthetic a priori* judgments. That is, the nature of the forms of intuition regarding space and time cannot be proven from any concepts alone, and thus analytically. That is, it is not part of the concept of "space" itself that it is Euclidean, in the way that it is part of the concept of "triangle" that it has three corners. Nevertheless, on the basis of pure intuition, we can see that our particular concepts of space and time are necessary, known with apodictic certainty. No empirical knowledge can have such an apodictic character, it is always contingent. Therefore the concepts of space and time must be known *a priori*, though not analytically.

Kant's argument, as presented in the *Prolegomena to Any Future Metaphysics*, is a transcendental argument, starting with the claim that we have pure mathematical knowledge. The question is how? Among the two types of judgment, there is the *explicative* or *analytic* which adds nothing beyond what is given in the concept itself. All such judgments are *a priori* and are justified on the basis of the principle of contradiction. The other type of judgment is *ampliative* or *synthetic* in nature. In this case, the judgment adds something beyond what is contained in the concept itself. Among these are *a posteriori* judgments, justified empirically. However, for Kant, there is a vital category of *synthetic* judgments, which are known *a priori*. That some judgments fall into this category can be seen by looking to the realm of mathematical judgments. According to Kant, mathematical judgments are all *synthetic*. They cannot be *analytic*, because nothing in the conclusion is contained in the concepts themselves. Some synthesis must be involved, since the conclusions cannot be established simply using the principle of contradiction. Moreover, mathematical judgments are known with apodictic certainty – that is, absolute necessity. Necessity, however, cannot be known empirically. Therefore, mathematical propositions are *synthetic a priori* judgments (Kant, 2001, pp. 266-8). Because they are not *analytic*, such judgments must come via intuition rather than by analysis of concepts alone. This intuition must be pure, or free from empirical sources. Mathematical judgments are gained from pure intuition. But how can it be that we intuit anything purely or *a priori*? And what is the nature of such intuition which, since it is *a priori*, must take place without an object of intuition? That is, pure intuition must precede the empirical intuition of the object.

> Therefore in one way only can my intuition anticipate the actuality of the object, and be a cognition *a priori*, viz., *if my intuition contains nothing but the form of sensibility, which in me as subject precedes all the actual impressions through which I am affected by objects*. (Kant, 2001, p. 282)

In other words, the only way it is possible for *synthetic a priori* judgments to exist is for them to exist as a precondition for sensibility, by providing the form of how the objects of sense appear to us. *Synthetic a priori* judgements do exist in pure mathematics since such judgments are not *analytic* or *a posteriori*. Therefore, so goes the argument, there are such preconditions brought to experience by our intuition.

At the foundation of pure mathematics, for Kant, are the concepts of space and time, representing the quintessential concepts of pure intuition. The concepts of space and time compose the structure of empirical intuitions, and if we remove all actual intuition of empirical objects, the concepts of space and time remain as forms of possible experience. The form of space is the Euclidean space, having three dimensions such that, "not more than three lines can intersect at right angles in one point" (Kant, 2001, pp. 284-5). As a pure intuition, this judgment is apodictically certain, yet cannot be determined from the concept of space itself. Kant provides a further argument to back up this claim. This argument involves a supposed paradox, wherein two figures are given, whose complete spatial description of shape and dimension are identical, yet the two figures cannot be made to coincide. An example is a hand and its image in the mirror (the glove which fits the original could not fit its counterpart). Kant argues that there is nothing internal to the figures by which the understanding alone could differentiate these two figures. It is only in relation to space as a whole that one can tell the figures apart. Therefore, the pure intuition of the form of space itself is a prerequisite for this judgment of incongruity. Kant says,

> Hence the difference between similar and equal things which are not congruent (for instance, helices winding in opposite ways), cannot be made intelligible by any concept, but only by the relation to the right and the left hands, which immediately refers to intuition. (Kant, 2001, p. 286)

This is clearly a rejection of Leibniz's view of space as the relation between objects themselves. It is also a rejection of Newton's substantival view of absolute space as something existing independently of the mind, in the world itself. Space is instead a relation imposed by our own cognition on what we perceive; "pure space is not at all a quality of things in themselves but a form of our sensuous faculty of representation" (p. 288).

Kant distinguished between *constitutive* principles and *regulative* principles. Constitutive principles concern the possibility of experience or appearances. Merely regulative are "those principles that are to bring the existence of appearances under rules *a priori*." (Kant, 1998, pp. A179, B221). The possibility of experience is necessarily given by pure intuition; therefore such *synthetic a priori* principles are constitutive of experience. Since *actual* existence is not given with necessity, principles concerning it are merely regulative, that is, not necessary, acting as rules for the synthesis of experience out of perception.

Although this is a transcendental argument, starting from the premise that there is *synthetic a priori* knowledge, the resulting explanation for the validity of such judgments in turn offers support for the claim that there can be such knowledge. Kant's unique approach introduces the idea that our cognition imposes certain constraints on all that we can experience. This accounts for the apparent necessity of certain judgments about the world. Indeed, for Kant, the *a priori* conditions of the possibility of experience simply are the objectively valid universal laws of nature.

Kant's view on the *aprioricity* of the principles of Newtonian physics and of Euclidean space-time was, of course, shattered by the development of non-Euclidean geometries, which hinted that the framework of Euclidian geometry was not in fact necessary *a priori*. The subsequent arrival of relativity theory confirmed this in beyond a reasonable doubt. The characteristic, which Kant sees as intuitively certain, is that space has the form dictated by Newtonian physics. This conflicts directly with the geometry of non-Euclidean relativity theory. Not only does modern physical theory deny the content of Kant's thought, it also throws doubt on his methodology. What Kant took to be apodictically certain according to pure intuition is in fact shown to be false, and, therefore, most definitely not apodictically certain. Nevertheless, neo-Kantian philosophies hung on with great tenacity. The challenge for any Kantian theory is to show that any physical principles at all can be proven to be synthetic *a priori* truths in the Kantian sense. What Kant took to be intuitively certain was shown to be wrong. So how can one argue that any other principles can be known with certainty in a similar way?

It is not possible to argue from actual scientific theories that any principles are necessary. This is merely an empirical claim and is open to refutation, especially in light of scientific revolutions. On the other hand, what transcendental arguments can be given that any principles are necessary? Pure intuition cannot provide self-evidently valid principles, since, as we see with Kant, what might appear to be apodictically certain only appears so due to limitations of the imagination, rather than cognition itself.

The challenge for the neo-Kantians is to square the Kantian philosophy with the advent of relativity, which explicitly denounces this form of space and time. One option is to insist that Kant is correct. This means either maintaining that relativity is wrong or that it applies only to scientific space and time, while Kant's notion of space and time applies to our psychological concept of space and time. The other option is to reject the content of what Kant thought was necessarily given by pure intuition. To remain in line with the tenets of Kantian critical philosophy, this means discovering another set of concepts which stand in the same relation to our knowledge of the physical world as do Euclidean geometry and Newtonian physics for Kant, but which conform to the principles of the theory of relativity.

One of Einstein's criticisms of neo-Kantianism, in this latter form, is that it seems to be an irrefutable theory. Just as there seems to be no transcendental argument for the absolute necessity of any given neo-Kantian principle, there does not seem to be an argument that there cannot be any such principle. As such, in any physical theory, one can always posit some "synthetic *a priori* principle". Einstein says,

> I am even of the opinion that this standpoint can be rigorously refuted by no development of natural science. For one will always be able to say that critical philosophers have until now erred in the establishment of the a priori elements, and one will always be able to establish a

> system of a priori elements that does not contradict a given physical system. (Einstein, 1924, pp. 1688-89)

4.2.2. Logical Positivism and Constitutivity

The logical positivist or empiricist[12] response to Kantianism is to deny the apodictic certainty of any principles. On the other hand, they did not reply with strict empiricism either – that the laws of nature can simply be inferred from the data through generalization. Instead, they acknowledged the contribution Kant had made in recognizing some *a priori* component of scientific theories. That is, inseparable from a physical theory is that which is brought to it by us before any observable physical content can have any meaning. A conceptual framework must be erected first, just as for Kant there are preconditions necessary for the possibility of experience. The essential difference between Kant and the logical positivists is the precise nature of such principles. Kant was limited by the logical, mathematical, and scientific viewpoint of his time, but the logical positivists argued that specific structures, such as Euclidean space-time, are not given with apodictic certainty. Rather, the space-time structure which must be in place is chosen as a matter of convention or by "coordinating definition". It is necessary that such a structure be in place to provide meaning for empirical science, but the exact nature of the structure is open to choice. Schlick is explicit in his delineation of empiricism from Kantianism,

> [M]ere sensations and perceptions are not yet observations and measurements; they only become so by being ordered and interpreted. Thus the forming of concepts of physical objects unquestionably presupposes certain principles of ordering and interpretation… An

[12] Among philosophers of this school (e.g. Schlick, Reichenbach, Carnap) there were of course differences among their views. For a much more complete history of the development, context, and analysis of their views see (Friedman, 1999; Howard, 1994).

empiricist, for example, can acknowledge the presence of such principles; he will deny only that they are synthetic and *a priori* in the sense [of having the property of apodicticity]. (Schlick, 1979, pp. 323-4)

Once the framework is established, then, in combination with observation, the physical theory falls into place. And so, just as the geometry of space-time is not simply given by pure intuition, it cannot be established by purely empirical discovery either. "[I]t is in no way a straightforward empirical matter of fact whether space is Euclidean or non-Euclidean" (Friedman, 1999, p. 7). The issue is taken to be closely analogous to the axiomatic structure of logic or pure geometry. Nothing about the world, or our cognition, imposes specific axioms of geometry, but once selected they completely define the geometric structure which follows from them. Likewise for the structure of space and time.

Reichenbach is explicit in his separation of the two distinct aspects of Kantian principles: apodictic certainty and constitutivity. He rejects the apodictic certainty of any principles. In so doing, Reichenbach does not entirely reject the Kantian approach, since he embraces their "constitutivity". Apodictic certainty and constitutivity need not necessarily go hand in hand as they do for Kant (Reichenbach, 1965). Intuition has no role to play in specifying particular principles with necessity. Any supposed such intuition, like that for Kant, cannot have *a priori* grounding since progress in the empirical sciences can always override it in the future. However, what is recognized is that without sufficient non-empirical definition, empirical laws cannot be meaningful. For example, attempts at empirically discovering the curvature of space by measuring it are bound to fail if they implicitly rely on light traveling in straight lines. But that light does travel in straight lines cannot be tested absent some coordinative definition. Some discussion among the positivists revolved around the exact nature of such definitions, but the general consensus was that their status is best understood as conventional, with there being some disagreement and discussion as to whether other restrictions must be considered regarding that choice. In Reichenbach's terms, only once these "axioms of coordination" are established can "axioms of connection" be well-defined and have meaning. We see in this distinction the emphasis on function, on the necessity of a framework in which to define theories which fall under its scope, which motivates Flores' move to distinguish between framework theories and interaction theories as we saw in Chapter 3. Reichenbach, too, classifies Newton's laws of motion as an upper-level theory, as axioms of coordination, and Newton's universal law of gravitation as an axiom of connection which can only be given concrete meaning within the framework established by the laws of motion. We see in Reichenbach, Schlick, and Carnap rigorous attempts at making this divide between axioms of coordination and axioms of connection sharp and coherent.[13]

We also find here the seeds of the fall of logical empiricism. Though there has probably been historic misrepresentation and certainly no one single logical empiricist position, this strict distinction between axioms of coordination and axioms of connection as different kinds of scientific propositions did not withstand attacks from Quinean holism, according to which there can be no such in principle distinction. The logical positivists also face objections for their adherence to strict epistemological (and hence purely philosophical) strictures, such as verificationism, as leading to philosophical advancement in the philosophy of science.

[13] For a much more detailed discussion of this problem for positivism see (Friedman, 1994; 1999)

4.2.2. Summary

Nevertheless, we have now a basis to think that there is a need in science for principle theories in order to frame further scientific inquiry in the physical world, and we have come a substantial way towards illuminating this relationship. However, there is more to be said. We have established, as was first noted by Kant and was still recognized by the positivists and Einstein, that it is necessary to have this framework; however, it has not been resolved what the nature of these principles must be. What is the role played by intuition, or by empirical discovery, or are they purely conventional stipulations chosen on pragmatic grounds? Can we meaningfully make a principled distinction between principles of coordination as different in kind from other propositions in the theory? And finally, what role does explanation play in all of this?

Kant argued that there are preconditions which must be in place for the possibility of there being any physical experience at all. For him these preconditions took the form of necessary *a priori* conditions given by pure intuition. Newton, for Kant, had clarified and formalized these preconditions for experience. The arrival of general relativity, and its use of non-Euclidean geometry, showed that Kant had been wrong in thinking that those preconditions for experience were necessary in the sense Kant had argued. However, Kant's point was recognized: there did need to be preconditions for experience that had to be in place prior to scientific observation. It is just that those principles were chosen contingently to establish the framework. It is necessary to have them, but what they are is a matter of choice.

The principles of a foundational principle theory must be of this sort. They must supply the framework, or the preconditions, which establish the meaning of the measurement of physical properties. Measurability is a necessary condition for doing science, and establishing a conceptual framework is a necessary condition for measurement and observation. Kant recognized this, but he was mistaken about the source and the nature of the principles which establish that framework. The positivists saw that flaw in Kant, but also accepted the necessity of having a framework. They saw it as a contingent choice. They too were mistaken about the source and nature of the framework which must be established.

There are a number of problems for the logical empiricist approach, which come from opposing sides. One problem is that, though there might be a meaningful distinction between axioms of coordination and axioms of connection, the choice of coordinative definitions is held to be entirely conventional, letting in an unacceptable degree of arbitrariness for the realist. It will be argued below that the choice of constitutive principles is not entirely conventional, that there are significant philosophical considerations which come into play in determining such principles.

From the other side, it is argued that positivism fails to even make an adequate distinction between purely constitutive principles and empirical laws. This is a line taken early on by Einstein, saying that the distinction must be made, but where it is made is itself arbitrary. Later developments in the philosophy of science also posed challenges to the logical empiricists. Kuhn (1962) argues that there are no philosophical arguments, such as the epistemological ones adhered to by the logical empiricists, which can be made that justify the choice of one theoretical framework over a different framework. I will argue that there are philosophical considerations which dictate to some extent where constitutive principles must play a role. Einstein presents us with typically insightful thoughts on the matter. Perhaps there is no principled place to make the distinction; nevertheless, given the structure of science and its concepts at a particular place in time, this decision is not entirely arbitrary, but based on conceptual inadequacies of the theory or theories in place.

4.3. Conceptual Foundations

DiSalle (2006) provides an analysis of the conceptual foundations of physical theories in a particularly nuanced manner. DiSalle's analysis places emphasis less on the nature of the conceptual framework of theories and more on the evolution of such frameworks, but in the process identifies some of the essential qualities of the frameworks which make such evolution possible. By approaching the task in this way, DiSalle is able discern where concept revision comes to be seen as necessary and where convention plays a role.

When we look to historical examples in the evolution of our theories of space and time, and the actors who play the role of developing them, we see that there are in fact remarkable similarities in terms of how they come to the conceptual framework which they do. This will hopefully shed some light on what the nature of these principles, which serve as the framework for scientific endeavors, must be like.

Kant introduces the idea that such frameworks must be there, and what has been up for debate is what they must be like, if they must be like anything at all. DiSalle shows us that there are some aspects of forming principle theories which at times does in fact become necessary – not in Kant's sense of *a priori* necessary, but necessary in order to overcome something like what Kuhn describes as a period of crisis. What this crisis amounts to, as DiSalle shows us, is the emergence of conflicting concepts within the current scientific framework. Until this crisis is resolved, not much progress, at a fundamental level, can continue. Of course the more intractable the conceptual inconsistencies are perceived to be, the more impressive the new theory which manages to resolve the crisis will be seen. Important examples of this include the theories of Galileo, Newton, and Einstein.

The manner in which the new principles resolve the old concerns reveals that aspect of principle theories which we are trying to uncover – that aspect which makes them foundational theories in need of no further interpretation – and which will hopefully aid us in resolving the interpretational issues which are presented to us by quantum mechanics.

4.3.1. Galileo

To investigate this further, it is useful to look at DiSalle's analysis in some detail. Let us go all the way back to Galileo and the revolution that he produced. Galileo recognized that the traditional Aristotelian concept of natural motion was in fact incoherent. The Aristotelian concept of natural motion is based primarily on the composition of an object, and its tendency to move towards its natural place in the universe. All objects are made up of the four basic elements, earth, air, water, and fire. The natural place for the element of earth is at the center of the universe. Hence objects made primarily of earthly stuff will naturally move downwards, towards the center of the earth, unless otherwise forced. Fire, the lightest element, will have a tendency to rise from the center of the universe. The proportion of different elements in an object dictates its weight. This then determines the speed at which an object moves according to its natural motion. Heavier bodies will fall faster, proportional to their greater weight. The Aristotelian system also holds that the earth is stationary, as it is the center of the universe. Neither is there any natural motion which would alter that condition. Applications of this framework, as later analyzed by Galileo, reveal serious inconsistencies.

Aristotelian evidence that the center of the universe is the non-rotating center of the earth, is provided by the fact that a stone dropped from a tower will land at the foot of the tower. If the earth were rotating, by the time the stone hits the ground, moving as it would directly towards the center of the earth, the base of the tower would have moved along with the rotating earth. However, as Galileo points out, this argument for the non-rotation of the earth fails because it is circular. As Galileo notes, if the earth is rotating, then that movement would be transferred to the horizontal motion of the stone in addition to its vertical motion, thus explaining the fact that the stone lands at the foot of the tower.

If the Aristotelian insists that such horizontal motion is not transferred, as should be the case given the natural motion of bodies according to Aristotelian physics, then there is a conflict with *everyday* experiences, where we do not even consider doubting this transference of horizontal motion. On a moving ship, one does not adjust for this movement if one drops a ball from the top of the mast. Instead, that motion is transferred. Indeed were one in the hull of the ship, there would be no way to determine if the ship is in motion or not based on the relative movement of objects in the hull. Many other examples demonstrate this as well.

Aristotle offers an *ad hoc* account of this type of "violent", or unnatural motion, possessed by a projectile with horizontal motion. As the projectile deviates from natural motion, the unnatural motion must be initiated externally; it must be a form of violent movement. Therefore, Aristotle tries to solve the problem by postulating that the air closes in behind the object and forces the horizontal movement. This solution is rather problematic for obvious reasons. But if this solution, even as *ad hoc* as it is, is adopted, then the original argument for the non-rotating earth is circular, assuming that the horizontal component of motion cannot persist without an external cause.

Galileo further points out the internal inconsistencies of the Aristotelian view with the following thought experiment:

> *Salviati.* But, even without further experiment, it is possible to prove clearly, by means of a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material and in short such as those mentioned by Aristotle. But tell me, Simplicio, whether you admit that each falling body acquires a definite speed fixed by nature, a velocity which cannot be increased or diminished except by the use of force or resistance.
>
> *Simplicio.* There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by addition of momentum or diminished except by some resistance which retards it.
>
> *Salviati.* If then we take two bodies whose natural speeds are different, it is clear that on uniting the two, the more rapid one will be partly retarded by the slower, and the slower will be somewhat hastened by the swifter. Do you not agree with me in this opinion?
>
> *Simplicio.* You are unquestionably right.

> *Salviati.* But if this is true, and if a large stone moves with a speed of, say, eight while a smaller moves with speed of four, then when they are united, the system will move with a speed less than eight; but the two stones when tied together make a stone larger than that which before moved with a speed of eight. Hence the heavier body moves with less speed than the lighter; an effect which is contrary to your supposition. Thus you see how, from your assumption that the heavier body moves more rapidly than the lighter one, I infer that the heavier body moves more slowly.
>
> *Simplicio.* I am all at sea because it appears to me that the smaller stone when added to the larger increases its weight and by adding weight I do not see how it can fail to increase its speed or, at least, not to diminish it.
>
> *Salviati.* Here again you are in error, Simplicio, because it is not true that the smaller stone adds weight to the larger.
>
> *Simplicio.* This is, indeed, quite beyond my comprehension. (Galilei, 1954, pp. 62-3)

Here, the Aristotelian view, depending on how it is applied, produces contradictory results. The Aristotelian system maintains concepts regarding motion which are incompatible upon application.

Galileo argues that the principles of motion must be changed. In particular, the principles which must be changed are conceptual ones. The concept of motion itself needs to be altered due to conflict within the traditional Aristotelian model. Galileo recognized the departure of the Aristotelian model from principles implicit in everyday phenomena and was able to formulate a new principle, the nascent version of Newton’s principle of inertia. Though the correct rendering of this principle was only fully formed through the later work of Descartes, Huygens, Newton, and others, the principle of relativity still bears Galileo’s name.

4.3.2. Newton

Newtonian mechanics also rests on constitutive principles, which are formed out of the ashes of previous theories that Newton had the insight to see as inconsistent, both internally and with observation, and therefore headed for crisis. DiSalle demonstrates that this is the case in at least two separate instances, where Newton sees the problem with Descartes' framework, and analyzes that problem in order to develop new principles, which stand as constitutive of the concepts needed to do physics. DiSalle focuses on the concepts of absolute time and absolute space. These become, under Newton, defined concepts, which can in turn be used to define further the measurable quantities of classical mechanics. These definitions were deemed as necessary by Newton in order to construct an empirical science at all.

The Cartesian view, a mechanistic philosophy predominant in contemporary physical science, contains within itself two separate, and incompatible, approaches. One approach focuses on the mechanical explanation of motion following from the work of Galileo. The idea is that uniform motion persists unless influenced from without, and this influence requires a mechanical explanation. Such an influence must involve the direct impact of one body on another for it to be mechanically intelligible.

The second approach develops out of Descartes' philosophical, *a priori*, perspective. For Descartes, the essential property of material substance is extension, making it distinct from spiritual or mental substance. Space, as a non-mental substance, must likewise have extension. Thus, space, having the essence of material substance, extension, must also be material. This *a priori* conception of space and substance serves as the basis for Descartes' theory of planetary motion. If body and space are substances with extension, then the entire universe is entirely full of matter. The universe, as an infinite plenum, only allows motion as circulations about various centers, because matter moving from one location can only move if that location is vacated, and so on. The motion of the planets and celestial bodies can then be explained mechanically by the introduction of the motion of fluid vortices, which carry the planets in their orbits. The universe is a plenum completely filled with matter, whose motion could only be accounted for by the existence of vortices. Since "space" too is a fluid, motion on this philosophical view is not motion with respect to any kind of absolute space, but motion with respect to the immediately adjacent fluid medium.

For Newton, these two separate approaches, the geometrical and the mechanical, were incompatible with each other, and prevented any clear conception of empirical measurement. The two viewpoints appear to be at odds over the concept of motion, which reveals, on deeper analysis, that they are also at odds with respect to the concepts of space and time. Within the Cartesian system, there appears to be a vicious circularity regarding the definition of motion and body. Without a single, coherent concept of motion, measurement and any meaningful empirical investigation could not be done. Therefore the preconceptions of space and time needed to be analyzed such that this could be accomplished.

For Cartesian mechanics, the motion of the planets around the sun could be causally explained by the movement of the vortices as carried from the rotation of the sun at the center. However, from the viewpoint of the earth, using the Cartesian philosophical perspective, Descartes could say that the earth was entirely at rest, given that it was surrounded by the fluid, and relative to it, immobile. Thus, there is a tension in the Cartesian framework regarding the concept of motion, which Newton articulates:

> [T]he individual parts of the heavens, and the planets that are relatively at rest in the heavens to which they belong, are truly in motion. For they change their positions relative to one another (which is not the case with things that are truly at rest). (Newton, 1999, p. 413)

Additionally, DiSalle addresses the famous thought experiment Newton devised, which envisions a bucket full of water twisted on a rope, and which has traditionally been taken to be an argument by Newton simply meant to show that space is absolute and not relative. DiSalle points out that this interpretation is misguided, and that the real point behind the thought experiment is to show that the Cartesian position is inconsistent, even within its own physical theory. First, we imagine a bucket of water suspended from a rope. In it, the surface of the water is flat. Then the rope is wound tightly, and with the bucket and water at rest, the bucket is released with a rapid rotation so that the unwinding of the rope will drive the rotation. First, the bucket will spin rapidly relative to the water, with the water surface remaining mostly flat. But the motion of the bucket will be transferred to the water, and the surface of the water will become concave as it eventually comes to rest relative to the motion of the bucket. If the bucket is stopped, the water will continue rotating, no longer at rest relative to the sides of the bucket, and with its concave shape until it eventually returns to the initial state (Newton, 1999, pp. 412-3).

From the Cartesian point of view of rotating vortices, when the bucket is released, the water begins to rotate, and it is in motion relative to the surrounding bucket. As the water becomes more concave, its motion relative to the bucket becomes less and less, until at its greatest concavity – an effect we can measure – it has ceased moving altogether from the Cartesian standpoint. Now, however, immediately after the bucket is stopped, the same concave shape is apparent, but the motion of the water is at its greatest, just as it had been when the bucket was initially released. But of course, then the water had been flat. Finally the water will stop its motion relative to the bucket and will also be flat. In other words, according to the Cartesian philosophical standpoint, precisely the same motion of the water at two different times, i.e. motion relative to the sides of the bucket, produces very different dynamical results. At one time that motion is associated with a concave surface to the water, at another a flat surface.

The dynamical results of the water climbing the sides of the bucket is, for Newton, the objectively measurable phenomenon. Cartesian motion can say nothing about this phenomenon. Yet, the dynamical phenomena must provide the measure of motion. More condemning for the Cartesian system is the contradiction which arises from the explanation of the bucket experiment with the causal explanation for the movement of the planets and stars. The Cartesian explanation depends on the centrifugal forces, along with the resistance of the fluid in other vortices, to describe the motion of the planets. But these forces are ruled out by the philosophically motivated mechanical conception of movement. For the Cartesian, non-rectilinear motion needs to have a mechanical cause to explain any deviation from natural motion. Planets would continue their straight line path except that the fluid in the vortex alters that motion to constrain the planet’s movement in orbit. But on the

Cartesian philosophical theory, the planet has no motion at all, for it is at rest with respect to the medium surrounding it. A planet has, at once, non-rectilinear motion, which needs to be explained by some force acting upon it, and no motion, which precludes the possibility of forces acting on it. So the entire theory of planetary motion in the solar system is self-contradictory.

If motion is defined in the Cartesian manner, then no planet or star will be in motion, or at least, it becomes impossible to say what its motion might be or mean. More importantly, it is impossible to say whether a body is free of forces acting upon it, and impossible to say whether it is in uniform motion or not. Ultimately this leads to a breakdown in the ability of the physical science to function.

> The crux of Newton's dynamical argument, then, is that the Cartesian definition ignores the aspects of motion that are central to Cartesian physics. It defines a univocal velocity for every body – indeed, every particle – in the Universe. But it does not offer any physical measure of the accelerations and rotations that are central to our understanding of the fundamental causal interactions. (DiSalle, 2006, p. 33)

Newton also takes up the definition of uniformly moving time in his analysis of motion, since it is a concept, which, along with space, is integral to the understanding of the concept of motion. Newton evaluates the concepts that are involved in the contemporary view of time, argued for by Leibniz, according to which, like space, time is a purely relational construction. The assumptions of this approach were ones which Newton also held and which supported his view, but which, when brought to light, exposed problems with the relationalist view. As for Cartesian physics, the relational view of time implicitly requires a notion of absolute simultaneity, or the notion of succession. Newton makes this explicit. Newton's laws of motion served to define the notion of uniform time, specifically determining how to differentiate between equal and unequal time intervals. Among the implicit assumptions shared by Newton and his contemporaries is that there is a "genuine physical distinction between inertial motion and non-inertial motion, and that there is an unambiguous way of determining all of the forces involved in every non-inertial case" (DiSalle, 2006, p. 22). Newton's three laws provide a means of differentiating between inertial and non-inertial motion, first by defining it in terms of the presence or absence of external forces. The other two laws establish the means of determining those forces or the absence of them.

Thus, what we see in Newton's reasoning is the analysis of the concepts which were being used in contemporary physics, both explicitly and implicitly. What he recognized was that some of these concepts were in need of more precise definition. Indeed, to make the concept of motion, as understood at the time, meaningful, those presuppositions behind it needed to be first developed into a coherent conceptual framework. DiSalle's argument is that Newton's development of the principles of motion was not simply the positing of hypotheses about the existence of absolute space and time. Rather, Newton was defining the concepts necessary for making hypotheses that could be meaningfully understood and tested in the first place.

4.3.3. Einstein

Following DiSalle, we now turn to Einstein and his work in developing the special and general theories of relativity. Einstein saw that the conflict which had arisen between electromagnetic phenomena and the Newtonian theory of motion was rooted in the implicitly held concepts of space and time. The central problem emerging at the time was an apparent conflict between Newtonian dynamics and Maxwellian electrodynamics. Newtonian dynamics adhered to the principle of relativity, that the physics in one system is the same for any system in uniform motion with respect to it. Electrodynamics stands as a possible exception to this principle, since it only holds in a frame at rest with respect to the aether. This was complicated by the inability to measure any electrodynamic phenomena in motion with respect to the aether, most famously demonstrated in the Michelson-Morley (1887) experiment. Lorentz explains this failure by appealing to molecular forces, which contract proportionally to movement through the aether, thereby accounting for this null result.

Both Lorentz's and Einstein's theories account for the empirical data. The crucial difference, according to DiSalle's analysis, is that Einstein recognizes that the Newtonian framework on which Lorentzian dynamics rests is conceptually inadequate. The notion of an inertial coordinate system in undefined. For this we require a kinematic description of motion, for which the concept of time must be defined. The Newtonian system fails to do so because it relies on an intuition of simultaneity which cannot be connected to any empirical definition. It depends on a notion of the instantaneous propagation of gravitational force. However, approximating this empirically relies on physical processes and ultimately on light signaling. But this approximation completely fails if light fails to obey the laws of velocity addition.

According to DiSalle, the breakdown occurs because the "intuitive *theory* of simultaneity" fails to meet the "intuitive *criterion* of simultaneity" (2006, p. 111). That is, operating to form the contemporary theory of simultaneity is a conception of its role in the theory of space and time. However, Einstein determines that the actual theory departs from the common sense conception of the role simultaneity ought to play. Specifically, "[Einstein] seeks a *criterion* of simultaneity that is independent of position and motion, that has a foundation in physical laws that are independent of any observer, and that makes simultaneity a symmetric and transitive relation" (DiSalle, 2006, p. 110). The condition which Einstein establishes is that, "we establish *by definition* that the 'time' required by light to travel from *A* to *B* equals the 'time' it requires to travel from *B* to *A*" (Einstein, 1905b, p. 894). This satisfies the criterion Einstein requires.

This definition does not, however, establish whether or not the velocity of light is *in fact* invariant. The invariance of Maxwell's equations could be explained by the universal contraction hypothesized by Lorentz. However, what the Newtonian framework of Lorentzian dynamics lacks, Einstein's theory of special relativity has, and that is a clear and meaningful definition of simultaneity, and, therefore, a clear and meaningful definition of time, space, and motion. Lorentz must explain the theory and the Lorentz invariance of otherwise disconnected phenomena, whereas Einstein's theory covers all of this with the definition of simultaneity, and there is no need of postulating any sort of hypothetical explanation. Einstein recognized the implicit role that signaling had in defining the inertial frame, but that it had been possible to overlook this until then. In overlooking it, however, the concept of an inertial frame had not been given a clear meaning.

We can see how Einstein's postulate regarding the propagation of light takes on an *a priori* character. It is not simply the result of inductive generalization. If this were the case, the constancy of the speed of light would require explanation. Rather, the postulate acknowledges the constitutive role of the velocity of light and uses it to impose a structural framework wherein physical explanations can be made. As DiSalle (2006, p. 118) notes, from this standpoint it makes no sense to demand an explanation for the principle itself. To do so must always be circular, just as asking for an explanation for force and acceleration in Newtonian dynamics is misplaced. These concepts serve as defining principles which can impose meaning on the concept of an inertial frame. There can be no external justification for such constitutive principles.

In the development of general relativity, a similar conceptual analysis takes place. Einstein's analysis discovers in the Newtonian system another instance where implicitly held views, upon analysis, cannot serve to form coherent definitions without arbitrary stipulation. Implicit in Newton's measurement of absolute acceleration, and hence inertial frames, is the ability to distinguish between bodies with inertial motion and bodies in gravitational free-fall. However, just as, upon analysis, it turns out that the notion of absolute simultaneity rests not on actual empirical principles but on abstractions, so does the idea that we can distinguish a center of mass in gravitational free-fall from one in inertial motion. But if they cannot be distinguished, i.e. if the equivalence principle holds, an acceleration relative to the center of mass can only be relative acceleration. Choosing the center of mass as an inertial frame is only an arbitrary choice of a coordinate system. The apparent inconsistency, from the Newtonian point of view, is that two frames can be seen as inertial and yet be in relative acceleration to each other. Accepting the equivalence principle means implicitly accepting the notion of geodesic motion, where, "what is distinct about free-fall corresponds to what is distinct about geodesic trajectories: the only objectively distinguishable state of motion corresponds to the only geometrically distinctive path in a generally covariant geometry" (DiSalle, 2006, pp. 131-2). The apparent contradiction from the Newtonian framework is, under Einstein's framework, the precise means by which we measure the curvature of spacetime.

4.4. Analysis

DiSalle's overall thesis demonstrates a number of points about theory building. Among the points that are made are broad arguments that previous philosophies have run roughshod over the more subtle issues involved, historically, in the evolution of space-time theories in physics. In particular, DiSalle takes issue with the Kantian approach, with the logical positivists, and the Kuhnian perspective. What DiSalle tries to draw out is that all of these philosophical approaches are too simplistic in their analysis of theory construction. On the other hand, DiSalle is concerned to maintain the idea that philosophical analysis, at least in certain situations, has a strong role to play in theory development. It is just that that role has been misrepresented. What is important is the role of conceptual analysis, which takes the existing theory and analyses its presuppositions in the face of contingent empirical facts that have arisen since the time of the theories' original inception.

The reason we have looked at DiSalle's analysis in such depth, from Galileo through Einstein, is to demonstrate the consistent pattern of conceptual analysis in theory generation. It also shows that this analysis specifically addresses only particular kinds of theories. These are theories which serve as preconditions for the possibility of scientific knowledge by establishing a consistent conceptual framework that defines the meaning of empirical investigations under it. In DiSalle's investigation, the most fundamental of these are theories regarding space and time. There are, arguably, other framework theories, based on principles which serve to set the framework in which empirical questions can be asked. Examples of such theories might be thermodynamics, and from the point of view of this paper, quantum mechanics. Theories of space and time occupy a unique position, in that the level at which they function is so high that they must be constructed as framework theories. They establish the structure within which all physics operates. This also explains the *a priori* character they seem to have, as recognized by Kant and the logical positivists. The justification for such theories is not solely empirical, for they actually serve to define what counts as an empirical justification in the first place. Theories of space and time, as developed by Newton and Einstein, define the structure in terms of which the notions of causal interaction and measurable physical phenomena are meaningful.

Space-time theories, are going to be "framework" theories in the nomenclature of Flores, because of the role which they must play in physics. Indeed, they are the ultimate class of all framework theories, setting up the conditions for doing any meaningful physics in the first place. As we saw in Chapter 3, any framework theory will also be a principle theory, insofar as it is based on conditions or principles constitutive of that framework. We see, therefore, that in the particular case of space-time theories such as Newton's or Einstein's, the principle theories are in fact of a particularly foundational nature, and why. These broad theories establish the structure on which other theories must be built. There is, therefore, a connection between being a foundational theory and being a principle theory.

4.4.1. Holism

Here we must pause to consider a significant objection, one which was also posed to the logical empiricists both by Einstein and Quine. This is that there can be no principled distinction between those aspects of a theory which are constitutive and those parts which are empirical. If this is the case, then it might put any foundational role that principle theories could play in jeopardy by collapsing any unique and vital function they might fulfill.

Quine (1951) presents a view of holism which regards theories as a complicated conjunction of statements. Therefore, in testing a hypothesis, we are really testing the theory as a whole, since the hypothesis cannot be meaningfully tested in the absence of the rest of the theory.

> If this view is right, it is misleading to speak of the empirical content of an individual statement – especially if it be a statement at all remote from the experiential periphery of the field. Furthermore it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements which hold come what may. Any statement can be held true come what may, if we make drastic enough adjustments elsewhere

> in the system… Conversely, by the same token, no statement is immune to revision. Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle? (Quine, 1951, p. 40)

This Quinean holism means, not only that any element of a theory is in principle revisable, but that any distinction between axioms of coordination/constitutive elements of a theory and axioms of coordination or empirical/factual elements of a theory is undermined. In turn, this undermines the functional distinction between principle/framework and interactionist/constructive theories, since both must be part of a theory's holistic structure and from a logical point of view on a par.

Although holism, and the idea that no hypothesis can be tested in isolation, has come to be known as the Duhem-Quine Thesis, Friedman (1994; 1999) notes that there are important differences between the holism of Duhem and that of Quine. The logical empiricists did not fail to recognize the problem of holism from very early on. While there is significant danger of lapsing into Quinean holism, whereby the distinction between conventional and factual is meaningless (as occurs with Schlick's conventionalism), this does not have to be the case, at least not immediately. Carnap (1937) accepts Duhemian holism, that a hypothesis cannot be tested in isolation and that any statement in a theory, constitutive (L-rule) or empirical (P-rule), is open to revision. However, he still maintains that there can be a distinction between L-rules and P-rules. The difference is that in revising an L-rule the language of the theory is altered, whereas the revision of a P-rule does not change the language itself, but only the empirical statement within the given language (Friedman, 1994, p. 31). This ability to hold onto the distinction between non-empirical statements and empirical ones, while accepting general revisability, is what marks the distinction between Duhemian holism and Quinean holism.

Howard (1994) explores the depth of the relationship between the thinking of the logical empiricists and Einstein. This relationship traces back to the earliest attempts at expounding the logical empiricist program. As we have noted, the development of logical empiricism was strongly encouraged by the advent of relativity theory. Howard even suggests that it may have been Einstein who first floated the idea that the *a priori* character of some physical principles is better seen as conventional. However, Einstein did see such conventions as necessary for science. He says, "[the conventional "categories"] appear to be *a priori* only insofar as thinking without the positing of categories and of concepts in general would be as impossible as breathing in a vacuum" (Einstein, 1949b, p. 674).

Where the logical empiricists and Einstein eventually departed was with Einstein's much more holistic view. As noted, choices in *a priori* principles are conventional. Thus, the choice between a Euclidean geometrical structure and a non-Euclidean structure as a theory's *a priori* principles is determined, not empirically, but for pragmatic reasons. The choice is more or less conventional. However, Einstein's conventionalism does not exist at this level alone. It is also a matter of convention as to where one makes the division between which elements of a theory are *a priori* and which are *a posteriori*. Carnap's dissertation (1921) provides an example of this idea. It is not only that there can be a choice among geometrical structures, which then, along with empirical considerations, determines your measuring rod. It is also the case that one may instead choose to select one's measuring rod by convention, thereby determining one's spacetime metric. Einstein's position is that there is no in principle distinction between axioms of coordination and axioms of connection, or constitutive principles and empirical claims. However, though there is no fixed line, one must be drawn in order to test a theory. There must be constitutive principles, though what they are and how they must be chosen is not determined. This necessary condition for testing is in line with Duhemian holism, in that only a theory as a whole has content and can be tested, and that any principle is open to revision. Einstein provides a useful analogy:

> All that is necessary is to fix a set of rules, since without such rules the acquisition of knowledge in the desired sense would be impossible. One may compare these rules with the rules of a game in which, while the rules themselves are arbitrary, it is their rigidity alone which makes the game possible. (Einstein, 1954a, p. 292)

He follows this up saying,

> The question as to which of the propositions shall be considered as definitions and which as natural laws will depend largely upon the chosen representation. It really becomes absolutely necessary to make this differentiation only when one examines the degree to which the whole system of concepts considered is not empty from the physical point of view. (1954a, p. 293)

In other words, the axioms of coordination are arbitrarily chosen, and the distinction between axioms of coordination and axioms of connection is also arbitrarily drawn.[14]

Just as Friedman's account of Carnap does, Einstein seems to thread the needle between Duhemian holism and Quinean holism, though leaning more towards Quine than did Carnap. He does this by allowing that while there is no principled

---

[14] A historical note: Howard (1994, pp. 97-98) notes that Einstein's holism and its impact on verificationism as well as the distinction between analytic and synthetic predates the publication of Quine's "Two Dogmas of Empiricism" (1951).

distinction between constitutive principles and empirical ones, in practice the divide can and must be made. However, we are not provided with much information on what this divide might consist in apart from mere psychology perhaps. Carnap, as we saw, tries to spell it out in terms of the basic language of the theory.

I argue that the analysis provided by DiSalle provides a way to shed light on this issue. I think that it can give us a robust enough picture of theory revision and structure to construct a viable option between the pitfalls presented by holism, Kuhn, and concerns regarding unwanted degrees of arbitrariness in theory formation. We can in fact accept some degree of theoretical holism and even embrace it – that theories are only meaningful as a whole, and that there is no in principle distinction between constitutive principles and empirical ones. However, we learn from DiSalle that contingent aspects of scientific progress mean that, upon serious conceptual analysis, certain concepts of the body of theory reveal themselves to be in conflict either with the main body of the theory itself or with empirical facts which arise. In principle, we could revise the theory anywhere so that it can absorb or adjust to this conflict. However, we revise it where we see it (or where individuals like Newton or Einstein see it), and those conceptual locales become established as the defining or constitutive propositions in the body of the theory. That is, while there is no strict determination of where theoretical revision occurs, it is deemed necessary that revision should occur for reasons of internal consistency, and there are plenty of reasons, given the particular contingent facts about the actual state of science at the time, which explain where the revision takes place and which principles become constitutive.

This is where the philosophical considerations DiSalle is concerned to demonstrate enter into the picture. Recognizing that the theory as a whole is incomplete or internally inconsistent and determining not just what is easiest to revise, but even understanding *how* it might be revised, requires serious conceptual analysis of the theory from a standpoint, which to some extent, is outside of the theory itself. This picture provides a response to the Kuhnian, since there is rational progression from one theory to the next. For those involved in this process, there cannot be any incommensurability between the old framework and the new, since it requires deep understanding the conceptual limitations of the old theory if we are to develop a new structure which is holistically sound. We can also drop the need for a principled way to divide our theoretical language into two distinct parts, axioms of coordination and axioms of connection using Reichenbach's terms. We can see, however, that any concept revision will require non-empirically justified principles, to establish the meaning of the theory. The epistemological divide is therefore rooted in the state of science and in the particular conceptual analysis that takes place. Though conventional to a certain extent, there are good philosophical and contingent empirical reasons for choosing the conventions that get chosen. This results in a sort of structured holism.

4.4.2. Unification and Explanation

From a holistic standpoint, lack of conceptual coherence represents a breakdown in meaning within a given theoretical structure. Since a theory stands or falls in its entirety, any element of the theory which is incompatible with the rest shows that the theory must be altered to maintain that coherence. Of course, a theory may function successfully for a long time, perhaps indefinitely, with hidden underlying conceptual inadequacies. This was the case with Newtonian physics for example. As we saw, it operated with an implicit understanding of the notions of simultaneity and of the ability to distinguish inertial motion from gravitational motion. For much of the long history of Newtonian physics, that these were not well defined simply did not matter. However, contingent empirical discoveries made it clear that there were underlying conceptual inconsistencies that had to be resolved for future science to be well defined. Hence, in some circumstances involving foundational theories regarding notions like space, time, and causation, conceptual clarification becomes a necessary aspect of theory progression.

This also allows us to discuss more concretely the relationship between principle or framework theories and the unificationist program in scientific explanation of Friedman and Kitcher. Unification by covering the most facts with the least argument patterns (Kitcher, 1989) is the product of bringing more phenomena under one theoretical structure. Principle theories can offer explanation by unification in this sense as discusses in Chapter 3. Principle theories can also have significant foundational merit in some cases because they establish the conceptual framework necessary for a theoretic structure with empirical meaning, by providing the preconditions for the explanation and understanding of phenomena that fall under the theory as established in this chapter. That is, theories such as this are necessary for any explanation at all because they provide the conceptual framework.

We can also say something about the historical connection between conceptual analysis of this sort and Friedman/Kitcher unification. When it becomes apparent that a particular framework is in a state of crisis, it is because of the emergence of phenomena unanticipated by it and about which the theory can say nothing. That is, a problem becomes apparent because of disunity at the level of the phenomena, and the intractability of the problem can sometimes point to an underlying conceptual problem. This is what Einstein was able to see. In cases of where conceptual problems are resolved, it will often be that they are noticed because of problems with unification at the level of the phenomena. Likewise, the resolution of their conceptual issues will often allow the possibility Friedman/Kitcher-type unification and explanation of the problematic phenomena. We see this in the special theory of relativity. The necessity of conceptual revision becomes evident because of the apparent conflict between Newtonian mechanics and Maxwellian electrodynamics. Einstein's analysis establishes the constitutive framework defining a functioning concept of simultaneity and of spacetime. The conceptual analysis allows for the unification of the fields of electrodynamics and mechanical dynamics.

The tools necessary for evaluating the framework, of necessity, come from outside of it. This requires a broader perspective, from which it is possible to reestablish a meaningful definition of those concepts necessary to do physics. This was the case in developing Newtonian mechanics, special relativity, and general relativity. This will often lead to the unification of new sets of phenomena. This is a function of how the crisis presents itself. For this unification to be possible, the conceptual framework must be such that the empirical terms employed are well defined. This requires conceptual analysis according to which the principles that play the appropriate constitutive roles can be established.

The concepts of the prevailing framework come up against empirical facts which do not fit. As DiSalle, shows, philosophical analysis is then the vitally important tool in determining just what conceptual presuppositions are at stake and which are in conflict with the new physics. This in turn leads to further analysis as to how to carry on by resolving the apparent inconsistencies. As a procedural fact, at least historically, this analysis comes at a time of crisis, which highlights the problem. This process generates a conceptual framework that allows for meaningful scientific explanation.

4.5. Conclusion

In summary, some types of theories are necessary as the preconditions necessary for defining empirical measurement and hence, the preconditions for scientific or empirical explanation. The most obvious, and perhaps only clear historical, example of this is found in space-time theories, hence their special relevance in the history and philosophy of physics. These theories are framework theories in Flores' sense. They establish the framework within which other theories can be formulated and within which questions can be asked with the possibility of getting empirically meaningful answers. Thus it is necessary that, as both Kant and the positivists realized, these theories must have an *a priori* character that is not based strictly on empirical discovery since they define the nature of that empirical discovery. Therefore, these theories are constructed in part via a process similar to definition. As definitions they are, therefore, principles restricting the meaning of empirical claims. As a matter of fact, not epistemic necessity, new principles are formed when it becomes the case that we need a new constitutive framework, when the old framework becomes insufficient in light of empirical discoveries that eventually come to be seen as falling outside the scope of that conceptual structure. The principles arise, as a matter of fact, from the necessity of resolving conceptual conflict. Framework theories of this foundational type are generated out of a need to resolve conceptual conflict. "This interpretive aspect of the laws of physics is the source of their a-priori and seemingly unrevisable character; their actual revisability reflects what a stringent requirement it is upon such a theory, that it be capable of bringing the relevant phenomena within its interpretive grasp." (DiSalle, 2006, p. 161) In other words, the crisis arises from conceptual conflict or lack of coherence and this drives the need for conceptual analysis and revision.

To reiterate, we are talking about a small class of theories. This is not meant to be an explication of all physical theories or a general philosophy of science. In Chapter 3 we saw that both principle theories and constructive theories aim at explanation, but the mode of explanation defines the distinction. Principle theories explain by way of unification. The upper-level type of theory that we have been looking at in this chapter works at the conceptual level and established the preconditions for explanation via unification. To clarify, these upper-level theories, such as those discussed involving space and time, must be principle theories. Their purpose is to define the conceptual structure of physics. Recall that a call for the interpretation of some theory often stems from some explanatory failure. Therefore, a principle theory that succeeds from an interpretational standpoint will fulfill a suitable explanatory function either by unifying or, as an upper-level principle theory, by constituting the explanatory framework itself. The theories we have been looking at in this chapter fulfill this function through the resolution of conceptual conflict and hence the establishment of preconditions for scientific explanation. As DiSalle says,

> When we ask how the principles of a theory are to be interpreted, or how the structure associated with a theory is to be interpreted, we have already lost sight of the genuine content of those principles. For the principles are not, after all, purely formal principles in need of interpretation; rather, they are themselves principles of interpretation. (DiSalle, 2006, p. 160)

The foundational conceptual work in these theories is the deepest and most basic precondition for explanation, of increasing understanding, by conceptual revision to formulate a coherent whole out of previously inconsistent conceptual parts, thereby establishing an explanatory framework.

Once again, to remind ourselves of where we are, recall that our aim in continuing this line of investigation is to determine the viability of using the developments of quantum information theory to solve the interpretational problems historically attending quantum mechanics. We have ruled out certain types of approaches which this perspective might seem to engender, namely the instrumental approach of subjective Bayesian quantum mechanics and any sort of ontic or constructive approach using information. This still leaves the possibility of developing a principle theory along the lines laid out by Einstein.

One such approach has been carried out by CBH and further by Bub. We saw in the last chapter that it could not be seen as unifying in the sense of Kitcher and Friedman. Do the information-theoretic principles presented allow the conceptual analysis necessary for developing an interpretation of quantum mechanics constitutive of the concepts which need to be resolved? I would argue that they do not. Among the central concepts which sit at the center of the storm are those of measurement and causation. Like space and time, these concepts are among the most fundamental and basic for the understanding of empirical science. Therefore, like in the case of theories of space and time, a clear and consistent conceptual scheme regarding these notions must be in place prior to any other physical science. The constitutive approach along the lines outlined by DiSalle is thus entirely appropriate.

The CBH approach, however, does not establish for these concepts any clearer meaning, reconciling somehow the intuitive *theory* of measurement and causality which fails to meet the intuitive *criterion* of measurement and causality. The argument is that if the information-theoretic principles hold, measuring instruments must ultimately be viewed as information sources, or as black boxes. In the sense that it does any analysis, it seems to be an argument for instrumentalism. If the principle of the constancy of light is constitutive of the notion of simultaneity by revising that concept such that it must be a relative description, then the no-cloning principle restricts the concept of measuring instrument such that quantum mechanics must ultimately be only about prediction and can say nothing of how measurement results come about. This is essentially the argument for complementarity couched in information-theoretic terms. Unless somehow there is some ontological role for information, the no-cloning principle is equivalent to the claim that the structure of quantum mechanics is non-commutative. So this is not a new argument that quantum mechanics is best seen as instrumentalist. The information-theoretic aspect of the argument plays no role.

Leaning on the analogy with special relativity a bit further, the principles of special relativity are constitutive of the concept of simultaneity and thereby of a coherent notion of time, space, and motion. There is a principled derivation behind this definition of simultaneity. And we can accept this principled reason because without it there is no coherent framework. This is not the case for CBH. We might think that because there is a principled argument for instrumentalism, that the same sort of thing is being done. One reason that this is not the case is that the information-theoretic principles, as such, are not doing any work which the structure of quantum mechanics does not already provide. The no-cloning principle implies instrumentalism. But the non-commutivity of quantum mechanics implies instrumentalism in just the same way. But I take it that there is supposed to be something additional going on by using information-theoretic principles. It is not clear what this is.

There is something of a self-supporting, or circular character to Einstein's argument. Normally this might be considered a bad thing, but as we have seen, it is a fundamental aspect of constitutive principle theories. It is also what is lacking in the CBH approach. The structure of the argument seems to be the following: Quantum mechanics can be axiomatized by some set of information-theoretic principles. If this set of principles is true of the world (as they appear to be), then quantum mechanics is best seen as an instrumentalist theory (along with the specific details of the theory). I take it that the overall CBH objective is to establish not just the consequent of this conditional, but also the further conclusion that the antecedent is the best representation with which to understand quantum mechanics. The same is the case in the argument for special relativity. If its two postulates are true of the world (as we have reason to believe they are), then the concept of simultaneity must be a relative one (along with the specific details of the theory). But there is more going on here. To this we add that if the concept of simultaneity is not understood in this way, then there is no clear meaning of essential empirical terms such as space, time, and motion. Therefore, we ought to accept these principles since they are constitutive of this concept of simultaneity. The justification for the principles is that they are constitutive in a way that is necessary for establishing the meaning of physical concepts and an explanatory framework.

The CBH argument does not seem to have this feedback loop which is vital for a constitutive principle theory. The objection does not rest simply on the fact that the principles imply instrumentalism and that is objectionable (though I think that it is). The objection is that the analogy with special relativity does not hold up. There is not the additional constitutive aspect such that if instruments are not considered black boxes, then there is no clear meaning of the relevant empirical terms. That is, it is necessary to understand instruments as black boxes just as it is necessary to have this new conception of simultaneity for the sake of providing a well definedconceptual framework. The issue is indeed how to understand concepts like measurement and causation in quantum mechanics, but the argument here does not seem to go any way towards proving any conceptual insight. The approach establishes it a as postulate of the theory that these are irresolvable concepts. It may be that this is the best way to see quantum mechanics; that Bohr was always right. But the CBH argument does not have the same constitutive characteristic as does special relativity or Newton's laws of motion which justify those principles. At the end of the argument, the troublesome concepts are no less so. So there is no reason, as there is for special relativity, to accept the principles as constitutive in a way that clears up interpretive issues. And because the principles are not constitutive, there is no more reason to prefer them as the basis for an argument that measuring instruments are ultimately black boxes over that given according to the Copenhagen interpretation. That is, the information-theoretic aspect is not operational in any justification for instrumentalism, if there is one.

The conclusion of this discussion will need to wait until the next chapter where an important extension of the CBH program advanced by Bub and Pitowsky (2007) is discussed. We are in a better position now to offer insight into how this strategy, of developing a principle theory around quantum mechanics, could be successful. Likewise, the language of quantum information theory does seem to be an ideal candidate for attempting this, in the same way that the development of non-Euclidean geometries provided a language and broader framework within which Einstein could develop a theory of relativity. So while the CBH program itself is not successful, it does point the way to a potential framework, or how to find one. The Bub and Pitowsky extension will be considered in the next chapter.

Chapter 5: Bub and Pitowsky

5.1. Introduction

In a recent paper by Bub and Pitowsky (2007), the authors pursue the analogy between quantum mechanics interpreted as quantum information theory and special relativity. As in Bub (2004a; 2004b), quantum mechanics is presented as a principle theory, but the emphasis is shifted away from the direct implications of being a principle theory. Instead, the primary lesson to be taken from special relativity and its success is the explanatory structure it offers, which is seen as specifically due to a shift from a dynamic viewpoint to a kinematic one. It is this shift which is taken to make special relativity preferable over Lorentzian dynamics. The project for Bub and Pitowsky is to make a similar shift in quantum mechanics, thus offering a realistic information-theoretic interpretation of quantum mechanics.

The Bub and Pitowsky paper works on a number of levels. In the broadest sense, it offers a comparison between various interpretations of quantum mechanics, specifically between the Oxford Everett view and Bub and Pitowsky's new information-theoretic position, with somewhat less emphasis on contrasts with Bohm and GRW. The problem for quantum mechanics is defined in terms of two measurement problems, the *big measurement problem* and the *small measurement problem*. The big measurement problem is the standard measurement problem dealing with the apparent "collapse" of the quantum state. The small problem is how to account for the classical characteristics of the macroworld given its quantum underpinnings. The small measurement problem is more easily dealt with using a physical solution by appealing to the process of decoherence. Essentially, the authors argue, the big measurement problem arises when one adheres to what they call the two dogmas of quantum mechanics. These two dogmas consist in 1) the demand for complete dynamical analysis of a measurement and 2) the insistence that the quantum state has ontological significance representing what is true and false in the world. If we can reject these two dogmas, then the traditional measurement problem in its two distinct forms goes away. It seems that the main project of the paper is to show how it is possible to reject the two dogmas without thereby falling into instrumentalism. It is here that the analogy between the information-theoretic approach to quantum mechanics and special relativity comes in to play. The argument is that the explanatory structure of special relativity which makes it uniquely successful in contrast to Lorentz's dynamical theory can be mirrored in quantum mechanics by undertaking a shift from a dynamical perspective to a kinematic one, and that this allows the rejection of the two dogmas, while at the same time maintaining a realist position, as is done in special relativity.

Like the argument for a principle theory approach to quantum mechanics, this view offers a similar and related meta-theoretical shift, though instead of focusing on the principle/constructive theory distinction, the focus is on the distinction between the structure of kinematic explanation and dynamical explanation. It is necessary that we investigate this conceptual shift to see how it works and whether or not it is successful. This will be an instructive endeavor for the purposes of this dissertation.

The strategy is related to the principle theory approach, but it again misses the underlying significance which is foundationally relevant in special relativity but still seems to be lacking here.

The significant shift which takes place in this information-theoretic interpretation is to the kinematic perspective. This is a powerful theoretical shift, but the significance behind it is missed both by Bub and Pitowsky, and by Janssen, whose work on special relativity motivates much of the work done here. For Janssen (Janssen, 2002; 2007), the kinematic stance that special relativity has makes that theory superior to Lorentz's precisely because it offers what he calls a common origin inference structure, whereas Lorentz's theory must accept the Lorentz invariance of completely different kind of forces as an unexplained coincidence. While the quantum information-theoretic interpretation takes the kinematic perspective, it does not result in a similar type of common origin inference. Moreover, I think there are compelling reasons to think that there are deeper issues involved with special relativity than simply being able to postulate a structure that can act as a common origin. Its particular foundational strength is indeed unifying in nature, but it stems from the conceptual work it does to clarify the physical terms out of which we construct kinematic frameworks. This ties back to the arguments made in Chapters 3 and 4.

5.2. Bub and Pitowsky's Overall Picture

5.2.1. Layout: Two Dogmas; Two Problems

The structure of the issue for Bub and Pitowsky is that the traditional problems associated with quantum mechanics, on a foundational level, can be recast and made distinct by considering the separate concerns of the big and the small measurement problems. The big measurement problem in the words of the authors is "the problem of explaining how measurements can have definite outcomes, given the unitary dynamics of the theory: it is the problem of explaining *how individual measurement outcomes come about dynamically*" (Bub & Pitowsky, 2007, p. 5). When one thinks of the standard and intractable measurement problem of quantum mechanics, as famously illustrated in Schrödinger's cat problem, this is the problem. In its standard representation, the problem gets started with the linearity of the wave function. The quantum state of the system to be measured and that of the measuring instrument become coupled according to Schrödinger's equation when they interact. A system which is in a superposition of the states to be measured will become entangled, because of the linearity of Schrödinger's equation, with the measuring instrument. Thus we have a state which is not in either of the possible measurement outcome states. Of course this is not what is observed. When the experiment is complete, we observe either one or the other outcome.

The small measurement problem is "the problem of accounting for our familiar experience of a classical or Boolean macroworld, given the non-Boolean character of the underlying quantum event space: it is the problem of explaining the *dynamical emergence of an effectively classical probability space of macroscopic* *measurement outcomes* in a quantum measurement process" (Bub & Pitowsky, 2007, p. 5). This problem is basically that of explaining how the macroworld arises out of the quantum world. How is it that we observe classical objects rather than entangled objects? This is taken to be a genuine problem, but one which is comparatively easy to resolve with a physical solution, namely decoherence. The authors are more concerned with the big measurement problem, though solving the small measurement problem does have a role to play in their overall program. From a theoretical standpoint, if the small measurement problem can be solved in the context of this information-theoretic approach, it can be solved to just the same extent using the same dynamical basis of decoherence in other interpretations. However, just as Oxford Everettians use decoherence as a fundamental constituent in their theory, Bub and Pitowsky utilize decoherence to buttress the analogy between their approach and that taken in special relativity. As we shall see, decoherence, and the resolution of the small measurement problem, is taken to be a proof of the completeness of the information-theoretic approach, and so evidence for its viability.

The big measurement problem is, of course, the perennial foundational issue in quantum mechanics which has persisted now nearly a century. It is also the impetus for the plethora of interpretations of quantum mechanics. Traditionally, the standard interpretation is characterized by the Copenhagen interpretation. It is not a straightforward historical task to describe the components of the Copenhagen interpretation and authors disagree on its fundamental tenets. Nevertheless, we can say that the "collapse" of the wavefunction is an accepted principle. This is the idea that the entangled state collapses stochastically into one of its measurement states upon observation. The wavefunction itself is seen as a device for making probabilistic predictions, but not as representing any ontological state in the world. As we have discussed, other interpretations have since been put forth. Among these are hidden variable solutions such as that offered by Bohm, wavefunction collapse theories such as GRW, and many-worlds interpretations such as Everett. None of the interpretations of quantum mechanics is without its share of philosophical problems and detractors.

The persistence of the measurement problem is due to the acceptance of what Bub and Pitowsky have called the two dogmas of quantum mechanics. That is, if we accept these two dogmas or assumptions, perhaps only implicitly, it is inevitable that we will run up against the big measurement problem. The first dogma is attributed to John Bell (1990) and it is that "measurement should never be introduced as a primitive process in a fundamental mechanical theory like classical or quantum mechanics, but should always be open to complete analysis, in principle, of how the individual outcomes come about dynamically" (Bub & Pitowsky, 2007, p. 5).

I think that this dogma is based on perceived conceptual constraints regarding the concept of measurement itself. Bell's warning in the referenced work is concerned with the use of the very word "measurement" in discussions about foundational issues in quantum mechanics. His concerns are twofold. The first worry with the term "measurement" is that it "anchors [in quantum mechanics] the shifty split of the world into 'system' and 'apparatus'" (Bell J. S., 1990, p. 34). The second worry is that the use of the word "measurement" imports all sorts of meanings from ordinary language which are most likely inappropriate in the quantum context.

In particular, the term "measurement" connotes the idea that the result tells us something about what was there prior to the measurement, about some pre-existing property of the object which the measurement uncovers for us. This is a warning against using measurement as a primitive in quantum theory.

The second dogma has to do with what the quantum state is interpreted as representing. It is that "the quantum state has an ontological significance analogous to the ontological significance of the classical state as the 'truthmaker' for propositions about the occurrence and non-occurrence of events, i.e., that the quantum state is a representation of physical reality" (Bub & Pitowsky, 2007, p. 5). It is not surprising that the quantum state came to be seen this way, arising as it did from classical mechanics, where there is no problem viewing the state as a description of the world, and indeed it is natural to do so. The classical state is a description of the properties of particles, and whether or not this description is true or false is determined by the actual existence of particles with those properties in the world.

The two dogmas are not to be given up lightly. That is, one is not simply being stubborn or naive in adhering to them. On both philosophical grounds and theoretical ones, the two dogmas are not illegitimate concerns. In particular, if one is concerned about problems of realism it might appear that the dogmas are indeed indispensible to quantum mechanics and any physical theory. That is, on the face of it, the two dogmas appear to be essentially realistic principles required to avoid sinking into instrumentalism.

The main thrust of this paper by Bub and Pitowsky is then not simply to point out that there are two such dogmas underlying the measurement problem and then to reject them. One could easily take this step and thereby concede that quantum mechanics is a purely instrumental theory. The primary goal for Bub and Pitowsky must be to show, having recognized these two implicit assumptions, how we can in fact reject the two dogmas of quantum mechanics and still maintain a realist position in regards to the theory of quantum mechanics. In order to do this, the apparent need for realism to adhere to the two dogmas must be shown to be unwarranted, or they must be significantly revised or replaced. The authors also have an interest in showing that their resolution, in rejecting the two dogmas, is superior to a solution to the measurement problem given by an interpretation of quantum mechanics that accepts the two dogmas.

In order to see how Bub and Pitowsky argue that it both possible, and preferable, to reject the two dogmas, we must first see how the two dogmas lead to the measurement problem. The second dogma, that the quantum state has an ontological significance, sets us up for the measurement problem. If we take the quantum state to have such ontological significance, then, in conjunction with the linear dynamics of the quantum mechanics, we must deal with the problem of explaining how the world goes from this state – which, quantum mechanically, can in general be described as an entangled state between the system being measured and the measuring device, and which, according to the second dogma, we take to say something real about the world – to a state with the definite outcomes which we experience as resulting from measurements. In particular, the ontological status of the classical state is that it divides the world into events that do take place and those that do not. In the standard quantum mechanical view, the system being measured may, in general, be in a superposition of states. This system, upon interacting with the measurement device becomes entangled with that device, thereby leading to a state in which the measuring device is in a superposition of the possible outcome states. This is of course something we never experience. In the case of Schrödinger's cat, not only is it something we do not experience, but what it might mean to say that the cat is in a superposition between being a live cat and a dead cat seems inconceivable.

There are various solutions to this big measurement problem, as we have seen and discussed. One option is to remain an instrumentalist about quantum theory, and understand the theory as a purely predictive instrument telling us nothing about the world. This is a direct rejection of the notion that the quantum state represents some ontological aspect of the world, i.e. the second dogma. Motivated by the desire to avoid instrumentalism, many interpretations have therefore accepted the second dogma because of the link between its rejection and instrumentalism. Furthermore, it seems that the particular form which the solutions to the resulting measurement problem have taken has been more or less dictated by a motivation to adhere to the first dogma, that is, to give a dynamical account of measurement. It does not seem that the first dogma leads to the measurement problem as such, but it has constrained the solutions offered to it by necessitating a dynamical explanation for this "collapse" upon measurement. Once the measurement problem is established by accepting the second dogma, if one also maintains the first dogma, then solutions to the measurement problem must be such that they provide, in some fashion, a dynamical explanation.

It may also be that the second dogma must be rejected to solve the small measurement problem. The small problem is how to account for the experience of a classical event space given the more basic quantum structure of the world. The proposed solution to this problem relies on the physical process of decoherence. In this process the interaction of the microsystem being measured, the macro-measuring device, and the environment are all taken into account. Dynamically, what happens is that the portions of the quantum state of this system that interfere with one another very rapidly become very small compared with the diagonal elements of the density operator for the system. This diagonalization essentially creates an emergent Boolean structure of macro-events which remains stable. If this is to work as a solution to the small measurement problem, we cannot treat the quantum state as having ontological significance. Otherwise, we still have a quantum state, where, though *effectively* diagonalized to a preferred basis, this diagonalization remains only effective as trace elements of superposition remain. Nor is an outcome selected, only an emergent Boolean event space. This may simply mean that one must solve the big measurement problem in order to solve the small problem.

Working backwards, the problem for Bub and Pitowsky is to argue that the first dogma – that measurements must open to complete analysis – can be rejected. Indeed, they argue it *must* be. For this, there are two reasons. The first is that the dynamical solutions to the big measurement problem which have been proposed are philosophically unsatisfactory, and much has been said on this subject in the literature. To a large extent, the proposed solutions cover the apparent space of logical possibilities when it comes to resolving the measurement problem dynamically. This might lead one to suspect that no satisfactory solution can be forthcoming and that therefore the first dogma is inappropriate.

More importantly, and centrally for the program of Bub and Pitowsky, the particular basis for resolving the big measurement problem dictates that the first dogma be given up. This then seems to lead back to the big measurement problem; but now, having rejected the first dogma there can be no dynamical solution to it, nor ought there to be one. To escape the big measurement problem, therefore, we must reject the second dogma – that the quantum state must be taken to have some sort of ontological significance. In other words, if any dynamical solution to the big measurement problem is in principle barred, then the only way to "solve" the big measurement problem is to avoid getting into it altogether. Since adherence to the second dogma leads to the measurement problem, the only way to avoid the problem is to also reject the second dogma.

But how to do this without descending into instrumentalism? The purpose behind the Bub and Pitowsky paper is to answer this question. The problem, as touched on earlier, is that the second, as well as the first, dogma seems integral to maintaining a realist interpretation of quantum mechanics. The first dogma seems philosophically and epistemologically an inherent request for a realist scientific theory as a condition for measurability. Measurement is that key component of science which connects our theories of the world to the world itself. Though there is certainly no single accepted definition of what it is to be a realistic scientific theory, in general, it means that our theories in some way reflect an actual physical world and our theories purport to describe that world. Measurement is the means by which science interacts with that world. If Bell is correct, the term “measurement” implies objective reality in the sense that it means uncovering and perhaps quantifying something that was already there in some manner, separate from whatever is doing the measuring. Linked with this idea is the notion that measurement acts as an accurate and objective reflection of this physical reality. Therefore, it must be open to a causal analysis or this connection becomes tenuous. Indeed one definition of the *anti*-realist approach to measurement is conventionalism. A standard definition of conventionalism is that “measurement procedures do not provide evidence of quantities that exist independently of our efforts to measure” (Trout, 2001, p. 271). Later: “The realist account of measurement treats the act of measurement as a product of a causal relation between an instrument (broadly interpreted) and a magnitude” (Trout, 2001, p. 272). A realist interpretation of a theory seems to require that there be a relation of dependence between real physical conditions and measurement outcomes. Thus, given the factors in this assumption, the dependence must be based on a causal relationship between the world and the measuring instrument. If physics, as a realist endeavor, is to describe the world, then on this view, this causal relationship must also be open to analysis.

The second dogma flows from classical mechanics where the classical state is taken to represent facts about the world. In this context, this is almost a definitional statement of what realism is. Physical theories, under realistic criteria, are supposed to tell us something about the world, something beyond the fact that certain regularities hold. Realism just is the idea that some part of the theory genuinely reflects or models something about the way the world is. Einstein says:

> If one asks what is characteristic of the realm of physical ideas independently of the quantum-theory, then above all the following attracts our attention: the concepts of physics refer to a real external world, i.e., ideas are posited of things that claim a "real existence" independent of the perceiving subject (bodies, fields, etc.), and these ideas are, on the one hand, brought into as secure a relationship as possible with sense impressions. (Einstein, 1971, p. 321)

It is the role of the concepts in our theories to, as far as possible, be about things in the world.

These concerns of Einstein's are also related to concerns about giving up the first dogma, that measurement be open to complete dynamical analysis. Einstein continues,

> Moreover, it is characteristic of these physical things that they are conceived of as being arranged in a space-time continuum. Further, it appears to be essential for this arrangement of the things introduced in physics that, at a specific time, these things claim an existence independent of one another, insofar as these things "lie in different parts of space." Without such an assumption of the mutually independent existence (the "being-thus") of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation. (1971, p. 321)

Also,

> [I]f one renounces the assumption that what is present in different parts of space has an independent, real existence, then I do not at all see what physics is supposed to describe. For what is thought to by a 'system' is, after all, just conventional, and I do not see how one is supposed to divide up the world objectively so that one can make statements about the parts. (Einstein 1969, 223-4, trans. by Howard 2004)

For Einstein, we see that his criteria for objectivity are conditions for *reality*. That is, physics is only possible when we have objectivity, and this objectivity must be underwritten by a world where there exists the possibility of separability and locality. Thus, objectivity is a metaphysical condition. In a strong sense, this is put forth like a Kantian necessary *a priori* principle which is necessary for doing physics.

One can categorize the various proposed solutions to the measurement problem according to how they answer the aspects of the problem by addressing the assumptions regarding the linearity of quantum mechanics, that the dynamics of quantum mechanics is linear, or the eigenvalue/eigenstate link, that only eigenstates with probability 1 or 0 are determinate. The various interpretations can also be categorized in terms of how they relate to the two dogmas suggested by Bub and Pitowsky. For the most part, interpretations of quantum mechanics can be divided into two classes: First are those which deny the second dogma, such as the Copenhagen interpretation, which remain consciously instrumental. On the other hand, there are those that accept that the quantum state describes, in some sense, what the world is like analogous to the way the classical state does. This is to accept the second dogma according to Bub and Pitowsky. This is true of the Bohmian and GRW approaches as well as the Everettian approach to quantum mechanics. This then of course leads straight to the big measurement problem. All of these interpretations attempt to resolve the problem in a way which brings to the fore a dynamic solution in some way. For the Bohmians and GRW, this involves adding structure to quantum mechanics, which provides a dynamical explanation for measurement outcomes, thus implicitly accepting the first dogma. Everettians avoid the big measurement problem by claiming that all possible outcomes do in fact occur, thus rejecting the idea that measurements have one particular outcome which needs to be explained. Workable versions of this interpretation rely on the dynamics of decoherence to account for the meaning of probabilities in a quantum universe where all possible outcomes do occur.

Though the argument does not seem to be made explicit in Bub and Pitowsky, it is clear that we are supposed to take these other interpretations as being inadequate. Part of this comes, I think, from the latent sense that their problems have not been overcome as a matter of historical fact, and that they open up equally unclear philosophical questions in addition to the ones they purport to solve. This discussion has been well documented in nearly all of the literature involving the philosophy of quantum mechanics from its inception onwards.

Bub and Pitowsky try to shed light on this discussion by putting it into the context of looking at these solutions as similar to the solutions offered by Lorentz to the problems developing between Newtonian mechanics and electromagnetism. The Lorentzian theory explained or accounted for the discrepancy using the contraction hypothesis. This theory has often been viewed in light of the challenge to it put forth by Einstein's theory of special relativity. For the most part, special relativity is perceived as the correct account of the phenomena while Lorentz's, though empirically adequate, has since been judged as inadequate by contrast. Standard reasons given for this inadequacy appeal to notions of the theory being *ad hoc* or by diagnosing the problem as a failure to recognize that Newtonian spacetime was simply ill-defined without a clear definition of simultaneity.

For Bub and Pitowsky, as we shall see, the essential difference between Einstein and Lorentz is the shift to a kinematic explanation where a dynamical explanation fails on philosophical grounds. Similarly, the authors trace the problems with other interpretations of quantum mechanics to this failure to make the shift to a kinematic framework. So in a manner analogous to the way in which Lorentzian dynamics fails against Einstein's special theory of relativity, other interpretations of quantum mechanics fail in comparison with the information-theoretic interpretation. More will be said on this later.

5.2.2. Treatment

The basis of the Bub/Pitowsky program is the recognition of, and the elevation of, an apparent empirical regularity to the status of fundamental principle. This is the "no cloning" principle carried over from CBH. This approach is done explicitly in the mode of special relativity. Acceptance of the no cloning principle immediately requires the denial of the first dogma. The no cloning principle, or more accurately the "no broadcasting" principle, is essentially a no-go theorem which disallows the existence of a universal cloning machine, and which functions in a manner similar to the way the second law of thermodynamics prohibits the existence of a perpetual motion machine of the second kind or the way the light postulate imposes universal limitations on the velocity of light.

An important consequence of the no cloning theorem is that it implies that there is an inherent loss of information in a measurement process, regardless of the dynamics of that process. It also follows from this that in principle there can be no complete dynamical account of the transition which takes place in a quantum measurement process. (For more on the no cloning principle and what it entails see (Bub & Pitowsky, 2007, pp. 20-22)). The first dogma is the position that measurement should always be open to complete dynamical analysis. The no cloning principle directly contradicts this possibility. Therefore, if the no cloning principle in fact holds for our world, then the first dogma *must* be rejected.

Essentially, if we accept the no cloning principle, and must thereby deny any dynamic account of quantum measurement, then we are left in an irresolvable position if we accept the second dogma – that the quantum state has an ontological significance. This is because, as we saw above, if the second dogma is accepted, then we are forced into the big measurement problem. However, without access to any dynamic explanation to account for the apparent transition from a superposed quantum state to the actual outcomes we observe, as dictated by the no cloning principle, the big measurement problem becomes impossible to solve. The "collapse" can only be viewed as impossible to analyze further. This is the instrumentalist Copenhagen interpretation. Therefore, if the no cloning principle holds, we are forced to reject, in some manner, the second dogma as well as the first if we are to avoid this consequence.

This is what is dictated by the no cloning theorem. It also seems to be the case that the authors argue that it has been the non-recognition of this principle in a clear way which has led to the measurement problem.[15] If we reject the two dogmas, both because they underlie the measurement problem, and because the no cloning principle, if accepted, requires their rejection, then the big measurement problem is merely a pseudo-problem. I take this to mean that an alteration in perspective or in our conceptual framework ameliorates the impact of the problem, resolving it by

[15] I think that it could be argued that it was articulated by Bohr early on. At the time, however, it was far less clear why this should be accepted as a fundamental principle until other avenues had been more thoroughly explored.

avoiding it, rather than attempting to take it on and solve it with a dynamical solution which adequately explains it.

In order to reach this view, we must reject the notion that the quantum state has some sort of ontological significance, in particular, a significance like that generally associated with the classical state, which we take to represent the actual position and momentum of particles in the real world. Bub and Pitowsky therefore take the quantum state to be "a derived entity, a credence function that assigns probabilities to events in alternative Boolean algebras associated with the outcomes of alternative measurement outcomes" (Bub & Pitowsky, 2007, p. 15). In Chapter 2, I argued that an approach looking quite similar to this, that of Fuchs, who also denies the ontological significance of the quantum state, but who instead sees it as a function of our subjective beliefs regarding measurement outcomes, must ultimately be understood to be an instrumentalist theory. The question is: is there anything different going on here? Where is realism going to enter the picture on the information-theoretic interpretation?

Another way to put the question is to ask why we should accept this information-theoretic position over, for example, an Everettian or Bohmian one? We might instead reject the no cloning principle as fundamental, and thereby have room to develop a realist interpretation of quantum mechanics. So why should we accept this principle as fundamental?

The answer to this question is a proposed conceptual shift suggested by following the no cloning principle to its logical conclusions. Doing so not only requires the rejection of the two dogmas discussed above, but it also suggests an alternative solution to the problem of instrumentalism. The theoretical holism of this approach, if it works, lends it strength, as happens in the case of special relativity. Elevating the light postulate to a fundamental principle does a number of things simultaneously. It acknowledges an empirical regularity that seems to hold in all known circumstances; accepting it rejects the unexplained hypothesized dynamical phenomena of Lorentz contraction; and following it to its logical conclusion offers the solution to the problems at hand in the special theory of relativity and the structure of Minkowski spacetime. Specifically, no cloning indicates, like the light postulate does for Einstein, that the correct explanatory posture in the relevant theory is *kinematic* and not *dynamic*. Accepting no cloning requires the shift (by forcing out the two dogmas), but on same grounds offers the resolution by providing the grounds for a realistic interpretation by providing the structure of the proposed kinematic framework. In this sense, it is argued that the information-theoretic approach is analogous to special relativity, and that it is likewise the preferable theory or interpretation as opposed others on offer, specifically those offering a purely dynamic solution.

Bub and Pitowsky draw on an analysis of special relativity by Janssen (2007).[16] Janssen argues that the fundamental shift which special relativity brought to bear on the apparent inconsistencies between Newtonian mechanics and Maxwell's equations was an explanatory shift away from dynamics to a kinematic framework.

The contrast most relevant is with the dynamical explanations offered by Lorentz to explain the inability to experimentally observe any aether shift. Lorentz posits a kind of intermolecular interaction, which is brought on by moving through

[16] See also (Janssen, 2002).

the aether, in just such a way that material bodies would contract to the extent that would exactly compensate for the expected light shift in moving through the aether, thereby accounting for any null result. This explanation was viewed as inadequate by Einstein, who with the principle of relativity and the light postulate accounted for these phenomena with special relativity. The theory of special relativity suggests that the correct way to see physical interactions is as taking place in Minkowski spacetime as opposed to the Euclidean space and time of Newtonian physics. In making this shift, there is no longer the need for any dynamical explanation of phenomena such as length contraction and time dilation. Instead, these phenomena are simply considered to be consequences of the kinematic structure imposed on all physics by the framework of Minkowski spacetime.

Just as the principle of relativity and the light postulate constrain the geometrical structure of spacetime to Minkowski spacetime, so the no cloning (or no broadcasting) principle and no superluminal signaling principle impose probabilistic constraints on the correlations between events. This can be represented by the projective geometry of Hilbert space structure. The structure of the problems for special relativity and quantum mechanics are then strikingly similar. There appear to be deep problems or inconsistencies and so dynamical solutions are proposed (e.g. Lorentz or Bohm/GRW/Everett). There appear to be empirical regularities, once recognized, from which the structure behind the phenomena can be derived. If these are elevated to fundamental principles, they offer a structure through which to interpret the phenomena. In particular, the no cloning principle makes a claim on the measurement process that must hold regardless of the particulars of the dynamics.

The same can be said for the light postulate. Regardless of the dynamical story which is told in a particular case, certain symmetries must hold due to the structure of special relativity. This suggests that just as special relativity was successful – by making the conceptual shift from problematic hypothetical dynamical solutions to inconsistencies, to a kinematic framework understood as being explanatorily prior and more fundamental – so might quantum mechanics.

By kinematic, Bub and Pitowsky mean pre-dynamic, by which they mean "generic features of … systems, independent of the details of the dynamics" (Bub & Pitowsky, 2007, p. 6). The correct way to see quantum mechanics, information-theoretically, is to take the Hilbert space as the kinematic framework for the physics of an indeterministic universe. That is, the projective geometry of Hilbert space, a non-Boolean event space, imposes "structural probabilistic constraints on correlations between events (associated with the angle between events)" (Bub & Pitowsky, 2007, p. 6). The dynamics of any events in the quantum world are constrained by the kinematic framework in which they take place. This is the same as for events in the framework of special relativity, for which the dynamics of any given event are constrained by the Minkowski spacetime in which it occurs.

The dynamics of quantum mechanics remains the unitary dynamics. The shift in perspective, to a kinematic framework, however, means that the dynamics of any given event "evolves the whole structure of events with probabilistic correlations in Hilbert space" (Bub & Pitowsky, 2007, p. 7). That is, the unitary dynamics of quantum mechanics describes the evolution of set of possible events. This is distinct from the classical structure where the dynamics evolves the state, as truthmaker, from one "actual co-occurrence of events to a subsequent co-occurrence of events," and distinct from taking the quantum state to do the same. This is a shift from seeing the unitary evolution of quantum mechanics as a dynamic description of how events in the world change, to one where the change is in the structure of the possible event space, along with constraints imposed by the Hilbert space nature of that structure. The quantum state, $|\psi >$, does not act like a description of the actual events which take place or do not take place in the world, thereby denying the second dogma. Instead it acts as a credence function for keeping track of objective probabilities of possible events.

A number of things seem to happen when you take this standpoint. First, the general features of quantum mechanics, which seemed to beg for explanation, are built in as part of the kinematic structure. Phenomena such as entanglement and interference are not things to be explained but are aspects of the kinematic constraints of the Hilbert space structure. Again, this is analogous to the situation in special relativity where general phenomena, such as Lorentz contraction, need no explanation from the point of view of special relativity. It simply arises out of the Minkowski spacetime structural constraints. Lorentz contraction is a pre-dynamic aspect of the kinematics of the theory.

Secondly, independent dynamical explanations for particular events become secondary to adherence to constraints imposed by the kinematic framework of Hilbert space. In a thought experiment proposed by Bell (1987), we see that a single event, when seen from different reference frames, can have different dynamical explanations, dependent on the frame. However, the overriding explanation for the occurrence in all frames is a relativistic one dependent on the kinematic structure of Minkowski spacetime. Many became familiar with this kind of "paradox" as novice students of special relativity, in which the description of an event dynamically varies depending on the frame in which it is described. Invariably, the paradox is explained by kinematic constraints imposed by special relativity. In this case, the thought experiment involves three spaceships *A*, *B*, and *C*. *A* is equidistant from *B* and *C*, and *B* and *C* are attached by a thin taut thread. *A* sends a signal to both *B* and *C* to begin accelerating. As they accelerate, the thread undergoes Lorentz contraction from the inertial frame of *A*. Eventually, it will be too short and snap under the tension. From different inertial frames the explanation for the source of the tension, a Lorentz-invariant force, changes. From the inertial frame where the rockets end up at rest, the explanation is that they decelerate at different rates, hence the tension is caused. From the perspective of the inertial frame in which *A*, *B*, and *C* are initially at rest, the moving thread undergoes a Lorentz contraction in the direction of its motion, which increases with the velocity of the spaceships, and the thread eventually breaks because this contraction is resisted by the thread being tied to B and C, which maintain a distance apart greater than the contraction requires. The dynamic particulars no longer play the role of fundamental explanation as they can differ for the same event depending on the frame in which they are described.[17]

In quantum mechanics, this all goes to minimize concerns about giving up the first dogma – that measurement must be able to be analyzed dynamically. As we see in special relativity, dynamics is not primary. Indeed, dynamical description may

---

[17] This is actually not the conclusion which Bell draws. He argues that at least for pedagogical reasons the better explanation would be a dynamical one.

differ depending on the inertial frame for the very same event. So it seems more difficult to argue that the dynamical analysis must be fundamental. Likewise, for quantum mechanics, the relevant explanatory stance to take is the kinematic one. This is, therefore, an argument justifying the denial of the first dogma – that measurement must be open to a complete dynamical analysis – or at least providing a justification for why we need not insist upon it. Thus, while the no cloning principle requires giving up the first dogma, it also shows why this might not be problematic, just as insisting on a particular dynamical description in special relativity is not necessary, or even possible, given the framework of special relativity.

The switch to a kinematic framework is also central to giving up the quantum state as some sort of ontological truthmaker. It allows the quantum state to be a derivative structure operating in the theory as a credence function. The quantum state is a cataloging device for the outcome probabilities of events in the Boolean algebras belonging to the non-Boolean Hilbert space corresponding to particular measurements. As such, the quantum state does not correspond to any description of the world and so in turn we are not led into the measurement problem. Giving this up, as we have said, seems like it might lead directly to instrumentalism. The move, however, is to replace the element of realism, which the state provided in classical mechanics with another, alternate type of structure, the Hilbert space kinematics. That is, we are not merely rejecting the quantum state as a real description of the world and saying that quantum mechanics is an instrumental device used to generate this credence function for making predictions. Instead quantum mechanics is about the kinematic structure of the world, just as special relativity is. Later we will consider just how close this analogy is.

But one might immediately wonder on what "realistic" entities such as tigers supervene, as the authors put it. For interpretations that accept the second dogma, that the quantum state has an ontological significance, this question is answered by how that view interprets the quantum state. For Bohm, objects supervene on the underlying particle configuration. For collapse theories such as GRW, macro-objects supervene on the collapsed wavefunctions. For Everettians, macro-objects supervene on elements of the quantum state, all of which exist in separate worlds. For the information-theoretic interpretation, objects are said to supervene on "events defining a 2-valued homomorphism in the emergent Boolean algebra" (Bub & Pitowsky, 2007, p. 18). This interpretation comes out of the dynamics of the information-theoretic interpretation.

What is important for the dynamics of a given theory is that they be consistent with that theory's kinematic structure. This is particularly important here, where the ontological significance of the quantum state has been jettisoned, and the realism of the theory rests on its kinematic structure. It is important to make a distinction between the two levels of the dynamics we might be talking about, the micro and the macro-levels, and then consider the relationship between them, which is an emergent one. At the macrolevel, there are objects such a measuring instruments (or tigers), which behave like and interact with other macro-objects in a classical manner. This is a distinct notion of dynamics from the traditional notion of quantum dynamics, which is unitary quantum evolution. The trick is to show that through the dynamical process of decoherence, which is entirely consistent with the kinematic Hilbert space structure, the classical, or Boolean, event space of classical mechanics emerges as also entirely consistent with the kinematic structure of quantum mechanics. This is meant to be analogous to cases in special relativity, like that of the spaceship example above, where the possibility of a particular dynamic story explaining the Lorentz contraction, which is consistent with the kinematics of Minkowski spacetime, shows that the theory is complete.

This consistency proof is meant to accomplish two things for Bub and Pitowsky. The first is that it solves the small measurement problem. Recall that this problem was how to account for the emergence of a Boolean classical world given the underlying non-Boolean quantum structure. Here we are given a quantum mechanism for explaining the classical probability structure of the macro-world, as well as maintaining the consistency of the appearance of such a macro-world given its quantum nature.

Perhaps more important for the Bub and Pitowsky program are claims that the existence of such a consistency proof underpins the realistic status of the information-theoretic interpretation. On realism, the authors say,

> The possibility of a dynamical analysis of measurement processes consistent with the Hilbert space kinematic constraints justifies the information-theoretic interpretation of quantum mechanics as realist and not merely a predictive instrument for updating probabilities on measurement outcomes. (Bub & Pitowsky, 2007, p. 8)

Although it is not explicitly stated, it cannot be the case that this possibility is offered as a sufficient condition for realism. The authors must mean that it is a necessary condition for any realistic theory. That is, this piece of the puzzle must come together. If it does not, then instrumentalism is not the only concern, so too is completeness. The advantage for the information-theoretic approach is the claim that this consistency is all that is needed, as opposed to this AND a dynamical account of individual measurement outcomes. Once you take the kinematic framework of Hilbert space seriously, there seems to be something on which to hang realism, and then for the sake of completeness you must also show that this perspective is consistent with and accounts for our experience of macro-world classical events and not quantum ones. This can be done with the theoretical apparatus of decoherence. Nevertheless, I think that for Bub and Pitowsky this consistency is given a great deal of weight in legitimizing the program. It seems that it legitimizes the kinematic perspective as a structure that is quantum, but from which one would expect to find classical-like macrostructures given the appropriate dynamical conditions.

To summarize then, Bub and Pitowsky argue that the measurement problem (most significantly the large problem, but also the small problem) can be solved if the, so-called, two dogmas are dropped. At face value, if this is done, it seems that quantum mechanics can only be an instrumentalist theory. However, the crux of the argument is the introduction of the idea that quantum mechanics is correctly viewed as a theory whose most fundamental explanatory structure is a kinematic one. If no cloning is elevated to a principle, the first dogma must be rejected. There can be no dynamical explanation for measurement outcomes. This further suggests a kinematic switch based on constraints imposed by the no cloning principle. This Hilbert space kinematic framework replaces the realist structure which the two dogmas were bolstering. By analogy then, this information-theoretic interpretation of quantum mechanics is no more instrumentalist than Einstein's special relativity. In special relativity, we also have the elevation of empirical regularity to fundamental principle. The light postulate suggests that the correct standpoint is to take a kinematic view of spacetime, that is, the geometry of Minkowski spacetime. Particular dynamical explanations involving specific forces are all secondary to the restrictions imposed by the overall kinematic structure. For both the information-theoretic interpretation of quantum mechanics and special relativity, there are important completeness proofs which show that any particular dynamical story will align with the kinematic structure dictated by the theory.

The final question is how does this information-theoretic interpretation stand up against other interpretations of quantum mechanics such as Everettian or Bohmian theories? In this particular paper, the authors do not explicitly argue that other proposed solutions to the measurement problem fail, but it is intimated that the information-theoretic interpretation is to be preferred.

5.3. Understanding Special Relativity

By way of an analogy between analogies, this one, like that between CBH and special relativity, merits closer inspection as it presents a very intriguing new perspective on quantum mechanics. As we have seen previously, however, it requires close inspection to determine where an analogy succeeds and where it fails. In this instance, as opposed to CBH, some of the strengths of a constitutive principle theory approach are more explicitly pushed and the analysis tries to pull them out of the interpretation, again in comparison to special relativity and its highly successful life as a foundational theory. But does it work? Is the analogy to special relativity close enough to share in its apparent realism? What role does and ought realism play? And do the analogies on these points hold up?

The central issues involved here are deep and important ones in the philosophy of science. Those are realism and explanation. As we have been doing all along, it is important to pay close attention to what it is we ought to expect from a scientific theory, and specifically a foundational one. The two notions of realism and scientific explanation are highly intertwined, and for the most part it does seem reasonable to argue that we want our scientific theories to be genuinely explanatory and to offer a realistic interpretation of the phenomena in their domain. We want our theories to tell us about the world and not merely be instruments of prediction. "Explanation" in an instrumental theory amounts to being an explanation of how we reached the theoretical results or predictions that we did. What formula was used, what assumptions, etc. For scientific explanation, however, the explanation should go some way to helping us understand how the predicted experimental results came about or how they fit in with other things we understand about the world, not merely how we predicted them. That is, it is about describing something in addition to the theory itself. Questions of scientific explanation are therefore inherently tied to the concept of realism, i.e. the world pushing back. For Bub and Pitowsky, we saw that a switch in explanatory priority, from a dynamical perspective to a kinematic one, is supposed to allow for a realistic interpretation of quantum mechanics.

For Bub and Pitowsky, as for Bub (2004b), there remains a principle theory-based approach to quantum mechanics. Specifically, this is that it is a theory structured on information-theoretic principles. However, diverging from Bub and CBH, it is not solely in virtue of the principle theory characteristics of the information-theoretic interpretation that the interpretational work is being done. For Bub and Pitowsky, the no cloning principle indicates that there ought to be a new emphasis placed on the kinematic perspective, and that the principle helps to facilitate this explanatory shift by adding realistic structure to the theory. This shift to a kinematic framework is an important addition which is in line with the arguments from Chapter 4, that a foundational theory plays the functional role of a framework theory. The nature of a foundational physical theory is that it establishes the conceptual foundation of empirical meaning. This seems to be part of what is at work both in special relativity and in the Bub/Pitowsky approach to quantum mechanics, and their similar focus on the kinematic framework, as opposed to particular dynamical explanations for the phenomena concerned. Without such a kinematic framework, for any foundational physical theory, the notion of dynamical interaction within that theory is ill-defined. The shift to a kinematic framework is tied directly with the shift from a poorly defined constructive theory to a defining principle or framework theory. This is the case in going from Lorentzian dynamics to the principle theory of Einstein's special relativity. So, following lessons learned in Chapter 4, the pertinent question at this stage is whether or not the Bub/Pitowsky program is equally successful in constitutive analysis and resolution.[18]

[18] As an aside, there is, I think, a distinction between being a framework theory in general, and being a theory with a kinematic framework, and also a theory with a robust kinematic framework like special relativity, which is aided by the rigorous geometry of Minkowski spacetime. It seems at least conceptually possible that a framework theory is not kinematic in nature. However, any kinematic theory is by its functional nature a framework theory of some sort. It sets the conditions or framework in which the interaction theories under it can operate. Furthermore, it might be argued that there is something to be added to a kinematic theory with the addition of a rigorous geometric structure such a

As we saw in Chapter 3, for Flores (1999) and for Einstein (1954b), there is a distinction to be drawn between types of theories and also the types of realism they characterize. Constructive or interaction theories are often preferred because they trade on entity realism. Principle or framework theories concern nomological realism. I have also argued that this distinction operates in tight conjunction with a standard distinction made among types of scientific explanation; between the causal-mechanical and unification models respectively. The Bub/Pitowsky position relies on a notion of realism and it is worth disambiguating that notion given the information-theoretic interpretation's kinematic structure, and then asking what type of realism they gain, and asking if all types of realism are on a par when it comes to foundational questions. Finally, is realism ultimately what we are after in a foundational theory?

5.3.2. Janssen's Argument

Much of the analysis of special relativity by Bub and Pitowsky stems from an analysis done by Michel Janssen (2007). Here Janssen is concerned to show that special relativity is preferable to Lorentzian dynamics due specifically to its kinematic stance on explanation. We can discuss this, along with other work by Janssen (2002, 2004), within the context of our previous analysis on principle theories and foundational issues in physical theories.

Minkowski spacetime. This seems to have been the case with special relativity. I do wonder if this mathematical clarity and simplicity does not in itself invest the theory with some sort of realistic feel, in virtue of some sort of clarity or understandability, apart from any actual ontological significance over and above its just being a kinematic framework.

Janssen argues that standard accounts of how Lorentzian dynamics fails and Einstien's special theory of relativity succeeds are wrong. Generally speaking, the standard claim is that Lorentz's theory proposes to resolve the conflicts between Newtonian mechanics and electrodynamics, but that it is *ad hoc* in the manner of its explanation while special relativity is not, especially with respect to the Michelson-Morley experiment's null results. For a more complete discussion see Janssen (2002, pp. 431-441). Einstein's expressed position shows concern that Lorentz's contraction hypothesis is 1) put forth specifically to account for only one experimental result, and 2) that even by the standards of Lorentz's theory, the contraction hypothesis is highly contrived. While Lorentz (1998) admits to being guilty of the first charge, it is not decisive in determining *ad hoc*-ness. If the explanation fits the more general theory and is supported by plausibility arguments based on, for the time, reasonable assumptions about molecular structure, then only accounting for specific phenomena is not detrimental. On the second charge, Lorentz disagrees, and, moreover, it is a charge leveled against Lorentz's original contraction hypothesis and not the generalized contraction hypothesis[19].

Another account of what makes an explanation *ad hoc* is given by Popper. A hypothesis which is inherently not falsifiable is one which is *ad hoc*. The contraction hypothesis was, thereby, accused of being *ad hoc* due to the fact that in principle one

---

[19] The original contraction hypothesis, independently found by FitzGerald and Lorentz, assumes that material bodies moving through the aether at velocity v contract by the factor: $\sqrt{1 - v^2/c^2}$. What Janssen calls the generalized contraction hypothesis is that "a matter configuration producing a certain field configuration in a frame at rest in the ether will, when the system is set n motion, change into the matter configuration producing the corresponding state of the field configuration in the frame moving with the system" (2002, p. 425). This generalized assumption will explain a broad class of phenomena, including the electron's frequency of oscillation and mass depending on their velocity with respect to the aether. The generalized contraction hypothesis amounts to the assumption that all laws are Lorentz invariant.

could not empirically test that there was or was not any such contraction, given that any measuring instruments being used to measure it would undergo a similar contraction, thereby making the contraction unobservable and the contracton hypothesis in principle unfalsifiable. This accusation turned out to be premature. When the generalized contraction hypothesis is considered and properly amended, the theory is testable and can be shown to be epirically equivalent to, and so just as testable as, Einstein's special relativity. Therefore, Lorentz's hypothesis is not *ad hoc* in the sense of being unfalsifiable in principle.

Grünbaum suggests that the problem with Lorentzian mechanics is the hypothsized existence of the aether and Newtonian space-time, which are in principle unobservable. This is in line with Mach's positivism and even Occam's razor. Janssen rejects this type of criticism as a general argument, wary of a wholesale rejection of all unobservables.

Janssen, having rejected these other criticisms of Lorentz's theory, argues that the fundamental characteristic which sets special relativity apart as a superior theory is that it offers a common cause for various phenomena, while for Lorentzian dynamics, the obvious connections between those various phenomena remain unexplained coincidences. Janssen makes a direct comparison with the historic rivalry between Ptolemaic models and the Copernican model of the solar system. Here, too, there are two formalisms which both accurately account for the motion of the sun, planets, and the earth in terms of making empirical predictions and describing the phenomena. What Copernicus offers is a reinterpretaion of the formalism which is supperior on the basis of a common-cause argument. As Janssen notes, the transformation in our physical worldview was not completed with Copernicus, but continues through the work of Galileo, Kepler, and culminates in the work of Newton. While for the Ptolemaic system, the correlations between the various movements of the planets with the sun remain unexplained coincidences, for the Copernican system, since the planets revolve around the sun, these correlations are explained in terms of a single model. The correlation between the apparent motion of the sun with the motion of the planets is due to the motion of the earth around the sun.

Janssen argues that the same situation is the case in the rivalry between Lorentz's theory and Einstein's. The tension between Newtonain mechanics and Maxwell's equations stems from the fact that Maxwell's equations hold only in the frame of reference where the aether is at rest while Newtonian mechanics holds in all inetrtial frames. To solve this problem, Lorentz introduces fictive space-time coordinates, which depend on that frame's velocity relative to the aether, and fictive electric and magnetic fields as functions of the fictive space-time coordinates. In these terms, Lortenz was able to construct Maxwell's equations that hold in any frame, regardless of its motion through the aether, that is, Lorentz invariance. The second order affects of the difference between real fields in frames moving at different velocities through the aether should produce a difference in interference patterns in sufficiently accurate aether-drift experiments. The Michelson-Morley experiment found no such difference. In order to account for the null result of the Michelson-Morely experiment, or any similar experiment, Lorentz hypothesized that the matter configuration of objects must contract as it moves through the aether. This is a theory in which *all* laws governing matter are Lorentz invariant, as are those governing electromagnetic fields. What is lacking, however, is any reason for this invariance in the laws governing matter. Why should such disparate and apparently unconnected types of forces such as those governing fields and those governing matter all be Lorentz invariant? One possible solution, considered at the time by Lorentz and others, was that matter is simply governed by the laws governing electric and magnetic fields. This, however, turned out to be problematic and unfeasible to formulate. Janssen concludes that there remains significant unexplained coincidences in Lorentz's theory.

Once again, we can look at the two different theories of Lorentzian dynamics and special relativiy in terms of their structure. Lorentzian dynamics represents a constructive theory while special relativity is a principle, or framework theory. As I have argued previously, it is not simply in virtue of being a principle theory that special relativity is more foundationally satisfactory. However, being a principle theory allows the possibility of resolving fundamental inconsistencies which arise from previously under-analyzed frameworks. For Janssen, special relativity, in the two principles of the relativity postulate and the light postulate, subsequently formulated in the structure of Minkowski spacetime, offers a common cause with which physics can explain all of the phenomena Lorentz leaves as happy coincidences. It is this recognition of a new spacetime structure, as opposed to Newtonian space and time still used in Lorentz, which alters the framework for doing physics, and which thereby provides a common structure which explains the Lorentz-invariance of *both* electromagnetic and material phenomena.

5.3.3. Critique

In a later presentation, Janssen (2007) is concerned to ward off objections posed by Brown and Pooley (2006) and Brown (2005) that Minkowski spacetime is not the sort of thing which can do any explanatory work. Rather, it is simply taking the facts which are described by Lorentz and asserting them as fundamental principles. As Lorentz himself put it,

> I cannot speak here of the many highly interesting applications which Einstein has made of this principle [of relativity]. His results concerning electromagnetic and optical phenomena... agree in the main with those which we have obtained in the preceding pages, the chief difference being that Einstein simply postulates what we have deduced, with some difficulty and not altogether satisfactorily, from the fundamental equations of the electromagnetic field. (Lorentz H. A., 2003, pp. 229-30)

Brown and Pooley contend, "In our view, the appropriate structure is Minkowski geometry *precisely because* the laws of physics, including those to be appealed to in the dynamical explanation of length contraction, are Lorentz covariant" (2006, p. 10), and "From our perspective, of course, the direction of explanation goes the other way around. It is the Lorentz covariance of the laws that underwrites the fact that the geometry of space-time is Minkowskian" (2006, p. 14). Brown and Pooley have a number of points to make. The first is that whatever type of explanation Minkowski spacetime may offer, it is not the sort of explanation found in a constructive theory. It does not tell us why laws are Lorentz invariant in a constructive manner, i.e. causal-mechanically, nor does it tell us how, for example, length contaction comes about dynamically. Second, as noted above, the order of explanation is wrong in Janssen. Facts about the laws being Lorentz invariant makes

Minkowki spacetime geometry the appropriate structure to use, rather than the other way around. Finally, Minkowski spacetime is, in a sense, a structural device used as shorthand for saying that all laws are Lorentz invariant. As such it is not a *real* structure. It has no ontological or causal role to play as a substance. Therefore, the common cause argument in favor of special relativity is a non-starter.

Janssen (2007) replies this line of objections. Janssen modifies the language slightly from "common cause" to "common origin inference", but the argument is still that the superiority attributed to special relativity over Lorentzian dynamics lies in the fact that it can provide a common origin for all of the Lorentz-invariant laws which must hold given the structure of Minkowki spacetime, a kinematic theory. There is also a shift in emphasis to the kinematic structure not present in his earlier paper (Janssen, 2002), perhaps to add weight to the common origin argument and shifting away from the causal language by focusing on the kinematic/dynamic explanatory distinction.

What Janssen is struggling with is how to coherently argue that the structure of Minkowski spacetime can be a common cause or act as a common origin in any way. It is essential that Janssen be able to establish something along these lines to show why special relativity is more fundamental than Lorentzian dynamics. Brown is correct in arguing that Minkowski spacetime cannot be a common cause explanation for relativistic phenomena. However, this is to miss the relevant features of the theory. This debate confounds two distinct and valid modes of scientific explanation. The language used by Janssen and Brown suggests that it is the causal-mechanical view of scientific explanation which characterizes Minkowski spacetime, and hence special relativity, as explanatory. Causal-mechanical explanation, however is within the purview of constructive theories. This, therefore, requires some kind of entity realism, and Brown correctly argues Minkowsky spacetime is not a substance, and so cannot give rise to common-cause explanations. At the same time, Janssen's analysis is correct; it is the kinematic explanatory structure of special relativity which gives it its foundational strength. The type of explanation involved is precisely that which belongs to principle or framework theories, that is unificationist explanation.

Technically, according to the definition of kinematic as pre-dynamic or "generic features of… systems, independent of the details of the dynamics" (Bub & Pitowsky, 2007, p. 6), the kinematics of a theory operate essentially as constraints on the dynamics in physical theories. This fits well with the functional dimension of the distinction between principle theories and constructive theories. Therefore, any principle theory fits within this definition of being kinematic. We might say that what Janssen is getting at is that special relativity, in being essentially about kinematics by positing constraining principles, via the structure of Minkowski spacetime, succeeds as a framework theory, and, moreover, that is because it provides a single unifying structure under which all of the relevant phenomena can be cast and explained.

What justifies the rejection of Lorentzian dynamics, in a Newtonian spacetime structure, in favor of a Minkowski spacetime structure, or in other words, the shift from a dynamical theory to a kinematic one, is not simply the fact that special relativity is a principle theory. Being a theory of kinematics is not, once again, inherently superior. This claim requires justification. Why should we suppose that the kinematic explanation is superior? For Janssen, it is contained in the idea that Minkowski spacetime provides the source of explanation for various previously unexplained issues, where the dynamical theory fails to forge a link between different types of Lorentz-invariant laws. The problem with this is that the ability to provide a common cause or a common origin explanation does not necessarily stem from kinematics. Indeed a well placed dynamic or constructive hypothesis can give rise to such a common cause explanation. An example of this can be found in the kinetic theory of gases. Here, rather simple constructive hypotheses serve to unite and explain the principles of thermodynamics by way of providing the common cause of interacting gas molecules. This shows that a good common-cause explanatory theory need not be a kinematic theory, so it is not kinematic emphasis, as such, that is important.

One might reply that Minkowskian kinematics is preferable just because it is the *only* single structure which accounts for all of the phenomena in question. That is, rather than providing a causal explanation, Janssen's argument might be understood to be invoking a unificationist model of explanation in the Friedman/Kitcher sense. As we have discussed, this is a viable notion of scientific explanation, and the one at work in principle theories. However, while this may be the case, it does not provide definitive desiderata regarding Janssen's versus Brown's arguments.

As stated above, the difference between asserting that all laws operate according to Minkowskian geometry is only a formal step away from saying that all laws are Lorentz invariant. The essential question is: which is the explanans and which is the explanandum. Because the step is a formal one and not a conceptual one, the argument from a common origin for different laws being Lorentz invariant does not have much force for Brown and Pooley:

> We agree that… according to our preferred dynamical interpretation, the Lorentz covariance of all the fundamental laws of physics is an unexplained brute fact. This, in and of itself, does not count against the interpretations: all explanation must stop somewhere. What is required if the so-called space-time interpretation is to win out over the dynamical interpretation… is that it offers a genuine explanation of Lorentz covariance. This is what we dispute. Talk of Lorentz covariance "reflecting the structure of space-time posited by the theory" and of "tracing the invariance to a common origin" needs to be fleshed out if we are to be given a genuine explanation here… Otherwise we simply have yet another analogue of Moliere's dormative virtue. (Brown & Pooley, 2006, p. 13)

And

> In our view, neither of these papers succeed in clarifying how space-time structure can act as a "common origin" of otherwise unexplained coincidences. One might, for example, go so far as to agree that all particular instances of paradigmatically relativistic kinematic behaviour are traceable to a common origin: the Lorentz covariance of the laws of physics. But Janssen wants us to go further. He wants us to then ask after the common origin of this universal Lorentz covariance. It is his claim that this can be traced to the space-time structure posited by Minkowski that is never clarified. (Brown & Pooley, 2006, p. 14)

Janssen (2007) accepts that Minkowski spacetime does not offer a causal explanation, as it were. However, shifting to a kinematic perspective in order to cover more phenomena under one structure leads to a situation where the choice, with no other deciding factors, comes down to explanatory preferences. On this front, neither Brown and Pooley nor Janssen make a definitive case. As we have seen, different types of scientific explanation may play different, but equally legitimate roles in science. It seems clear from what has been said that Janssen is arguing that special relativity is the more fundamental theory because it can explain by unification, while Brown and Pooley argue that the dynamical theory is more fundamental because it can explain causal-mechanically. It would appear that the two parties are arguing past one another due to the lack of a shared conviction regarding what counts as explanation. Determining which is the cart and which is the horse requires bringing more considerations into the picture.

The protractedness of the debate between Janssen and Brown and others (Brown & Pooley, 2001; Janssen, 2002; Janssen, 2007; Balashov & Janssen, 2003; Brown, 2005; Brown & Pooley, 2006) itself serves as evidence that a new approach is warranted. It is important to note that the relative perseverance of both positions speaks to the fact that both have a handle on an element of truth. At the heart of the issue, though, is that both parties are employing somewhat circular arguments from the perspective of a preferred route to explanation. From the Brown and Pooley perspective, to say that all laws are Lorentz invariant without explanation of that fact is simply to say that all laws operate in a Minkowski spacetime geometry. They are equivalent formulations; one just posits as facts what the other posits as a geometrical structure, but there is still no more explanatory or other philosphical benefit. Describing phenomena from a Minkowski framework adds nothing over and above the Lorentzian view. It gives no common cause explanation for the fact that “all laws are Lorentz invariant”, nor is it any more unifying than accepting that statement as brute fact. And since Lorentzian dynamics does provide a causal explanation for phenomena, it is to be prefered.

From Janssen's perspective, that "all laws are Lorentz invariant" is a fact that requires explanation. There is no dynamical, or causal-mechanical explanation which is any 'deeper', but special relativity, with its Minkowski spacetime structure as a unifying theory does the explaining. What we have here though is a situation where we have two theories, one constructive, and the other principle, which have the same empirical content. Two issues make it difficult to select one over the other as explanatorily prior based on the framing of this debate. The first is that in any situation such as this, there is no principled method for choosing the causal-mechanical explanation over the unificationist explanation of the same phenomena. Both explain, but in different ways. In this particular case, however, the problem is more complex in that in neither approach does the type of explanation pursued entirely succeed as it has been put by Janssen or Brown. While the dynamical theory does have a mechanism, contraction, which explains phenomena, the mechanism itself, and in particular why all things obey it, is left unexplained causally. As far as unification goes, as we have said, the kinematics of Minkowski spacetime, taken on its own, is not more unifying than the claim that "all laws are Lorentz invariant." So although it is a unifying geometrical structure, it is not on its own any more explanatory.

I do want to argue, along with Janssen, that special relativity is explanatorily more fundamental than Lorentzian dynamics. However, the reason behind this has been missed by both Janssen and Brown.

To demonstrate this, let us recall Janssen's comparision of special relativity with the Copernican system employed to make the argument that it is the common-cause aspect of special relativity which renders it superior to Lorentzian dynamics. I would add that what really cemented the place of the Copernican system over the Ptolemaic model was not simply a common cause explanation attributed to the structure of the system with the sun at the center of the solar system. Rather, it came after some time with Newton's introduction of a common cause explanation uniting that structure along with other phenomena. On the one hand, we might view this as the hypothesizing of a true common cause – the force of gravity in the universal law of gravitation. This then would explain the success of a constructive theory finally offering a causal explanation for the various phenomena we now know to be attributable to gravitational forces. However, the story is not so simple due to the mysterious nature of this action at a distance. This lack of causal mechanism means that no satisfactory causal explanation can be offered. What cemented the ascendency of Newtonian mechanics, and thus the Copernican revolution, was the foundational analysis by Newton, who not only provided a single model to explain multiple facts, such as falling bodies, planetary motion, and tidal events, but who recognized that the description of such facts could only be made coherent within a framewok where the notions of force and motion had clear empirical meaning (see Chapter 4 and DiSalle, 2006).

Simplicity in theory building is undoubtedly a virtue. In this, the Copernican system represented in Kepler's laws was a preferable theory to the Ptolemeic one. However, there remains no *reason* why we should think of it as a better model of what the solar system is like. The Ptolemaic system relies on various unexplained coincidences to save the phenomena, but Kepler's system also provides no more explanation for why that model is any more true of the world. The same is true of the debate between Janssen and Brown regarding Lorentz and Einstein. What matters is the conceptual reconciliation introduced by special relativity in defining concepts which had previously been poorly defined as discussed in Chapter 4, not a simpler model per se.

This idea of unification is of central concern. The manner in which this is important, however, is different from Janssen's notion of a common origin explanation. While this can be valuable, foundational theory formation involves not merely the explanatory unification of phenomena, but the resolution of concepts which previously had been in conflict. This constitutive conceptual work is the key aspect of a foundational physical theory. This argument is made in Chapter 4.

What this tells us is that both Janssen and Brown are right in their respective criticisms of the other's standpoint, but both are wrong in assessing what it shows. At best, we have a stalemate and we cannot tell which is the cart or the horse in terms of whether Minkowski spacetime explains Lorentz invariance in all physical laws, or whether the fact that all laws are Lorentz invariant justifies the use of the Minkowski spacetime framework. Janssen is correct, however, along with what I take to be the standard view, that special relativity is a more fundamental theory than Lorentzian dynamics. What differs in Einstein's special theory of relativity is the conceptual analysis of previously vague concepts such as simultaneity. This points the way to the appropriate interpretation. Without the conceptual work which Einstein's principles do, there is no explanatory framework at all, either unificationist or causal-mechanical. Likewise, Minkowski spacetime is the conceptual framework to make sense of physics, the conceptual foundation for doing physics. Therefore, it is primary and it precedes the dynamics on a theoretical level. It would be wrong to say that this provides a common-cause explanation for the Lorentz invariance of physical laws, but it does tell us why the laws of physics must be Lorentz invariant given the conceptual foundations of spacetime physics. They must be in order for empirical claims to have meaning. This provides the possibility of explanation via conceptual resolution as well as a justification for preferring special relativity over Lorentzian dynamics.

Having said this, it follows that, among other things, a foundational theory such as special relativity will have many of the aspects which have been under consideration above. A foundational theory, because of the role it plays, will be a framework theory, and so a principle theory; it will be a kinematic theory in at least the weaker sense of simply being pre-dynamic; and it will often involve unification at the level of the phenomena. This goes a long way to explain why so many of these characteristics have been put forth to account for the success of theories such as special relativity, which has all of these characteristics.

5.4. Return to Quantum Mechanics

5.4.1. On the Issue of Realism

Now we return to the program of Bub and Pitowsky. Up until now we have been discussing issues of explanation. Janssen does not tackle the question of realism. His primary concern is to show why special relativity is a preferable theory to Lorentzian dynamics. His answer is based on the explanatory unification offered by special relativity which is lacking in Lorentz's theory. Upon further analysis, we see that it is not just in virtue of there being a single kinematic geometric structure, i.e. Minkowski spacetime, that we should take special relativity as being more fundamental. Rather, this is best understood as arising from the additional element of constitutive conceptual analysis. The central question for Bub and Pitowsky, however, is whether or not the information-theoretic interpretation of quantum mechanics can genuinely be seen as a realist interpretation of quantum mechanics. The reason that this question is so important is because in other interpretations of quantum mechanics the underlying basis for adhering so insistently to the two dogmas is because giving them up seems to come at the expense of giving up realism. Bub and Pitowsky argue that with the quantum information-theoretic interpretation this is not the case.

So what kind of realism are we dealing with and how do we get it? Generally, I believe, when one thinks of realism it is entity realism which one has in mind. This is to be a realist about particles and waves and causal-mechanical explanations. When Einstein at times seems to favor constructive theories, it is on realist grounds. Again, "[w]hen we say we have succeeded in understanding a group of natural processes, we invariably mean that a constructive theory has been found which covers the processes in question" (Einstein, 1954b, p. 228). On the other hand, a principle theory could certainly be a realist one. That is, realism and principle theories are not incompatible with one another. However, it is not in virtue of being a principle theory that quantum mechanics is realist in this sense. Likewise, a kinematic theory is not realistic in light of being kinematic as far as entity realism is concerned. This seems to be one of the primary objections which Brown asserts against arguments in favor of special relativity over Lorentz. Minkowski spacetime is a theoretical structure, not an entity, so it cannot be considered a common cause for any phenomena. It is not the type of thing which interacts in a causal manner (the case is different for general relativity). Therefore, it cannot contribute to an entity-realist interpretation or *act* as a common cause.

On the same grounds, I do not think that we could argue that Bub and Pitowsky's interpretation gives us any kind of entity realism. So there must be something else. Let us consider another kind of realism – nomological realism. This is the belief that certain physical principles or laws are true of the world. There are two questions which go along with this type of realism: 1) What makes a theory realist in a nomological sense, and 2) Is this kind of realism interesting from an interpretational standpoint?

5.4.2. Principles and Kinematics

The first thing to look at is to examine what work is being done by the idea that the information-theoretic interpretation is a principle theory approach. Another way to pose this question is to ask if this approach is equally valid supposing we do not posit the no cloning theorem? Does the interpretation rely on this principle in the same sense that special relativity needs the relativity and light postulates? Or could we simply begin with the kinematic perspective of Hilbert space and go from there and equally well reach the conclusion as argued in Bub and Pitowsky without any talk of underlying principles? Given that the principles in question are where the notion of information theory comes into this interpretation, answering these questions will also determine how instrumental for this interpretation information theory is. That is, is this interpretation information- theoretic, or is all the work done by the kinematic structure? These questions warrant further investigation.

The no cloning principle, as we have seen, does provide a motivation for taking the kinematic perspective on quantum mechanics if it is true of the world. Together with the no superluminal signaling principle, the no cloning principle restricts the correlations between events. This probabilistic structure can be represented by the projective geometry of Hilbert space structure. That is, we have a kinematic framework, a set of pre-dynamic constraints, well represented mathematically by Hilbert space.

But what is doing the work for this interpretation? Is it the principle, or the kinematic structure it motivates? Previously, I argued that the principle-theory approach is not sufficient. But is it necessary now for this new kinematic approach? This is an important question, and to answer it we must look again at the model of special relativity. Here, it would seem that the light postulate and the relativity postulate *are necessary* for the foundational significance of the theory. The light postulate is so important because it is what does the conceptual work. Without it, the important constitutive element of special relativity is lacking, and then special relativity and Minkowski spacetime is, as argued by Brown, simply the positing of Lorentz-invariance.

So does the information-theoretic interpretation of quantum mechanics have this constitutive character? It comes back to that. In Chapter 4, it was argued that, on the basis of the three information-theoretic principles given by CBH, including the no cloning principle, it is not clearly the case that that interpretation of quantum mechanics has a constitutive character. Perhaps, though, the constitutivity of the information-theoretic principles is best understood only in conjunction with the switch to a kinematic explanatory framework and thereby the program can be said to be successful.

There are, however, several vital disanalogies with special relativity. The first is that in special relativity, we do not need to look beyond the first principles of the theory for further justification. We have the groundwork for a constitutive theory of space and time with the light postulate, because it re-characterizes the very meaning of simultaneity and absolute time, and with the principle of relativity, since this partly defines the applicability of empirical concepts. Historically, Einstein did not even take to Minkowski's geometrical model at first. It is not the ability to characterize special relativity in a kinematic structure such as Minkowski spacetime which makes it interpretationally preferable to Lorentz's theory. It does, however, follow from the conceptual work that was done that some sort of kinematic framework is possible, and that it will codify the conceptual analysis in the new physics. Therefore, if the information-theoretic approach is to be justified, we must find in the scheme as a whole something similar that might be going on. This is, I think, the implicit aim for Bub and Pitowsky.

When we look at the Hilbert space kinematic structure proposed by Bub and Pitowsky, the most distinct and important difference with special relativity is that while the kinematic structure of Minkowski spacetime is indeed the structure doing the primary explanatory work, there is a dynamic story to be told about how an individual outcome comes about. The story differs depending on the frame from which it is told, but there is a story. We are not left with only the kinematic framework as an explanatory structure. This structure gives the translations between reference frames, which allow us to see that forces observed in different reference frames are in fact the same force simply observed from different frames. What the kinematic structure explains is not the individual results per se, but how they can appear to have different explanations from the perspective of different inertial frames, and yet be the same. As noted by Cassirer,

> For [relativity]… true objectivity never lies in empirical determinations, but only in the manner and way, in the function, of determination itself. The space and time measurements in each particular system are relative; but the truth and universality, which can be gained nevertheless by physical knowledge, consist in the fact that all these measurements correspond mutually and are coordinated with each other according to definite rules. (Cassirer, 1953, p. 381)

The kinematic structure of Hilbert space for the information-theoretic interpretation of quantum mechanics, on the other hand, is not playing the same role. In fact, it *blocks* the general possibility of any such transformations like those we see in special relativity. The Hilbert space is a kinematic framework, which is a probabilistic structure of possible events. It has nothing to say about which events actually occur, but rather places restrictions on the possibility of being able to open measurement to full dynamical analysis. This is a limitation interpreted as arising from the kinematic structure of quantum mechanics.

Returning to the question of what makes a theory realist in a nomological sense, there are two cases to consider. The first is that principles or laws are merely posited and thereby conclusions and predictions can be generated. Here we are presented with laws which under rigorous empirical testing appear to hold, and so in that respect we can indeed consider them to be real laws. But this has really very minimal content. The example of Kepler's laws is a case in point. This sort of nomological realism is not particularly interpretationally interesting. The second case is where, as we find in Newton's laws or special relativity, the principles are not only expressions of strong empirical generalizations but are ones which take those generalizations and use them to formulate meaningful concepts without which the physics has no coherent basis. The foundational significance of these principles comes from that constitutive character. They are part of the conceptual apparatus necessary for meaningful physics. In this case, the nomological realism is substantial, but it must also be relativized to the framework under which those laws are supposed to hold, since they are in fact the defining structure imposed on the empirical relations under it.

So what should we now say regarding special relativity and realism? I think there could be a question about why we should take special relativity to be a genuinely realist theory, though I think most would tend to think it is. On the one hand, one might contend that special relativity is an instrumentalist theory – that it simply posits, without explanation, certain laws and so we have a minimalist nomological realism based on its kinematic perspective akin to Kepler's laws. However, special relativity goes beyond this in justifying those principles due to their constitutive nature. Therefore, we have nomological realism with foundational strength. That is, in an almost Kantian sense the principles are not simply taken to be generalizations which hold empirically, they are principles which hold AND which serve to structure the conceptual apparatus through which physics is carried out. This lends them a kind of necessity beyond being realist in a strictly empirical sense.

I think, however, that the tendency not to regard special relativity as an instrumentalist theory rests on two, perhaps not clearly compatible, but nevertheless compelling reasons. The first is that it is always possible to provide a complete dynamical account of an event in any given reference frame, and this allows for the possibility of causal-mechanical explanation. This is not due to special relativity's explanatory focus on a kinematic framework in general; rather it is a contingent fact about the theory that the particular framework allows for it, albeit with restrictions on the causal structure imposed by Minkowski spacetime. Any particular event can be explained dynamically within a given inertial reference frame. The kinematic framework of special relativity then allows us to show how observers in different inertial frames might explain the same event differently and how the explanations relate according to the appropriate transformations.

This leads to the second consideration for deeming special relativity realist. In the context of general relativity, Einstein locates the realism as coming from those things which are invariant from any observational perspective.

> The physically real in the universe of events (in contrast to that which is dependent upon the choice of a reference system) consists in *spatiotemporal coincidences*.* [Footnote *: and in nothing else!] Real are, e.g., the intersections of two different world lines, or the statement that they *do not* intersect. Those statements that refer to the physically real therefore do not founder on any univocal coordinate transformation. If two systems of the $g_{\mu\nu}$ (or in general the variables employed in the description of the world) are so created that one can obtain the second from the first through mere spacetime transformation, then they are completely

> equivalent. For they have all spatiotemporal point coincidences in common, i.e., everything that is observable. (Einstein, Letter to Paul Ehrenfest of 26 December 1915, 1998)

This is a very sparse relative of entity realism. Nevertheless, the realism here comes from the fact that an event takes place and that fact is agreed upon by all observers. E.g. the string between the rockets breaks. On Einstein's view presented here, the kinematic structure of special relativity does not come into play. That structure is the theory of how different observers describe the event differently even though it is the same event.

I do not want to spend too much time determining just why special relativity should be taken as a realistic theory. It is enough to show that 1) the question for authors like Janssen and Brown is not that of realism and 2) if we do look at that question, any standard notion of special relativity's realism does not come from the kinematic structure of the theory as such. Realism is not really under suspicion when it comes to special relativity, nor is the source of that realism the kinematic nature of the theory. This is not to say that kinematic features of a theory cannot make it a realistic theory; it is just that in this case the kinematic structure is superfluous to the question of realism in special relativity. Therefore, analogies focusing on a similar kinematic structure are not entirely useful for questions of realism.

5.4.3. Unification and the Nature of the Problem

Given the above considerations, I do not think that it is viable to consider the information-theoretic interpretation of quantum mechanics as realistic. It is more important to come back to the question of whether or not the principles on which it is based are appropriately constitutive. But a lack of realism does not immediately indicate that the approach has failed. It was not, after all, Janssen's aim to show that special relativity is a better candidate for a realist theory than Lorentzian dynamics. It was to show that special relativity offers a more unifying theory. So can that be considered the case for the Bub and Pitowsky program?

I think that Bub and Pitowsky are on the right track in considering the constitutive elements of quantum mechanics. The dogmas or assumptions represent precisely the questionable concepts at issue. One of the concepts underlying the problems for quantum mechanics is in the second dogma, that the quantum state has some kind of ontological significance. Rejecting the dogmas of quantum mechanics is like rejecting the dogmas of absolute simultaneity or the necessity of Euclidean geometry for Einstein. Perhaps this rejection of the dogmas is possible, but it must be replaced or accounted for with more crystallized physical concepts, as was the case in special relativity and general relativity. More work remains to be done in quantum mechanics. The central difficulty for Bub and Pitowsky is that the concept or dogma under attack in not absolute simultaneity but realism itself. That is a significant undertaking.

The strategy is analogous to that of special relativity. The problematic concepts must be analyzed from a broader framework. In the case of special relativity, the implicit dogma taken from Newtonian mechanics was that of absolute simultaneity. By adopting the light postulate, Einstein showed, in conjunction with the relativity postulate, that this dogma must be replaced within a broader perspective of spacetime. The issue for the Bub/Pitowsky approach is that since the dogmas under scrutiny are essentially ones relating to the concept of realism, finding a broader framework is much more difficult. Indeed, in Einstein's conceptual analysis of simultaneity, the guiding principles were empirical conditions closely associated with the realistic ones expressed in the two dogmas. The notion of simultaneity must be empirically meaningful, and this meant that it must be tied to our ability to determine when two events occur simultaneously. The light postulate is that epistemic principle. It defines the limit, and now not simply the empirical limit, but the theoretical one, on causal structures, on making measurements, and hence defines the very notion of simultaneity. This is closely related to the dogma that all measurements must be open to complete dynamical analysis or else lack clear meaning. When we ask how we know two events are simultaneous, the answer can only be verified with a causal signal. The only universal signal is light. Therefore, the *meaning* of simultaneity is dependent on this principle. The principle of relativity is a realistic principle postulated as a condition for doing physics. That the laws of physics must be the same in any inertial reference frame is a demand that measurement be objective. In quantum mechanics, the nature of the question is much more fundamental.

The question posed by Bub and Pitowsky concerns how giving up the two dogmas of quantum mechanics can result in a realistic interpretation of quantum mechanics. The proposed solution to the question appeals very strongly to the case of special relativity. However, the essential difference is that special relativity is a realist theory, but not in virtue of its principle or kinematic structure. That structure makes it an interpretationally successful theory because of the constitutive and conceptually definitive nature of its principles; but that is not a question of realism.

Bub and Pitowsky do not solve the problem of realism that way. But it points to a very deep issue. The program, like special relativity, requires realism. The concept in need of revision is realism itself. This is much more ambitious than the paper suggests. Following the model of special relativity will not solve the problem in the way the authors planned. That method will not underwrite realism since that is not the strength of special relativity. Realism is a given for special relativity. The real work that relativity does as a principle theory is to define the concept of simultaneity. For Bub and Pitowsky, realism is at the heart of the question. So their interpretation must look at how special relativity is a constitutive theory.

Can we tease the concept of realism from the two dogmas? Is the concept of realism muddled, and unclear and so that is where the problem is coming from? Do we need broader framework to answer these questions and what could that be?

5.5. Conclusions

Ultimately the problems are multiple. Bub and Pitowsky are concerned with a realistic interpretation of quantum mechanics. This they do not provide. One issue is that the model, the kinematic explanatory structure of special relativity, is not that theory's source of realism. That comes from extra features of the theory, dynamical features. There is, however, something which is foundationally important about special relativity. Janssen argues that this is due to its ability to attribute a common origin explanation via its kinematic structure. Even if this were correct, the information-theoretic approach lacks any common origin explanation in quite the same way. It does connect phenomena such as interference and entanglement, but these phenomena were never unexplained coincidences as was the Lorentz invariance of wholly different kinds of forces. They are predicted by standard quantum mechanics. Finally, and most importantly, Janssen's position is not quite right in my view. It is not the common origin explanation which makes special relativity preferable over Lorentzian dynamics. That special relativity has a particular kinematic structure is related to its underlying foundational strength, and so too is its common origin explanation. This underlying strength is the constitutive character of the principles involved. I would not say that this lends it realism, nomological realism, but it does provide a deeper sense of necessity in the Kantian sense of a priori necessity. These principles must be in place, and the ones Einstein developed are not in need of further interpretation because they are the fundamental interpretive principles, establishing a consistent and meaningful conceptual framework.

We can still ask at this point, is the information-theoretic interpretation as presented by Bub and Pitowsky a preferable position to hold over other interpretations of quantum mechanics such as Everett, Bohm, or GRW, and on what grounds? It cannot be preferable on entity realist grounds, since it does not make any claims for entity realism. However, I think that it acts as a convincing promissory note. Even without the deep constitutive work like that done by special relativity, the information-theoretic interpretation relative to some broader framework might be like special relativity relative to general relativity, where the light postulate is understood in a larger framework.

To ask why one might take the information-theoretic interpretation as correct is to ask why we should think the no cloning principle holds. This is analogous to asking why the speed of light is constant. In a sense the question goes outside the scope of the theory. Nevertheless it is a relevant physical and philosophical question. The type of answer given gets to the heart of the difference between special relativity and the information-theoretic interpretation.

It is not often asked of special relativity, why is the speed of light constant from the perspective of special relativity. Nevertheless, there are two levels at which we find answers. First, consider special relativity without the addition of general relativity. In this case, there is a kinematic framework, but it could just as easily be viewed as a mathematical summary of Lorentzian dynamics, as argued by Brown, except for the philosophical considerations which emphasize the necessity of the light postulate in establishing conceptual clarity regarding the measurability of simultaneity. The primacy of Minkowski spacetime as a kinematic framework is thereby established. From the perspective of general relativity, the light postulate is unified within a still larger conceptual framework. At the end of the day, our reasons for accepting this principle, and therefore the switch to a kinematic explanatory framework, hinge on the work being done by that kinematic framework in terms of conceptual clarification. The Bub/Pitowsky approach does not provide the clarification of concepts such as realism, measurement, and causation, but it may pave the way, by analyzing the roles of these concepts and in pursuing a constitutive line of theory building.

To clarify, I am not arguing that there are no other types of acceptable physical theories. There are other types of theories which are common causal, dynamic, kinematic, principle, constructive, and various combinations of these. All can be highly successful theories in their place. However, when it comes to foundational issues, the nature of the task is such that a fundamental theory will very likely be as much about our conceptual framework as about empirical modeling. How do we know when this is necessary? In a sense, this is the hard part. It takes recognizing that the current science is in the midst of a crisis and that the crisis stems from an inadequate conceptual grounding and not simply empirical inadequacies. It seems more than reasonable to think that quantum mechanics is and has been in just such a crisis. The difficulties involved are fundamentally conceptual in nature; hence, the resolution must come from an analysis of our current conceptual structure. In this sense, Bub and Pitowsky are on the right track. What has been achieved is an analysis in the negative sense, i.e. an analysis showing that there is a conceptual issue at stake and pinpointing the implicit assumptions lying at its source, the two dogmas. The Bub and Pitowsky approach is to develop a realist interpretation of quantum mechanics by switching to a kinematic explanatory framework. As this chapter shows, this does not succeed. However, indirectly the outline for a positive analysis has also been established. The resolution must come at the conceptual level with a theory of constitutive principles. But what this resolution is has not been established. The no cloning principle, in establishing a kinematic perspective over a dynamic one does not succeed in establishing a realistic interpretation of quantum mechanics. It is not necessarily the role of constitutive principles to establish such a realistic theory. However, when it comes to quantum mechanics, the analysis of this chapter has shown that the motivation to find such a theory comes from the fact that the conceptual issues relate to the concept of realism itself. The challenge is how to reconcile the notion of realism with what quantum mechanics tells us.

Chapter 6: Where do we go from here?

DiSalle argues that the functional role of space-time theories as foundational theories lies in establishing objectively meaningful physics. Therefore, they require extra-empirical, constitutive principle-based theory construction. Such principle theories are what allow us to define an explanatory structure and therefore the empirical meaning of basic physical concepts such as motion, space, time, and measurement.

Quantum mechanics also seems to fall into this class of theories. It is not (yet, in any case) a space-time theory, but it nonetheless is a theory tied to the very structure of empirical measurement, observation, and causation.

The constitutive role of quantum mechanics has not been as clear as it perhaps should have been. Galileo laid down the inherently constitutive nature of space-time theories 400 years ago, Kant reified it in the annals of philosophy. Quantum mechanics has only existed less than 100 years. Approaches to solving the problems which quantum phenomena have presented have been more or less constructive approaches – that is, approaches which operate within the boundaries established for physics by other framework theories such as Newton's and Einstein's. In a sense, this history is analogous to the mechanical philosophy in its attempt to understand motion, or Lorentzian dynamics within the framework of Newtonian mechanics. These theories were operating in a framework in which all of the relevant concepts had yet to be fully defined. Hence no sufficiently clear resolution was found until the framework itself was the object of analysis.

Perhaps quantum mechanics is in such a state. Quantum mechanics was born out of clashes, the need to reconcile alternate conceptual schemes. Eventually, quantum mechanics settled on the appropriate equations and calculus for making accurate predictions. In a technical sense, quantum mechanics relieved the tension between the new puzzling phenomena and the existing frameworks, but it never really resolved the philosophical puzzles. That is, the theory's development managed to bypass conceptual problems with predictive adequacy, but ultimately did not disentangle the underlying conceptual issues.

There are a variety of pieces of evidence that quantum mechanics warrants this type of conceptual analysis.

The first is simply the abundance of proposed solutions to the problems fundamental to quantum mechanics, where none seems entirely satisfactory, and about which there is no consensus among physicists and philosophers. Given that the solution space for the measurement problem is pretty well outlined within the given framework, the lack of consensus seems to indicate that something fundamental in the conceptual framework itself may be in need of analysis and alteration. If a problem is intractable, yet the space of the potential solutions appears to have been exhausted, it suggests the need to expand the solution space.

The second and third pieces of evidence have to do with where, specifically, the problems of quantum mechanics seem to lie. The standard philosophical concerns with quantum mechanics have to do with the measurement problem and EPR-style correlations. These are essentially problems directly involving the notions of measurement and causality. As such, quantum mechanics is a theory which is fundamentally about our concepts of causation and definitions of empirical measurement. These concepts, like those of space and time, are essential to the basic understanding of the physical world. Moreover, as for theories of space and time, it is necessary that such concepts be well defined. The nature of the concepts involved and the intractability of the problems suggests that quantum mechanics ought to be subject to conceptual analysis, and that the right sort of solution will be found in an appropriate constitutive theory. That means developing a quantum theory as a theory not simply of empirical generalization, but as one which partially defines the concepts necessary for establishing a coherent causal structure and meaningful picture of measurement in the physical world.

Third, quantum mechanics faces extrinsic difficulties regarding its compatibility with relativity theory. This also has been seen to be a particularly intractable problem, though theoretical advances are being made. Once again, the intractability and the nature of the problem suggest that the problem is a deep and fundamental one. Given that relativity theory, as we have now seen in some depth, is a foundational theory regarding the structure of space-time functioning as a framework theory, a resolution of this problem will involve a quantum theory commensurate with a theory of space and time. Therefore, not only is quantum mechanics, as it stands, a theory involving the causal structure of the world, but this lack of unity suggests that quantum mechanics ought to be, at the very least, compatible in its causal structure with the preeminent theory of space-time. As we have already seen, theories of space and time are of necessity framework theories which serve to define the conceptual notions of causation and measurement.

For all of these reasons, a reasonable interpretive strategy would be to understand quantum mechanics as a theory which needs to be constructed in a fashion very much like that which has been demonstrated to be the standard for space-time theories. That is, it needs to be constructed as a framework theory, with due attention and analysis paid to the concepts presupposed in its foundations. The first step in this is to analyze those presuppositions and see how they lead to the problems which arise in quantum mechanics. Taking our cue from the historical precedents, we look to places of apparent contradiction. The analysis of Bub and Pitowsky discussed in Chapter 5 pursues this course.

If we look at the issues involved in quantum mechanics – the standard problems, or puzzles, and the various solutions to them – it seems that there has been an attempt to resolve deep issues in a more or less constructive fashion, following the first dogma, after having run into the measurement problem due to adherence in the second dogma. I have argued that, because of the combination of the explanatory aims of a constructive theory and the peculiar nature of quantum theory, the success of this type of approach is doubtful, at least without significant prior conceptual work. Looking at the perennial problems and the proposed solutions, it is clear that the struggle, at its roots, is one involving the meaning of measurement within the quantum framework, and more deeply, the meaning of causality in this physical system. For Bub and Pitowsky, the two dogmas are at the heart of the measurement problem. As we have seen, the two dogmas are also rooted in realist views of physical theories.

The problem here is in fact deeply analogous to that of Einstein prior to his formulation of relativity theory. Einstein saw that clashes between Newtonian mechanics and electromagnetism were rooted in ill-defined concepts which precluded the possibility of objectively meaningful measurements. So measurement can play a role in the discovery of poorly grounded concepts which have not been analyzed. Ultimately, it is measurement which must be defined for empirical investigation to take place. The possibility of the measurement of motion, of force, or of mass were the subjects of definition for Galileo, Newton, and Einstein. In order to provide definitions which resulted in conceptually clear notions of measurement, these theories constructed principles which provided the conceptual clarity and framework to give meaning to the current notions of physical measurement. Kant thought that these principles arose from our own *a priori* intuition of the world. The positivists thought that, while necessary, these principles were purely conventional. It has been shown by DiSalle that the development of these principles shows that there is certainly an element of definition, and therefore a conventional choice. However, there is also serious conceptual analysis of the preceding framework, which brings to light those concepts which require further definition in order to meaningfully carry out empirical science. This conceptual analysis results in the reconciliation of disparate frameworks. This resolution is a prominent aspect of the conceptual analysis which goes into the principle theories constitutive of foundational physics.

This analysis of theory analysis should now, I think, aid us in approaching the issues surrounding quantum mechanics. That is, we might now know what we are looking for. It indicates that there may be the need for conceptual revision in quantum mechanics, and physics in general, if we are to make sense of the peculiarities of quantum mechanics. There seems to be at least two ways to see what is at issue. One approaches the analysis as internal to quantum mechanics and the other as external. The first follows more directly from the CBH and Bub and Pitowsky program[20]. In this case, quantum mechanics, analyzed in information-theoretic terms sheds light on the nature of measurement and the limitations imposed upon it. This approach takes a somewhat Bohrian view of quantum mechanics. Measurement results can only be understood objectively in a classical sense. That is, Booleanity is necessary for objectivity. Informational constraints imposed by the quantum structure require that measuring instruments must ultimately be black boxes which cannot be analyzed dynamically. To ask why the world cannot be objective "all the way down" is answered by the no cloning principle. However, as I have argued, this is not analogous to special relativity where the analogous question is why there is no absolute simultaneity, which is answered by the light postulate. In the case of special relativity, there is the external justification that such a concept of simultaneity is required to define the notion of inertial frame, and hence motion, space, and time.

The second possible approach adopts this type of external analysis. The challenge lies not in revising the concepts of quantum mechanics to better bring to light the structure that makes it the case that quantum mechanics places limits on

[20] This line of thought stems from personal communication with Jeffery Bub.

objective measurement. Rather, the challenge is to revise the concepts of objective measurement and causation, so that they can encompass quantum mechanics, instead of stipulating that some area of physics is in principle unamenable to such analysis.

It is worth repeating that there are a number of areas in which problems arise regarding quantum mechanics. Some arise from within quantum mechanics which appears to conflict with our traditional understanding of the explanatory role that a physical theory ought to play. This conflict is articulated in the measurement problem and the EPR thought experiment. On a separate level, quantum mechanics stands in conflict with another fundamental physical theory, general relativity.

Perhaps these two concerns regarding quantum mechanics, internal and external, are not separate from one another. The first, from within quantum mechanics, seems to arise from conflict with the highly successful formalism of quantum mechanics with deeply felt intuitions about causation and measurement. That is, preconceptions regarding what the physical world must be like from a causal or realist perspective. Einstein denied that the causal structure offered by quantum mechanics could even allow the possibility of physics. On the other hand, the conflicting theories of quantum mechanics and relativity both have something to say about the causal structure of the world, implicitly and explicitly. Here are frameworks whose apparent incompatibility lies in their causal structure. Both areas of problems, however, come down to issues regarding the same fundamental concepts of causation and the possibility of measurement. This motivates the external approach.

What this leads one conclude is that what is needed is a conceptual revision, much like that carried out in the history of science by Newton and Einstein. The analysis should show where hidden, or overlooked, conflicting conceptual schemes exist. This in turn, might lead to resolutions both between the differences with quantum mechanics and relativity theory, but might also shed light on the ever present difficulties in quantum mechanics. Einstein, in analyzing the principle of simultaneity brings in conceptual structure from electrodynamics (light) and extends it to the notion of simultaneity, space, time and motion in the realm of all physics. Electrodynamics conflicted with the contemporary spacetime theory of the time, Newtonian mechanics. Einstein recognized this conflict as indicating the source of conceptual inadequacy and therefore the area in need of revision. Quantum mechanics is in just the same sort of conflict with the current theory of spacetime, general relativity.

It is not within the scope of this document to propose such a resolution. However, it does seem that the guiding instinct of CBH and Bub and Pitowsky to create a principle theory approach to quantum mechanics was quite inspired, for that is exactly what is needed. However, the approaches of CBH and Bub and Pitowsky do not have those characteristics which make a principle theory constitutive in the right way. At least not clearly so. They are information-theoretic principles which do allow the derivation of quantum-like theories, but this is not sufficient for an interpretation of quantum mechanics. What appears to be needed is some sort of conceptual analysis which will shed light on those concepts which one suspects are at the root of the problems.

But where could this come from? And can the theoretical framework of information theory come to our aid? As hinted at above, I think that ultimately one must look to the conflict between relativity theory and quantum mechanics. Not only is there a struggle between concepts here, but general relativity adheres much more closely with our intuitive notion of the causal structure of the world (hence, one might add, its much broader acceptance). What we take to be "measurable" is not a concept constant to both theories. We need to find the conceptual basis of the problem and resolve it.

Where might such a solution come from and what might it look like? By way of example, we might consider an information-theoretic principle such as the holographic principle. The holographic principle is often presented as a bound on the information contained within a volume of space such that it is proportional to the area of the surface of that volume in Plank units[21]. This bound appears in black hole thermodynamics, but it has links with general relativity, thermodynamics, and quantum gravity. Some work has been done to show that quantum relations such as the uncertainty principle can be derived from the holographic principle.

This is speculative on several levels. The first is that the standing of the holographic principle itself still remains conjectural in the field of quantum gravity. Second, the steps to derive aspects of quantum mechanics from it are still in their infancy. However, my proposition is that an information-theoretic principle such as the holographic principle could stand to play a large role in the foundational questions of quantum mechanics for all of the reasons argued in the preceding analysis. If the holographic principle does hold, or if there are other principles which are also

[21] See Appendix for a very brief review of the holographic principle.

implicated in relativistic structure, quantum structure and information theory, and from which quantum mechanics, perhaps with other constraints, can be derived, at least in the appropriate limiting situations, then it seems that it might offer the possibility of solving some of the conceptual problems.

The primary aim of introducing the holographic principle and what it might entail is to explore a concrete example of the type of principle which we might be looking for, given the parameters which were set out after much analysis of theory construction and the state of quantum mechanics. In the first place, this example holds out the hope that there could in fact be such constitutive principles which help enlighten the issues surrounding quantum mechanics. It also shows that information theory is in fact a likely place to look – if for no other reason than because it offers a framework for providing very general constraints.

But how might the holographic principle act as a constitutive principle, providing a framework to define the concepts of causation and measurement so that physical enquiry in the quantum realm is made meaningful? Following the analysis of DiSalle, we saw a fairly general methodology. There seems to be a fact which holds. It is not explained, but it seems to hold and is connected with fundamental concepts. Elevate it to a principle, no longer in need of explanation, and see what it tells us about concepts in physics. Light seemed to be constant in all reference frames. Einstein employed is as a defining principle for time and simultaneity, thus characterizing inertial frame. There appeared to be an equivalence between inertia and gravitational force. Einstein elevated this to a principle, thus constituting the spacetime manifold. Following this pattern, the holographic principle seems to hold.

Elevate it to a constitutive principle. What does it tell us about the explanatory framework, causation, and measurement?

It is not clear what constitutive framework this might offer. What this kind of external approach may offer beyond the CBH approach is the promise of unification with other conceptually diverse frameworks. What the holographic principle has over this, if it holds and if connections with quantum structure can be derived, is that from an information-theoretic point of view, there seems to be a connection between the geometry of spacetime and the quantum world, which is tied to the limits placed on the amount of information that a region of spacetime can contain and the speed at which it can be processed. In itself, unification is not sufficient to establish a constitutive theory. However, following the model of conceptual analysis discussed above, unification often indicates the place to look when it is unification between two theoretical structures that appear to be incompatible with one another.

If this dissertation has been successful, then what has been shown is that quantum information theory may very well have a role to play in the philosophical debate over quantum mechanics. That is, one of the questions the dissertation is asking is: is quantum information theory the type of thing that can provide any philosophical insight? We have seen that the most obvious ways that this might be the case fail. The growth of the field of quantum information theory does not legitimize the view that the world is made of information. Nor do the advances in quantum information theory justify the conclusion that quantum mechanics is about knowledge. Both of these approaches depend for their motivation on illegitimate uses of the concept of "information". First, information it not the type of thing of which the world can be made up. To think so is to make a category mistake. Second, the use of the word “information” in information theory has given rise to a confusion with the concept of information in its standard use. Such an equivocation is unjustifiable given the nature of the concept of information in the technical sense.

Though these tantalizing approaches are unjustified, quantum information theory may still have a philosophical role to play, only more subtly. Indeed, if this dissertation is successful, then we can say more. The use of quantum information-theoretic principles may be the best strategy for moving forward regarding the philosophical issues of quantum mechanics. A look at foundational theories such as Newtonian mechanics and relativity theory reveals that such theories are necessary as preconditions for the possibility of explanatory structure. We have seen that this structure is not given with *a priori* certainty, but it does generally arise via a particular kind of conceptual analysis, which is motivated by incompatible frameworks available at the time. While not recognized, as has been the case for space-time theories, a strong argument can be made that quantum mechanics – because of the particular nature of its conceptual puzzles, its fundamental content, and its conflict with the current theory of spacetime – should be considered to be in this category of foundational framework theory.

If that case can be made, then the resolution to the conceptual problems in quantum mechanics requires conceptual analysis in such a way that a theory of the appropriate constitutive principles, which resolve the conceptual issues, is defined. As such, the argument motivates a move away from other interpretations of quantum mechanics. Moreover, it explains the lack of consensus in accepting any of them due to their constructive approach, where such an approach is, in this case, not the correct explanatory tool for the job. The argument does not necessitate an information-theoretic approach. If constitutive principles are required, they may be found elsewhere. However, the current and potential conceptual scope of quantum information theory makes it the most promising area in which to look for future directions. Just as developments in non-Euclidean geometries unknowingly paved the way for Einstein's theory of relativity by introducing novel conceptual structures with which to describe the basic concepts of space and time, so might developments in quantum information theory open up new conceptual structures with which to analyze the basic concepts of causation and measurement.

> The whole of science is nothing more that a refinement of everyday thinking. It is for this reason that the critical thinking of the physicist cannot possibly be restricted to the examination of the concepts of his own specific field. He cannot proceed without considering critically a much more difficult problem, the problem of analyzing the nature of everyday thinking. (Einstein, 1954a, p. 290)

Appendix: The holographic principle - briefly

The holographic principle is closely associated with another bound, the Bekenstein bound. This bound was first articulated by Bekenstein (1973). Bekenstein (1981) argues that the entropy of a system is bounded by its mass and size. The arguments for the bound stem from black hole theory and fundamental thermodynamic principles. Following Bekenstein (1973), we begin with the observation shown by Hawking (1971) that the horizon area of a black hole never decreases. Bekenstein notes that this is a property shared in general by entropy.

A black hole is characterized by only three quantities: mass, angular momentum, and charge. Therefore, the collapse of a large system of matter with many degrees of freedom into a black hole appears to violate the second law of thermodynamics. It goes from arbitrarily large entropy to none at all. This is also the case when a system is lost in an existing black hole. It is argued that a reasonable solution to this thermodynamic problem is to connect the entropy loss with the gain in area mentioned above, since the area must always increase and it is well defined. That is, we take the black hole to have an entropy S_{BH} equal to its horizon area *A*, modified by a number the order of unity. This is provided by Hawking (1974):

$$S_{BH} = \frac{A}{4}.$$

Bekenstein concludes:

> Suppose that a body containing some common entropy goes down the black hole. The entropy of the visible universe decreases in the process. It would seem that the second law of thermodynamics is transcendent here in the sense that an exterior observer can never verify by

> direct measurement that the total entropy of the whole universe does not decrease in the process. However, we know that the black-hole area "compensates" for the disappearance of the body by increasing irreversibly. It is thus natural to conjecture that the second law is not really transcended provided that it is expressed in a general form: *The common entropy in the black-hole exterior plus the black-hole entropy never decreases*. This statement means that we must regard black-hole entropy as a genuine contribution to the entropy content of the universe. (Bekenstein, 1973, p. 7)

This is known as the generalized second law.

The analogy drawn between black hole entropy and thermodynamic entropy by Bekenstein is made far more robust by the discovery of Hawking radiation (1974; 1975). That is, if Bekenstein's argument is to be carried though, then we must take seriously the idea that if a black hole has entropy, it must also have a temperature, since it has mass. If this is the case, then a black hole with a temperature must radiate. Hawking confirmed this, thus solidifying the notion of black hole entropy beyond a mere analogy.

Further possible implications of this bound come from recognizing that it is not necessarily the case that the generalized second law must hold. That is, it is not thus far established as a law of nature. It could be violated if systems of fixed mass and size, with arbitrarily large entropies, were dropped into black holes. Black hole horizon area is strictly dependent on mass, so this scenario would violate the generalized second law since the expansion of the black hole horizon area would not compensate for the entropy lost. What this suggests is that we should demand that the generalized second law hold, by stipulation, and this then requires that entropy must be bounded on all matter according to its mass and size. This is Bekenstein's bound (1981):

$$S \leq 2\pi ER,$$

where *E* is the total mass-energy of the system contained in the sphere whose radius is *R*. This holds only for weakly gravitating systems. So the entropy *S* of a system of known energy is constrained by its surface area. Note the central role which the constraint of the second law of thermodynamics plays for this derivation.

The Bekenstein bound is a strong motivating factor for the holographic principle. Bekenstein also makes an explicit claim that thermodynamic entropy and Shannon entropy are equivalent.

> Thermodynamic entropy and Shannon entropy are conceptually equivalent: the number of arrangements that are counted by Boltzmann entropy reflects the amount of Shannon information one would need to implement any particular arrangement. The two entropies have two salient differences, though. First, the thermodynamic entropy used by a chemist or a refrigeration engineer is expressed in units of energy divided by temperature, whereas the Shannon entropy used by a communications engineer is in bits, essentially dimensionless. That difference is merely a matter of convention. (Bekenstein, 2003)

Here entropy is taken to be a measure of uncertainty, or lack of information. This claim requires further analysis which this dissertation cannot address. For discussion on the relationship between thermodynamic entropy and Shannon entropy see Leff and Rex (2003).

The holographic principle, according to Bousso, is that "A region with boundary of area *A* is fully described by no more than *A*/4 degrees of freedom, or about 1 bit of information per Planck area" (2002, p. 14). Bousso (2002, p. 19) also argues for a related, but more general *Covariant Entropy Bound: the entropy of any light-sheet of a surface B will not exceed the area of B:*

$$S[L(B)] \leq \frac{A(B)}{4}.$$

A light sheet is a 2 + 1 dimensional hypersurface generated by nonexpanding light rays orthogonal to *B*. There are other variations on the holographic principle, and none are confirmed, though it is believed that some version of the bound will turn out to be true[22].

In a non-rigorous way, the derivation for the holographic principle begins with black hole physics. If the energy in a finite region of space surpasses a critical density, then that region collapses into a black hole with the entropy we have seen. Moreover, the entropy of a given region of space cannot be larger than the entropy of the largest black hole the size of that area. The maximal entropy for a region is proportional to its surface area, and, surprisingly, not its volume. The bound applies to statistical entropy, a notion of entropy more general than any specific thermodynamic interpretation. It does not make any assumption about the microscopic properties of matter and so places a fundamental limit on the number of degrees of freedom in the world (Bousso, 2002, p. 36).

There are two broad formulations of the holographic principle[23]: 1) The strong holographic principle – states that the information which an outside observer can derive from the surface of a black hole is proportional to the surface area of the event horizon. This allows there to be something behind the horizon, or the "screen", but only that the screen filters the information which the observer can access. 2) The weak holographic principle – states that all of the information entering the event horizon of a black hole is encoded on the surface of the horizon and is proportional to

[22] For a good review of the holographic principle see Bousso (2002).
[23] See Smolin (2001, pp. 169-78).

the surface area. Here, there is nothing behind the "screen", and the universe can be described entirely by the "screens", hence the "holographic" principle.

Apart from what the holographic principle might mean, it is interesting as a fundamental principle, or at least a fact which might lead to one. On the one hand, it is a principle which stems from black hole physics, but it also relates to the number of quantum states which can occupy space.

This is born out in various implications of the holographic principle. First, in a paper by Jacobsen (1995), the Einstein equation is derived from the proportionality of entropy and horizon area together with the fundamental thermodynamic relation $\delta Q = TdS$ which relates heat, entropy, and temperature. If thermodynamic principles are not to be violated, then if energy flows through a horizon, so must entropy, meaning that the size of the horizon must change in proportion to the energy flux across it. This implies a curvature of spacetime, and the deduction of the Einstein equation. So not only is the holographic principle a very interesting principle arising out of spacetime physics, it seems that if it is assumed as a fundamental principle, general relativity can be deduced from it.

Work done by Bousso and others to show that aspects of quantum mechanics also follow from the holographic principle (see Bousso (2004), Per and Segui (2005), Chen (2006)). Bousso begins by assuming the holographic relation

$$S \leq \frac{\Delta A}{4 l_{\mathrm{Pl}}^2}.$$

From this he derives the Bekenstein bound. Following an earlier paper (Bousso, 2003), Bousso shows that a generalized covariant entropy bound implies the

Bekenstein bound. As an intermediate step, we can see that the Bekenstein bound expresses the constraints of the holographic principle on the physics of flat space.

Bousso argues that if we imagine a weakly gravitating mass such as earth, ΔA on the order of *GMR*, where *G* is Newton's gravitational constant, *M* is the mass, and *R* is the radius of the sphere into which it fits,

$$\Delta A \approx GMR.$$

Hence

$$S \lesssim \frac{MR}{l_{\mathrm{Pl}}^2/G}.$$

This bound on entropy is then the Bekenstein bound up to an order of one.

Then Bousso goes on to show that if the position and momentum uncertainties of a particle are too small then the Bekenstein bound would be violated. This limit turns out to be

$$\delta x \delta p \gtrsim l_{\mathrm{Pl}}^2/G.$$

Planck's constant emerges as a derived quantity:

$$\hbar \approx l_{\mathrm{Pl}}^2/G.$$

A slightly different derivation of this fundamental quantum mechanical relation is also offered by Chen (2006).

Per and Segui (2005), following a method similar to Bousso's to derive the time-energy uncertainty relation from the generalized covariant entropy bound.

$$\Delta t E_1 \geq \pi \hbar/2$$

The authors take this to mean that the holographic principle not only poses a limit on information storage, but also a bound on the maximum speed of information processing.

Although much work still needs to be done, there are promising results which suggest that more general quantum structure might be related to the holographic principle.

Bibliography

Albert, D. Z., & Galchen, R. (2009, March). A Quantum Threat to Special Relativity. *Scientific American* .

Balashov, Y., & Janssen, M. (2003). Presentism and relativity. *British Journal for the Philosophy of Science , 54*, 327-46.

Bekenstein, J. D. (1973). Black Holes and Entropy. *Physical Review D , 7* (8), 2333-2346.

Bekenstein, J. D. (1981). A universal upper bound on the entropy to energy ratio for bounded systems. *Physical Review D , 23*, 287–298.

Bekenstein, J. D. (2003). Information in the HOLOGRAPHIC UNIVERSE. *Scientific American , 289*, 58-65.

Bell, J. S. (1964). On the Einstein Podolsky Rosen Paradox. *Physics* , 195-200.

Bell, J. S. (1987). How to teach special relativity. In *Speakable and Unspeakable in Quantum Mechanics* (pp. 67-80). Cambridge: Cambridge University Press.

Bell, J. S. (1990). Against 'measurement'. *Physics World , 8*, 33-40.

Born, M. (Ed.). (1971). *The Born-Einstein Letters; Correspondence Between Albert Einstein and Max and Hedwig Born.* (I. Born, Trans.) New York: Walker and Company.

Bousso, R. (2002). *The holographic principle.* Retrieved from arXiv:hep-th/0203101v2.

Bousso, R. (2003). *Light-sheets and Bekenstein's bound.* Retrieved from arXiv:hep-th/0210295v2.

Bousso, R. (2004). *Flat space physics from holography.* Retrieved from arXiv:hep-th/0402058v2

Boyd, R. N. (1973). Realism, Underdetermination, and a Causal Theory of Evidence. *Nous , 7*, pp. 1-12.

Boyd, R. N. (1984). The Current Status of Scientific Realism. In J. Leplin (Ed.), *Scientific Realism* (pp. 41-82). Berkeley: University of California Press.

Brown, H. (2005). *Physical Relativity. Space-time structure from a dynamical perspective.* Oxford: Oxford University Press.

Brown, H., & Pooley, O. (2001). The origins of the spacetime metric: Bell's Lorentzian pedagogy and its significance in general relativity. In C. Callender, & N. Huggett (Eds.), *Physics Meets Philosophy at the Plank Scale* (pp. 256-72). Cambridge: Cambridge University Press.

Brown, H., & Pooley, O. (2006). Minkowski space-time: a glorious non-entity. In D. Dieks (Ed.), *The Ontology of Spacetime* (pp. 67-89). New York: Elsevier.

Bub, J. (2004a). *Why the quantum?* Retrieved from arXiv:quant-ph/0402149v1.

Bub, J. (2004b). *Quantum mechanics is about quantum information.* Retrieved from arXiv:quant-ph/0408020v2.

Bub, J. (2006). *Quantum information and computation.* Retrieved from arXiv:quant-ph/0512125v2.

Bub, J., & Clifton, R. (1996). A uniqueness theorem for 'no collapse' interpretations of quantum mechanics. *Studies in the History and Philosophy of Modern Physics , 27*, 181-219.

Bub, J., & Pitowsky, I. (2007). Two dogmas about quantum mechanics. *arXiv:0712.4258v2 [quant-ph]* .

Carnap, R. (1921). *Der Raum: Ein Beitrag zur Wissenschaftslehre.* Jena: Dissertation.

Carnap, R. (1937). *The Logical Syntax of Language.* (A. Smeaton, Trans.) London: Kegan Paul.

Cassirer, E. (1953). *Substance and Function and Einstein's Theory of Relativity*. (W. C. Swabey, Trans.) New York: Dover Publications, Inc.

Chen, J.-Z. (2006). *Uncertainty Relation from Holography Principle.* Retrieved from arXiv:hep-th/0412171v7

Clifton, R., Bub, J., & Halvorson, H. (2003). *Characterizing quantum theory in terms of information-theoretic constraints.* Retrieved from arXiv:quant-ph/0211089v2.

Cushing, J. T. (1998). *Philosophical Concepts in Physics.* Cambridge: Cambridge University Press.

DiSalle, R. (2006). *Understanding Space-Time.* Cambridge: Cambridge University Press.

Einstein, A. (1905a). On the electrodynamics of moving bodies. In A. Einstein (Ed.), *The Principle of Relativity* (pp. 35-65). New York: Dover.

Einstein, A. (1905b). Zur Elektrodynamik bewegter Körper. *Annalen der Physik* , 891–921.

Einstein, A. (1924). Review of "Kant und Einstein". *Deutsche Literaturzeitung , 45*, 1685-92.

Einstein, A. (1949a). Autobiographical Notes. In P. A. Schilpp (Ed.), *Albert Einstein: Philosopher-Scientist.* Evanston, IL: Library of Living Philosophers, Inc.

Einstein, A. (1949b). Remarks Concerning the Essays Brought Together in this Cooperative Volume. In P. A. Schilpp (Ed.), *Albert Einstein: Philosopher Scientist* (Vol. 7, pp. 665-88). Evanston, IL: The Library of Living Philosophers.

Einstein, A. (1954a). Physics and Reality. In A. Einstein, *Ideas and Opinions* (pp. 290-323). New York: Crown Publishers, Inc.

Einstein, A. (1954b). What is the Theory of Relativity? In A. Einstein, *Ideas and Opinions* (pp. 227-232). New York: Crown Publishers, Inc.

Einstein, A. (1971). Quantum-Mechanik und Wirklichkeit. In M. Born, A. Einstein, H. Born, & M. Born (Ed.), *The Born-Einstein Letters* (M. Born, Trans., pp. 168-173). Macmillan.

Einstein, A. (1998). Letter to Paul Ehrenfest of 26 December 1915. In R. Schulmann, A. J. Kox, M. Janssen, & J. Illy (Eds.), *The Collected Papers of Albert Einstein, Volume 8: The Berlin Years: Correspondence, 1914-1918* (p. 173). Princeton: Princeton University Press.

Einstein, A., Podolsky, B., & Rosen, N. (1935). Can Quantum-Mechanical Description of Physical Reality be Considered Complete? *Physical Review* , 777-80.

Flores, F. (1999). Einstein's theory of theories and types of theoretical explanation. *International Studies in the Philosophy of Science , 13* (2), 123-134.

Flores, F. (2005). Interpretations of Einstein's Equation E = mc2. *International Studies in the Philosophy of Science , 19* (3), 245-260.

Friedman, M. (1974). Explanation and Scientific Understanding. *Journal of Philosophy* , 5-19.

Friedman, M. (1994). Geometry, Convention, and the Relativized A Priori: Reichenbach, Schlick, and Carnap. In W. Salmon, & G. Wolters (Eds.), *Language, Logic, and the Structure of Scientific Theories. Proceedings of the Carnap-Reichenbach Centennial, University of Konstanz, 21-24 May 1991* (pp. 21-34). Pittsburgh: Pittsburgh University Press.

Friedman, M. (1999). *Reconsidering Logical Positivism.* Cambridge, UK: Cambridge University Press.

Frisch, M. (2005). Mechanisms, principles, and Lorentz's cautious realism. *Studies in History and Philosophy of Modern Physics , 36*, 659-679.

Fuchs, C. A. (2002). *Quantum Mechanics as Quantum Information (and only a little more).* Retrieved from arXiv:quant-ph/0205039v1.

Galilei, G. (1954). *Dialogues Concerning Two New Sciences.* (H. Crew, & A. de Salvio, Trans.) New York: Dover Publications Inc.

Ghirardi, G. C., Rimini, A., & Weber, T. (1986). Unified dynamics for microscopic and macroscopic systems. *Physical Review D , 34*, 470-479.

Halvorson, H. (2004). *Remote preparation of arbitrary ensembles and quantum bit commitment.* Retrieved from arXiv:quant-ph/0310001v2

Hawking, S. W. (1971). Gravitational radiation from colliding black holes. *Physical Review Letters , 26*, 1344-6.

Hawking, S. W. (1974). Black hole explosions. *Nature , 248*, 30-31.

Hawking, S. W. (1975). Particle creation by black holes. *Communications in Mathematical Physics , 43*, 199-220.

Howard, D. (1994). Einstein, Kant, and the Origins of Logical Empiricism. In W. Salmon, & G. Wolters (Eds.), *Language, Logic, and the Structure of Scientific Theories. Proceedings of the Carnap-Reichenbach Centennial, University of Konstanz, 21-24 May 1991* (pp. 45-105). Pittsburgh: University of Pittsburgh Press.

Jacobsen, T. (1995). *Thermodynamics of Spacetime: The Einstein Equation of State.* Retrieved from arXiv:gr-qc/9504004v2.

Janssen, M. (2002). Reconsidering a Scientific Revolution: The Case of Einstein versus Lorentz. *Physics in Perspective , 4*, 421-446.

Janssen, M. (2007). Drawing the line between kinematics and dynamics.

Kant, I. (1998). *Critique of Pure Reason.* (P. Guyer, & A. W. Wood, Trans.) Cambridge, UK: Cambridge University Press.

Kant, I. (2001). *Prolegomena to Any Future Metaphysics.* (P. Carus, & J. W. Ellington, Trans.) Indianapolis, IN: Hackett Publishing Company, Inc.

Kitcher, P. (1989). Explanatory unification and the causal structure of the world. In P. Kitcher, & W. Salmon (Eds.), *Minnesota Studies in the Philosophy of Science, Vol. 13* (pp. 410-503). Minnesota: University of Minnesota Press.

Klein, M. J. (1967). Thermodynamics in Einstein's Thought. *Science , 157*, 509-516.

Kuhn, T. S. (1962). *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

Landauer, R. (1991). Information is Physical. *Physics Today* , 23-29.

Landauer, R. (1996). The physical nature of information. *Physics Letters A , 217*, 188-193.

Leff, H. S., & Rex, A. F. (Eds.). (2003). *Maxwell's Deom 2.* Philadelphia: Institute of Physics Publishing.

Leplin, J. (2000). Realism and Instrumentalism. In W. H. Newton-Smith (Ed.), *A Companion to the Philosophy of Science* (pp. 393-401). Oxford: Blackwell Publishers.

Lloyd, S. (2006). *Programming the Universe: A Quantum Computer Scientist Takes on the Cosmos.* New York: Alfred A. Knopf.

Lorentz, H. A. (1900). Electromagnetische Theorien physikalischer Erscheinungen. In *Collected Papers* (Vol. VIII). The Hague: Marin Nijhoff.

Lorentz. (1998). Draft of Letter to Einstein. In R. Schulmann (Ed.), *The Collected Papers of Albert Einstein* (Vol. 8, pp. 49-55). Princeton: Princeton University Press.

Lorentz, H. A. (2003). *The Theory of Electrons: And its Applications to the Phenomena of Light and Radiant Heat* (2 ed.). New York: Courier Dover Publications.

Maudlin, T. (2008). Non-local correlations in quantum theory: how the trick might be done. In W. L. Craig, & Q. Smith (Eds.), *Einstein, Relativity and Absolute Simultaneity* (pp. 156-179). New York: Routledge.

Michelson, A. A., & Morley, E. W. (1887). On the Relative Motion of the Earth and the Luminiferous Ether. *American Journal of Science , 34* (203), pp. 333-345.

Newton, I. (1999). *The Principia: Mathematical Principles of Natural Philosophy.* (I. B. Cohen, & A. Whitman, Trans.) Berkeley and Los Angeles: University of California Press.

Nielsen, M. A., & Chuang, I. L. (2000). *Quantum Computation and Quantum Information.* New York: Cambridge University Press.

Per, M. A., & Segui, A. (2005). *Holographic Cosmology and Uncertainty Relation.* Retrieved from arXiv:gr-qc/0502025v1.

Petersen, A. (1963). The Philosophy of Niels Bohr. *Bulletin of the Atomic Scientists , 19*, 8-14.

Putnam, H. (1975). *Mathematics, Matter and Method: Philosophical Papers* (Vol. 1). Cambridge: Cambridge University Press.

Quine, W. V. (1951). Two Dogmas of Empiricism. *Philosophical Review , 60*, 20-43.

Reichenbach, H. (1965). *The Theory of Relativity and A Priori Knowledge.* (M. Reichenbach, Trans.) Los Angeles: University of California Press.

Salmon, W. C. (1984). *Scientific Explanation and the Causal Structure of the World.* Princeton: Princeton University Press.

Salmon, W. C. (1989). *Four Decades of Scientific Explanation.* Minneapolis: University of Minnesota Press.

Salmon, W. C. (1993). The Value of Scientific Understanding. *Philosophica , 51* (1), 9-19.

Schlick, M. (1979). Critical or Empiricist Interpretation of Modern Physics? In H. L. Mulder, & B. F. van de Velde-Schlick (Eds.), *Philosophical Papers* (P. Heath, Trans., Vol. I, pp. 322-334). Dordrecht: Reidel.

Shannon, C. E. (1948). A Mathematical Theory of Communication. *The Bell System Technical Journal , 27,* 379–423, 623–656.

Smolin, L. (2001). *Three Roads to Quantum Gravity.* New York: Basic Books.

Timpson, C. G. (2004). *Quantum Information Theory and the Foundations of Quantum Mechanics.* Retrieved from arXiv:quant-ph/0412063v1.

Timpson, C. G. (2005). *The Grammar of Teleportation.* Retrieved from arXiv:quant-ph/0509048v1.

Timpson, C. G. (2006). *Philosophical Aspects of Quantum Information Theory*. Retrieved from arXiv:quant-ph/0611187v1.

Timpson, C. G. (2007). *Quantum Bayesianism: A Study*. Retrieved from http://users.ox.ac.uk/~bras2317/qb_s.pdf.

Trout, J. D. (2001). Measurement. In W. H. Newton-Smith (Ed.), *A Companion to the Philosophy of Science* (pp. 265-276). Malden, MA: Blackwell Publishers.

Tumulka, R. (2006). *A Relativistic Version of the Ghirardi-Rimini-Weber Model.* Retrieved from arXiv:quant-ph/0406094v2

van Fraassen, B. (1989). The Charybdis of Realism: Epistemological Implications of Bell's Inequality. In J. T. Cushing, & E. McMullin (Eds.), *Philosophical Consequences of Quantum Theory: Reflections on Bell's Theorem* (pp. 97-113). Notre Dame, IN: University of Notre Dame Press.

Weaver, W. (1963). Recent Contributions to the Mathematical Theory of Communication. In C. E. Shannon, & W. Weaver, *The Mathematical Theory of Communication* (pp. 1-28). University of Illinois Press.

Wheeler, J. A. (1990). Information, physics, quantum: The search for links. In W. Zurek (Ed.), *Complexity, Entropy and the Physics of Information* (pp. 3-28). Redwood City, CA: Addison-Wesley.

Zeilinger, A. (1999). A Foundational Principle for Quantum Mechanics. *Foundations of Physics , 29*, 631-43.