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Evaluating the Quantum Postulate in the Context of Pursuit

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Abstract

The purpose of this dissertation is to contribute to our understanding of scientific theory pursuit by providing a detailed case study on the development of quantum theory.

In 1900, Max Planck introduced the notion of 'energy elements' into his attempt to account for the observed anomalous blackbody radiation spectrum. Despite the fact that the physical interpretation of this notion was ambiguous, a similar idea was later taken up by several scientists. These investigations eventually led to the formulation of quantum mechanics, one of our most successful physical theories to date. However, the intervening years of study were marked by theoretical uncertainty and inconsistency, during which time scientists had to proceed according to a patchwork collection of principles and heuristics.

I first elaborate on why this case should be considered an instance of piecemeal pursuit by presenting the historical 'quantum conjectures' that were being used in different contexts. These conjectures gave quite varied interpretations of what quantization might refer to. By comparing these conjectures, I identify a general *quantum postulate* that captures the underlying assumption that is common to all the cases. I argue that it is possible to consider a general postulate about quantization even when its proper application is ambiguous in a given context, and that the postulate can be separated from different elements of the framework being used to investigate it.

I show that the quantum postulate can be deemed promising by analysing the support it gains using a Bayesian framework. I first defend the use of such a framework by considering the purported inconsistencies in Planck's introduction of his quantum conjecture and how we should handle these. I then explicate two cases of support for the postulate. First, I show how we can use a particular solution to the Bayesian problem of old evidence to interpret the support the quantum postulate received by accounting for phenomena that had no previous explanation. Finally, I show that the quantum postulate is also supported by a unification argument, where unification is interpreted as informational relevance between the different domains of inquiry.

Keywords: quantum postulate, context of pursuit, quantum theory, scientific theories, history of physics, Bayesian epistemology, inconsistency, old evidence, unification

To my grandma, my biggest fan

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Chapter 1

Introduction

1.1 The Context of pursuit

In the past few decades there has been growing acknowledgement of the idea that philosophers of science can both learn from and contribute to the study of the process of constructing scientific theories. Much of this discussion arose out of Reichenbach's famous distinction between the "context of discovery" and the "context of justification," first formulated in his (1938) article, where he calls the first a matter of the "psychology of scientific discovery" and the latter a matter of the "logic of science" (p. 36). Since then, this distinction has generally been interpreted as a kind of division between the process of coming up with a scientific theory, and the process of testing said theory in order to determine whether one is justified in accepting it. For much of the twentieth century, the dominant view among philosophers of science was that philosophical investigations should be confined to the context of justification. For instance, Karl Popper believed that there was no "logic of scientific discovery" despite the English title of his famous work (1959). According to him, the process of discovery was a topic for empirical psychology rather than logical analysis due to an ineliminable creative element, and that the initial stage of conceiving or inventing a theory was not relevant for the practice of philosophy (1959, p. 7). This separation of discovery and justification arose from a belief that a hypothesis arrived at by pure luck or guesswork may still be tested via methods we have determined to be well-justified and accepted as scientific knowledge, despite its questionable origins. Of course, the process of scientific inquiry does not proceed overwhelmingly by pure luck or guesswork, and it is a rare occurrence for theories to simply present themselves to scientists wholesale, ready for testing and subsequent acceptance or rejection.

In recent years, some have argued that the distinction between discovery and justification cannot be upheld (Arabatzis, 2006). Others have argued that Reichenbach's position was misinterpreted, and that the distinction was never meant to be a temporal one, as a chronological division between initial discovery and subsequent justification, but simply as a directive for separating out the psychological components of theory construction from the justificatory ones (Nickles, 1980b). Furthermore, the view began to emerge that the attempt to find an infallible logical procedure for justification ignores the complicated processes scientists actually undergo, and that a more historically oriented viewpoint can help overcome this imbalance.

Several foci of study emerged from this line of thought. Some authors have turned their attention to the investigation of methods by which scientific theories are constructed or 'discovered.' Although there is a general consensus that there exists no infallible set of rules by which scientists can construct new theories, authors have drawn attention to important processes that are often used in such development (Nickles 1980b, Meheus & Nickles 2009). Such analyses agree on the idea that we should take into account the process of developing and constructing theories. As a result, these studies tend to be focused on episodes of actual scientific development, such as Lindley Darden's work on the discovery of biological mechanisms (Darden, 2009).

In addition to the work on the process of discovery, Larry Laudan (1977) argued for an understanding of scientific growth primarily in terms of progress in solving problems, and introduced the term 'context of pursuit' to draw attention to the fact that before deciding whether to accept or reject a theory, scientists must determine whether or not a fledgling theory is worth pursuing for a variety of reasons, both epistemic and pragmatic, often in cases where the theory is not developed enough to warrant acceptance in the usual sense. Laudan's discussion centres on the question of rationality in science in such contexts, but several authors have picked up on this theme and introduced different ways to evaluate theories in the context of pursuit. For instance, Laurie Anne Whitt argues for a combination of conceptual and empirical dimensions to theory appraisal, and discusses possible indices of theory promise focusing largely on the usefulness of analogies from a more established domain to a newly developing line of inquiry (1992). More recent work by Dunja Šešelja and Christian Straßer (2014) has discussed the question of epistemic justification in the context of pursuit, where they have presented a coherentist approach to evaluating theoretical frameworks. These authors also provide case studies to help support their analyses (Whitt 1990, Šešelja & Weber 2012). Peter Achinstein has approached the topic of pursuit by considering general features of the reasoning employed in this context that tend to differ from a purely justificatory perspective, also using a case study (Achinstein, 1993). What these discussions have in common is their characterization of pursuit as occurring after a theory has already been articulated, at which point a judgment must be made about whether to pursue it or not. This allows for a contrast with the context of discovery, before such a theory exists, and the context of justification, at which time scientists are deciding whether or not to accept such a theory.

The purpose of my dissertation is to contribute to our understanding of pursuit by providing a detailed case study on the development of quantum theory. One might consider this to be an investigation of the context of discovery rather than pursuit, but I claim that one thing that becomes clear in my analysis is that pursuit and discovery are not always easily separable. In what follows, I use the language of 'pursuit' but I do not see my project as being closer in kind to Laudan's conception of science than to those who choose the terminology of 'discovery.' Indeed, my case study is concerned with both of these contexts, but I find that the language of 'pursuit' better captures the process in question than the term 'logic of discovery.' I will also show that despite the fact that I characterize my project as one that is focused on pursuit, there is an essential difference between my goal and that of authors I have discussed above. This difference consists in the fact that authors like Whitt and Šešelja & Straßer are trying to answer the question "*Is* a given theory worth pursuing?" My analysis will instead focus on issues related to the queston, "*How* should we proceed in the context of pursuit?" This means that I will be concerned with how we can identify and evaluate promising features of theories in this context. I will now introduce my case study, and explain why it is particularly appropriate for an investigation of the process of pursuit.

1.2 Quantum theory as case study

In the latter years of the nineteenth century, various components of classical physics — including classical mechanics and Maxwell's electromagnetic theory — seemed to be quite well confirmed by almost all experiments up to that point. However, around that time, a few experimental results began to point to the inability of classical theories to account for all phenomena. In 1900, Max Planck introduced a notion of discretization, or quantization, in his treatment of a blackbody in an attempt to account for the observed radiation spectrum. Despite the fact that the idea of quantization was ambiguous in terms of its specific physical interpretation, a similar idea was later taken up by several scientists, most notably, Einstein and Bohr. The investigations into quantization that took place during this time eventually led to the formulation of quantum mechanics, one of our most successful physical theories to date. However, the intervening years of study were marked by theoretical uncertainty and inconsistency, during which time scientists had to proceed according to a patchwork collection of principles and heuristics. There was no theoretical framework that was even a candidate for acceptance, and so it is difficult to see how we could characterize this as the justification of a theory.

Instead, we can consider this period as one in which scientists were engaged in both discovery and pursuit. On the one hand, we might say that scientists were working to discover the theory of quantum mechanics, where 'discovery' refers to the first articulation of the modern mathematical form of the theory. On the other hand, this period provides an excellent example of scientists working in the context of pursuit. For instance, Planck's work is often referred to as his 'theory of blackbody radiation', and Bohr's work is often referred to as the 'old quantum theory.' Scientists evidently judged each of these individual theories to be worth pursuing. However, one theme that will arise from my analysis is that an understanding of pursuit in one individual context must take into account its relation to the other domains, despite the absence of an overall coherent framework in which all of these investigations could be situated.

There is a significant amount of work addressing the modern formulation and interpretation of quantum mechanics, as well as many treatments of the 'old quantum theory,' beginning with Bohr's atomic model of 1913. However, despite the fact that the developments of 1900 to 1913 are historically well-documented, the philosophical literature on this period is more limited. While John Norton has written on this topic, his focus has been on the justification of certain aspects of the theory, in the traditional sense of justification as making it reasonable to accept the ideas in question, primarily through deductive arguments.[1] Leplin (1980) and Nickles (1980a) recognise the significance of this period, and both refer to it as an example in their suggestions of how to understand the process of pursuit. However, this period has not yet been subject to a detailed historical analysis from the perspective of theory pursuit. I submit that such an analysis yields insights on

[1]See for instance Norton (1987) on Planck's theory of blackbody radiation and Norton (1993) for an analysis of the argument for quantization.

scientific methodology and, in particular, for the process of pursuit in the absence of an overarching theory that is guiding the inquiry.

By restricting my attention to the years roughly up to 1913, I focus on the ways in which quantization was applied in different experimental contexts, and how this advanced the development of quantum theory in a 'piecemeal' manner. My purpose is to provide a reconstruction of the arguments made and experiments performed during this developmental period in order to better understand the process of piecemeal pursuit that I claim was operative at the time.[2] The term 'piecemeal pursuit' is meant to reflect several facts. First, the notion of quantization was being deemed promising in various contexts, but there was no overarching theoretical framework, and so the investigation of quantization had to be pieced together from application in these different domains. Second, each domain was itself something of a piecemeal investigation, since elements of classical theories were being used in conjunction with conjectures and hypotheses that did not clearly fit within classical frameworks.

In what follows, I take it as given that scientists were interested in the notion of quantization because it had the potential to solve unaddressed problems, and that they were trying to construct a theory that could account both for the successes of classical physics, and that included some kind of quantum postulate. This attitude of situating scientists in the midst of pursuit is nicely summed up by Planck in the Preface to the Second Edition of his *Theory of Heat Radiation*:

> [I]t follows from the nature of the case that it will require painstaking experimental and theoretical work for many years to come to make gradual advances in the new field. Any one who, at present, devotes his efforts to the hypothesis of quanta, must, for the time being, be content with the knowledge that the fruits of the labor spent will probably be gathered by a future generation. (Planck, 1913/1914, p. ix)

My analysis provides a more detailed explanation of how and why such a postulate had the potential to succeed in this endeavour.

[2]That said, some of the experimental work I discuss, such as Millikan's work on the photoelectric effect, occurred after 1913. I include it because it is directly related to specific elements of theoretical work that arose before this date, and was relatively disconnected to the theoretical developments in quantum theory that were occurring after this date.

1.3 Formal framework

I will be limiting my analysis to broadly epistemic factors. While I would certainly not deny that science is a social activity, and that a full understanding of whether it is wise to pursue a theory will take social and pragmatic factors into account, I put these considerations aside for the purpose of this particular study. Furthermore, none of what I will say hinges on a particular conception of the end goal of science beyond a commitment to its adequacy in accounting for the phenomena in which we are interested. Part of my goal is to show that many of the same epistemological features we take to be indicative of justification are also applicable when we consider whether a theory is worth pursuing or not, but this does not mean that such features necessarily indicate truth. In parts of my analysis, I will use Bayesian epistemological tools. Specifically, I will be working within a framework very similar to the one Abner Shimony calls 'tempered personalism' (1970). There are several reasons that make this epistemological framework appropriate for my purposes. First, consider John Earman's characterization of theory acceptance.

> [T]here is no natural Bayesian explication of theory acceptance, save in the case where the probability of the theory is 1. Since scientists qua judges of theories are almost never in a position to justify such an acceptance, the Bayesian prediction is that rarely is a theory accepted in the epistemic sense. Similarly, when theory choice is a matter of deciding what theory to devote one's time and energy to, the Bayesian prediction is that in typical situations where members of the community assign different utilities to such devotions, they will make different choices. Thus, from either the epistemic or practical perspective, the Bayesian prediction is for diversity. (Earman, 1992, p. 194)

I will be arguing that this diversity of views is an important part of the kind of theory pursuit I am interested in. In particular, we will see that scientists tried to give different physical explanations of experimental results, and that scientists had different levels of commitment to various claims. This framework is thus suited for an examination of the pursuit of a quantum theory. Shimony's view in particular captures what I consider to be my general purpose for using Bayesian epistemology. That is, I consider probabilistic reasoning one of the best ways to formalize some of the steps of scientific inference, and I believe that it can yield results that are not obvious without the formalization. I thus treat it as a useful tool that provides normative recommendations for a rational agent in understanding the relations between a claim and its support, as well as between different pieces of evidence.

Another important feature of tempered personalism is that "the individual investigation delimits an area in which probabilities are calculated" (Shimony, 1970, p. 99). Thus, in our case, probabilities are assigned in the context of particular investigations of energy quantization. How this is being done will be discussed in more detail, but it fits well with the notion of piecemeal pursuit since I claim that the idea of quantization was being investigated in various disparate domains. Finally, the 'tempered' part of tempered personalism comes from the prescription of open-mindedness towards new, seriously proposed hypotheses; that is, scientists should at least consider such hypotheses when there are good reasons to do so. Although we will need to make judgments that are not prescribed by the Bayesian framework to determine whether a hypothesis is a serious proposal, I take this to be a good general methodological rule. We will thus see that there were uses of the idea of quantization that were applied in cases that were ultimately unsuccessful, but it would be unreasonable to expect that they should have been ruled out at the outset.[3]

Furthermore, I believe that the use of this framework helps to sharpen some methodological points. For one thing, we see that the use of this framework requires that in each particular context, we must make an effort to clarify what information should be included in the background knowledge. This will result in a closer identification of the kinds of assumptions scientists were making when considering various experimental results to be evidence for quantization. This will also lead to an examination of how perceived inconsistencies in these background assumptions were treated. For another thing, in order to evaluate probabilities in any meaningful way, we will need to be able to link the hypothesis with evidence statements, and I argue that this requires the inclusion of the parameter h. This helps to highlight the fact that it was the introduction of Planck's constant that turned experimental results into evidence for a hypothesis of quantization, and not a hypothesis about the specific mechanism that caused the observed behaviour of energy.

As an added benefit, this analysis can contribute to the project of Bayesian epistemology by providing a plausible example of how it can be used in real

[3]There are also more specific elements of tempered personalism that make it an appropriate characterization of the reasoning I am interested in, such as the assignment of degree of belief 0 to the belief that a given theory is literally true, but these will be considered in more detail in later chapters.

scientific contexts. While the tools being developed in Bayesian epistemology are becoming more powerful, the literature linking these concepts to historical case studies has not appeared to keep pace. By applying elements of this case study to Bayesian analyses of specific theoretical features, I am able to evaluate the appropriateness of these analyses as explications of scientific reasoning. Specifically, I evaluate a particular Bayesian solution to the problem of old evidence by deciding whether it is a good explication of the reasoning operative at the time, and consider how the solution needs to be modified to properly capture the case.

1.4 Argument structure

In what follows, I will first elaborate on why this case should be considered an instance of piecemeal pursuit by presenting the historical 'quantum conjectures' that were being used in different contexts to account for anomalous behaviour. These conjectures all made use of the notion of quantization, but gave quite varied interpretations of what this quantization might refer to. By comparing these conjectures, I identify a general *quantum postulate* that captures the underlying assumption that is common to all of them. This shows that it is possible to consider a general postulate about quantization even when the proper way to apply or interpret the postulate is ambiguous in a given context.

In Chapter 3, I argue that the quantum postulate can be separated in an epistemic sense from different elements of the framework being used to investigate it. This is done by examining the way that the particular quantum conjecture of light quanta guided research in the context of experiments on the photoelectric effect. While the conjecture of light quanta was crucial for the investigation of this phenomenon, I argue that the experimental results forced agreement on a general postulate about quantization, but did not provide direct support for the existence of light quanta.

I go on to show that the quantum postulate can be deemed promising by analysing the support it gains using a Bayesian framework. The main task of Chapter 4 is to defend the use of the tempered personalist framework by considering the purported inconsistencies in Planck's introduction of his quantum conjecture and how we should handle these. I also give a brief example of how we can use the framework to explicate support by way of correct prediction in the case of the photoelectric effect. In the next two chapters, I use the Bayesian framework to explicate two special cases of support for the quantum postulate. I first show how we can use a particular soution to the Bayesian problem of old evidence to interpret the support the quantum postulate received by accounting for certain phenomena that had no previous explanation in Chapter 5. In so doing, I evaluate and provide some modifications to the solution in question that I claim arise naturally out of historical considerations. Finally, in Chapter 6, I show that the quantum postulate is also supported by a unification argument, where unification should be interpreted as informational relevance of the various disparate domains of inquiry.

From the above arguments, I extrapolate some general conclusions about the process of piecemeal pursuit, arising from both the relation of the specific domains of applicability to the quantum postulate, and features of the postulate itself. For one thing, this characterization helps to demonstrate how a general postulate can be supported by evidence from a particular domain, even when the postulate itself is not an explicit part of the framework being used in that context. This shows that we can identify specific elements of theories that are promising, even when there is no overall theoretical framework that is guiding the investigation. Furthermore, experimental results can be interpreted in such a way as to develop a new theory, even when experiments are conducted largely in the framework of the old theory.

We are also able to see that the quantum postulate was promising due to its combination of wide applicability, and specificity. The wide applicability of a postulate is important not only because scientists may happen upon an appropriate application, but also because these applications help scientists narrow down to a common core that is supported in each context that should be the focus of further inquiry. However, this wide applicability must be tempered by a form of specificity, which in this case takes the form of the inclusion of Planck's constant h. Not only does this specificity allow for features such as prediction and accounting for evidence to gain support, the inclusion of this constant allows the experimental results to yield information about the postulate itself in the form of measurements of the constant. The importance of the quantum postulate is borne out by features that persisted in the transition from the piecemeal application of quantum conjectures to the theory of quantum mechanics. The fundamental quantum of action h continues to play a crucial role in quantum mechanics, and though our understanding of the concept is different, the basic idea of quantization of action remains.

Overall, this analysis shows that the feature of promise need not attach to entire theories. Instead, we can consider specific elements of emerging theories, and so the pursuit of a theory, or a particular part of a theory, can be deemed promising even if the theory as a whole contains theoretical weaknesses or even inconsistencies. In this case, a 'quantum theory' was not a fully consistent theory, and certainly not a candidate for acceptance. Nevertheless, there were specific components of the ideas found at that time that we can see were worth pursuing for empirical reasons.

As a reconstruction of an argument, I do not claim that my representation always accurately captures the thought processes of the scientific community at the time. However, as an account of pursuit, I consider it important to deal primarily with information that was generally known at the time, lest the reconstruction become an idealized and unachievable norm. Thus, although my project is one of philosophical reconstruction, historical facts about the nature of evidence available at the time play a crucial role. Indeed, in this sense, I take to heart Smith's (2010) comments on the relation between history and philosophy of science: a philosophical account of support for a claim must take into account the kinds of things that were considered evidence for that claim by attending to history. This approach allows me to supplement a formal analysis that makes normative claims about how an agent should reason with historical content.

Chapter 2

Quantum Conjectures in 1900–1916

2.1 Introduction

In the previous chapter, I argued for the importance of better understanding the context of pursuit when pursuit occurs as a piecemeal process, and suggested that a case study on the early development of quantum theory could help us gain insight into this process. In this chapter, I further explain why we should consider this as an instance of piecemeal pursuit. To this end, I present an overview of some of the ways that the idea of quantization was being applied in physics from the years 1900 to 1916. I will distinguish between several distinct ‘quantum conjectures’ that were being used in different domains to try to explain anomalous behaviour, and argue that we can extract a general ‘quantum postulate’ from these conjectures that captures the core common assumption among these uses.

One of the central assumptions in classical physics is the idea that energy is correctly described in continuous terms. However, in the late nineteenth-century, various experiments and observations began to point to the inadequacy of classical theories in accounting for certain phenomena. Max Planck’s introduction of “energy elements” in 1900 as a way to recover the behaviour of blackbody radiation was a groundbreaking step in physics, and was followed by other applications of the idea of quantized amounts of energy by scientists in diverse contexts. However, these were not simply applications of a single, unambiguous hypothesis of energy quantization. Planck’s quantum conjecture had to do with the interaction between electromagnetic radiation and matter. Einstein, in his work on light quanta, characterized electromagnetic radiation itself as being quantized. He also applied a quantum conjecture to the description of the energy of a physical molecule in his work on the specific heats of solids. Bohr used a quantum conjecture to define the stable states of an atom to account for the emission spectra of hydrogen gas. Thus, these tentative conjectures were based on quite different assumptions about the physical reasons for quantization. Indeed, it would be more accurate to characterize Planck, Einstein and Bohr as appealing to various members of a family of quantum conjectures, all regarding the behaviour of energy in particular contexts. Despite these differences, there was clearly an important commonality among these conjectures, as evidenced by the fact that this body of work eventually led to the development of a quantum theory.

There is no shortage of detailed historical treatments of the idea of energy quantization in the period from 1900 to 1916.[1] Such work frequently draws attention to Planck's reluctance in accepting Einstein's proposal of light quanta, as well as Einstein's claim that Planck's ideas were a much more drastic break from classical theories of energy than Planck himself believed.[2] These disagreements were ostensibly about the import of the idea of energy quantization for a possible revision of classical physics, so this issue is closely related to the question of exactly how and where a hypothesis of quantized energy is applicable. Philosophers who are interested in the process of theory pursuit have noted the importance of this period. For instance, Leplin (1980) refers to this period as an example of the process of theory construction and mentions the quantization of energy in various forms. Nickles (1980a) discusses how scientists at this time were able to rationally violate what one might have taken to be general constraints on theory development in seeking to solve particular problems. The historical accounts all present these varied applications as important steps in the pursuit and construction of a quantum theory. While I certainly do not wish to deny that this is true, I believe that the very fact that these differences exist has important implications for the characterization of theory pursuit and, to my knowledge, an analysis that takes this into account has not yet been provided.

Although there is no general consensus about the proper way to characterize the end goal of a physical theory, I will assume for my purposes that a theory must at least be empirically adequate over the domain it purports to cover. Thus, in the

[1] Among others cited, see for instance Barkan (1993), Darrigol (1992), Mehra & Rechenberg (1982).

[2] See Kuhn (1978), Klein (1965).

context of my discussion, I assume that scientists were trying to construct a theory that could both incorporate new ideas about quantization in order to account for observations that were anomalous in the framework of classical theories, while maintaining the successes of those classical theories in the domains where they were highly accurate. My main goal in this chapter will be first, to articulate what was common between the conjectures and second, to argue that if we accept the idea that this particular quantum postulate was being supported by various experimental phenomena, we must recognise the fact that this postulate should be divorced from the specific physical assumptions underlying it in each case, and instead be seen as indicating that a change is required at the level of fundamental kinematical laws of classical theories. I will not be concerned here with exactly how the quantum conjectures were being supported by the various experimental phenomena: the explication of the nature of this support will be the role of later chapters. For now, I take for granted that each discussed phenomenon does indeed provide support for the idea of quantization.

The chapter will proceed as follows. The next section presents in detail the different quantum conjectures of Planck, Einstein, and Bohr that I have already mentioned. In each case, I carry out a brief exposition of the work in question, drawing attention to the differences between the conjectures in terms of the physical assumptions that the conjectures imply. I then argue that, despite these differences, we can articulate a statement that I call "the quantum postulate," abbreviated QP, that picks out a common assumption between its various uses, and that I formulate as follows: "There is a universal, nonzero parameter h with the dimensions of action ($energy \cdot time$) that can be used to impose a quantization condition on quantities that were previously considered to be continuous in such a way that reduces to the specific conjectures in each of the domains." I submit that this captures the core assumption common to all the appeals to quantized energy, and that it is not tied to a particular physical cause of experimental results. Indeed, I show that scientists themselves were not committed to the physical causes of quantized behaviour. Finally, I will explain how the quantum postulate as I have articulated it possesses a combination of the features of wide applicability and specificity. The wide applicability is important not only for pragmatic reasons, but also because this is what allows for the identification of a common core between its various applications. In this case, the fact that the postulate was applicable in increasingly diverse ways indicated that a conceptual change was required in certain fundamental classical descriptions of the world. However, any such description would need to be very specific, in the sense that we would require a precise way of interpreting the observations of relevant phenomena. Here, the specificity comes by way of an inclusion of the parameter h. These features will be important for my analyses of evidence for the postulate in subsequent chapters.

2.2 The quantum conjectures

2.2.1 Blackbody radiation

It was the investigation of blackbody radiation which first gave rise to the consideration of an explicit quantum conjecture by Planck. In this chapter, I will not discuss how Planck arrived at his formula for the emission spectrum of blackbody radiation.[3] I focus instead on Planck's attempt to provide a theoretical interpretation of this radiation equation and his appeal to the idea of quantization. We will see that there are various ways in which we might interpret Planck's quantum conjecture, and it is not clear which version should be attributed to him if it is even indeed the case that he was committed to a single meaning.

The formula for the spectrum of blackbody radiation suggested by Planck and confirmed by experiments at both high and low frequency is given by the expression

$$u = \frac{8\pi h\nu^3}{c^3} \cdot \frac{1}{e^{h\nu/kT} - 1}, \tag{2.1}$$

where u is the energy density as a function of ν, the frequency of radiation, T is the temperature, and h and k are constants introduced by Planck. We now call k Boltzmann's constant, and h Planck's constant. The attempt to provide a theoretical explanation for this experimentally confirmed equation is what gave rise to the idea of quantization. Planck employed a model of a blackbody that arose out of Kirchhoff's Law, which posited that the behaviour of radiation was independent of the specific properties of the system. He thus incorporated Hertzian resonators in this description — small, nonresistive, oscillating electric circuits. Since no theory of electrons existed at the time, Planck used the simplest model possible.[4]

In his 1901 presentation of the derivation of the radiation law, Planck drew on previous work to outline the problem. As he already had a relation between the

[3] This will be discussed in Chapter 4. For helpful treatments, see Klein (1961), Kuhn (1978), Gearhart (2002).

[4] Planck was drawing on his earlier work in thermal radiation in which he modeled a system as composed of these resonators. See Darrigol (1992), p. 35.

energy density u and the mean energy E of a single resonator, as well as relations between E and temperature T by way of the entropy S of a single resonator, a major part of Planck's task was to determine the relation between E and S, the mean energy and entropy of a single resonator in the system. To do this, he considered a large number N of identical resonators, and took the total entropy of the resonators S_N to be $k \log W + const$, in analogy to the entropy found in kinetic gas theory. In order to find a value for W, the probability that the system of resonators possesses energy U_N, Planck assumed that the energy can be considered in portions of yet undetermined size ϵ. Rather than simply taking the limit as $\epsilon \to 0$ as one would expect for continuous amounts of energy, he used this discretized consideration of the energy U_N to calculate the number of ways the energy elements could be divided between the N resonators, thus yielding a value for W. This yielded an expression for S_N in terms of energy E and energy element ϵ, and in turn, an expression for the entropy S of a single resonator. The form of S is restricted by Wien's displacement law, which gives a relation between the radiation density of a frequency range and the temperature of the body. This yielded the result that ϵ must be proportional to ν, and Planck introduces the constant h to express that proportionality as $\epsilon = h\nu$. The last part of the paper is dedicated to calculating numerical values for Boltzmann's constant k and Planck's universal constant h based on experimental work on blackbody radiation.[5]

The foregoing is a simplified version of Planck's presentation of 1901, which also differed from his 1900 presentation. Nevertheless, we can extract much of interest from this and Planck's subsequent work on radiation. The main point will be that it is possible to attribute two different quantum conjectures to Planck, and that it is not clear which of these Planck was genuinely committed to, if he indeed had a single conjecture in mind. Both conjectures will arise from an interpretation of the "energy element" $\epsilon = h\nu$, and what it might mean for energy of frequency ν to be considered in amounts of ϵ.

One way to read Planck's quantization condition is to attribute the discontinuous nature of energy to the behaviour of the resonators modeling the blackbody. In this case, one assumes only that we must describe the energy that a single resonator possesses in integral multiples of $h\nu$. This is how historians such as Klein have read Planck, who says for instance "Planck had quantized only the

[5] Many will be familiar with the fact that some of Planck's assumptions here appeared to be inconsistent. While I will have more to say on this matter in the next chapter when I address the issue of how experiments supported the quantum postulate, it is not crucial for my purposes here.

energy of the material oscillators and not the radiation" (Klein, 1961, p. 477). This is also how Einstein explicates Planck's assumption: "The energy of an elementary resonator can only assume values that are integral multiples of $h\nu$; by emission and absorption, the energy of a resonator changes by jumps of integral multiples of $h\nu$" (Einstein, 1906/1989, p. 195).[6] In 1901, in describing the total energy of the resonators, Planck writes,

> Es kommt nun darauf an, die Wahrscheinlichkeit W dafür zu finden, dass die N Resonatoren insgesamt die Schwingungsenergie U_N besitzen. Hierzu ist es notwendig, U_N nicht als eine stetige, unbeschränkt teilbare, sondern als eine discrete, aus einer ganzen Zahl von endlichen gleichen Teilen zusammengesetzte Grösse aufzufassen. (p. 556)

or,

> It is now a matter of finding the probability W, so that the N resonators together possess the vibrational energy U_N. Here, it is necessary to interpret U_N not as a continuous, infinitely divisible quantity, but as a discrete quantity composed of an integral number of finite equal parts.

Planck later developed this idea further into a conjecture that the oscillators could absorb energy continuously, but could only emit it in quantized amounts. For instance, in the 1913 edition of his *Theory of Heat Radiation*, he writes,

> Whereas the absorption of radiation by an oscillator takes place in a perfectly continuous way, so that the energy of the oscillator increases continuously and at a constant rate, for its emission we have . . . the following law: The oscillator emits in irregular intervals, subject to the laws of chance; it emits, however, only at a moment when its energy of vibration is just equal to an integral multiple n of the elementary quantum $\epsilon = h\nu$, and then it always emits its whole energy of vibration $n\epsilon$. (Planck, 1913/1914, p. 161)

This particular quantum conjecture is thus that an oscillator of frequency ν can only emit energy in integral amounts $h\nu$, despite the fact that we would have previously assumed that there was no discrete restriction on this quantity.

[6] I have changed Einstein's $R\beta/N$ to h for consistency of notation.

An alternative reading of Planck's quantum conjecture is that he was specifically imposing a quantization condition on the phase space of a resonator when calculating its entropy. Let us consider Planck's words when introducing the notion of energy elements in 1900.

> If E is considered to be a continuously divisible quantity, this distribution is possible in infinitely many ways. We consider, however — this is the most essential point of the whole calculation — E to be composed of a very definite number of equal parts and use thereto the constant of nature $h = 6.55 \cdot 10^{-27} erg \cdot sec$. This constant multiplied by the common frequency ν of the resonators gives us the energy element ϵ in erg, and dividing E by ϵ we get the number P of energy elements which must be divided over the N resonators. If the ratio is not an integer, we take for P an integer in the neighbourhood. (Planck, 1900/1967, p. 84)

The inclusion of the caveat, "in case P is not an integer," seems to indicate that he considers the possibility that the energy elements are merely a convenient calculational device, and do not reflect a physical fact. To better understand what this might mean, I return to Planck's calculation of the entropy of a single resonator, which required a determination of the probability W of a state of the system. Although Planck was guided by Boltzmann's earlier statistical treatment of the kinetic theory of gases, his treatment differs from Boltzmann's in an important way. First of all, Boltzmann was clear that his division of phase space into regions was merely a calculational convenience, and he quickly went on to consider the limiting case where the elements go to zero (Kuhn, 1978). For Boltzmann, the size of the partition is not important since, for an ideal gas, changing the size of a phase-space element changes the entropy by an additive constant, but in classical thermodynamics, only entropy differences are physically significant. In Planck's work however, the size of the partition is of crucial importance because it determines the probability W of a thermodynamic state, which in turn determines the absolute value of the entropy. In this context, the absolute value of the entropy is meaningful because it is needed to recover the radiation law. In the second edition of his *Theory of Heat Radiation*, Planck describes the elements in phase space as follows:

> That such a definite finite quantity really exists is a characteristic feature of the theory we are developing, as contrasted with that due to

> *Boltzmann*, and forms the content of the so-called *hypothesis of quanta* (Planck, 1913/1914, p. 125).

Kuhn has argued that this kind of conjecture should not be connected to quantization of energy specifically. It is not clear what physical significance we should attribute to this specific partition of phase space.

These two conjectures show that even the mere introduction of the idea of quantization was ambiguous in terms of its physical interpretation. However, we note that its application was limited to the phenomenon of blackbody radiation, and the interaction between radiation and matter, since this is what Planck was attempting to understand at this time. In the next quantum conjectures, we will see that the idea of quantization was applied beyond this domain.

2.2.2 Light quanta

The first notion of quantized energy in Einstein's work appeared in his 1905 paper on light quanta. He begins this paper by saying that the wave theory of light "has been excellently justified for the representation of purely optical phenomena and it is unlikely ever to be replaced by another theory," thus indicating his commitment to classical methods and concepts in many contexts (Einstein, 1905, p. 91). However, he points out that these equations govern observations of optical phenomena which refer to averages over time rather than instantaneous values, the fact of which raises the possibility of different laws governing light phenomena on a different timescale, such as its creation and conversion. These considerations lead Einstein to suggest that the idea of quantization should be applied to light itself rather than in the interaction of radiation with matter.

Einstein first shows that when assuming the validity of the doctrine of equipartition to describe the velocities of the molecules, using Maxwell's theory to analyse the energy of resonators and gas molecules in a volume surrounded by reflecting walls leads to the prediction that the amount of radiation will continue to grow with the frequency of radiation, a result we now refer to as the ultraviolet catastrophe. Einstein saw the potential in Planck's work for solving this problem, but provided a more rigorous underpinning for the idea of quantization.

He proceeds to the suggestion of light quanta by considering the entropy in a system of radiation. He proves that the entropy of monochromatic radiation of sufficiently small density varies with volume according to the same rules as the entropy of a perfect gas or dilute solution, then continues in an analogy with the kinetic theory of gases by interpreting this entropy as a function of the probability of the state. By comparing the expression for the entropy of the system of monochromatic radiation with the expression for entropy according to Boltzmann's principle, Einstein is able to conclude that "Monochromatic radiation of low density behaves . . . in a thermodynamic sense, as if it consisted of mutually independent energy quanta of magnitude $h\nu$" (1905, p. 102).

There are several features of Einstein's work here that indicate that he is not simply applying Planck's quantization conjecture, but is instead providing his own version of such a conjecture that is merely inspired by Planck's work. First is the fact that he provides a different argument to justify the appeal to such a hypothesis. While Planck was representing a blackbody as a system of vibrating resonators that could absorb and emit energy, Einstein considered a system with n moving points in a given volume, in analogy with gaseous systems. In contrast, Planck's appeal to Boltzmann's work in the kinetic theory of gases seems to stem primarily from its mathematical expediency in leading to the correct radiation formula. Einstein's analogy with kinetic theory is carefully formulated so that although its applicability is limited only to domains of low-density radiation, the argument is much better grounded than Planck's.[7]

Most importantly, Einstein's quantum conjecture is given specifically in terms of light phenomena. He presents the major assumption of this paper as follows:

> According to the assumption considered here, when a light ray starting from a point is propagated, the energy is not continuously distributed over an ever increasing volume, but it consists of a finite number of energy quanta, localised in space, which move without being divided and which can be absorbed or emitted only as a whole. (Einstein, 1905/1967, p. 92)

Therefore, this assumption is applicable to phenomena that Planck never considered, such as Stokes's law, the ionisation of gases, and the photoelectric effect. While we will see evidence below that the degree to which Einstein was genuinely committed to a literal interpretation of physical light quanta is questionable, it is significant that his conjecture refers to radiation *in vacuo* in a way that Planck's clearly does not. Indeed, there is nothing in Planck's discussions of blackbody radiation that indicate that his conjecture of quantized energy was meant to be exported to other domains.

[7]See Norton (2006) for a reconstruction of the reasoning.

> [P]revious electron theories suffer from an essential incompleteness which demands a modification, but how deeply this modification should go into the structure of the theory is a question upon which views are still widely divergent. . . . [Some physicists, including Einstein] even believe that the propagation of electromagnetic waves in a pure vacuum does not occur precisely in accordance with the Maxwellian field equations, but in definite energy quanta $h\nu$. I am of the opinion, on the other hand, that at present it is not necessary to proceed in so revolutionary a manner, and that one may come successfully through by seeking the significance of the energy quantum $h\nu$ solely in the mutual actions with which the resonators influence one another. (Planck, 1909/1915, p. 68)

Thus, the suggestion of light quanta is evidently a more radical use of quantization than Planck's quantization of oscillator energy. Einstein's subsequent work on the specific heat of solids stretched the idea of quantization even further.

2.2.3 Specific heats

At the beginning of the twentieth century, a problem with the theory of specific heats loomed over the scientific community. Lord Kelvin, in a lecture to the Royal Society in 1900 described it as a "cloud which has obscured the brilliance of the molecular theory of heat and light during the last quarter of the nineteenth century," (1901, p. 527). Despite the many successes of the atomic-molecular theory of gases, the application of the Maxwell-Boltzmann doctrine of the equipartition of energy demanded by classical physics yielded mixed results in the prediction of specific heats of various substances. This doctrine posited that a substance in thermal equilibrium had its energy equally divided between all its available degrees of freedom. More specifically, each degree of freedom should contribute $(1/2)kT$ to the energy. However, its use in the prediction of specific heats of many common monatomic and diatomic gases were at variance with observed values. Furthermore, while its application in the Dulong-Petit law successfully predicted the specific heats of certain crystalline structures at high temperatures, the same law yielded values that were significantly higher than the observed values for substances such as diamond at room temperature.[8] In what follows, I will not address the quantum

[8]The Dulong-Petit law provided the molar specific heat capacity of a substance as a constant with units Joules/Kelvin. This constant is now expressed as $3R$, where R is the universal gas constant. It is also worth noting that while this law can be understood in terms of the

treatment of gases, since a fuller treatment of such phenomena was not forthcoming until later years.[9] However, as early as 1907, Einstein was able to use a quantum conjecture quite successfully in accounting for the values that were lower than expected of the specific heats of certain solids and, in particular, that of diamond.

Einstein's 1907 paper was the first attempt at using a quantum conjecture in a context other than that of radiation phenomena. He proposed that the idea that energy should be considered in quantized amounts be applied to the energy of molecules of solids and. In his explication, Einstein did not require that the energy take on exact quantities, but only that the energy take on values infinitesimally close to discrete values (Einstein, 1907/1989, p. 217). Using this conjecture in combination with a canonical distribution to describe the system, he calculated the mean energy of an oscillator with frequency ν,[10]

$$\bar{E} = \frac{(R/N_0)h\nu/k}{e^{h\nu/kT} - 1}. \tag{2.2}$$

By making the simplifying assumption that all the atomic vibrations were independent and of the same frequency ν, Einstein was able to calculate the average energy of one oscillator (1907, p. 186). The energy of one mole of such a solid would be

$$E = 3R\frac{h\nu/k}{e^{h\nu/kT} - 1}, \tag{2.3}$$

and the specific heat can be calculated by differentiating the energy with respect to temperature. Note that as $kT/h\nu$ approaches 0, $h\nu/kT$ grows large, and the expression goes to 0. In the limit as $kT/h\nu$ goes to 1 (and larger), the expression takes the value of $3R$. As Klein explains, "If the specific heat is plotted as a function of temperature, or rather of $(Tk/h\nu)$, one obtains a curve that rises smoothly and monotonically from zero at the origin and approaches the equipartition value, $3R$, asymptotically, when $(Tk/h\nu)$ becomes large" (1965, p. 176). We can see the resulting graph from Einstein's 1907 paper in Figure 2.1.

equipartition theorem, it was first proposed as a phenomenological law to describe the observed specific heat capacities of certain substances.

[9]We now know that these divergences between predictions and observations occur because of quantum effects at relatively low temperatures. Roughly, some degrees of freedom require a certain minimum temperature before they are activated and can store energy, so certain substances at low temperatures have low specific heats.

[10]I have written Einstein's equations replacing the constant β with the equivalent h/k to render my notation consistent.

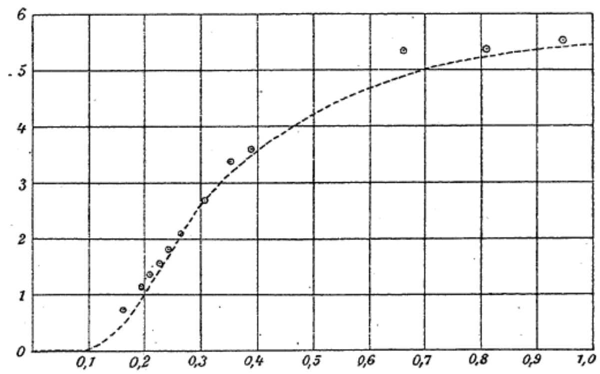

Figure 2.1: Graph of dE/dT from Einstein (1907)

The contradictions between the experimental observations and theoretical predictions based on equipartition occurred primarily among lighter atoms, which could be expected to vibrate at higher frequencies. Einstein's quantized treatment thus fit well with these observations since they would be expected not to conform to the equipartition predictions, but to be much lower. For the first time, there was a plausible explanation for why the specific heats of these substances were so much lower than the Dulong-Petit values.

This version of a quantum conjecture is clearly a much broader interpretation of the notion of energy quantization, despite the fact that Einstein's argument for this energy quantization is based on ideas in Planck's radiation theory. Having begun the paper by showing that the molecular-kinetic theory of heat combined with Planck's relation between the average energy of an oscillator and the radiation density yields the untenable Rayleigh-Jeans law, Einstein concludes that the molecular-kinetic theory must be amended in some way in order to obtain the experimentally verified Equation 2.1. As Einstein phrases it, "this stipulation involved the assumption that the energy of the elementary structure under consideration assumes only values that are infinitesimally close to $0, \epsilon, 2\epsilon$, etc." (1907/1989, p. 217). He explains, "we had to make the assumption that the mechanism of energy transfer is such that the energy of elementary structures can only assume the values $0, h\nu, 2h\nu$, etc." (p. 218). Therefore, the idea of energy quantization here refers specifically to the idea that the phase space of an oscillator of frequency ν is not equally occupied over time; instead, a system occupies only those regions that correspond to energy of integral multiples of $h\nu$. This conjecture was thus not limited to the context of radiation, but any elementary structure that could be modeled as an oscillator. In this context, the energy of an oscillator has to do with its position and momentum, which seems *prima facie* to be unrelated to the constitution of electromagnetic radiation. Thus, Einstein's quantum conjecture here presents another very different application of the idea of quantization.

2.2.4 Atomic theory

The final quantum conjectures I will address come out of Bohr's model of the hydrogen atom.[11] Bohr motivates his 1913 paper by noting that certain experimental results on α-ray scattering seem to support Rutherford's atomic model, but that this model comes up against conceptual and theoretical problems not encountered in alternative atomic models such as Thompson's. One important issue is that Rutherford's model requires the existence of stable states that cannot be determined based on classical electrodynamics. Furthermore, the quantities present in the Rutherford model do not provide enough information to determine a characteristic length for the radius of the atom. Bohr notes that the introduction of h provides this information, since its units and dimensions make it possible to calculate the length of the atom which turns out to be of the expected order of magnitude based on other experiments. Thus, Bohr's use of the quantum postulate is directed towards providing a preliminary theory of the structure of the atom, in contrast to the work on radiation that came before. However, his quantum conjecture is again notably different from Planck's, even though the fundamental idea of quantization comes directly from Planck's work.

In the first section of the paper, Bohr shows how applying assumptions found in Planck's theory of radiation to ideas on the atomic structure of hydrogen results in an account of how electrons might be bound to a positive nucleus in stable states. He first considers an atomic model in the style of Rutherford, consisting of a system of a positively charged nucleus with an electron orbiting this nucleus. He describes how a classical treatment of energy radiation by the electron would result in a continuously shrinking orbit, with large, continuous quantities of emitted radiation, such as are not observed in experiments. Here is where Bohr brings in assumptions from Planck's work, which he phrases as follows.

> Now the essential point in Planck's theory of radiation is that the energy radiation from an atomic system does not take place in the continuous

[11] See Heilbron & Kuhn (1969) for a detailed treatment of this episode.

> way assumed in the ordinary electrodynamics, but that it, on the contrary, takes place in distinctly separated emissions, the amount of energy radiated out from an atomic vibrator of frequency ν in a single emission being equal to $\tau h\nu$, where τ is an entire number, and h is a universal constant. (Bohr, 1913, p. 4)

It is crucial that Bohr had to determine how he might apply something like Planck's quantum conjecture to an atomic system. In order to do so, he posited the existence of stable states, with electrons orbiting the nucleus in definite orbits. He initially considered the emission process from an atomic system as taking place in quantized amounts, dependent on the parameter h. He assumed that the radiation is monochromatic, and that the amount of energy emitted is equal to $h\nu$, where ν is the frequency of the emitted radiation (p. 8). More importantly, Bohr then goes on to show how a modification of this conjecture can be used to recover the same results. Rather than assuming that the stationary states correspond to emissions of integral quanta, Bohr assumes "that the frequency of the energy emitted during the passing of the system from a state in which no energy is yet radiated out to one of the different stationary states, is equal to different multiples of $\omega/2$, where ω is the frequency of revolution of the electron in the state considered" (p. 14). Thus, he eliminates the reference to actual quanta of energy, and relates instead the frequency of emitted radiation to the frequency of revolution of an electron in one of the stable states. Bohr's ultimate statement of his conjecture is the following.

> If we therefore assume that the orbit of the electron in the stationary states is circular, the result . . . can be expressed by the simple condition: that the angular momentum of the electron round the nucleus in a stationary state of the system is equal to an entire multiple of a universal value, independent of the charge on the nucleus. (Bohr, 1913, p. 15)

Using this conjecture along with his elementary atomic model, Bohr was able to account for the Balmer formula, which described the discrete spectral lines observed when hydrogen gas is heated.[12]

Although Bohr explicitly references Planck's quantization conjecture, Bohr's interpretation of quantization is a novel one. First, it is clearly the case that his quantum conjecture differs from those of Planck and Einstein. Several quotes from Bohr emphasize the fact that this application is different from what has come before. For instance,

[12] I omit the details of Bohr's numerical argument here, but they will be presented in Chapter 5.

> It is readily seen that there can be no question of a direct application of Planck's theory. This theory is concerned with the emission and absorption of energy in a system of electrical particles, which oscillate with a given frequency per second, dependent only on the nature of the system and independent of the amount of energy contained in the system. (Bohr, 1922, 10)

This is in contrast to an atomic system, where the frequency depends on the energy of the system. Therefore, despite the reference to the emission of discrete amounts of energy, this is not a straightforward application of Planck's idea. This difference is unsurprising. While quantum hypotheses raised in the context of blackbody radiation and light quanta were dealing with thermodynamical phenomena, and thus evaluating behaviour on a large scale, Bohr was using a quantum hypothesis in the investigation of individual atomic structure. Unlike Einstein's conjecture of light quanta, Bohr does not hypothesize about the constitution of electromagnetic radiation itself.

2.3 The general postulate

In the previous section, I emphasized the different quantum conjectures in use in various domains of application. We have seen that these conjectures differed quite widely in terms of the scientists' intended scope, as well as their physical underpinnings. Planck's conjecture was one about the behaviour of radiation in its interaction with 'resonators'; Einstein hypothesized the existence of physical quanta of light, as well as suggesting that the energy of an oscillator as a model of solid matter could be quantized; Bohr's conjecture was used to determine the stable states of his atomic model of hydrogen. Despite these differences, it is clear that there was some common idea that linked all of these applications. I submit that the core assumption that we can infer from all of the quantum conjectures can be articulated in the following way: "There is a universal, nonzero parameter h with the dimensions of action that can be used to impose a quantization condition on quantities that were previously considered to be continuous, in such a way that reduces to the specific conjectures in each of the domains." Each of the more specific claims about radiation or physical systems can be seen as a particular instance of quantization. I claim that scientists were implicitly seeking to find support for the general postulate, as evidenced by their attempts to find more ways to apply the idea to different systems despite disagreements or ambivalence about the underlying physical mechanisms causing the quantized behaviour.

Such ambivalence can be observed in several contexts. I have already discussed the varying ways in which we might understand Planck's quantum conjecture. It was not clear whether the quantization was meant to apply to the physical resonators modeling a blackbody, or merely in a mathematical description of phase space when calculating the entropy. Lorentz, for instance, said,

> [W]e cannot say that the mechanism of the phenomena has been unveiled [by Planck's theory], and it must be admitted that it is difficult to see the reason for this partition of energy by finite portions, which are not even equal to each other, but vary from one resonator to the other. (Lorentz 1909, quoted in Jammer 1966, 24)

Even Planck's assumption of resonators was not meant to be a literal description of the physical system; instead, he was relying on Kirchhoff's law which states that the radiation in blackbodies is dependent purely on temperature and not on their specific material. Whether this was an appropriate model or not, the fact that quantization was a crucial feature for the recovery of the distribution law for blackbody radiation emerged quite clearly. Subsequent applications of quantization can be seen as ways that scientists were exploring the possibility of a general postulate that would still recover the distribution law.

We should also consider the fact that Einstein was likely not as committed to the existence of physical light quanta as one might have assumed. Although in the first part of his 1905 paper Einstein phrases the underlying assumption as one about light quanta, his primary argument is about the behaviour of monochromatic low-density radiation. Consider what Einstein writes to Lorentz in 1909: "As far as the light quanta are concerned, it seems that I did not express myself clearly. For I am not at all of the opinion that light has to be thought of as being composed of mutually independent quanta localized in relatively small spaces" (Einstein 1909/1995, p. 123). This might seem quite surprising, but it likely reflects his recognition of the fact that Maxwell's equations for electromagnetic radiation were extremely well confirmed in certain domains. Indeed, at the start of the paper, Einstein says that "The wave theory of light which operates with continuous functions in space has been excellently justified for the representation of purely optical phenomena and it is unlikely ever to be replaced by another theory" (Einstein, 1905, 91). Thus, even though he called his 1905 paper "revolutionary," what he was committed to was the idea that a new concept of quantization would have to be incorporated in some domain of description of the behaviour of light, and not necessarily to the physical existence of the light quanta. This attitude is displayed in the final section of his (1909), where he sketches a possible interpretation of the meaning of light quanta in which the energy of the electromagnetic field is localized in singular points, and the fields associated with each point superpose in such a way as to recover the wave description of the field. However, he goes on to say, "I am sure it need not be particularly emphasized that no importance should be attached to such a picture as long as it has not led to an exact theory" (Einstein, 1909/1989, p. 394).[13]

Similar considerations are true for Bohr's use of quantization. Despite his discussion's use of mechanical concepts such as the angular momentum of an electron, he says that "there obviously can be no question of a mechanical foundation of the calculations given in this paper" (Bohr, 1913, p. 15), indicating that such links between the quantum conjecture and any physical accounts are speculative at best. Bohr himself did not claim that his use of quantization explained anything in a deep sense in his atomic model.

> I am by no means trying to give what might ordinarily be described as an explanation; nothing has been said here about how or why the radiation is emitted. (Bohr, 1922, p. 13)

This is particularly apt given that Bohr never accepted Einstein's light quanta explanation. Instead, Bohr seems to treat his quantum conjecture as simply another application of a general idea of quantization in the progression towards a theory that includes the notion in a precise way.

2.4 Implications

The foregoing discussion underlies my claim that there was a process of piecemeal pursuit that was occurring at this time: the applicability of the general postulate in a variety of disparate contexts was providing support for the postulate despite the absence of an overall theoretical framework in which the postulate could be consistently embedded. One might object that simply ignoring certain aspects of the quantum conjectures as originally presented might render the postulate so vague that it is no longer useful as a promising idea. However, consider two points. First,

[13] Darrigol (2014) provides an excellent account of Einstein's vacillating commitment to the idea of light quanta and quantization in general in the decade after 1905.

in the context of pursuit, I would argue that it is quite realistic to be working with a very indefinite postulate, since we lack a theory that precisely defines the entities about which we are theorizing. Second, although the postulate is extremely noncommital with respect to underlying causes, its inclusion of the specific parameter h renders it precise enough to be useful for further pursuit. In this section, I discuss these points and their implications for our understanding of theory pursuit that this suggests.

First, we see how the existence of 'piecemeal' attempts to apply the idea of quantization helped scientists to infer that something fundamental about previous theories had to be changed. Any individual application of the idea of quantization could be considered merely as a way of trying to recover the phenomenon in question, and as we have seen, could be attributed to various possible physical causes. Indeed, we can understand Planck's original introduction of the idea in just these terms. However, the increasingly broad applicability of the notion of quantization in different domains began to eliminate the possibility that previous continuous descriptions of quantities could be wholly accurate. After all, an explanation of blackbody radiation in terms of some mechanism in a resonator's interaction with radiation could not be used in a straightforward way to quantize the phase space of a harmonic oscillator being used to model a diamond. Thus, the 'indefinite' nature of the postulate is actually an indication of how universal it is, and the universality of the general quantum postulate made it increasingly clear that some fundamental aspect of previous descriptions of physical systems had to change.

This point can be considered more generally for any process of pursuit that is similarly piecemeal in nature. In this particular example, I claim that the increasing number of applications of the idea of quantization indicated that there was something necessary about this description at a fundamental physical level. However, I believe that even in cases where a previously accepted description of fundamental processes is not being challenged, we can see that broad applicability has a benefit beyond 'fruitfulness,' where this refers to the possibility of finding additional applications. A methodology of pursuit that tries to identify principles that are widely applicable and then seeks to apply them in as many settings as possible is desirable not only because such a practice may allow scientists to come upon a number of appropriate applications of the principle, and to explain more phenomena. Rather, such a practice allows scientists to identify what is common among these various applications in order to decide what should be retained in any theory going forward. As the number of successful applications grows, we obtain more information about what a proper generalization should be, and which parts of the principle are crucial in accounting for the phenomena. In our example, if the application of quantization to the phenomenon of specific heats had failed, then it would have been reasonable to think that the general postulate that was being supported would make some reference to electromagnetic radiation; however, the successful application in that context indicated that we should broaden our perspective.

A crucial aspect of the general quantum postulate that I did not focus on in this chapter was the importance of the inclusion of the theoretical parameter of Planck's constant. We saw that h, with its dimension of action, made it possible to quantize the various systems and thus linked the different contexts. In the next chapters, I will discuss in more detail how h both allowed for precise predictions that could be tested, and also how its inclusion made the experimental observations yield information about the quantum postulate by measuring the size of h. I claim that any widely applicable principle must be tempered by some kind of specificity in order to obtain good scientific results.

This combination of broad applicability and specificity is a desirable feature of a principle in the context of pursuit. While my particular analysis identifies a postulate that is applicable at a very fundamental physical level and where specificity comes from a universal theoretical parameter, these features could very well be interpreted differently in different contexts. It is possible that one way to apply a principle very widely would be to find a physical mechanism that helps to account for phenomena being studied in different domains. According to my characterization, it would also be important for such a principle to have a level of specificity, such that it could yield precise and testable predictions in a variety of contexts. Even better would be the ability to use experimental results to infer something about the nature of the principle under examination.

Finally, this study provides us with an example of how we can identify a postulate as promising without its being embedded in a larger theoretical framework that is deemed worthy of pursuit. A specific conjecture that seems promising in one particular domain is generalised so that a broader postulate is applicable in a variety of domains, and the nature of the postulate indicates that a new theory will be forthcoming. Thus, we have a case where we have no full coherent framework, but where a postulate can be identified from among a background framework and deemed promising, and supported by the evidence.

2.5 Conclusion

I have argued for my characterization of the applications of a general postulate about quantization as a process of piecemeal pursuit. This is due to the fact that the various quantum conjectures in use differed significantly in their interpretation of what should be considered as quantized, and also in terms of their physical explanations of the observed behaviour. The application of the general quantum postulate in ever broader contexts indicated that it was indeed a universal principle, and that scientists should be attempting to find an appropriate generalization of the postulate in any developing theory. In the following chapters, I will discuss the evidential relations between the experiments in each context and the general quantum postulate.

Chapter 3

The Photoelectric Effect

3.1 Introduction

In this chapter, I examine the way in which the phenomenon of the photoelectric effect influenced and constrained an episode of theory pursuit. I will show how a scientist need not be committed to all of the elements of a framework in order for research to be guided by that framework, and that despite disagreements about some of these elements, there can be agreement about certain aspects of the framework based on experimental results.

Most people now take for granted the importance of Einstein's explanation of the photoelectric effect for the development of quantum theory. Yet, when Einstein first provided a tentative explanation of the effect in 1905 by way of his light quanta postulate, the reaction from the scientific community was quite skeptical. This was because a hypothesis of localized light quanta did not seem as though it would be able to account for the experimentally well-established diffraction phenomena, which was explained by the wave theory of light. Despite various misgivings, great strides in quantum theory were made from 1905 to 1921, as evidenced by Einstein's Nobel prize in the latter year.

Much of the support for Einstein's ideas came from Millikan's experiments on the photoelectric effect, published in 1916. Yet, Millikan was one of many to disagree with Einstein about what the underlying theory explaining the photoelectric effect might be. This shows that when we consider the context of pursuit, we should be careful about exactly what is being pursued, and what is being confirmed. In this case, we should consider Millikan as pursuing a very specific hypothesis about the relationship between two quantities in the experimental setup of photoelectric phenomena, in order to confirm its accuracy. While scientists were perhaps trying to ultimately articulate a theory that could consistently and accurately account for a growing body of experimental results, and that could also recover the successes of classical theories, there was no theoretical framework that could achieve all of these demands at the time of Millikan's work. This chapter provides a historical case study of how pursuit and confirmation of a particular element of a framework can contribute to the articulation of such a theory.

I first provide a brief overview of Einstein's argument for light quanta as it relates to the photoelectric effect. In Section 3.3, I discuss Millikan's experimental results. I then provide an analysis of the significance of these results for understanding the context of pursuit in Section 3.4. These arguments are significant for my characterization of the process of pursuit, but this episode will also be important for my later explications of support using a tempered personalist framework. In Chapter 4, I will return to this example in order to explicate the support that the quantum postulate gained through Einstein's correct prediction of a particular formula, and Chapter 6 will appeal to Millikan's measurement of the value of Planck's constant.

3.2 Einstein and light quanta

We now understand the photoelectric effect as the emission of streams of electrons from certain metallic surfaces that are charged to an electric potential when these surfaces are exposed to incident ultraviolet light. This phenomenon was first observed by Philipp Lenard in 1887, who conducted experiments on illuminated metals and the resulting "cathode rays" that were emitted from those surfaces. While Lenard was able to document various ways in which rays were emitted in different circumstances, he had no ready explanation for the behaviour of these rays. Contrary to the predictions of classical theory, the energy of the emitted rays was independent of the intensity of the incident light, depending instead on its frequency. Although this was a puzzling result, there did not seem to be a pressing need for an explanation, since the experiments were not particularly precise, and thus did not point definitively to a problem that had to be solved.[1]

The first link between any kind of quantum postulate and the photoelectric effect was made by Einstein in 1905 when he initially put forth the possibility of the existence of light quanta. As we saw in Chapter 2, the primary motivation for the postulate arose in analogy with the kinetic theory of gases. After presenting his

[1]See Hendry (1980) for more details.

analogical argument, Einstein gives a statement that perfectly accords with an attitude of pursuit:

> If monochromatic radiation — of sufficiently low density — behaves, as far as the volume-dependence of its entropy is concerned, as a discontinuous medium consisting of energy quanta of magnitude $h\nu$, *it is plausible to investigate* whether the laws on creation and transformation of light are also such as if light consisted of such energy quanta.[2] (Einstein, 1905/1967, p. 102, emphasis added)

He then goes on to consider how this hypothesis might account for several phenomena, including the observations of the photoelectric effect. For Einstein among others, the observations of these phenomena pointed to the existence of anomalies in classical theories. These anomalies suggested the necessity of developing a new theory; a theory that treated energy discretely that could be consistently applied over many domains. The problem that had to be solved was that certain wave aspects of light were so well-established that no one was prepared to give them up. Indeed, it was a reasonable constraint that any future theory would have to account for diffraction phenomena as successfully as the wave theory.

At the same time, energy was displaying seemingly contradictory particle-like behaviour in certain contexts. These facts needed to be reconciled in some way. Thus, Einstein's attitude here is that the idea he has put forth is a promising one and should be pursued, perhaps to make progress in finding a more generally applicable theory. He goes on to show how pursuing the idea he has put forth in the context of the photoelectric effect could contribute to the articulation of such a theory.

Einstein hypothesized that each light quantum penetrating the surface layer of bodies has a definite amount of energy that is proportional to the frequency of the light. Once a quantum has penetrated the surface, its energy might be transferred to an electron in the substance. If the amount of transferred energy is sufficient, the electron does a certain amount of work to escape the surface, where the amount of work required is determined by the experimentally controlled potential difference between the substance and its surroundings. Any additional energy is manifested as the kinetic energy of the escaped electron. The energy of an electron escaping the

[2] I continue to replace β with the equivalent h/k for consistency of notation.

surface could be described by the equation

$$\frac{1}{2}mv^2 = V \cdot e = h\nu - p. \tag{3.1}$$

V is the potential difference against which an escaping electron is just able to drive itself before coming to rest, e is the charge of that electron, ν is the frequency of the incident light, p is the work required for an electron to escape the surface (characteristic to the substance), and h is Planck's constant. This hypothesized interaction allowed Einstein to derive a prediction for the relation between the voltage (potential difference) and the frequency of light. Specifically, he predicted that

$$V = \frac{h}{e}\nu - p/e, \tag{3.2}$$

or that the relationship between V and ν would be a linear one, and that the slope of this line would take the value h/e.[3]

Due to the limited experimental precision of Lenard's results, it was impossible to know whether this relation was borne out by observation. While it was known that the value of V increased with ν, there was not sufficient evidence to determine whether the relation was linear as opposed to some other increasing relationship. Einstein was able to show an agreement to an order of magnitude between his prediction of the potential difference required to stop the emission of cathode rays and Lenard's experimental setup, but this was not nearly precise enough to conclude that his equation must hold. Neverthless, this work did allow him to predict that graphing the relation between the potential difference and the frequency of the incident light would yield a straight line with slope h/e. This prediction was crucial for identifying a possible direction along which pursuit might occur.

Early reactions to Einstein's light quanta hypothesis were skeptical. Kuhn, for instance, says about the 1905 paper, "As to the photoelectric and related effects, little evidence was available" (1978, p. 221). Furthermore, consider the reaction from several leading scientists in their bid for Einstein's membership into the Prussian Academy of Sciences in June 1913:

> In sum, it can be said that among the important problems, which are so abundant in modern physics, there is hardly one in which Einstein did not take a position in a remarkable manner. That he might sometimes

[3] This is the equivalent of Einstein's notation, which I have changed for consistency with Millikan's work.

> have overshot the target in his speculations, as for example in his light quantum hypothesis, should not be counted against him too much. (Planck et al., 1913/1995, p. 337–338)

There were, after all, experimentally well-grounded objections to the light quanta hypothesis. For instance, Jammer discusses experiments performed by Lummer and Gehrcke, which seemed to indicate that light quanta, if they existed, would have an extension of over a metre (Jammer, 1966, p. 43). Millikan mentions that such a hypothesis cannot account for observed interference phenomena (Millikan, 1916, p. 355). Nevertheless, some scientists deemed Einstein's treatment of the photoelectric effect very promising, and general opinion changed significantly in reaction to Millikan's very precise experiments performed in 1914. Indeed, Einstein was awarded the 1921 Nobel Prize in Physics not for his work on special or general relativity, but for his work on quantum theory; in particular, his work on specific heats and the photoelectric effect are cited. In the speech, Arrhenius says,

> Einstein's law of the photo-electrical effect has been extremely rigorously tested by the American Millikan and his pupils and passed the test brilliantly. Owing to these studies by Einstein the quantum theory has been perfected to a high degree and an extensive literature grew up in this field whereby the extraordinary value of this theory was proved. Einstein's law has become the basis of quantitative photo-chemistry in the same way as Faraday's law is the basis of electro-chemistry. (Arrhenius, 1922/1967)

In the speech, Arrhenius does not distinguish between different quantum conjectures, and presents Einstein's explanation of various phenomena explicitly in terms of light quanta. He makes it sound as though Millikan's work provided solid support for Einstein's light quanta conjecture. However, I will argue that this is a bit too simplistic a characterization of what Millikan achieved with his experiments, and that although it is certainly the case that Einstein's light quanta conjecture successfully guided numerous investigations, it is still desirable in the context of pursuit to be cognizant of which elements of a framework are genuinely being supported by experiments.

3.3 Millikan's experiment

While Einstein's paper is couched in terms of light quanta, one can pursue the general idea of light behaving in the way Einstein describes without being committed to their existence. As part of my analysis here, I wish to draw special attention to the fact that the purpose of Millikan's 1916 paper was not to defend or vindicate Einstein's light quanta hypothesis: instead, it was to use experiments on photoelectric phenomena to determine the value of h with as high a precision as possible. This fact has been recognised by several historians of science, most recently perhaps by Allan Franklin (2013) and Roger Stuewer (2014), and is a significant facet of an account of the pursuit of a quantum theory. We will see that Millikan's determination of the value of h required several steps. One step in particular provided almost conclusive proof that the relationship between V and ν was exactly the linear relationship that Einstein had predicted in 1905.

Millikan outlines five experimentally verifiable relationships contained in Einstein's photoelectric equation, Equation 3.1. The most important ones for our purposes are the following:

1. There is a linear relation between V and ν.
2. $\frac{dV}{d\nu}$, or the slope of the V-ν line is numerically equal to h/e.
3. At the critical frequency ν_0 at which $v = 0$, $p = h\nu_0$, i.e. that the intercept of the V-ν line on the ν axis is the lowest frequency at which the metal in question can be photoelectrically active. (1916, p. 356)

He first discusses previous experiments on the photoelectric effect. These were both experimentally less reliable, and focused on a limited number of wavelengths, such that it was impossible to draw confident conclusions about the relationships between experimental values. In fact, several experiments attempting to determine the numerical value of Planck's constant disagreed about the correct value (Franklin, 2013, p. 577). Millikan's group developed an accurate way to measure the photoelectric effect for a much larger range of wavelengths than was previously possible. He conducted experiments on several photoelectric materials and reported his results on the alkali metals sodium and lithium.

The determination of h proceeded as follows. First, experiments were performed in order to determine the potential difference required to stop all photoelectric emission for a particular frequency of light ν on a sodium metal surface. This was

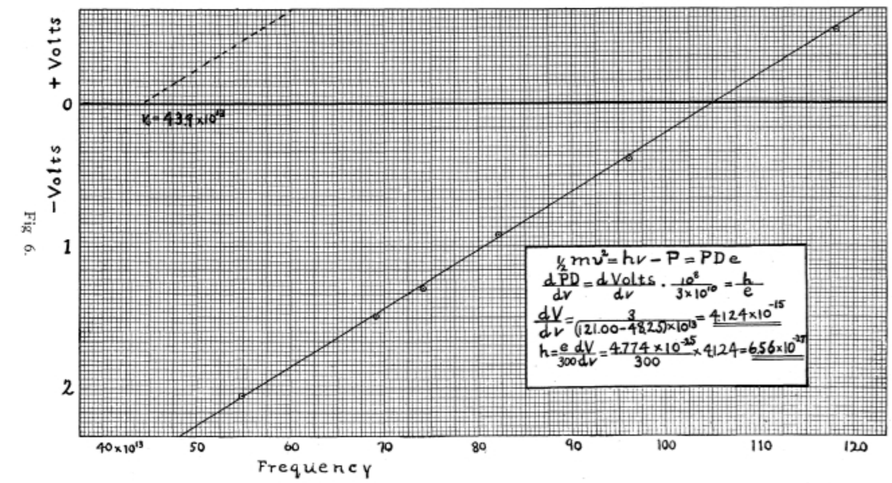

Figure 3.1: Graph from Millikan (1916), p. 373

repeated for several values of ν. This value was not measured directly: for any given ν, different voltages were applied to the experimental apparatus, and the resulting photocurrent was observed. Several of these observations yielded enough data to graph a line of best fit, whose potential-difference intercept was then determined. These intercept values were plotted on a V-ν graph in order to determine the general type of relation, which turned out to be clearly linear, as demonstrated in Figure 3.1.

Setting the slope of this line to h/e, and using his previously determined value for e, Millikan was able to calculate a value for h, which was 6.56×10^{-27}.

It was necessary to conduct this part of the experiment in a non-perfect vacuum as the observations had to be taken over a long period of time, and in the best achievable vacuum the values of some observables changed drastically after a short initial period. However, higher quality data could be obtained by taking a small number of observations in the best achievable vacuum. Thus, Millikan then adjusted the experiment so that he was able to take measurements for two different frequencies of light in the highest attainable vacuum. Having already clearly established the linear relationship between V and ν using several data points, Millikan used this new data to determine the slope of the line in an alternative way. The mean value from these observations yielded a value of $h = 6.569 \times 10^{-27} erg \cdot sec$ with an error of no more than .5 percent.

He also reported on similar experiments performed on lithium metal which yielded a mean value of $h = 6.584 \times 10^{-27} erg \cdot sec$ (p. 376, typo corrected), with an uncertainty as much as 1 percent. Finally, Millikan calculates the value of h using the same method as Planck's original calculation, but using more recent experimental and theoretical values of the necessary variables since he estimates that Planck's original calculation contained an uncertainty of at least 8 percent. This yielded a value of $h = 6.57 \times 10^{-27} erg \cdot sec$.

The above summary shows very clearly that Millikan's main concern was the confirmation of Einstein's equation expressing the relation between frequency and potential difference in the photoelectric effect, and the subsequent measurement of h. This was not an experiment designed to test the light quanta hypothesis. He characterizes the inquiry as one that would allow him to "assert whether or not Planck's h actually appeared in photoelectric phenomena as it has been usually assumed for ten years to do" (Millikan, 1916, p. 360). He also discusses the work of scientists such as Hughes, Richardson and Compton in terms of their determinations of h. Thus, we can infer that in Millikan's view, what scientists were really able to take away from Einstein's 1905 paper was the applicability of the parameter h to a new domain based on Equation 3.1, which guided the research programs on the photoelectric effect for the next decade.

3.4 Context of pursuit

This episode demonstrates several points relevant for the context of pursuit. First, we have an example of the fact that the ability of a hypothesis to qualitatively explain a phenomenon is not enough to constitute good evidence for that hypothesis. If it were, Einstein's first paper would have been taken to be a much stronger argument, as it was able to provide a perfectly good explanation of the photoelectric effect that was in accord with the best experiments of the time, along with other observed phenomena such as Stokes's Rule.

> Stokes's Rule . . . was well established. But, in the absence of a quantum theory applicable to spectral frequencies, Einstein's suggestion did little to explain the phenomenon and offered no guidance at all for further research. As to the photoelectric and related effects, little evidence was available. (Kuhn, 1978, p. 221)

Aside from the fact that there were elements of his light quantum hypothesis that appeared to be in conflict with accepted wave theories of light, his work was also lacking a certain level of experimental precision that would lend credibility to his conjecture. Later, despite the fact that few if any of the earlier conflicts with classical theories of light were resolved, Millikan's far more precise determinations of the value of h using photoelectric phenomena were striking.

This period also provides a clear example of how scientists might reasonably disagree about the significance of certain anomalies with established theories, and exhibits some of the different ways that they might proceed in the context of pursuit to account for these anomalies. In 1916, Millikan was ready to accept Einstein's predicted linear relationship between V and ν. Yet, he still rejected the original explanation for that equation, namely, the hypothesis of light quanta. Indeed, he describes the hypothesis as "bold, not to say reckless," and calls it "a form of quantum theory which has now been pretty generally abandoned" (p. 355). In fact, consider what Millikan says in his 1917 book, *The Electron*.

> Despite then the apparently complete success of the Einstein equation, the physical theory of which it was designed to be the symbolic expression is found so untenable that Einstein himself, I believe, no longer holds to it, and we are in the position of having built a very perfect structure and then knocked out entirely the underpinning without causing the building to fall. It stands complete and apparently well tested, but without any visible means of support . . . Experiment has outrun theory, or better, guided by erroneous theory, it has discovered relationships which seem to be of the greatest interest and importance, but the reasons for them are as yet not at all understood. (p. 230)

Given this attitude, it is reasonable that he would try to provide alternatives to the light quanta conjecture that would still explain why this equation might be physically instantiated. In the last section of his paper, Millikan provides such an alternative.

He proposes that while a body may contain mostly oscillators of a characteristic frequency, these may be mixed with a small number of oscillators of different frequencies. These oscillators would absorb energy from light of their particular frequency until their energy reaches some critical value. The emitted corpuscle may then impact other oscillators, which may in turn emit corpuscles of their characteristic amount of energy. Since the substance is composed largely of oscillators of this frequency, emitted corpuscles will overwhelmingly possess this amount of energy. We see that Millikan's suggestion bears a closer resemblance to Planck's quantum conjecture about quantized oscillator energy, and explicitly distances itself from Einstein's light quanta conjecture.

This suggestion made use of the idea of resonance, whereby some mechanism in the atom is triggered by light of the appropriate frequency. In addition to its relation to Planck's conjecture, it was also related to earlier suggestions such as Lenard's, who had already speculated that it was just such a phenomenon that was producing the photoelectric effect. Similarly, it appeared that such an explanation would be able to account for the phenomenon of blackbody radiation.[4] Johannes Stark was an enthusiastic early proponent of the quantum theory, and appealed to ideas about quantization from 1905 onward, but he was also considering some physical process of the oscillators as being the underlying cause of quantization.

> In my opinion it is not necessary to postulate a discontinuous structure of the radiation energy, which flows with the velocity of light in ether; rather there exists in the process considered, which is governed by the elementary law, a specific type of action exerted by the electromagnetic resonators. (Stark 1908, 768)

Like Planck's view, such posits shifted the anomalous behaviour to the processes of matter interacting with energy rather than light itself. After all, it seemed at the time that diffraction phenomena could only be explained by the existing wave theory of light, which was enormously successful in most domains. Of course, Einstein was fully committed to certain aspects of the wave theory:

> The wave theory of light which operates with continuous functions in space has been excellently justified for the representation of purely optical phenomena and it is unlikely ever to be replaced by another theory. (Einstein, 1905/1967, p. 91)

However, he viewed the conflict between such a wave theory and the light quanta postulate as a problem to be solved, rather than an insurmountable difficulty for light quanta. After all, he is clear that these theories may be applicable in their own domains.

[4] See Hendry (1980) for more details.

> One should, however, bear in mind that optical observations refer to time averages and not to instantaneous values and notwithstanding the complete experimental verification of the theory of diffraction, reflexion, refraction, dispersion, and so on, it is quite conceivable that a theory of light involving the use of continuous functions in space will lead to contradictions with experience, if it is applied to the phenomena of the creation and conversion of light. (Einstein, 1905/1967, p. 91–92)

Thus, Einstein recognises that these different theories seem to hold in their limited domains, and that it should be possible to account for both diffraction and the photoelectric effect. For instance, while he says that it has "not yet been possible to formulate a mathematical theory of radiation that would do justice to both the undulatory structure and the . . . inferred . . . quantum structure" of radiation, (1909/1989, p. 394), we saw in Section 2.3 how he approached the problem by considering some constraints on the formulation of such a theory. While others were rejecting light quanta because it was inconsistent with classical ideas, Einstein was urging the idea that the two conceptions were not inconsistent, and says about his sketch, "All I wanted is briefly to indicate . . . that the two structural properties (the undulatory structure and the quantum structure) simultaneously displayed by radiation according to the Planck formula should not be considered as mutually incompatible" (*ibid*).

This type of disagreement with respect to how theorizing should proceed is unsurprising, as this is how new alternatives arise. Some scientists may be committed to the older theories and seek to modify those; some others may suggest completely new ideas. Regardless of these disagreements, there are ways in which such lines of inquiry and subsequent research impose constraints that must be satisfied in all further research. In our particular case, the experimental work that arose from the photoelectric effect showed that Einstein's Equation 3.1 was almost certainly accurate. This result constrained the possible theories that were subsequently developed.

As Millikan explains, the fact that Einstein's equation holds shows that the escaping electron must, at some point, absorb at least energy of amount $h\nu$. If one does not want to accept Einstein's light quanta hypothesis, the fact that electrons are emitted immediately upon the surface's illumination can only be explained by assuming that corpuscles already possess that energy. This implies that there are oscillators in the body 'loading up' to the value $h\nu$. Now, if these oscillators lost energy whenever they are not exposed to radiation, they would eventually lose enough energy such that they would require hours of illumination to display photoelectric effect. This is shown not to be the case by experiment. Therefore, they cannot lose energy while not being illuminated, or, in other words, we must assume a "discontinuous or explosive emission of the energy absorbed by the electronic constituents of atoms" (Millikan, 1916, p. 385). Thus, Millikan agrees that the confirmation of Einstein's equation sets a constraint on any subsequent theory that oscillators must show such a discrete, 'explosive' emission.[5]

The foregoing discussion shows very clearly that one need not be committed to all of the elements of a suggested theory or framework in order for research to be guided by that framework. Instead, scientists might agree that there is a general idea that is promising, but have distinct opinions on what should be considered crucial elements that must be included in that framework. Although this work is now retrospectively taken to be good evidence for the photon theory of light,[6] at the time, Millikan characterized his work not as supporting Einstein's photon theory, but merely as confirming Einstein's equation. It would be a misrepresentation of the situation to claim that at the time the experiment was conducted, the hypothesis of light quanta was clearly vindicated since, even then, virtually no scientists were ready to declare this an acceptable theory. However, this work was considered good evidence for the importance of incorporating h into the description of the behaviour of energy in this domain. Thus, even in cases of high predictive accuracy, it is natural to separate a confirmation of an equation or determination of a theoretical parameter from the confirmation of all of the elements of the framework guiding the research.

It is also worth mentioning that while such different attitudes in the context of pursuit are all rational, they will clearly not all lead to an ultimately correct theory. For instance, Stark attempted to use quantum theory to explain the Doppler effect of canal rays, incorporating h into an account of the excitation of electrons by ions. Stark did not at first subscribe to the idea of energy quanta per se, but rather some

[5]This argument is similar in structure to those given by Ehrenfest and Poincaré, explicated in Norton (1993), in which an inference is made from experimental results to a conclusion about discontinuity. It is perhaps the case that Millikan did not know of these arguments. Whether this is the case or not, it is telling that Jeans, in his *Report on Radiation Theory* 1924, reported these arguments, and yet still thought it was worthwhile to give other empirical arguments for a quantum postulate, which we will see in greater detail in Chapter 6.

[6]Franklin (2013) outlines some of the textbook discussions of Millikan's experiment, as well as Millikan's own later work; many of these tend to treat this experiment as excellent evidence for the hypothesis of light quanta.

kind of process attributable to the resonators being used to describe the process. This attempt was ultimately unsuccessful. We now know that Millikan's suggestion of modifying Planck's theory of radiation would also not be successful. However, such varied approaches to the development of a new theory are reasonable and important.

3.5 Conclusion

In this chapter, I have discussed the way in which the light quanta conjecture guided research on the photoelectric effect, despite scientists' disagreements about the significance of the conjecture itself. I showed that while the results forced agreement about certain aspects of a future theory, this did not require the acceptance of all the elements of the framework in which the phenomenon was being investigated. This demonstrates more generally the possibility of using hypotheses to guide research while coming up with different possibilities to explain the results in the context of pursuit.

In the chapters to come, I will be appealing to this example again to explicate support for the general quantum postulate in a Bayesian framework. This will include discussion of Einstein's precise prediction of Equation 3.2, and Millikan's measurement of h. These analyses will help clarify my claim that the inclusion of Planck's constant in the quantum postulate was crucial for the support the postulate received.

Chapter 4

Blackbody Radiation, Inconsistency, and Tempered Personalism

4.1 Introduction

In Chapters 2 and 3, I argued for the characterization of theory pursuit in the early stages of quantum theory as a piecemeal process, and showed how scientists need not be committed to particular conjectures in order to guide research and further develop theories. In this chapter, I will discuss the origin of Planck's original quantum conjecture, and introduce the Bayesian framework in which I will be evaluating the evidential relations in subsequent chapters. These analyses will show how a formal framework can be used to interpret the epistemic force of the features in question.

Section 4.2 is an examination of the genesis of the idea of quantization in Planck's treatment of blackbody radiation. I draw attention to the criticisms made by Einstein and Jeans of Planck's work, and I claim that previous philosophical discussions have not paid close enough attention to the nature of these criticisms: while there was of course a conceptual problem of combining classical principles with a quantum hypothesis, Einstein's criticism does not attribute an outright contradiction to these claims, and Jeans's criticism focused on empirically motivated considerations. I argue that a historically influenced reconstruction of the arguments for quantization must be able to account for this, and that a Bayesian tempered personalist framework is well-suited to this task. In Section 4.3, I lay out the framework that I will be utilizing and suggest that we should understand contradictory statements simply as statements that are being applied to a more limited domain. I will discuss the idea of setting a prior for the quantum postulate at the time of Planck's introduction of the idea of quantization. Finally, I will provide an example of how the general quantum postulate was supported by Millikan's experiments on the photoelectric effect as discussed in the previous chapter.

4.2 Inconsistency and Blackbody Radiation

I focus here on the criticisms of the original presentation of the quantum postulate in order to examine the way inconsistency was addressed when it first appeared. I argue that the nature of the criticisms had an empirical aspect as well as a conceptual one, and that this has often been overlooked by previous philosophical discussions of Planck's theory. These facts provide some guidance in terms of how we can reconstruct the process of pursuit.

In Chapter 2, we saw that in 1900, Planck proposed a formula for the emission spectrum of blackbody radiation that provided the correct values for all experimental observations of the spectrum of frequencies at various temperatures. Unfortunately, Einstein pointed out that certain of Planck's assumptions in the derivation of this formula were questionable in terms of their consistency (Einstein, 1906/1989). This fact is now well-known, and many discussions take for granted that there is a fundamental inconsistency in Planck's assumptions. Yet few philosophical treatments address Einstein's actual criticism.

I will argue that in keeping with Einstein's analysis, it is possible to read Planck's assumptions in a way that are not actually inconsistent. I will also argue that the main concern about Planck's introduction of quantization for scientists such as Einstein and Jeans was less the oft-cited conceptual problem of attributing conflicting properties to resonators, but the fact that such assumptions required some kind of independent motivation, or that they actually led to predictions that were not borne out by observations. I begin by presenting Planck's argument of 1901 and follow this with a discussion of the criticisms of this argument given by Einstein and Jeans and some implications thereof.

We have already seen that Planck had to infer the form of the radiation law for blackbodies based on two equations of limited validity, and provide a theoretical interpretation of such a law only afterward. In his treatment of the radiation, he calculates restrictions on the equilibrium entropy of a blackbody at a particular temperature in two different ways, then compares the two forms to obtain a more precise expression of the entropy. Subsequent rearrangement yields the energy distribution law sought for, Equation 2.1. The purported inconsistency arises from the two different ways of restricting the entropy expression.

In the first restriction, Planck calculates how entropy would be expressed based on the idea that the energy is divided into parts. To do this, he considers a large number N of identical resonators of frequency ν. He takes the total entropy S_N to be $k \log W + constant$. He proceeds to find the probability W so that the N resonators possess vibrational energy U_N. Crucially, he makes the assumption that U_N can be considered as a collection of P elements of energy of size ϵ, where the size of ϵ is not yet determined. The number of ways to divide the P elements between N resonators is given by

$$W = \frac{(N+P-1)!}{P!(N-1)!}, \tag{4.1}$$

which, by ignoring the 1's in relation to large N's and P's and applying Stirling's formula, can be approximated as

$$W = \frac{(N+P)^{N+P}}{P^P N^N}. \tag{4.2}$$

If entropy is $S = k \log W$, then

$$S_N = k\left[(N+P)\log(N+P) - P\log P - N\log N\right]. \tag{4.3}$$

However, the total entropy S_N is just the entropy of a single resonator S, times the number of resonators N. Furthermore, the total energy of the resonators, U_N is equal to $P\epsilon$, the number of energy elements times the amount of energy in each element, and U_N is also equal to NU, where U is the average energy of a resonator. Thus, P/N is equivalent to U/ϵ, and Equation 4.3 can be rewritten as

$$S = k\left[\left(\frac{U}{\epsilon}+1\right)\log\left(\frac{U}{\epsilon}+1\right) - \frac{U}{\epsilon}\log\frac{U}{\epsilon}\right]. \tag{4.4}$$

This gives us the first restriction on the entropy of a resonator in terms of its energy U.

In the second restriction on entropy, Planck refers back to earlier work in which he calculated the relation between radiation density u_ν and the average energy of a resonator U,

$$u_\nu = \frac{8\pi\nu^2}{c^3}U. \tag{4.5}$$

He did this by using classical theory to determine the expressions for the absorption and emission of energy respectively, and equating them to obtain the relation that holds at the equilibrium state. To be more precise, Planck used his relation between a resonator's absorbed energy and the intensity of the incident radiation to calculate the relation between the average energy of a resonator and the intensity of a monochromatic polarized ray K. With this relation between K and U, along with the Kirchhoff-Clausius law, one obtains a restriction on the form of u_ν. Planck then uses this information to derive a form of Wien's Displacement Law, $S = f(\frac{U}{\nu})$, which can be seen as a restriction on the form of any expression for entropy in the equilibrium state. Planck applies this to his earlier expression of entropy to obtain the fact that the energy element ϵ must be proportional to ν. This gives the following expression for the entropy of a resonator.

$$S = k\left[\left(1+\frac{U}{h\nu}\right)\log\left(1+\frac{U}{h\nu}\right) - \frac{U}{h\nu}\log\frac{U}{h\nu}\right] \tag{4.6}$$

Here, h and k are universal constants. By differentiating Equation 4.6 by U, we obtain,

$$\frac{dS}{dU} = \frac{k}{h\nu}\log\left(\frac{h\nu}{U}+1\right). \tag{4.7}$$

Setting the result equal to $1/T$ yields

$$U = \frac{h\nu}{e^{h\nu/kT}-1}, \tag{4.8}$$

and substituting Equation 4.5, we obtain the final energy density formula, Equation 2.1,

$$u = \frac{8\pi h\nu^3}{c^3}\frac{1}{e^{h\nu/kT}-1}.$$

The inconsistency arises from the two different ways of calculating the equilibrium entropy. As we have seen, the former case assumes that the energy of the resonator takes on integral multiples of an undetermined amount ϵ. In the latter case, the relation between u and E was one that Planck had previously calculated based on Maxwell's theory of electromagnetism, which, in Einstein's words, "does not recognise *distinguished* energy values of a resonator" (Einstein, 1906/1989, p. 196). That is, in calculating the *mean* rate of absorption of energy by a resonator, the values representing the change in energy of the resonator are expressions of periodic functions that do not restrict their possible values to discrete ones. This is the case because the expression of the force acting to change the state of the resonator is modelled as a gradual process, which is necessary in order for the Newtonian equations of motion to hold so that acceleration is well defined at every moment. Therefore, the latter formulas assume that the resonators absorb radiation continuously.[1]

One reason we might be concerned about this is that in these assumptions, we have two statements that seem to contradict each other, namely, "Resonators absorb radiation continuously," and "Resonators absorb radiation in discrete amounts." This is enough to render any result unsound, insofar as we can derive anything from such a contradiction. This is generally how the inconsistency is described in the literature as well. Consider, for instance, the following descriptions.

> The inconsistency arises from the fact that in deriving [the radiation equation] theorists on the one hand appealed to a quantum postulate, according to which the energy levels of the 'resonators', or 'radiation oscillators' that give rise to the blackbody radiation, are quantized, but on the other hand they appealed to results drawn from classical electrodynamics, according to which these energy levels can vary continuously. (Saatsi, 2014, p. 2951)

> In his work on black-body radiation, Planck combined a quantum hypothesis with classical electrodynamics, and came up with the first theoretical derivation of the empirical black-body law. Of course the quantum hypothesis was inconsistent with classical electrodynamics, and as Poincarré [sic] acerbically pointed out, we can derive anything we want from a contradiction. (Brown, 2014, p. 3093)

Yet this explication does not fully capture the reaction of either Einstein or Jeans, although both criticized the tension between the assumptions in subsequent work.

As explained above, the fact that Maxwell's equations are used to calculate the mean energy of a resonator requires that the energy of the resonators varies

[1]My exposition of the problem follows Jeans's presentation of calculating the energy of a resonator and subsequent explanation.

continuously, and yields a particular value for the mean energy. However, this is not equivalent to saying that the only way that a resonator could possess this mean energy value is to behave specifically in this continuous manner. This is, in fact, consistent with the idea that resonators behave in such a way that there are discrete jumps between energy levels, but that the mean energy nevertheless has the value described by the continuous process. Planck seems to take into account such a possibility as well, when he says,

> It is true that this inconsistency is greatly reduced by the fact that, in reality, only mean values of energy are taken from classical electrodynamics, while, for the statistical calculation, the real values are used. (Planck, 1913/1914, p. viii)

Of course, this raises the question of why such discrete resonators would behave in such a way. Indeed, this seems to be how Einstein addresses the flaw in Planck's reasoning. He starts by saying that, in Planck's theory, "[The underlying assumption is that] although Maxwell's theory is not applicable to elementary resonators, nevertheless the *mean* energy of an elementary resonator in a radiation space is equal to the energy calculated by means of Maxwell's theory of electricity" (Einstein, 1906/1989, p. 196, emphasis in original). He then goes on to explain why this would be puzzling.

> This proposition would be immediately plausible if, in all those parts of the spectrum that are relevant for observation, $\epsilon = (R/N)\beta\nu$ were small compared with the mean energy $\bar{E}_\nu$ of a resonator; however, this is not at all the case, for within the range of validity of Wien's radiation formula, $e^{\beta\nu/T}$ is large compared with 1. It is easy to prove that according to Planck's theory of radiation, within the range of validity of Wien's radiation formula, $\bar{E}_\nu/\epsilon$ has the value $e^{-\beta\nu/T}$, thus, $\bar{E}_\nu$ is much smaller than ϵ. Therefore only a few resonators have energies different from zero. (p. 196)

The criticism here appears to be that there is no reason to expect that the use of Maxwell's equations in such circumstances would yield the correct value for the average energy, and so it is in fact surprising that it does. After all, if the energy elements were small compared to the mean energy, it would be reasonable for calculations of the integrals to 'smooth out' so to speak, but we are instead applying the technique to values that are either zero, or quite large. Thus, Einstein seems to be drawing attention not to an impossibility, but to what might be seen as a strange coincidence that calls out for further investigation. In fact, Einstein does not even assign a conceptual incompatibility to the properties of resonators in this part of the argument. Nor does he think that such an assignment of properties allows us to derive any result whatsoever. On the contrary, the criticism is intended to show that Planck was implicitly assuming the existence of something like light quanta. The motivation behind this claim is to motivate the attempt to obtain the derivation in a way that does not use this problematic assumption.

Although Jeans seems to take the more standard view that Planck was attributing conflicting properties to the blackbody resonators, his criticism of Planck's radiation theory does not centre on the existence of inconsistent assumptions, but instead identifies how such contrary assumptions result in incorrect predictions. He says, "[Planck's method] is open to serious objections. For, in considering the partition of energy between the various resonators, it is assumed that the energy can only vary by jumps of amount ϵ, while, in considering the partition of energy between resonators and ether, it has to be assumed . . . that the energy of the resonators can vary continuously" (Jeans, 1924, p. 21). He then goes on to explain that this is problematic because if this were the case, we would expect the resonators themselves to conform to Planck's radiation formula, whereas the resonators interacting with the containing medium would tend towards the equipartition formula of classical mechanics.

> The result would be a compromise between the two laws. Worse than this, it would be a compromise which would depend on the relative numbers of Planck resonators and of free electrons, and as the ratio of these would vary from one substance to another, there would be no definite law of radiation — the same for all substances — such as is demanded by observation. (Jeans, 1924, p. 22)

Thus, according to Jeans, the problem with these assumptions is not merely the conceptual incompatibility of assigning the resonators conflicting properties, but also that doing so leads to a contradiction with an experimentally well-established principle, Kirchhoff's Law. While Jeans focuses on the empirical implications for the system, Einstein highlights the implications for individual Planck resonators. However, both are clearly examples that make specific inferences from the inconsistency to characterize the problem.

As I have mentioned, previous philosophical discussions of this inconsistency, while insightful, have tended to focus on the conceptual inconsistency rather than the more empirical criticisms. Most characterizations of Planck's work identify the problem as the fact that Planck assigned the resonators conflicting properties, or that the quantization assumption conflicts with certain tenets of classical mechanics, thus yielding a contradiction in the form of $(H \& \neg H)$. For instance, Badino says, "Thus, the two ways of calculating the energy of the oscillator rested upon contradictory assumptions: classical continuous emission/absorption of energy in the first case, discrete elements in the second" (2012, p. A29). This is the case even in the "content-driven" discussions that are concerned with the actual progression of scientific arguments.[2] While it is true that Einstein and Jeans both noticed this conceptual tension, I believe it is valuable to consider the further arguments they give. We have seen that Einstein's criticism calls into question whether there was indeed a genuine inconsistency, and that Jeans's discussion highlights the importance of empirical results, even in cases with a purported inconsistency. It is interesting to note that while both scientists criticize the assumptions being used, they also both have very specific predictions that these contrary assumptions would entail. This seems to give lie to the idea that from an inconsistent theory, one can derive absolutely anything. After all, as Vickers (2014) notes, "just because one *can* derive anything and everything with deductive logic doesn't mean that there is a danger one *will*" (p. 2900). Of course, several people have noticed this and there are various methods for dealing with inconsistent science. I suggest that a reconstruction of the investigations here should be able to account for the way in which scientists such as Einstein and Jeans engaged with the quantum postulate. This will require being very careful about what the domain of applicability is for any given assumption, and a philosophical analysis should be able to take this into account. This means we can make good use of a framework in which we identify the background assumptions being used in order to make testable predictions in conjunction with the postulate itself.

4.3 Tempered Personalism

I now turn to providing a basis for a formal analysis of the promise of QP. In order to do so, we should consider how we might deal with the inconsistency in our reconstruction.

[2]See Saatsi (2014) for a discussion of content-driven approaches to reconstructions of scientific arguments.

Philosophers have responded to the problem of inconsistency in several ways. While some have focused on developing sophisticated logical tools such as systems of paraconsistent logic that will tolerate contradictions, I take my project to be more in line with what Saatsi (2014) calls the 'content-driven perspective,' in which empirical success is analysed not in terms of logical tools, but by looking at the inferences we can make in particular cases. Saatsi and Norton (1987) are among those who take this approach. This is so even though I am situating my discussion in the framework of Bayesian epistemology.

A traditional Bayesian framework is committed to the idea that agents do not simply accept or reject statements, but that we have varying degrees of belief. Bayesians are then able to make use of the probability calculus to model degrees of belief in particular statements and combinations thereof, with rules governing the transition from a 'prior' degree of belief to a 'posterior' when new information is learned. A Bayesian framework is particularly apt because when we are considering evidence for a hypothesis, a probabilistic framework provides a useful language for expressing varying levels of promise. To quote Joyce, "since the data we receive is often incomplete, imprecise or equivocal, the epistemically *right* response is often to have opinions that are similarly incomplete, imprecise or equivocal" (Joyce, 2010, p. 283). This seems to me to express exactly the attitude a scientist should have in this context: in the face of new evidence that is not definitive in settling a particular question, the proper epistemic attitude is to allow for a range of possibilities when evaluating the probability of a hypothesis. In what follows, I will make use of 'imprecise credences,' as defended in Joyce (2010). I first present the basics of the framework as given, then comment on their interpretation in the context of pursuit.

1. A believer's overall credal state can be represented by a *family* C of credence functions defined on some Boolean algebra Ω. Facts about the person's opinions correspond to properties common to all the credence functions in her credal state.

2. If the believer is rational then every credence function in C is a probability, so $\forall c \in C, 0 \leq c \leq 1$.

3. If a person in credal state C learns that some event D obtains (and nothing else), then her post-learning state will be $C_D = \{c(\cdot|D) = c(X)[c(D|X)/c(D)] : c \in C\}$.

4. A rational decision maker with credal state C is obliged to prefer one action A

> to another A when A's expected utility exceeds that of A relative to every credence function in C. (p. 287)

The elements of C are probability functions that are compatible with given background information. I take a credal state to represent the doxastic state of an agent with the relevant background information. The same considerations that motivate the use of imprecise credences in general epistemology come to bear in the context of scientific pursuit. When new information is learned, all credence functions in C are updated accordingly, and new facts may emerge if all the updated credence functions agree on a certain property. For instance, if every member of C assigns X a greater degree of probability than Y, the agent can be said to have a higher credence in X than in Y.

One might consider this a subjectivist model in the sense that I do not claim that there is a unique credal state that all rational agents share. While imprecision expresses the idea that an individual agent does not have a sharp credence about particular statements, there also exist differences in overall credal states of different agents. However, the general attitude with which I approach this framework is influenced by 'tempered personalism,' a term first introduced by Shimony in order to characterize the use of Bayesian tools to describe scientific investigations. This approach requires that the framework be applied locally, and prescribes an open-mindedness that sets the prior of any seriously proposed hypothesis high enough such that it may be preferred to its rivals after sufficient evidence (Shimony, 1970, p. 101).

This approach has the desirable feature of being local in an important way. When I say local, I refer to the idea that the evaluation of a hypothesis or theory should be conducted within bounds that are largely specified by the domain of inquiry. This was how Shimony pictured the framework being used in the context of scientific inference, the idea being that "the individual investigation delimits an area in which probabilities are calculated" (1970, p. 99). Many others have also expressed the importance of considering evidence in local contexts rather than trying to determine the support for a hypothesis using a background of global knowledge. For instance, Brown (2014) says,

> I believe it is methodologically preferable to follow the more detailed and constrained programs of investigation that our best local accounts suggest and see where they lead. So long as we recognize the tensions between different programs of research and follow up thoroughly when

> new results suggest a possibility of reconciliation, we do best to continue to develop and extend our best local understandings of the evidence in each area. (p. 3089)

I believe this is an accurate way to capture the reasoning being utilized at the time if we attend to the discussion of Planck's inconsistency given above.

There, I emphasized that the problem scientists had with Planck's derivation of the radiation equation was not primarily that it contained a conceptual contradiction. Even if they did think that this was the case, no one thought they could derive absolutely anything from this set of assumptions. Instead, Einstein and Jeans were both concerned that they could derive a specific result that was undesirable or contrary to observations. One might be concerned that regardless of the nature of the criticisms, it is unclear how to apply a traditional Bayesian analysis due to the contradiction. However, I think it instead points to restrictions in how we should apply the framework; namely, it points to the fact that it is a mistake to think we must use the tools of Bayesian epistemology only to represent all thought processes of an omniscient agent. Instead, it should be used as a tool in localized contexts, just as suggested in the tempered personalist account. Before I turn to this analysis, let us consider a related issue of inconsistency that has also been discussed extensively, namely that found in Bohr's first atomic model.

One might think that the assumptions Bohr used in his first atomic theory were conceptually inconsistent. However, we must consider the fact that he was able to derive a prediction that could actually be compared with experimental results. We can model our own account of what goes on in our evaluation of all the phenomena on Bohr's suggestions: the 'inconsistency' is not one that went unnoticed in his case, yet it was possible to make certain predictions about spectral phenomena that were so precise, people took the results extremely seriously. While this result will be discussed in greater detail below, I will point out here that although Bohr uses classical mechanics to describe the motion of electrons within a system, he also specifically posited that those equations of motion were applicable only in stable states, and that such stable states existed. Thus, assumptions that might seem contradictory should actually be understood as applications of classical principles that were well-known to hold in certain domains to a new but limited domain.

The above considerations lead me to posit that we should consider each of the judgments prescribed by the Bayesian framework as being made over a restricted set of background knowledge, where logical omniscience does not hold. We identify the relevant set of background knowledge for each judgment by examining the domain of applicability in question. In most cases, a domain will be explicitly limited by the assumptions in the background information. Furthermore, the background information in each case is information that is accepted in the investigation being considered for the purposes of making a judgment. This does not mean that the contents of this background information can never be questioned in the future; simply that it constitutes accepted information in the context at hand. We will see that often, the content of the background knowledge is justified in ways that make the information plausible to the majority of scientists, and so serve well as shared assumptions in a given context.

4.4 Analysis

One might be concerned about how to determine a reasonable prior for Planck's quantum conjecture when it is first introduced. Because the conjecture, which I will here abbreviate QC, came about in response to a particular experimental problem, I will first address the issue of setting a prior for QC before these experimental results were known, and then consider the impact of these results on this value.

An obvious problem with setting an intial prior value for $P(QC)$ arises from the fact that such a conjecture seems to contradict important tenets of classical physics, i.e. that energy is continuous and wavelike. This could be problematic for two reasons. First, the degree of belief one should have in a statement that directly contradicts your background information should be 0. Second, the combination of a quantum postulate and classical physics might seem to contain a contradiction from which any statement can be derived, thus making the prior degree of belief for any evidence conditional on that background 1, rendering all possible evidence equally confirmatory.

However, we have already seen how these are not problematic in the framework we are using, mainly for reasons of appropriately limiting the content of B, our background knowledge. For one thing, doing so allows us to be explicit about why a quantum conjecture does not directly contradict information in B. Moreover, we can see how identifying what is included in B allows us to limit classical theories from being all-encompassing. For instance, relevant information to be included in B might be a statement such as, "In almost every physical domain studied thus far, the behaviour of energy has been accurately described by Maxwell's equations. These equations imply that energy behaves like a wave." It is true that any subsequent theory that is developed will have to be able to account for this behaviour, and it is thus prima facie unlikely that a postulate that energy behaves as a particle will be able to do so. However, this does not altogether preclude the possibility of such a postulate, *especially* in contexts where classical theories have been known to fail, and these are exactly the contexts in which quantization was being investigated. More importantly, this is precisely the kind of attitude that tempered personalism requires, such that a seriously suggested hypothesis may overcome others in light of sufficient data. In the framework of imprecise credences, this means that it must be possible for an agent's credal state to be such that all members of C will eventually assign the hypothesis a higher degree of probability than any other hypotheses.

The above discussion provides reasons for why the initial prior for Planck's QC should not be 0. Nevertheless, this value is still not very high. What is more relevant for our purposes is a consideration of the value of $P(QC)$ in relation to the experimental results on blackbody radiation, and an articulation of B that allows us to consider a change in $P(QC)$ once Planck introduced this conjecture in this context. I claim that this is the relevant point at which to consider $P(QC)$. Although scientists would not have considered a quantum conjecture before these experiments, I have already argued that a prior value for $P(QC)$ would be very low, and we must consider instead how these experimental results made scientists take the idea of QC seriously. In other words, how would an agent update a belief in QC after these results were known.

I will turn to a determination of what needs to be included in background knowledge B in order to make a judgment about the value of $P(QC)$, where QC is Planck's initial quantum conjecture. That is, we should determine what assumptions were necessary for the evaluation of this conjecture, and to try to properly delimit the domain to which the assumptions apply. Planck's quantum conjecture can be phrased in the following way: "In the context of blackbody radiation, resonator energies must be considered in terms of discrete packets of size $h\nu$, where ν is the frequency of incident radiation, and h is a nonzero constant." We need enough information in B so as to be able to relate Planck's quantum conjecture to evidence E, the formula for the energy density u_ν of blackbody radiation.

Some tenets of classical physics must be included. This is because QC, in conjunction with some of these results, is how we derive the result u_ν.

1. Kirchhoff's Law, which states that the radiation in blackbodies is dependent purely on temperature and not on their specific material.
2. The *mean* energy of a resonator is the value calculated through classical

means.

3. Stefan-Boltzmann Law, $u = \sigma T^4$.

4. Wien's displacement law, which relates the wavelength of the maximum density of emitted radiation in a blackbody to its temperature.

Recall that these assumptions, in conjunction with the QC, allows Planck to derive the empirically correct radiation formula. Thus, the claim is that QC is supported by the particular distribution formula for radiation because it, in conjunction with these elements of background information, make it possible to derive the formula for u_ν. I claim that the above principles are legitimately included in B, and accepted for the purposes of this investigation, primarily due to their phenomenological character, that is, because they had been inferred directly from experimental results. This previous confirmation of these laws is what made them acceptable in this context, without having to reference their derivability from more basic assumptions about the nature of energy. This emphasis on experimentally confirmed results is clearly reflected in the literature on blackbody radiation.

This analysis differs from John Norton's as given in his 1987 paper, in which he argues that there is a subtheory of Planck's derivation that recovers the radiation equation and eliminates the inconsistency on the nature of resonators. He shows that from certain posited properties of radiation, one can derive the essential classical results listed above and from these, one can then derive Planck's radiation equation. While this is an extremely interesting and important result, my goal is to show that the tempered personalist framework can explicate the reasoning in a way that is closer to that which was operative at the time, which is why I have claimed that the laws I have cited should be considered a part of the background B for a rational agent.

One might object that even the inclusion of these classical laws in B implies some commitment to the idea of energy continuity since they were developed in this theoretical framework. However, I would argue that this includes far too much in terms of commitments to other elements of the classical framework. After all, we might say that fluid behaviour is accurately described by differential equations, which only apply to continuous quantities. Yet this does not mean that one need be fundamentally committed to the continuity of the fluid in question. Instead, one might accept that such a theory very accurately describes fluid behaviour at a certain lengthscale, and that one is justified in using it in certain domains. Analogously, one can accept that Maxwell's equations accurately describe mean energy values — even in all of the contexts so far observed — without being committed to the idea that the energy must literally be continuous at all possible lengthscales, let alone that such assumptions are required for a phenomenological description of observed patterns of radiation distribution. In fact, Planck says something very similar.

> Without committing myself too much to details, I may still note the following: in hydrodynamics and in the theory of elasticity, matter will almost always be assumed as arranged continuously in space, and no one finds in these considerations . . . a contradiction with the generally accepted atomic structure of bodies. . . . I am well aware that this analogy is by no means complete, but it can hardly be entirely dismissed. (quoted in Gearhart, 2002, p. 201–202)

The tempered personalist framework makes explicit how we can include a very limited set of assumptions in an investigation, so long as there is some good reason to accept them in that context.

If one then considers Planck's quantum conjecture on a background that includes the ideas given above, one can derive the distribution formula for blackbody radiation, which is confirmed by Lummer & Pringsheim's experimental results. Thus, some (though certainly not all) of the credence functions in C will change so that QC becomes more likely than its negation. Because of this shift in credence functions, the resulting credal state will no longer unequivocally say that a classical view of energy is more likely than a theory that included QC. Thus, an appropriate epistemic response at the time of Planck's introduction of his conjecture would be to have a less precise credence about QC, but in a way that makes it reasonable to pursue a theory using this result as a guide for further research.

I have been focusing on Planck's specific quantum conjecture, and I have not yet claimed that this result provides support for the general quantum postulate, QP. This is because in the context being considered, there was not as yet a strong reason to think that a more fundamental amendment to principles of classical physics would be required. The results on blackbody radiation provides the universal QP a bit of support, since it demonstrates one context where the idea of quantization is able to solve a problem. However, it might very well have been the case that the specific conjecture could have been incorporated into a theory without further applications of the universal QP. An individual agent's credences will not yield a precise judgment of the promise of QP in relation to this episode, nor will all agents agree about the significance of a general postulate. However, it is reasonable that they would disagree, and this is what we see in terms of scientists' varying attitudes regarding Planck's quantum conjecture, and its import for any future theory.

In Chapter 3 though, we saw an instance where the experimental results did support the general quantum postulate rather than the specific conjecture in question. I turn now to the Bayesian analysis of this case as an example of a correct prediction of a subsequently tested formula.

4.5 Correct Prediction

In this explication, I argue that the confirmation of Einstein's formula can be understood as support for the general quantum postulate, but that it does not distinguish between specific quantum conjectures. This helps to clarify the claim that we should consider Millikan's experiment as supporting a general QP rather than the light quanta conjecture.

The quantum postulate gained support from Millikan's experiments on the photoelectric effect due to the fact that it allowed Einstein to make a very accurate prediction about an experimental result, namely, the linear relationship of slope h/e between V and ν. Predictive power can be cashed out in a Bayesian framework with a few provisos (Maher, 2004, 72). In general, if evidence E is a logical consequence of hypothesis H on background B, then $P(H|E) > P(H)$ provided that $0 < P(H) < 1$ and $P(E|\neg H) < 1$. In our case, H is QP and E is the expression of the linear relationship between V and ν. That is, E: "In photoelectric effect experiments, the stopping voltage V and the frequency of incident light ν display a linear relationship." We have seen how Einstein's light quanta conjecture, in conjunction with other assumptions about the absorption of energy in a photoelectric substance, yields the prediction that $\frac{1}{2}mv^2 = V \cdot e = h\nu - p$ regarding the energy being emitted. This entails the linear relationship between V and ν.

In order to draw the conclusion, we must also consider whether the provisos are satisfied in this case. The first proviso states that our credence in QP must not be either 0 or 1. As I have discussed, we assume that an agent's credal state is expressed by a family C of credence functions, all expressing possible credences in QP. I argued that such a credal state would contain multiple credence functions, all of which assign value greater than 0 to QP, but distributed largely at the lower end of the spectrum. Thus, the imprecise version satisfies the first proviso. As for the second proviso, which expresses the idea that one should not be sure that the linear relationship e obtains if QP were not the case, this is also satisfied as there was debate about the nature of the relationship between V and ν which was independent of the status of QP or any alternatives. After Millikan's experiments, it was reasonable to have a credence very close to 1 for the statement E. It follows that $P(QP|E) > P(QP)$, and QP received support from Millikan's demonstration of the linear relationship between V and ν.

I now turn to a defence of my identification of the general quantum postulate as the hypothesis receiving support rather than the light quanta conjecture, especially given that Millikan's experiments are now generally taken to be good evidence for the existence of light quanta. The first reason is simply that it is a more historically accurate reconstruction. After all, Millikan is specifically testing the numerical Equation 3.1 that Einstein provides, and while this may have arisen in part from the light quanta conjecture, I have already argued for the idea that one can consider the equation promising without accepting the underlying physical idea. There is nothing in Millikan's experimental setup that requires the assumption of light quanta.

More importantly, the support here does not differentiate between alternative explanations of why the linear relationship holds. This is because, if we were to consider the support provided to a general hypothesis about light quanta by the experimental results, we would still have to include a more specific posit such as Equation 3.1 in the background information to derive the specific linear relationship being tested. Given that Equation 3.1 is by no means sure in itself, we would be hiding a crucial piece of information in the background by doing this. Furthermore, we would need to include this equation in the background of any purported account of the behaviour of the energy in order for such an account to be supported by the results. But in that case, the support does not differentiate between the light quanta conjecture and any other account of the mechanisms governing the behaviour of energy. In contrast, Einstein's equation is excellently supported by the results, since a different postulated expression of the emitted energy would not yield the linear relationship between V and ν as a logical result. This is why it is reasonable to consider Millikan's results as strongly supporting Einstein's equation, but not necessarily light quanta, and thus makes it reasonable to hold the equation as experimentally well determined, but to pursue different explanations for the result.

4.6 Conclusion

In this chapter, I have argued that a Bayesian framework making use of imprecise credences and modeled on tempered personalism is an appropriate way to reconstruct the arguments arising from the investigation of quantum conjectures. This stems from the fact that the historical development of quantum theory, which involved seeming inconsistencies and experimental anomalies in different contexts, calls for evaluation of arguments within local frameworks. The addition of imprecise credences allows for a realistic representation of agents' doxastic states. I showed how we would define background information B in the evaluation of Planck's quantum conjecture to explicate the support it received in terms of experimental results on blackbody radiation.

I also focused on the significance of Einstein's correct prediction of the relationship between V and ν, and I argued that the confirmation of this prediction provided support to the general quantum postulate. However, we have also seen that one of the great advances made in Millikan's experiments was that the reliability of the experiments and the precision of the results enabled him to determine the value of the parameter h to a very exact degree. This is crucial, as it allowed for these results to be used in a comparative way with other measurements of h through experiments in different domains. The importance of this fact will be the subject of Chapter 6, but I first turn to a discussion of the ability of the quantum postulate to account for previously known phenomena.

Chapter 5

The Quantum Postulate and Old Evidence

5.1 Introduction

In this chapter, I argue that given some formal assumptions that capture certain intuitions that are reasonable and historically borne out, we can see how the Bayesian framework yields the result that an agent should judge that the quantum postulate was supported by accounting for previously known phenomena. I consider some ways in which the accommodation of previously known information was discussed, and I show how these can be captured in the Bayesian framework if we adopt a modified version of a solution to the problem of old evidence presented by Stephan Hartmann and Branden Fitelson in their 2015 paper. The paper provides a brief discussion of their general result, but it is left an open question whether it can be applied to any genuine scientific cases. I argue that historical considerations of the applications of a quantum postulate show that this explication does not quite capture an important feature of the support in this period: namely, that the quantum postulate was able to give an account of highly anomalous phenomena. I introduce two new assumptions to mitigate this problem, and I defend their applicability in the context being discussed. The support by way of old evidence contrasts with Millikan's work, which I explicated as support by way of a confirmed prediction of a specific formula.

I first provide a brief recapitulation of the relevant phenomena, namely, the specific heat of diamond and Balmer's formula for the spectrum of hydrogen. I contrast the application of a quantum conjecture to these domains with Einstein's use of light quanta to explain Stokes's Rule in order to highlight the importance of the fact that the phenomena being explained are anomalous. I claim that an explication of a reasonable agent's thought processes should reflect this fact. I then present Hartmann & Fitelson's solution to the problem of old evidence and show how their solution can accomplish this with the addition of a few new assumptions. I defend the idea that these assumptions accurately capture the historical cases in question, and show how this results in an appropriate explication of the support provided to the quantum postulate by these phenomena. I conclude that in the framework of imprecise credences, there is no definitive answer regarding how much the evidence boosts the level of promise of the quantum postulate. However, it is reasonable to think that being able to accommodate these phenomena did indeed contribute to a judgment that the quantum postulate was promising.

5.2 Phenomena to consider

In Section 2.2.3, I discussed Einstein's treatment of the specific heats of solids and, in particular, his use of a quantum conjecture to explain the observed value of the specific heat of diamond. Recall that, before 1907, no one had considered the idea that Planck's work on blackbody radiation could be related to observations of specific heats. Einstein's quantization of the phase space of an oscillator modeling a molecule of diamond allowed him to give an account of why the specific heat of diamond was so much lower than expected. Thus, his analysis turned the observed specific heat of diamond at room temperature into evidence for the quantum postulate. The importance of this fact can be inferred from scientists' reactions to this result: in the years following Einstein's work, Walther Nernst performed new experiments on the measurement of specific heats at definite temperatures (Klein, 1965). In other words, this work prompted quick and enthusiastic uptake by Nernst and others, presumably because of its ability to account for a phenomenon that previously defied explanation by classical means.

We also saw in Section 2.2.4 that Bohr's use of a quantum conjecture was directed towards providing a preliminary theory of the structure of the atom. What is more relevant for this chapter is the fact that this account of atomic structure allowed Bohr to provide an account of Balmer's formula, which described the observed spectral emission lines of hydrogen gas when heated.

The Balmer formula is expressed as follows.

$$\lambda = B\left(\frac{m^2}{m^2 - n^2}\right) \tag{5.1}$$

where $n = 2$, m is an integer ≥ 2, B is a constant. Written in terms of frequency and explicit values for the constant, and generalized to allow for different integers for n and m, this becomes

$$\nu = \frac{2\pi^2 me^4}{h^3}\left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2}\right). \tag{5.2}$$

These precise spectral lines could not be explained on the classical theory. Indeed, one might say they were doubly anomalous. One would first expect, according to classical electromagnetic theory, that an electron orbiting a nucleus would emit energy proportional to its rotational frequency, and that this frequency would change continuously as the energy is emitted. Yet, the emitted radiation was of a number of specific frequencies, as manifested in a number of discrete lines on the spectrum. Furthermore, even if one were to accept the existence of stable states in the atom, classical physics predicts that the lines of the higher harmonics should be sums of the fundamental frequencies, whereas the observed spectral lines were expressed by Ritz in his ‘combination principle’ as differences between the harmonics (Jammer, 1966, p. 69). Therefore, this made Bohr’s accomplishment even more significant.

Although Balmer’s formula had been identified as accurately describing the observed spectral lines, there was no theoretical account of how and why these lines were produced until Bohr put forth his model of the hydrogen atom. On this model, electrons moved in stationary orbits that were determined in part by the parameter h. Upon transition from one energy level to another, an electron would emit a discrete amount of energy, $h\nu$. Bohr considered the case of a hydrogen atom, which was generally accepted as a system in which a single electron rotated around a positive nucleus of charge e. According to his earlier calculations, the binding of an electron to a positive nucleus in a transition to that stable state would result in emitted energy of amount

$$W_\tau = \frac{2\pi^2 me^4}{\tau^2 h^2}. \tag{5.3}$$

To express the amount of energy emitted by the system when transitioning from state $\tau = \tau_1$ to $\tau = \tau_2$, we have

$$W_{\tau_2} - W_{\tau_1} = \frac{2\pi^2 me^4}{h^2} \cdot \left(\frac{1}{\tau_2^2} - \frac{1}{\tau_1^2} \right). \tag{5.4}$$

This is simply an expression of the formula put forth by Balmer, where B is replaced by more specific constants. Thus, Bohr was able to use a quantum postulate to account for this previously observed phenomenon. The idea gained support when it was discovered that in conjunction with other specific assumptions, it could account for the anomalous spectral lines. Again, scientists were enthusiastic about the quantum postulate in the context of the old quantum theory despite the latter's incomplete nature once it became known that these facts were able to account for previously unexplained phenomena.

I have discussed two cases where a quantum postulate was able to account for experimental results that were already known. However, we can contrast this with another case of explanation of a previously known phenomenon, namely, Einstein's use of a quantum postulate in his 1905 light quanta paper to explain Stokes's Rule, which was an experimentally well-confirmed result dating back to 1852. Einstein considered observations of photoluminescence, where monochromatic light is changed to light of a different frequency when being absorbed and re-emitted by various forms of matter. He assumed that the original and the changed light consist of energy quanta, and that an incoming light quantum was responsible for an outgoing light quantum. By conservation of energy, he reasoned that the final energy of a light quantum would have to be less than that of an initial light quantum or, symbolically, $\frac{R}{N}\beta\nu_2 \leq \frac{R}{N}\beta\nu_1$. This is simply an expression of Stokes's Rule.

Despite the fact that Einstein's explanation of Stokes's Rule provided an account of a phenomeon that was previously known, scientists did not react with any great enthusiasm. One might argue that this is simply because most found the idea of light quanta too radical, and that such a hypothesis would need far more direct experimental evidence in order to be seriously taken up by many people. Yet, it is worth considering why this was such a contrast to the reception of Einstein's quantum postulate as applied to specific heats. Despite the fact that early applications of the postulate to specific heats could not be tested in experimentally precise ways, it was received as an extremely promising explanation of the phenomenon in question and triggered several rounds of new experiments almost immediately.

I claim that this was because the use of a quantum postulate provided an account of phenomena that were not only previously known and inexplicable by other means, but that indeed seemed to be in direct conflict with the dominant theories of the time. Such a feature makes the need for an account of the phenomenon much more urgent, and I claim this was operative in the cases of specific heats and spectral lines discussed above. I will now proceed to show how a particular account of old evidence can take this into account.

5.3 Bayesian analysis

I now turn to the task of providing a Bayesian explication of the support provided to a quantum postulate here, and defend the idea that Hartmann & Fitelson's analysis best represents the historical reasons that the promise of QP was raised by these cases. Recall that the goal of my analysis is to show that given some formal assumptions that I claim capture certain intuitions that are reasonable and historically borne out, we can see how the Bayesian framework yields the result that an agent, upon learning that the quantum postulate accounted for previously known phenomena, should raise their credence in the postulate. I will first briefly address the Bayesian problem of old evidence.

The "problem of old evidence" in Bayesian epistemology has been discussed extensively, and it is generally understood that there are several issues that might bear this name.[1] I restrict my attention to the following situation. Consider the time shortly after a theory has been formulated. In our case, we consider not a full theory, but a quantum postulate. We restrict our attention to a hypothesis H in that theory and assume other information is contained in background B. Imagine that it is discovered that a previously known fact E can be accounted for in some way by H. This newly discovered relation seems to affect the support for the theory in some way, such as in the cases I discussed above. Another classic example is found in the increased confidence in Einstein's theory of general relativity due to its explanation of the previously known precession of Mercury's perihelion. Therefore, intuitively and historically, it seems that old evidence can provide support for a new hypothesis. However, some have claimed that in the Bayesian framework, it is difficult to account for why this should be.

One issue is with the notion of what it means for evidence E to confirm a hypothesis H. Although there are several ways of explicating this process, the most basic is the idea that upon learning a piece of evidence E, an agent's degree of belief

[1] See, for instance Garber (1983), Jeffrey (1983), Christensen (1999).

in H should be revised according to the conditionalization formula,

$$Pr(H|E) = \frac{Pr(H) \cdot Pr(E|H)}{Pr(E)}, \tag{5.5}$$

where all quantities are understood to be conditional on background information B. The problem is that for any evidence E that is already known, $Pr(E)$ and $Pr(E|H)$ have the value 1, and so $Pr(H|E)$ has the same value as $Pr(H)$. Thus, any fact that is already known at the time that we are considering a new hypothesis H cannot raise our credence in H. If we think that E confirms H if and only if $Pr(H|E\&B) \geq Pr(H|B)$, then in this case, E does not confirm H. If we think that all of an agent's knowledge must be represented in the probability distribution, then E must be included in B. In this case, the problem is rooted in the assumption that an agent is perfectly logically omniscient and thus was aware of the relations between H and E as soon as theory T, which contains H, was formulated. If that is so, there is no new fact about the relation between E and H that can change the agent's credence in H. Thus, Garber (1983), Jeffrey (1983) and Eells (1990) among others discuss solutions that eliminate the assumption of logical omniscience on the grounds that it is an unrealistic representation of any agent's degrees of belief.

This is the strategy used by Hartmann and Fitelson in their 2015 solution. This solution relies on the claim that what is learned is some kind of relation, which may be logical, between a hypothesis H and evidence E, and that it is the discovery of this fact that provides support to the hypothesis.[2] This reflects the fact that what seemed to motivate scientists' further consideration of a quantum postulate was their learning that it accounted for previously known, anomalous phenomena. In Hartmann & Fitelson's approach, they introduce various formal conditions which, when satisfied, yield the result that learning that H accounts for E contributes to the support for H in an incremental sense.

The solution requires four ordinal assumptions for the support to go through:

1. $Pr(H|X\&\neg Y) > Pr(H|\neg X\&\neg Y)$

2. $Pr(H|X\&\neg Y) > Pr(H|\neg X\&Y)$

3. $Pr(H|X\&Y) > Pr(H|\neg X\&Y)$

[2] Hartmann & Fitelson discuss their solution in terms of the confirmation of a theory T rather than a hypothesis. Since I am interested in the role of QP specifically, I take it to be the hypothesis whose support is in question, and that the contents of B are accepted for the purposes of the consideration of H.

4. $Pr(H|X\&Y) \geq Pr(H|\neg X\&\neg Y)$

These statements X and Y have to do with whether a hypothesis adequately accounts for the evidence in question. X is the statement that hypothesis H adequately accounts for evidence E. Y is the statement that a different hypothesis or theory H' adequately accounts for evidence E. The proof proceeds by noting that these ordinal assumptions lead to the result that $P(H|X) > P(H)$. To see this, define the following probabilities in this way:

- $\mathfrak{a} = P(H|X\&\neg Y)$
- $\mathfrak{b} = P(H|X\&Y)$
- $\mathfrak{c} = P(H|\neg X\&\neg Y)$
- $\mathfrak{d} = P(H|\neg X\&Y)$

Also, $x = P(\neg Y|X), y = P(\neg Y|\neg X)$. The ordinal assumptions above can then be expressed as follows.

1. $\mathfrak{a} > \mathfrak{c}$
2. $\mathfrak{a} > \mathfrak{d}$
3. $\mathfrak{b} > \mathfrak{d}$
4. $\mathfrak{b} \geq \mathfrak{c}$

Now, consider the following, where $x > 0$ and $y < 1$.

$$\begin{aligned}
\mathfrak{a}x + \mathfrak{b}(1-x) &= (\mathfrak{a}y + \mathfrak{a}(1-y))x + (\mathfrak{b}y + \mathfrak{b}(1-y))(1-x) \\
&> (\mathfrak{c}y + \mathfrak{d}(1-y))x + (\mathfrak{c}y + \mathfrak{d}(1-y))(1-x) \\
&= \mathfrak{c}y(x + 1 - x) + \mathfrak{d}(1-y)(x + 1 - x) \\
&= \mathfrak{c}y + \mathfrak{d}(1-y)
\end{aligned}$$

Therefore, we have,

$$\mathfrak{a}x + \mathfrak{b}(1-x) > \mathfrak{c}y + \mathfrak{d}(1-y). \tag{5.6}$$

Now, given the definitions of $\mathfrak{a}$, $\mathfrak{b}$ and x, and the fact that
$1 - Pr(\neg Y|X) = Pr(Y|X)$, we have

$$\mathfrak{a}x + \mathfrak{b}(1-x) = Pr(H|X\&\neg Y)Pr(\neg Y|X) + Pr(H|X\&Y)Pr(Y|X) \tag{5.7}$$

By the rule of total probability, the right-hand side is equal to $Pr(H|X)$. Similarly, given the definitions of $\mathfrak{c}$, $\mathfrak{d}$ and y, we have

$$\mathfrak{c}y + \mathfrak{d}(1-y) = Pr(H|\neg X\&\neg Y)Pr(\neg Y|\neg X) + Pr(H|\neg X\&Y)Pr(Y|\neg X). \tag{5.8}$$

Again, the rule of total probability yields the fact that the right-hand side is equal to $Pr(H|\neg X)$. It follows from these that $Pr(H|X) > Pr(H|\neg X)$. This in turn entails that $P(H|X) > P(H)$ and, in fact, that the difference depends on the difference between the two sides of the inequality (5.6).

Although the conditions given by Hartmann & Fitelson are meant to be plausible enough to apply in any case, it is useful to consider what they mean in our particular contexts. The assumptions are all based on comparative probabilities of H, conditional on various combinations of ways that QP and classical theory can account for the phenomena of specific heat of diamond or Balmer's formula, or fail to do so. In the case of specific heats, we would have X and Y as the following.

- X: "QP adequately explains (or accounts for) E, the measured specific heat of diamond near room temperature."
- Y: "The assumptions of classical theory adequately explain (or account for) E, the measured specific heat of diamond."[3]

The first condition states that QP is more likely if it is able to account for the specific heat of diamond than if neither it nor any other theory could account for this phenomenon. Historically, it was this ability to explain the phenomenon that rendered it a promising postulate for many scientists, so the ordinal ranking is reasonable. The second condition says that QP is more likely if it accounts for the specific heat of diamond than if some other theory were the only one that could adequately do so. This reflects the idea that if classical theory could account for the

[3]I conduct my analysis in terms of specific heats, but the same points apply, *mutatis mutandis*, to the account of Balmer's formula by Bohr's old quantum theory.

specific heat values and QP could not, there would have been no reason to think of QP as being promising in relation to this phenomenon. The third condition says that QP is less likely in the case that only classical theory could explain the specific heat than in the case where both QP and classical theory are possible explanations of the specific heat. This seems reasonable, since even if classical theory were also able to account for the specific heat, the ability of QP to do so as well seems like it would render it at least somewhat interesting. The last condition states that QP is at least as likely if both it and another theory explain the specific heat than if no theory at all does so. Again, this seems reasonable: imagine that at the same time that QP was shown to be able to account for the specific heat, a plausible way of adjusting classical theories to account for the phenomenon was also suggested. One might not think that QP is extremely likely in this case, but its ability to account for the phenomenon should not make it *less* likely than previously.

Given these assumptions, Hartmann & Fitelson show that $Pr(QP|X) > Pr(QP)$. This means that, if one were to learn that QP accounts for the specific heat of diamond, then this raises one's degree of belief in QP. In fact, we can say something further. Let us consider the difference between $Pr(QP|X)$ and $Pr(QP)$:

$$Pr(QP|X) - Pr(QP) = Pr(\neg X)[Pr(QP|X) - Pr(QP|\neg X)]. \tag{5.9}$$

Let us suppose that the conditional probabilities for QP on X and $\neg X$ are fixed, regardless of the value of $Pr(X)$. Then the amount to which one's degree of belief in QP is raised is proportional to $Pr(\neg X)$, or inversely proportional to $Pr(X)$. This means that if prior to learning definitely that QP accounts for the specific heat, one already thought that this was likely to be the case, then learning this fact does not add as much to the support for QP as if one were more skeptical of the idea that QP could account for this phenomenon. In our particular case, it does appear that it would be relatively surprising that QP was able to account for the specific heat of diamond. This is, in part, due to the fact that the postulate itself was not obviously reconcilable with successful aspects of classical physics. What made it even more surprising was its application to a context that seemed unrelated to any of the contexts in which it was previously successful, i.e. to a theory of energy in matter rather than the context of energy transmission between radiation and matter. Thus, it seems reasonable that Einstein's demonstration that a quantum postulate could account for certain aspects of the specific heat of diamond was a not insignificant factor in raising scientists' degrees of belief in this hypothesis.

One might be worried that this implies that a hypothesis H is not supported by its account of previously known information that is not anomalous. However, it is important to note that the situation here is specifically about conditionalization upon learning that a relation exists between H and E, and thus does not affect the prior value of $Pr(H)$. Presumably, this is determined at the time of the initial advancement of H, and at this point, such a hypothesis is usually only suggested if it is able to take into account much of the same facts as the original theory. This explication may not be appropriate in contexts where a theory is explicitly designed to account for an anomalous phenomenon, but I claim that it is an accurate explication of the cases I discuss.

Although X and Y are to be conditionalized upon in the ordinal judgments, I am also concerned with the matter of credences in X and Y, so it is worth considering what other assumptions should be included in B in order to generate these credences. In the case of specific heats, we need to include the following assumptions, as taken from Einstein (1907, p. 219):

1. The atoms of a solid can be modelled as sinusoidal oscillators about equilibrium positions.
2. The molecular kinetic equation for energy of the oscillators can be applied.
3. The phase space of an individual oscillator of frequency ν can be quantized in areas $h\nu$.
4. Light atoms vibrate at higher frequencies.

The combination of these yield the fit to the equation for specific heat of diamond, which generates a high credence in X.

I now turn to the significance of anomalous phenomena I discussed in the previous section. Hartmann & Fitelson do not address this issue, but I have argued that an important aspect of the situation was that learning that a proposed theory accounts for a previously known phenomenon does not always seem to be evidence for that theory. The cases where we put the most weight on old evidence are those where it was believed that this evidence was not accounted for by any previous theory. In the classic example of general relativity being able to account for the precession of the perihelion of Mercury, it was intuitively seen as highly significant that Einstein's theory could account for this phenomenon, whereas being able to account for the observations that Newton's theory also covered would be expected of any competing theory, and would not support the new theory to a degree beyond the degree of support for the previous one. In the cases of both specific heats of diamond at room temperature and the spectral lines expressed by Balmer's formula, we have seen that historically, it was an important feature that classical theory yielded predictions that were in direct conflict with observations of this phenomenon.

I take this fact as an indication of certain conditions that a solution to old evidence should satisfy. One important one is that an agent's credence in a hypothesis should be boosted the most in cases where one learns that it can account for a phenomenon that previous theories could not account for. In other words, the incremental support for H (or in our case QP) should vary with an agent's credence in $P(Y)$. In Hartmann & Fitelson's solution, we see that one way that such a correlation holds is in the case where two separate conditions are satisfied:

1. $\mathfrak{a} - \mathfrak{c} > \mathfrak{b} - \mathfrak{d}$
2. $P(Y)$ and $P(X)$ are independent.

When the latter condition holds, $P(\neg Y|X) = P(\neg Y|\neg X)$, so that $x = y$. When this is true, we see that Equation 5.6 becomes $\mathfrak{a}x + \mathfrak{b}(1-x) > \mathfrak{c}x + \mathfrak{d}(1-x)$. If the first condition holds, the difference between the two sides of the equation varies with x, as desired. I argue that these conditions are satisfied in cases we would intuitively consider accounting for old evidence to have contributed to an agent's credence in a hypothesis.

First consider the assumption that $P(Y)$ and $P(X)$ are independent. $P(Y)$ refers to the credence an agent has in the proposition that classical theory accounts for the specific heat of diamond at room temperature, while $P(X)$ is the credence an agent has in the proposition that a quantum postulate accounts for the specific heat of diamond at room temperature. Before the quantum postulate was ever applied to this phenomenon, it was known that classical theory could not account for the specific heat, and so $P(Y)$ was very low. The subsequent use of the quantum postulate had no bearing on this fact. Thus, it is reasonable to think that at least in this case, these two probabilities are independent. Similarly, in the case of Balmer's formula, Bohr's introduction of old quantum theory to provide an account of this formula did not change a low credence in the proposition that classical theory could account for these spectral lines.

One might be concerned about this condition since the relations between $P(X)$ and $P(Y)$ are features of the probability space. Thus, there is perhaps simply a fact of the matter that a low credence in Y would affect the credence in X, and so we cannot assume independence. However, in the situation we are considering, we have explicitly eliminated the assumption of logical omniscience of the agent. This means that we can assign values to $P(X)$ and $P(Y)$ that make sense for an agent in that context, and at the time point that we are considering, it is reasonable to consider the two credences as being independent. After all, even if it was known that the classical theories did not account for the phenomena, the quantum postulate modifies a very specific aspect of classical theories. Presumably, there are countless other ways to modify classical theory that would result in hypotheses that would not account for the specific heat. Of course, this independence condition will not always hold: for instance, after the formulation of Bohr's correspondence rule, the ability of QP to account for phenomena in certain domains would be known to depend on the ability of certain classical descriptions of phenomena. Such relations would have to be evaluated with the individual cases in mind.

Now consider the condition that $\mathfrak{a} - \mathfrak{c} > \mathfrak{b} - \mathfrak{d}$. This is the same as a condition that $\mathfrak{a} - \mathfrak{b} > \mathfrak{c} - \mathfrak{d}$. The difference $\mathfrak{a} - \mathfrak{b}$ is the difference between $P(H|X\&\neg Y)$ and $P(H|X\&Y)$. The difference $\mathfrak{c} - \mathfrak{d}$ is the difference between $P(H|\neg X\&Y)$ and $P(H|\neg X\&\neg Y)$. I claim that it is reasonable to expect the first probability difference to be larger than the second.

This is because in the first case, we are assuming that H accounts for a phenomenon, and the difference relies on whether there is another hypothesis that does the same. In the second case, we are evaluating the probability of a hypothesis conditional on the fact that the hypothesis does not account for the phenomenon in question. It seems that the presence of an alternative explanation is meaningful in the first case, but not in the second. After all, when a hypothesis accounts for evidence, a reasonable thing to ask is whether there are alternative explanations for the phenomenon. But if a hypothesis does not account for the evidence in question, the latter fact seems to be irrelevant. In our particular case, this expresses the idea that the quantum postulate's ability to account for the specific heat of diamond when classical theory could not was significant. In contrast, credence in a quantum postulate that did not explain the specific heat would be low, regardless of whether classical theory could account for specific heat or not. Therefore, we would expect that the difference between $\mathfrak{c}$ and $\mathfrak{d}$ is much smaller than any difference between $\mathfrak{a}$ and $\mathfrak{b}$.

When the two above conditions are satisfied, the difference between $P(H|X)$ and $P(H)$ will vary with $P(\neg Y)$. Particular numbers will depend of course on the priors an agent assigns to all the conditional probabilities. However, this dependence captures the fact that accounting for something like specific heat, where $P(\neg Y)$ is very high, will provide relatively more support for QP than accounting for a phenomenon like Stokes's Rule, where $P(\neg Y)$ is considerably lower. The foregoing analysis is important because Hartmann & Fitelson's solution provides only a general formal outline. While it yields what seems like a desirable result, it was unclear whether it truly captured cases we would have wanted it to. I have argued that it can, and clarified the conditions under which accounting for a previously known phenomenon is considered confirmatory, showing how this fits into their suggested solution.

I will now consider how this analysis can be adjusted to be applicable in a framework of imprecise credences. Recall that an agent's credal state is composed of a collection of credence functions. Definite statements about an agent's credal state can only be made when all of the credence functions constituting C agree about a particular fact. Hartmann & Fitelson's result holds for any situation where the ordinal assumptions hold. Now, the prior values of $Pr(H)$ will differ from one credence function to another, as will the conditional probabilities $Pr(QP|X)$, so it follows that the difference between $Pr(QP|X)$ and $Pr(QP)$ will vary between the credence functions. It will thus be impossible to say how much support is provided to QP when X is learned, since there is no agreement across credence functions. However, so long as the conditions are satisfied by every member of C, the result that $Pr(QP|X) > Pr(QP)$ will hold in every one of those credence functions, and so we can say that an agent's credence in QP should be increased when learning X. Of course, this increase in promise of QP does not task every scientist with the pursuit of this postulate, since reasonable agents will differ on the level of promise assigned QP, even if all agree that they have learned that QP accounts for the specific heat. However, this is to be expected in the context of pursuit, since it is a feature and not a flaw that scientists pursue various lines of inquiry. Furthermore, the generality of the conditions is intended to make them universally acceptable, and, as I have argued, accurately capture the situation with specific heats. However, it is possible that for some particular agent, the credence functions comprising credal state C may not satisfy those conditions. Nevertheless, this would be a minority view, and is certainly permitted in the context of pursuit.

5.4 Conclusion

I have argued that a particular Bayesian explication can reveal how accounting for previously known phenomena provides support to a quantum postulate. Furthermore, by considering some historically motivated conditions, we are able to see how this formal result can reflect the importance of accounting for anomalous phenomena as opposed to non-anomalous results.

There are several reasons that this explication of the situation is appropriate. The introduction of statements X and Y reflect the importance of learning that a postulate can account for a given phenomenon, which I have argued is the best way to understand how scientists perceived the importance of the quantum postulate in the contexts being considered. Furthermore, there is no strict requirement on how X is satisfied: the phrasing is purposely sufficiently broad such that it allows for the way in which I have been considering the quantum postulate, as a postulate that does not necessarily have a firm physical interpretation, but that can nevertheless yield results when combined with other pieces of background knowledge.

This way of approaching the problem works well in a tempered personalist framework, where we are not assuming logical omniscience of the agents. Instead, we consider statements on an explicitly defined background B. As I have argued, including certain elements in B allowed scientists to consider X acceptable, that is, that QP in conjunction with the contents of B did account for the phenomenon under consideration. In this explication, I accepted that X is satisfied, but I argued for a role for $P(Y)$, since Y was not satisfied. This explication thus shows that phenomena that are not clearly anomalous in a given framework provide less support for a new hypothesis.

Chapter 6

Unification and the Quantum Postulate: Agreeing Measurements of Planck's Constant[1]

6.1 Introduction

In previous chapters, I have mainly discussed the support for quantum conjectures in the context of the individual domains in which these conjectures were being applied. In this chapter, I provide an analysis of the unificatory power of the quantum postulate. I argue that we can understand the experiments in different contexts as providing measurements of the parameter h. In a Bayesian framework, we can interpret each of these measurements as constraining the value of h and thus yielding information about the quantum postulate that can then be applied in other domains. This way of explicating unificatory power yields the result that the quantum postulate received support over and above that from each of the individual experiments in virtue of its ability to make the different domains relevant to one another.

I begin by outlining some of the ways unification has been characterized in the literature, then present the Bayesian notion of informational relevance that I will be defending. I provide a brief overview of the phenomena that were being unified, focusing in particular on how they provided measurements of h. I present an analysis in the tempered personalist framework I have been using to argue that the

[1]A version of this chapter has been published as "Unificatory Power in the Old Quantum Theory: Informational Relevance of the Quantum Hypothesis," *Philosophy of Science*, 82(5), 1200-1210.

quantum postulate received support from this unificatory power. Finally, I draw some conclusions for theory pursuit based on this analysis.

6.2 Unification

The idea that unification is a virtue of a scientific theory has a long history in philosophy of science, and has been presented in several guises. Accounts range over those focused primarily on the common causal origins of various phenomena to those emphasizing the importance of a common explanatory basis. Of course, these are not mutually exclusive ideas and a combination of these elements is common. William Whewell, for instance, emphasizes what he calls the Consilience of Inductions in his *Novum Organon Renovatum* (Whewell, 1989). An important part of Whewell's conception of science is the practice of providing a mathematical description of phenomena, which he calls an 'induction' due to scientists' addition of a formula to the characterization of observations. A Consilience of Inductions occurs when an induction obtained from one class of facts is found to apply to another class of facts, even though the two previously seemed to be unrelated. Whewell takes this to be a good indicator of probable truth. He says,

> [T]he evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence (Whewell, 1989, p. 153, emphasis in original).

Thus, he argues that when a hypothesis is able to explain or predict facts of a kind that were not used in its generation, the evidence for that hypothesis is stronger than it was before on the grounds that it could not be mere coincidence that two disparate phenomena are explained by the same hypothesis. Instead, it is much more likely that the hypothesis does actually apply in some way to all the phenomena it explains.

A more recent version of this argument is given by Michel Janssen, in his work on common origin inferences (2002). Janssen coined the term COI Stories, an acronym for common origin inferences, which he considers a subspecies of Inferences to the Best Explanation. Janssen explains what he means by a COI thus:

> A COI, like an IBE, can be defined as an inference from a statement of the form "If it were the case that X, then that would explain observations/phenomena a, b, c, ..." to "It is, in fact, the case that X." The distinguishing characteristic of a COI is that X – a statement, a model, or an idea ... – wields its explanatory power by tracing a number of otherwise puzzling coincidences ... to a common origin (Janssen, 2002, p. 464).

COIs thus arise out of what Janssen calls common-origin explanations, which are hypotheses or theories that tie together multiple phenomena. He believes that most COIs will be common-cause inferences, in which some kind of causal structure is posited as being responsible for several phenomena. Of course, the notion of 'causal structure' here is quite broad, possibly referring to some event or substance with causal efficacy, or a causal structure or mechanism broadly construed (Janssen, 2002, p. 12). The latter could presumably include such things as physical laws described by some mathematical structure, and so does not restrict a common-cause argument to traditionally conceived causal relations.

Janssen is concerned to argue that if a hypothesis is able to explain the occurrence of multiple previously unrelated phenomena, the hypothesis is thereby conferred *epistemic* warrant. He tries to show that a theory's possessing explanatory power does generally count as evidence for that theory.[2] Janssen marshals a variety of historical and scientific examples to support his claim that such COIs have played a prominent role in the history of science, including the argument for a Copernican system, Darwin's argument in the *Origin of Species*, Newton's argument for universal gravitation, and the move to special relativity.[3] Janssen's primary purpose is to show that there is a link between explanatory power and evidentiary warrant. However, at the end of the paper, Janssen points out that there is still a question of whether there is any epistemic force behind COIs as

[2] Here, theory stands as shorthand for anything between a well-formulated scientific theory, to a loose hypothesis.

[3] Interestingly, Janssen says that his COIs are analogous to Whewell's Colligation of Facts rather than the Consilience of Inductions. This is because the former refers to a process similar to finding a formula that describes particular observations of phenomena. Since individual observations are already separate instances, they must be unified by some conception. Finding a mathematical formula that captures the behaviour of the observed entities could be seen as finding a causal structure, in a very loose sense of the term, on Janssen's view. Whewell's Consilience of Inductions is instead comparable to "meta-COIs," inferences that posit a higher-level unification between two or more COIs.

opposed to their being of mere pragmatic value, and what this force might consist of. One such answer to this question is given by Wayne Myrvold, in his 2003 work on Bayesian unification.[4] I will argue that the epistemic force in this case can be seen as one of informational relevance.

In his paper, Myrvold shows that on a particular understanding of what it means for a hypothesis to unify phenomena, its ability to do so contributes directly to its support. He begins with a common definition of the *informational relevance* of a proposition P_1 to another proposition P_2, conditional on background B,

$$I(P_2, P_1|B) = Log_2 \frac{Pr(P_2|P_1\&B)}{Pr(P_2|B)}. \tag{6.1}$$

He then defines the quantity U as a measure of the extent to which a hypothesis H unifies P_1 and P_2,

$$U(P_1, P_2; H|B) = I(P_2, P_1|H\&B) - I(P_2, P_1|B). \tag{6.2}$$

This generalizes straightforwardly to a set of propositions $P_1 \ldots P_n$. He then shows that on two common candidates for the degree to which evidence supports a hypothesis, the quantity U contributes directly to the support of H by the evidence. Consider, for instance, the "degree of confirmation." This is simply one way to measure the degree of support for a hypothesis, but a similar result holds if one takes an alternative measure, Good's "weight of evidence." If we now consider evidence statements E rather than propositions P, the "degree of confirmation," measured by $log_2 \frac{Pr(H|E)}{Pr(H)}$, is identical to the definition of the informational relevance of E to H, so we can consider the informational relevance as a measure of evidential support. From Bayes' theorem, and the definition of informational relevance which is additive, one obtains for the informational relevance of pieces of evidence E_1 and E_2 to hypothesis H,

$$I(H, E_1\&E_2|B) = I(H, E_1|B) + I(H, E_2|B) + U(E_1, E_2; H|B). \tag{6.3}$$

Myrvold explains the significance as follows. "[T]he degree of support provided to H by E_1 and E_2 taken together is the sum of three terms: the degree of support of H by E_1 alone, the degree of support of H by E_2 alone, and an additional term

[4]Other related conceptions of unification can be found in Philip Kitcher's work on explanatory unification (1989) and William Wimsatt on robustness (1981). While the details of such discussions differ, it is clear that some version of this notion has played a role in several important episodes of scientific theorizing.

which is simply the degree of unification of the set $\{E_1, E_2\}$ by H. An analogous result holds for larger bodies of evidence" (2003, 412). Thus, the ability of a hypothesis to unify previously unrelated phenomena contributes directly to the likelihood of that hypothesis given the evidence.

In what follows, I will provide details of how the case under consideration provides an example of this feature. Briefly, the physical phenomena to be discussed were not clearly relevant to one another before considering the general quantum postulate. However, on the assumption of such a hypothesis, numerical values of quantities obtained from observations of those phenomena could be used to calculate the value of Planck's constant, h. All of these calculations agree to within an order of magnitude. The measured value of h via one type of phenomenon thus provided information about the measured value of h via a different phenomenon when assuming the quantum postulate. This captures a type of unificatory power that was epistemically significant and contributed to the justification of the pursuit of various theories that incorporated the idea of quantization, even in the absence of a well-developed quantum theory.

6.3 Informational relevance

Because this explication of unification is a Bayesian analysis, it will be important to be clear about the types of assumptions that are being included in the background information B. In the overview that follows, I will focus on the assumptions we should consider to be included in B in order to make inferences about the numerical value of h. I argue that the background information should be considered as including mainly claims about the function of the measurement apparatuses being used, and the mathematical expressions of phenomena inferred from observations. In this way, we see how the general quantum postulate was being confirmed with respect to this shared background that was accepted for the purposes of the context in question.

6.3.1 Blackbody radiation

As we have seen in Section 2.2.1, Planck's quantum conjecture was put forth in order to arrive at the empirically well supported radiation formula, Equation 2.1. He introduced the notion of an 'energy element' ϵ, of size $h\nu$, and then rewrote the previous equation in terms of wavelength,

$$E = \frac{8\pi ch}{\lambda^5} \frac{1}{e^{ch/k\lambda T} - 1}. \tag{6.4}$$

He was then able to use this empirically confirmed radiation formula to estimate the size of h. He used his formula to calculate the amount of radiation in air, and compared this with values obtained by Ferdinand Kurlbaum in his experimental work (1898). He then drew on observations made by Otto Lummer and Ernst Pringsheim, who were able to determine the wavelength of the maximum energy in air of blackbody radiation. The result was a numerical value for the parameter h, $h = 6.55 \cdot 10^{-27} erg \cdot sec.$[5]

I stressed in earlier chapters that Planck was focused on providing an account of observationally motivated descriptions of phenomena using a general model of 'resonators' while hoping that electron theory would later be able to fill in the gaps, so to speak, on how such mechanisms were taking place. As Gearhart has pointed out, Planck repeatedly stressed the need for a physical interpretation of the constants he introduced (2002, p. 200). Despite this, we can still see how the quantum postulate had unificatory power by examining its application in other phenomena. It is important to note that in the following presentations, I present examples where Planck's constant is invoked in various ways, often as a starting assumption allowing one to derive results. Thus, even though it is important for my argument that results of experiments can be construed as ways of measuring the value of h, scientists were for the most part neither directly trying to determine the value of h, or even taking the existence or value of h as a primary hypothesis in an H-D scenario. Nevertheless, the reconstruction I provide is based solely on information that was available at the time, and so the epistemic feature of unification was applicable whether it was explicitly recognised or not.

6.3.2 Light phenomena

I have already discussed Einstein's introduction of the light quanta conjecture, and Millikan's subsequent investigation of the photoelectric effect. While Millikan's work is clearly significant for its measurement of h, in this section, I will consider in more detail Einstein's original treatment of the photoelectric effect, and how it too can be taken as a preliminary constraint on the value of h.

[5] Error values were not reported in Planck's 1901 article.

Recall Einstein's Equation 3.1,

$$\frac{1}{2}mv^2 = V \cdot e = h\nu - p, \tag{6.5}$$

describing the relation between the potential required to stop electrons from escaping a metal surface, and the frequency of the incident light. We can consider this equation in terms of our discussion of informational relevance due to the fact that the experiments done on the photoelectric effect yield information about the size of h. One can derive a relation between the energy of electrons and the size of h based on the kinetic energy of the electrons being emitted. Einstein reasoned that $VE = Rh\nu/k - p$, where the body under investigation is charged to positive potential V, E is the charge of a gram equivalent of an ion, and p is the potential of negative electricity.[6] Experiments on the photoelectric effect provided observed values for the unknowns in this relation. Known quantities could then be inserted into this formula: R is a known constant, $E = 9.6 \cdot 10^3, P' = 0, \nu = 1.03 \cdot 10^{15}$. The value of ν corresponds to frequencies of ultraviolet light, and the other values are given for an experimental setup. The order of magnitude of V according to Lenard's results was 10^7. Einstein calculated the theoretical value of VE according to his theoretical assumptions, and found that his theoretical value of V was in good accord with the experimental results of Lenard. This provided a constraint for the value of h even though at the time it could only have been given within an order of magnitude. Because Einstein's β was equivalent to h/k and the order of magnitude of β had 10^{-11}, the measured value of a body's resistance in cases of the photoelectric effect constrained h to be of order of magnitude 10^{-27}.[7]

Let us now explain how this fits into the Bayesian framework by determining how the various experiments provide information about h. First, note that by beginning with the quantum postulate, one can calculate the average energy of the resonators Planck was considering in order to obtain the radiation formula Equation 6.4. However, this equation refers only to the form of a family of equations, where the value of h is not yet determined. Thus, let E_1 be the proposition expressing the results of Lummer and Pringsheim's work determining the maximum wavelength of blackbody radiation in air at a given temperature, "$\lambda_m T = 0.294 cm \cdot K$." Let E_2 be

[6]Einstein's original notation is $\Pi E = R\beta\nu - P'$, which I have changed for consistency. However, I follow Einstein in discussing quantities in terms of 'gram equivalents'.

[7]This is not the structure of Einstein's reasoning: he hypothesizes from light quanta to the result that the resistance should be a certain value, then confirms that this matches the experimental value.

the proposition that an experiment on the photoelectric effect would yield a result such that V is of the order of magnitude 10^7. From E_1, in conjunction with QP as applied in deriving Equation 6.4, we can calculate that $h = 6.55 \cdot 10^{-27} erg \cdot sec$. Similarly, the results of E_2 in conjunction with QP yield the result that h is of the order of magnitude 10^{-27}.

Before continuing with the analysis of the unificatory power of QP, I will consider how we can define background knowledge B to make such a judgment. In the case of blackbody radiation, I presented the assumptions necessary to derive the radiation equation in Section 4.2, and I argued that these assumptions were supported mainly by experimental results in the case of phenomenological laws such as Wien's displacement law and the Stefan-Boltzmann law, and did not include any fatal inconsistencies. It is thus possible to combine this information with the information required to infer a result in the case of the photoelectric effect. In the latter case, these assumptions require the assumptions that electrons within a metal can escape the surface with a certain amount of work, and that additional electron energy is manifested as kinetic energy. Since I am considering the unificatory power of the general quantum postulate, background information B would also include specific posits about how the general postulate is applied in each context: in the context of blackbody radiation, we would have a specific posit about the quantum postulate defining the size of energy elements, and in the case of the photoelectric effect, a posit that the quantum postulate determines the energy of individual quanta of light.

To return to the analysis of unification, we see that before the suggestion of QP, there was no way to use E_1 to yield information about E_2. Thus, the informational relevance of E_1 to E_2 on background B, given by Equation 6.1, was very low. After all, there was no way that Lummer & Pringsheim's experiments on blackbody radiation would be thought to constrain the behaviour of cathode rays, so $Pr(E_2|E_1\&B)$ should be the same as $Pr(E_2|B)$, thus assigning $I(E_2, E_1|B)$ the value 0. Compare this with the informational relevance value on the assumption of QP along with background B. This is given by the expression

$$I(E_2, E_1|QP\&B) = Log_2 \frac{Pr(E_2|E_1\&QP\&B)}{Pr(E_2|QP\&B)}. \tag{6.6}$$

The value of $Pr(E_2|QP\&B)$ is the probability that Lenard's results would obtain, which does not have a particularly high value if considered against a general background. However, once we consider E_1 as well, we can calculate a value for h

from the blackbody spectrum, thus constraining the value we would obtain from experiments on the photoelectric effect. This yields a very high value for $Pr(E_2|E_1\&QP\&B)$, arguably a value very close to one, thus making the value of the information relevance of E_1 to E_2 quite high.[8]

Now recall that the unificatory power of QP is given by Equation 6.2, which measures the difference between the relevance of E_1 to E_2 when including QP in the background knowledge, and excluding it. This nonzero value contributes directly to the degree of confirmation of QP by E_1 and E_2 as measured by Equation 6.3. Thus, by positing behaviour of radiation in terms of quanta of size $h\nu$, the form of the blackbody radiation spectrum constrained possible values of measurements conducted on the phenomenon of the photoelectric effect by providing information about the size of h. We see that QP gained confirmational power not only from the results of the individual experiments, but directly from the way in which it made these phenomena informationally relevant to one another.

6.3.3 Spectral phenomena

A quantum postulate and the value of h were also crucial in early characterizations of the structure of the atom, as well as the behaviour of line spectra. I have already discussed some of the details in Sections 2.2.4 and 5.2. In the latter, I described the process by which Bohr was able to account for the Balmer formula describing the emission spectrum of hydrogen gas. In this section, I explain how we can interpret the observation of these lines as providing information about the value of h.

Balmer's formula 5.1 had no known connection with the other phenomena discussed above until Bohr developed his model of the atom. We have seen how Bohr calculated relations between several observable quantities based on Planck's radiation theory utilizing h. We can reinterpret this as a way to turn the observed line spectra into information about the size of h by including in background knowledge B the fact that Balmer's formula could be used to describe emission spectra. According to Bohr's calculations,

$$\frac{2\pi^2 me^4}{h^3} = 3.1 \cdot 10^{15}. \tag{6.7}$$

The observed value was $3.290 \cdot 10^{15}$, which we see is remarkably close to Bohr's

[8]Again, this epistemic analysis does not conform in structure to the historical development of these ideas, as Planck worked backwards from the radiation formula to infer a quantum conjecture. Nevertheless, I claim that this logical structure holds for the experiments I address.

calculated value as given in Equation 6.7.

We can reverse the calculation in order to see how such an experiment would have constrained the value of h. We use the same experimental values that Bohr used for the charge of the electron $e = 4.7 \cdot 10^{-10}$ and the ratio of the charge to mass $e/m = 5.31 \cdot 10^{17}$, as well as the observed value of $3.290 \cdot 10^{15}$ and solve for h in the expression above. The result is $h = 6.38 \cdot 10^{-27}$. Thus, we see how Balmer's formula carried information about the size of h, which was also given by the blackbody spectrum.

In order to make the informational relevance explicit, let us take E_1, as above, to be the statement of Lummer & Pringsheim's results on the maximum wavelength of blackbody radiation in air, $\lambda_m T = 0.294 cm \cdot K$. Let E_2 here be that the value on the left side of Equation 6.7 takes on a value around $3.290 \cdot 10^{15}$. As before, a value of this constant without the assumption of QP could a priori have taken on an infinite range of values, and the result of measurements on blackbody radiation would not be expected to be informative about this. Thus, the informational relevance of E_1 to E_2 was low, if not zero. However, by assuming QP, the blackbody spectrum provides information about the size of h, thus constraining the possible values that the constant could take. This makes it much more likely that the value of the constant should be the one found, assuming that values close to the one calculated using Planck's radiation theory would be more likely than those that do not provide numerical agreement. This makes the informational relevance of E_1 to E_2 quite high on the assumption of QP, in contrast to its value without the assumption of QP. This yields a nonzero value for the unificatory power of QP with respect to E_1 and E_2, again contributing directly to the degree of confirmation of QP by those phenomena. Here, we add to background knowledge B Bohr's specific assumptions as outlined in Section 5.2, and a statement linking the general quantum postulate to the specific application to stable atomic states.

After Bohr's success with the hydrogen spectrum, other phenomena related to atomic spectra were used as explicit tests for the value of h. James Franck and Gustav Ludwig Hertz performed experiments on the energy of electrons colliding with molecules of an inert gas or metal vapour (1914/1967). In particular, their experiments with mercury vapour were able to help determine the value of h. This is interesting because, as Gearhart (2014) points out, Franck and Hertz did not mention Bohr's atomic theory in their initial work. However, it was later reinterpreted in the context of Bohr's theory, and we can see how this can be understood as using these experimental results to yield more information.

In these experiments, electrons of a certain kinetic energy were introduced into mercury vapour. It was known that at relatively high energies, the mercury gas became ionised. However, below this level but at certain energy thresholds, the electrons lost their kinetic energy; this was attributed to inelastic collisions between the free electrons and those bound to mercury atoms. The fact that these only occurred at discrete levels of energy of the introduced electrons was evidence for the idea that the mercury gas atoms could only absorb energy in those discrete quantities. These energy levels corresponded to the observed spectrum lines emitted by mercury gas.

Since the experiment involved only quantities that were pre-determined or measurable such as the energy of the introduced electrons, the voltage drop corresponding to the loss of the electrons' kinetic energy, and the frequency of emitted energy in the spectrum, these results were used to calculate a value for Planck's constant. Franck and Hertz calculated that h had the value $6.59 \cdot 10^{-27}$. An analysis of the informational relevance of this experiment can be made analogously to the ones given above.

6.3.4 Summary of Informational Relevance

I have presented several phenomena that were unified by the quantum postulate by emphasizing the importance of constraining and measuring the numerical value of Planck's constant, which was an integral feature of QP. Below is a table summarizing the values obtained from each of the phenomena discussed above. I also include the value of h Millikan calculated in his (1916), discussion of which is given in Chapter 3.

Summary of calculated values of h

Phenomenon	Value in $erg \cdot sec$
Blackbody radiation	$6.55 \cdot 10^{-27}$
Light quanta	Order of 10^{-27}
Photoelectric effect	$6.56 \cdot 10^{-27}$
Hydrogen emission spectrum	$6.38 \cdot 10^{-27}$
Mercury gas resonance radiation	$6.59 \cdot 10^{-27}$

These measurements are significant because they demonstrate the idea that various observations, understood in terms of constraining information about a parameter, are able to render previously unrelated phenomena relevant to one another by yielding information implicitly contained in those observations. By increasing the informational relevance of each phenomenon to the other, the unificatory power of the quantum postulate is raised. The above discussion considered only pairwise informational relevance relations, but the generalization to several phenomena yields the following (taking each E below to represent the results of experiments from the five phenomena listed in the table):

$$\begin{gathered} U(E_1, E_2, E_3, E_4, E_5; QP|B) = \\ I(E_1, E_2, E_3, E_4, E_5|QP\&B) - I(E_1, E_2, E_3, E_4, E_5|B). \end{gathered} \tag{6.8}$$

Thus, the unificatory power of the quantum hypothesis from the Bayesian perspective is nonzero, and the degree of confirmation of QP is increased not only by the individual phenomena, but by the fact that it makes those phenomena relevant to one another. This provides epistemic justification for pursuing the quantum hypothesis beyond the support the hypothesis gained individually from each phenomenon discussed.

It is relatively straightforward to understand this result in the framework of imprecise credences. Recall that an agent's credal state is represented by a family C of credence functions, each of which assigns a degree of belief to the general quantum postulate, based on the background information B I outlined above. Thus, an agent's credence in QP will be imprecise, since the different members of C will likely assign different values. However, the Equation 6.8 tells us that QP gains support from its unificatory power. Given that all of the experimental results yield values for h on B, it will be the case that any credence function in C will assign nonzero informational relevance to the E_i's, and thus that support for QP will be raised in virtue of this unificatory power for each credence of C. Since an agent with imprecise credences makes a judgment just in the case that all the members of C agree about some fact, we obtain the result that the agent's credence in QP should be raised due to its unificatory power. While there will likely not be a definite value by which $Cr(QP)$ is raised, this shows that informational relevance provides support to a quantum postulate, even for an agent who has an imprecise credence in the postulate. In the next section, I will draw some conclusions for the process of theory pursuit based on this interpretation of unification.

6.4 Further considerations

This analysis provides a formal explication of the fact that the quantum postulate received support in virtue of its ability to unify various phenomena, and it is worth noting that this type of argument was also historically considered to be of value. I claim that this arises partly from the fact that alternative methods of justification are often not available in early stages of a scientific theory's development. For instance, Norton details arguments given by Poincaré and Ehrenfest, in which they infer from observed phenomena to the result of quantum discontinuity (1993). These are obviously very strong reasons to take a quantum postulate seriously. However, due to the complex nature of such arguments, it is almost certainly the case that they would not have been undertaken without prior evidence of the desired result. In general, the ability to give a deductively valid argument for a hypothesis only follows the stage at which pursuit of the hypothesis is based on other reasons, and a unificatory argument can provide good epistemic warrant for designating a hypothesis promising before deductive justifications are available. Indeed, these arguments were not given until 1911 or later, despite the appearance of the quantum hypothesis as early as 1900. Moreover, James Jeans explicitly discusses Poincaré's argument and is aware of its significance for quantum discontinuity. Yet, in his 1924 *Report on Radiation and the Quantum-Theory*, he cites the determination of the value of h via multiple methods as important evidence for the necessity of some form of quantization. It is noteworthy that Jeans evidently still found it beneficial to present this unificatory argument, even with knowledge of a more traditional deductive justification.

Jeans's work also leads me to my next point, which is that this type of unification argument also underlines the importance of the consideration of a general quantum postulate rather than the specific quantum conjectures. Jeans stresses repeatedly that the physical interpretation of quantization is not clear merely from the mathematical descriptions of phenomena.

> It seems then to be abundantly proved that the transfer of energy must in some way take place by jumps or jerks of amount $\epsilon = h\nu$, but mathematical analysis gives no indication as to the physical nature of these processes. The physical problem as to when, where and how the jumps occur can be solved with much less certainty than the mathematical problem, of which the solution has predicted the occurrence of the energy jumps with a high degree of certainty. (Jeans,

> 1924, p. 29)
>
> [The laws] give no indication at all as to the seat of these quanta of energy, whether they are to be looked for in the radiation or in matter. (Jeans, 1924, p. 32)

These comments support my claim that the value of applying the quantum postulate in several contexts was not to be found in the details of the applications, such as with mechanisms of radiation exchange, but in the universal applicability of the parameter h. In a 1913 speech to the Physical Society of Copenhagen, Bohr says,

> Planck's theory would hardly have acquired general recognition merely on the ground of its agreement with experiments on blackbody radiation, but, as you know, the theory has also contributed quite remarkably to the elucidation of many different physical phenomena, such as specific heats, photoelectric effect, X-rays and the absorption of heat rays by gases. These explanations involved more than the qualitative assumption of a discontinuous transformation of energy, for with the aid of Planck's constant h it seems to be possible, at least approximately, to account for a great number of phenomena about which nothing could be said previously. (Bohr, 1922, p. 7)

Thus, the unificatory power of the idea of quantization was highly significant according to Bohr as well, but this wide applicability would not have been possible by focusing on individual conjectures rather than the general postulate. This is not to say the individual conjectures were not necessary: the very specific application of the idea of quantization in these contexts allows us to identify the particular assumptions being used in a very limited domain. This allows us to define the background information scientists were assuming in a consistent way. However, understanding the experiments as measurements of h allows us to see how we gain information about the general postulate.

This analysis is similar to that given of other episodes in history of science as well, such as Newton's argument for the inverse square force law, and his extension of this law to the theory of universal gravitation. By using Kepler's orbital laws, observations of the moon's orbit, and terrestrial phenomena, Newton is able to argue for the specific inverse square value of the force of gravitation. In William Harper's reconstruction of Newton's reasoning, "*a theory succeeds empirically by* *having its causal parameters receive convergent accurate measurements from the phenomena it purports to explain*" (2002, p. 185, emphasis in original).

Another excellent example is Jean Perrin's determination of Avogadro's constant. Perrin began with Avogadro's Hypothesis, which took into account the known relations between temperature, pressure and volume of gases, and hypothesized that equal volumes of different gases, under the same conditions of temperature and pressure, contain equal numbers of molecules, N (Perrin, 1913/1916, p. 18). Perrin used several different methods to calculate Avogadro's number, including work on Brownian motion, blackbody radiation and alpha decay, and showed that the number for N obtained by each method was extremely close — on the same order of 10^{22} in each case.[9] It is clear that there is a notion of informational relevance in this case, with phenomena from different contexts yielding information about the parameter N which could then be applied in other domains.

Wesley Salmon reconstructs Perrin's argument in terms of a common cause (1984). He argues that the agreement between the various methods of determining Avogadro's constant pointed to a common cause, and that this constituted evidence for the existence of atoms. One might claim that a similar argument can be made in the case of the quantum postulate, and that we should consider quantization of energy as the underlying reason that caused the results of the experiments. However, I would argue that characterizing the unificatory power of the quantum postulate as being due to a common cause would be to overstate the strength of the information available, since at the time in question, it was not at all clear how quantization might be occurring, and no account of the underlying mechanisms was forthcoming. In fact, there was significant disagreement on whether the quantization was merely a useful mathematical technique, or due to the nature of matter interacting with radiation, or perhaps due to the radiation itself. Despite this, it was clear that the quantum postulate did possess unificatory power in the informational relevance sense, and this minimal sense is all that is required to provide the hypothesis with some confirmational force.

This argument is perhaps more in line with Janssen's COIs, a broader notion than that of a common cause. I believe that this idea better captures the role of quantization of energy than a common cause, since a common origin can refer to something as weak as an 'embryonic idea.' Thus, one might say that the idea that energy of frequency ν at small scales should be considered in discrete packets of size $h\nu$ is what underlies the account of the various phenomena. However, the structure

[9] See Nye (1972) for a historical account of Perrin's work.

of my argument differs from Janssen's 2002 discussion of COIs in a few key ways.

First, the evidentiary warrant that accrues to the quantum postulate in this explication is not a matter of its explanatory power with respect to various phenomena as it seems to be in the quote above. Although it is important that by assuming the quantum postulate, one can account for several phenomena, the structure of my argument relies on the fact that the results of experiments on these phenomena can be turned into measurements of the parameter h, a crucial part of the quantum postulate, thereby yielding information that is relevant in different contexts. Thus, the evidence for the postulate comes not only from the fact that we can infer from the postulate to the phenomena, but also from the phenomena to something about the postulate. Second, although a common origin can be much vaguer than a common cause, it is still the case that "good COIs should at least provisionally identify some mechanism connecting the phenomena it groups together" (Janssen, 2002, p. 466). I argue that the quantum postulate was supported by the evidence despite the lack of a plausible mechanism underlying the process of radiation and matter interaction. Finally, at the end of the paper, Janssen points out that there is still a question of whether there is any epistemic force behind COIs, or if they merely have pragmatic value. My treatment of the quantum postulate within the Bayesian framework is meant to give an explicit account of how such an inference can have epistemic warrant.

More recently, Janssen has characterized the force of a COI not as a legitimate inference to the truth of a theory, but as an indicator that an idea is worth pursuing. I am wholly in agreement with this, and in fact, I take it that this accords well with how to understand the case I have presented. In particular, this fits well with an analysis of Einstein's treatment of Stokes's Rule 1905. Recall that Einstein showed that adopting the light quanta conjecture could account for the phenomenon of Stokes's Rule, $\frac{R}{N}\beta\nu_2 \leq \frac{R}{N}\beta\nu_1$. However, we can see through this reasoning that the quantum hypothesis entails Stokes's Rule, regardless of the size of h. Thus, the informational relevance of Stokes's Rule to any other piece of evidence is 0, and by definition, the unificatory power U of the quantum hypothesis for Stokes's Rule and any other piece of evidence is 0. As we have seen in the previous chapter, scientists did not seem to consider the explanation of Stokes's Rule a contribution to the support for a quantum postulate. However, it seems that even though Stokes's Rule does not contribute to the evidence for the quantum postulate by way of its unificatory power, it did provide at least some reason to think that the quantum postulate was worthy of pursuit. We can see thus see Einstein's argument in that paper, where he shows that a notion of quantization can be used to account for several phenomena, as putting forth an argument for the promising nature of the idea. Nevertheless, according to this analysis, the epistemic import of the postulate arises from the informational relevance that can be derived from it.

6.5 Conclusion

In this chapter, I have argued that the type of unification displayed by the quantum postulate can be understood in terms of informational relevance, which yields the result that in a Bayesian confirmational framework, this unificatory power contributed to the confirmation of a quantum postulate over and above the evidence taken individually. One of the important aspects of this informational relevance is the ability to turn experimental results into ways of constraining or measuring a physical parameter. I have argued that in many of these cases, an account of the mechanisms that would explain the observed behaviour were not available, which makes a mechanistic story for the unification more difficult to provide. However, the background knowledge to which one need be committed in order to accept the epistemic confirmational force of the unification argument was relatively minimal; a commitment to the reasonable accuracy of certain experimental apparatuses provides enough background on which to make probabilistic judgments about the evidence in question, without reference to the sometimes problematic theoretical explanations and derivations of the evidence. While I do not deny that causal explanations have their place in theoretical justification, I have provided an example where informational relevance lends a postulate epistemic force, even though a causal explanation is not available to us.

On the other hand, one might argue that what was driving scientists to pursue theories incorporating quantization in each of the domains in question was something like Kitcher's idea of having a unified explanation of the phenomena. This might very well capture scientists' motivations. However, the epistemic force of informational relevance is stronger since it makes clear that the experiments yielded information about the parameter in particular. On this account, merely providing a common explanation, such as Einstein's explanation of Stokes's Rule, does not help support the postulate. Thus, I argue that this type of unification, while related to these others, can be considered independently.

Chapter 7

Conclusion

In this dissertation, I have provided an evidentiary analysis of the early stages of the development of quantum theory through the lens of theory pursuit, and I argued that we can characterize this pursuit as a piecemeal process. The first half of the dissertation focused on explaining this characterization in detail.

I first showed that there were a variety of specific conjectures related to quantization being applied in different domains, with varied interpretations. I argued that we can nevertheless identify a broader quantum postulate that generalizes all these conjectures despite uncertainty or ambiguity about how to apply this postulate in specific contexts. I then argued that it is possible to separate the quantum postulate from different elements of the framework being used to investigate it by examining the way that the particular quantum conjecture of light quanta guided research in the context of experiments on the photoelectric effect. We saw that the conjecture of light quanta was crucial for generating a prediction that guided the experiments on this phenomenon, but that confirmation of the predicted formula did not necessarily support the light quanta conjecture itself. However, the experimental results did force agreement on a general postulate about quantization.

The term 'piecemeal pursuit' is meant to reflect several facts. First, the notion of quantization was being deemed promising in various contexts, but there was no overarching theoretical framework that included such a postulate, and so the investigation of quantization had to be pieced together from its application in different domains. Second, each domain was itself something of a piecemeal investigation, since elements of classical theories were being used in conjunction with conjectures and hypotheses that did not clearly fit within classical frameworks. By examining some of the historical criticisms of this tension in the context of Planck's original quantum conjecture, I argued that we should understand the seemingly inconsistent assumptions as applications of principles in very limited domains.

This made it possible to employ a broadly Bayesian framework in the last part of the dissertation to explicate some of the theoretical features that were operative in this process of pursuit. Using this framework, I was able to provide a formal explication of how agents with imprecise credences should consider the quantum postulate as being supported due to the features of correct prediction, explanation of previously known phenomena, and unificatory power. I first showed how the experiments on the phtooelectric effect provided support for the quantum postulate in terms of a correct prediction. I then argued that some of the support for the postulate came by way of its ability to account for old evidence. In the process, I suggested some modifications to a solution to the problem of old evidence by introducing two additional assumptions that I claimed arise naturally out of historical considerations. Finally, I showed that the quantum postulate is also supported by a unification argument, where unification should be interpreted as informational relevance of the various disparate domains of inquiry. This informational relevance arises by interpreting experiments in the different domains as yielding measurements of the numerical value of Planck's constant h.

We thus have an example of the possibility of using more traditional methods of confirmation to evaluate elements of theories in the context of pursuit. A crucial aspect of this argument was that these Bayesian analyses apply to the general postulate rather than either individual conjectures from each of the domains of applicability, or the broader theoretical frameworks guiding the investigations. Thus, we see that we can identify specific elements of theories that are promising, even when there is no overall theoretical framework that is guiding the investigation. We saw that while experimental results can force agreement about certain aspects of the theory under consideration, this can also easily foster disagreement about how best to proceed, such as whether one version of a theory should be pursued over another.

An important point is that this kind of divergence of opinion is in fact important in the context of pursuit for reasons both epistemic and pragmatic. In this case, the divergence of opinion had to do with how far one should generalize the quantum postulate. The generality of a hypothesis is an interesting feature because it has to do with how widely applicable it is, and a widely applicable hypothesis can be fruitful in its account of a variety of phenomena. However, this wide applicability is also important for two related but distinct epistemic reasons. The first is that the more applications of an idea that we have, the more we can use these applications to identify the essential elements of the hypothesis that are common to all its uses.

The second arises from the unification argument, in which the existence of multiple applications contributed directly to the support for the general postulate.

However, it was crucial that this wide applicability was tempered by the specificity of Planck's constant h. The inclusion of h is what made it possible to give a unificatory argument in terms of informational relevance. Furthermore, it was also responsible for features of the quantum postulate such as making predictions that were later verified, and accounting for previously known phenomena. This importance of h indicates that the methodological significance of wide applicability goes beyond a commitment to explanatory unification: in my explication, an important element of the unification arises from the possibility of gaining information about the hypothesis in question from the experimental results. Thus, even a general or widely applicable hypothesis must be rendered precise in some way.

This work has implications for theory pursuit because it shows that we can consider a particular postulate or hypothesis to be promising, even when no consistent, overarching scientific theory or framework is available in which to situate research. Indeed, it is possible to identify good epistemic reasons for pursuit, in addition to pragmatic ones. While the quantum postulate turned out to be universal in a very fundamental way, the features of wide applicability and specificity could potentially be used to identify promising features in other areas of science as well. While the details of each case would vary, and the meaning of being widely applicable and specific would require close study of individual cases, I take my case study to have provided an example of this kind of inquiry.

Bibliography

Achinstein, P. (1993). How to defend a theory without testing it: Niels Bohr and the "logic of pursuit". *Midwest Studies in Philosophy*, *XVIII*, 90–120.

Arabatzis, T. (2006). On the inextricability of the context of discovery and the context of justification. In J. Schickore, & F. Steinle (Eds.) *Revisiting Discovery and Justification: Historical and philosophical perspectives on the context distinction*, (pp. 215–230). Dordrecht, The Netherlands: Springer.

Arrhenius, S. (1922/1967). Presentation speech by S. Arrhenius, chairman of the Nobel committee for physics of the Royal Swedish Academy of Sciences. In *Nobel Lectures, Physics 1901–1921*. Amsterdam: Elsevier Publishing Company.

Badino, M. (2012). Productive conservatism: Planck's second theory of radiation. *Annalen der Physik*, *574*(2), A29–A31.

Barkan, D. K. (1993). The witches' sabbath: The first international Solvay congress in physics. *Science in Context*, *6*, 59–82.

Bohr, N. (1913). On the constitution of atoms and molecules, part I. *Philosophical Magazine*, *26*, 1–25.

Bohr, N. (1922). *The Theory of Spectra and Atomic Constitution: Three Essays*. Great Britain: Cambridge University Press.

Brown, M. B. (2014). The shape of science. *Synthese*, *191*, 3079–3109.

Christensen, D. (1999). Measuring confirmation. *The Journal of Philosophy*, *96*(9), 437–461.

Darden, L. (2009). Discovering mechanisms in molecular biology: Finding and fixing incompleteness and incorrectness. In J. Meheus, & T. Nickles (Eds.) *Models of Discovery and Creativity*, (pp. 43–55).

Darrigol, O. (1992). *From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory*. Berkeley: University of California Press. Retrieved from http://ark.cdlib.org/ark:/13030/ft4t1nb2gv/.

Darrigol, O. (2014). The quantum enigma. In M. Janssen, & C. Lehner (Eds.) *The Cambridge Companion to Einstein*, (pp. 117–142). Cambridge University Press.

Earman, J. (1992). *Bayes or Bust? A critical examination of Bayesian confirmation theory*. Cambridge, Massachusetts: The MIT Press.

Eells, E. (1990). Bayesian problems of old evidence. In C. W. Savage (Ed.) *Scientific Theories*, (pp. 205–223). Minneapolis, USA: University of Minnesota Press.

Einstein, A. (1905). Ueber einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt. *Annalen der Physik*, *17*, 132–148. Translated by D. ter Haar in *The Old Quantum Theory* 1967, pp. 91–107.

Einstein, A. (1905/1967). On a heuristic point of view about the creation and conversion of light. Trans. D. ter Haar. In *The Old Quantum Theory: Selected Readings in Physics*. Great Britain: Pergamon Press. Original work published 1905.

Einstein, A. (1906/1989). On the theory of light production and light absorption. Trans. A. Beck. In *The Collected Papers of Albert Einstein, Volume 2: The Swiss Years: Writings, 1900–1909 (English translation supplement)*, (pp. 192–199). Original work published 1906.

Einstein, A. (1907). Die Plancksche Theorie der Strahlung und die Theorie der spezifischen Waerme. *Annalen der Physik*, *22*, 180–190.

Einstein, A. (1907/1989). Planck's theory of radiation and the theory of specific heat. Trans. A. Beck. In *The Collected Papers of Albert Einstein, Volume 2: The Swiss Years: Writings, 1900–1909 (English translation supplement)*, (pp. 214–224). Original work published 1907.

Einstein, A. (1909). Über die Entwickelung unserer Anschauungen über das Wesen und die Konstitution der Strahlung. *Deutsche Physikalische Gesellschaft*, *7*, 482–500.

Einstein, A. (1909/1989). On the development of our views concerning the nature and constitution of radiation. Trans. A. Beck. In *The Collected Papers of Albert*

Einstein, Volume 2: The Swiss Years: Writings, 1900–1909 (English translation supplement), (pp. 379–394). Original work published 1909.

Einstein, A. (1909/1995). Letter to H.A. Lorentz. Trans. A. Beck. In *The Collected Papers of Albert Einstein, Volume 5: The Swiss Years: Correspondence, 1902–1914 (English translation supplement)*, (pp. 122–126).

Franck, J., & Hertz, G. (1914/1967). On the excitation of the 2536Å mercury resonance line by electron collisions. Trans. D. t. Haar. In *The Old Quantum Theory*, (pp. 160–166). Great Britain: Pergamon Press. Original work published 1914.

Franklin, A. (2013). Millikan's measurement of Planck's constant. *The European Physical Journal H*, *38*, 573–594.

Garber, D. (1983). Old evidence and logical omniscience in Bayesian confirmation theory. In J. Earman (Ed.) *Testing Scientific Theories: Midwest Studies in the Philosophy of Science, Vol. X*, (pp. 99–131). Minneapolis, USA: University of Minnesota Press.

Gearhart, C. A. (2002). Planck, the quantum, and the historians. *Physics in Perspective*, *4*(2), 170–215.

Gearhart, C. A. (2014). The franck-hertz experiments, 1911–1914: Experimentalists in search of a theory. *Physics in Perspective*, *16*, 293–343.

Harper, W. (2002). Newton's argument for universal gravitation. In I. B. Cohen, & G. E. Smith (Eds.) *The Cambridge Companion to Newton*, (pp. 174–201). Cambridge: Cambridge University Press.

Hartmann, S., & Fitelson, B. (2015). A new Garber-style solution to the problem of old evidence. *Philosophy of Science*, *82*(4), 712–717.

Heilbron, J. L., & Kuhn, T. S. (1969). The genesis of the Bohr atom. *Historical Studies in the Physical Sciences*, *1*, 211–290.

Hendry, J. (1980). The development of attitudes to the wave-particle duality of light and quantum theory, 1900–1920. *Annals of Science*, *37*(1), 59–79.

Jammer, M. (1966). *The Conceptual Development of Quantum Mechanics*. United States of America: McGraw-Hill Inc.

Janssen, M. (2002). COI stories: Explanation and evidence in the history of science. *Perspectives on Science*, *10*(4), 457–522.

Jeans, J. H. (1924). *Report on Radiation and the Quantum-Theory*. London: Fleetway Press, Ltd., 2 ed.

Jeffrey, R. C. (1983). Bayesianism with a human face. In J. Earman (Ed.) *Testing Scientific Theories: Minnesota Studies in the Philosophy of Science, Vol. 10*, (pp. 133–156). Minneapolis, USA: University of Minnesota Press.

Joyce, J. M. (2010). A defense of imprecise credences in inference and decision making. *Philosophical Perspectives*, *24*, 281–323.

Kitcher, P. (1989). Explanatory unification and the causal structure of the world. In P. Kitcher, & W. C. Salmon (Eds.) *Scientific Explanation: Minnesota Studies in the Philosophy of Science, Volume XIII*, (pp. 410–505). Minneapolis: University of Minnesota Press.

Klein, M. J. (1961). Max Planck and the beginnings of the quantum theory. *Archive for History of Exact Sciences*, *1*(5), 459–479.

Klein, M. J. (1965). Einstein, specific heats, and the early quantum theory. *Science*, *148*(3667), 173–180.

Kuhn, T. S. (1978). *Black-body Theory and the Quantum Discontinuity: 1894–1912*. New York, United States of America: Oxford University Press, Inc.

Kurlbaum, F. (1898). Ueber eine Methode zur Bestimmung der Strahlung in absolutem Maass und die Strahlung des schwarzen Körpers zwischen 0 und 100 Grad. *Annalen der Physik*, *301*, 746–760.

Laudan, L. (1977). *Progress and Its Problems: A Theory of Scientific Growth*. Berkeley and Los Angeles, California: University of California Press.

Leplin, J. (1980). The role of models in theory construction. In T. Nickles (Ed.) *Scientific Discovery, Logic, and Rationality*, (pp. 267–283). Dordrecht, Holland: D. Reidel Publishing Company.

Maher, P. (2004). Probability captures the logic of scientific confirmation. In C. Hitchcock (Ed.) *Contemporary Debates in Philosophy of Science*, (pp. 69–93). Malden, MA: USA: Blackwell Publishing.

Meheus, J., & Nickles, T. (Eds.) (2009). *Models of Discovery and Creativity*. Springer.

Mehra, J., & Rechenberg, H. I. (1982). *The quantum theory of Planck, Einstein, Bohr and Sommerfeld: Its foundation and the rise of its difficulties, 1900-1925, v. 1, pt. 1 of The historical development of quantum theory*. United States of America: Springer-Verlag New York Inc.

Millikan, R. A. (1916). A direct photoelectric determination of Planck's "h". *Physical Review*, *VII*, 355–388.

Millikan, R. A. (1917). *The Electron: Its Isolation and Measurement and the Determination of Some of Its Properties*. Chicago, IL: The University of Chicago Press.

Myrvold, W. C. (2003). A Bayesian account of the virtue of unification. *Philosophy of Science*, *70*, 399–423.

Nickles, T. (1980a). Can scientific constraints be violated rationally? In T. Nickles (Ed.) *Scientific Discovery, Logic, and Rationality*, (pp. 285–315). Dordrecht, Holland: D. Reidel Publishing Company.

Nickles, T. (Ed.) (1980b). *Scientific Discovery, Logic, and Rationality*. Dordrecht, Holland: D. Reidel Publishing Company.

Norton, J. (1987). The logical inconsistency of the old quantum theory of black body radiation. *Philosophy of Science*, *54*, 327–350.

Norton, J. D. (1993). The determination of theory by evidence: the case for quantum discontinuity, 1900–1915. *Synthese*, *97*, 1–31.

Norton, J. D. (2006). Atoms, entropy, quanta: Einstein's miraculous argument of 1905. *Studies in History and Philosophy of Modern Physics*, *37*, 70–100.

Nye, M. J. (1972). *Molecular Reality: A Perspective on the Scientific Work of Jean Perrin*. London: Macdonald & Co. (Publishers) Ltd.

Perrin, J. (1913/1916). *Atoms*. Trans. D. L. Hammick. London: Constable & Company Ltd.

Planck, M. (1900). Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum. *Verhandlungen der Deutschen Physikalischen Gesellschaft*, *2*, 237–245.

Planck, M. (1900/1967). On the theory of the energy distribution law in the normal spectrum. Trans. D. t. Haar. In *The Old Quantum Theory*, (pp. 82–90). Original work published 1900.

Planck, M. (1901). Über das Gesetz der Energieverteilung im Normalspectrum. *Annalen der Physik*, *4*, 553–563. Translated by Koji Ando.

Planck, M. (1909/1915). *Eight Lectures on Theoretical Physics: Delivered at Columbia University*. Trans. A. P. Wills. New York: Columbia University Press. Original work published 1909.

Planck, M. (1913/1914). *The Theory of Heat Radiation*. Trans. M. Masius. Philadelphia, USA: P. Blakiston's Son & Co., 2 ed.

Planck, M., Nernst, W., Rubens, H., & Warburg, E. (1913/1995). Proposal for einstein's membership in the Prussian Academy of Sciences. Trans. A. Beck. In *The Collected Papers of Albert Einstein Volume 5: The Swiss Years: Correspondence, 1902–1914*, (pp. 335–338). Princeton, New Jersey: Princeton University Press. Consultant: Don Howard, Original work published 1913.

Popper, K. R. (1959). *The Logic of Scientific Discovery*. Routledge.

Reichenbach, H. (1938). On probability and induction. *Philosophy of Science*, *5*(1), 21–45.

Saatsi, J. (2014). Inconsistency and scientific realism. *Synthese*, *191*, 2941–2955.

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

Shimony, A. (1970). Scientific inference. In R. G. Colodny (Ed.) *The Nature & Function of Scientific Theories*, (pp. 79–172). United States of America: University of Pittsburgh Press.

Smith, G. E. (2010). Revisiting accepted science: The indispensability of the history of science. *The Monist*, *94*(4), 545–579.

Stuewer, R. H. (2014). The experimental challenge of light quanta. In M. Janssen, & C. Lehner (Eds.) *The Cambridge Companion to Einstein*, (pp. 143–166). Cambridge University Press.

Vickers, P. (2014). Theory flexibility and inconsistency in science. *Synthese*, *191*, 2891–2906.

Šešelja, D., & Straßer, C. (2014). Epistemic justification in the context of pursuit: A coherentist approach. *Synthese*, *191*, 3111–3141.

Šešelja, D., & Weber, E. (2012). Rationality and irrationality in the history of continental drift: Was the hypothesis of continental drift worthy of pursuit? *Studies in History and Philosophy of Science*, *43*, 147–159.

Whewell, W. (1989). *Theory of Scientific Method*. United States of America: Hackett Publishing Company. Ed. Robert E. Butts.

Whitt, L. A. (1990). Theory pursuit: Between discovery and acceptance. *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, *1: Contributed Papers*, 467–483.

Whitt, L. A. (1992). Indices of theory promise. *Philosophy of Science*, *59*(4), 612–634.

William Thomson, L. K. (1901). Nineteenth century clouds over the dynamical theory of heat and light. In *Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light,*, (pp. 486–527). London: C.J. Clay and Sons, Cambridge University Press Warehouse. Also in Philosophical Magazine, vol. 2, pp.1–40, 1901.

Wimsatt, W. C. (1981). Robustness, reliability, and overdetermination. In M. Brewer, & B. Collins (Eds.) *Scientific Inquiry in the Social Sciences*, (pp. 123–162). San Francisco: Jossey-Bass.

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