A COHERENTIST APPROACH TO THE JUSTIFICATION OF SCIENTIFIC THEORIES

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ABSTRACT

A COHERENTIST APPROACH TO THE JUSTIFICATION OF SCIENTIFIC THEORIES

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Philosophers of science have long realized that it is not possible to decide which scientific theory is true just by relying on their empirical adequacy. That theories should possess other virtues in order to be accepted by the scientific community is well understood. Nevertheless, empirical adequacy remained as having a privileged value among these virtues. In this thesis I argue that scientific theories are accepted or rejected on the bases of an evaluation of their degree of coherence. In such a coherentist understanding, empirical adequacy still plays some role. However, this is an egalitarian approach where observational reports have no special status. By means of case studies form history of science I provided reasons to think that this coherentist approach is better suited to understanding scientific change as a rational process.

Keywords: Coherentism, Scientific Realism, Scientific Progress.

ÖZ

BİLİMSEL KURAMLARIN GEREKÇELENDİRİLMESİNE UYUMCULUK YAKLAŞIMI

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Bilim felsefecileri hangi bilimsel kuramın doğru olduğunun yalnızca kuramların gözlemsel başarısına dayanarak anlaşılamayacağının uzun zamandır ayırdındadırlar. Kuramların, bilim toplumunca kabul görmesi için başka niteliklerinin de olması gerektiği bilinmektedir. Bununla birlikte gözlemsel başarı, bu nitelikler arasında sahip olduğu ayrıcalıklı değeri korumaktadır. Bu tezde kuramların kabul edilmesinin ya da reddedilmesinin uyumluluk derecelerinin değerlendirilmesine dayandığını öne sürüyorum. Böyle bir uyumluluk yaklaşımında gözlemsel başarının yine önemi vardır. Ancak bu, eşitlikçi bir yaklaşımdır ve gözlem önermelerinin ayrıcalıklı bir önemi yoktur. Bilim tarihinden örnekler yardımıyla bu uyumluluk yaklaşımının bilimsel değişimi rasyonl bir süreç olarak anlamak için daha uygun bir yaklaşım olduğunu düşündürtecek nedenler sundum.

Anahtar Sözcükler: Uyumculuk, Bilimsel Gerçekçilik, Bilimsel İlerleme.

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TABLE OF CONTENT

CHAPTER

CHAPTER I

INTRODUCTION

Scientific realists claim that our scientific theories are, at least approximately, true. However, the difficulties of determining whether or not a given theory is actually true became a serious challenge to defending scientific realism, so much so that some philosophers of science argued that truth is not relevant to determining the success of scientific theories. Therefore, it is important to figure out some qualities of true theories, which are more readily recognizable than truth. Even if the truth of a theory cannot be demonstrated, it is necessary for a scientific realist to be able to argue that we are justified in believing these theories. The first obvious candidate for justification is successful prediction. It is quite a reasonable expectation that a theory, which correctly describes the laws of nature, will also be successful in its predictions of empirical phenomena. Therefore, successful predictions are thought to justify theories.

However, it soon became obvious that this criterion is not sufficient by itself to explain the history of science as a rational process. I will discuss the reasons for why it is insufficient in section 2.4. If we aim to select true theories and eliminate false ones, using *only* the predictive success criterion would not do the job. It is not simply that predictive success is insufficient for theory choice. Moreover, there are cases where empirically inadequate theories are given serious consideration and eventually became respectable scientific theories. That is, there may be reasons for—at least temporarily—ignoring empirical failures of a theory. Noticing these many philosophers of science introduced other criteria that accompany predictive power.

Kuhn, for example, suggested simplicity, broadness of scope, fruitfulness and consistency, as other virtues, of a scientific theory, that actually play a role in the decisions of scientists.[1] Another often cited virtue is explanatory power, which seems to be closely related to simplicity and broadness of scope. There are other criteria occurring in the literature such as parsimony, modesty and conservatism. Finally, sometimes scientists refer to "beauty" as a factor playing role in theory choice. As Kuhn noted for his own set of criteria, these are closely related and not exhaustive. No one ever attempted to provide a complete list of such criteria.

However, there are several difficulties concerning the use of these criteria. First, different philosophers of science generally mean slightly different things by these "virtues." Thus, for example, when we want to determine which of the two rival theories is more fruitful, different philosophers will suggest the use of different measures. Moreover, even if the task is left to actual scientists, as Kuhn indicates, "individuals may legitimately differ about their [criteria's] application to concrete cases."[2] Since the criteria are not well-defined, the judgement as to which of the two rival theories is superior with respect to a given criterion cannot be totally objective.

Secondly, it does not seem possible to build a hierarchy among the criteria. That is, even when we agree that one of the two rival theories has broader scope but the other is simpler, there seems to be no strict rule indicating which theory should be preferred. It seems that two rational scientists may differ in their theory choice, even when they agree about the merits of these theories. Moreover, how much say, explanatory power, can be sacrificed for gaining somewhat higher accuracy in predictions is something that cannot be settled once and for all.

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[1] Kuhn, 1977, pp. 321-322.

[2] Kuhn, 1977, p. 322.

These difficulties proved to be insurmountable. That is why Kuhn calls these criteria “values” as opposed to “rules” to emphasise the lack of unambiguous decisions in all cases. This is not to say that they are totally subjective and have no correlation with truth. As Steven Weinberg’s analogy, which he makes use of in explaining his conception of beauty, shows, the inability to clearly specify the criteria does not necessarily indicate that it is not objective:

> The horse trainer [who calls a horse beautiful] is of course expressing a personal opinion, but it is an opinion about an objective fact: that, on the basis of judgements that the trainer could not easily put into words, this is the kind of horse that wins races.[3]

Like Kuhn, Weinberg suggests that individuals may differ in their attribution of these virtues. However, they are still objective and rationality should be sought at the level of scientific community and not at the level of the individual. Even though individuals may *legitimately* differ in their judgements, if the scientific community as a whole made a choice—despite a few opposing views—that choice should not be understood as lacking good reasons. “[W]e do well to trust the collective judgement of scientists trained in this way.”[4]

It is also important to note that the use of these criteria is not strictly limited to cases of theory choice. Once their importance is accepted it would be natural to expect that they will guide the researchers in modifying even the only available theory of their domain. That is, when there is prospect of say, simplifying the theory, a researcher will aim at that, just like he aims to increase the accuracy of predictions of the theory. In order to understand what role these criteria play in the development of science one needs to be able to consider not only theory choice but the development of a theory in time and also the emergence of new theories.

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[3] Weinberg, 1993, p. 133.

[4] Kuhn, 1977, p. 321.

The development of scientific theories also puzzled some philosophers of science who tried to rely on empirical adequacy as a principal source of justification.[5] If the empirical success is the only basis for determining the truth of the theory, then the need to modify a theory would only emerge when an empirical inadequacy is encountered. There would simply be no clue as to how to develop a theory if all its predictions that are tested lies within the experimental error margins. Without empirical challenges the theory would be preserved intact. However, the history of science is full of debates over theories that seem to have no immediate empirical problems.

Kuhn's values and other similar criteria are introduced to avoid at least some of the problems that emerge due to a foundationalist approach to the justification of scientific theories. Foundationalism suggests that propositions can only be justified by being either "basic" or by being a consequence of justified propositions. Even though there are several versions of foundationalism, the essential theme is finding a secure foundation that can be used in justifying the rest of our beliefs.[6] The expectation that observation reports can provide this secure basis which will be used to justify scientific theories is unfounded not least because of theory-ladenness problem.

There are, however, other views as to how our beliefs are justified. The most well-known alternative to foundationalism is coherentism, which dispenses with the idea of a secure basis. In this thesis, I will argue that a coherentist approach is more appropriate to understand actual historical cases of both theory choice and the development of scientific theories. Many of the serious problems of philosophy

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[5] It was even argued that the context of discovery and the context of justification should be clearly distinguished. The most prominent figures defending this view are Carnap, Reichenbach and Popper. However, if a certain quality is said to provide justification for a theory, then it is hardly possible to argue that a scientist developing a theory will not aim at achieving it. Hence the expected justification process will also guide the discovery.

[6] For a classification of alternative versions of foundationalism and their problems see the second chapter of Haack, 1995.

of science, such as the underdetermination problem, will turn out to be much more easily handled by a coherentist approach to how our theories are justified.

In what follows I will first introduce the coherence theory of justification and then argue that most of the problems occurring due to the foundationalist approach to the justification of scientific theories dissolve in a coherentist approach. I will also argue that criterion other than the predictive accuracy are helpful in explaining the history of science to the extent that they are possessed by the theories with a high degree of coherence. Finally, by examples from the history of science I will argue that a coherentist approach is more appropriate to describing the development of science as a rational process.

CHAPTER II

THE SCEPTICAL ARGUMENT

The well-known sceptical challenge to justification rests on the premise that all propositions are justified by other propositions, which are themselves in need of justification. In order to avoid infinite regress, justification either ends abruptly at some unjustified proposition or is circular. In both cases one cannot claim to have genuine justification.

The foundationalist replies to this challenge by arguing that there are “basic” propositions which are justified without the use of other propositions (either because they are self-justificatory or justified by non-propositional means). Yet they are capable of providing justification to other propositions. In this way, foundationalists claim to terminate the justificatory chain and also avoid being dogmatic.

On the other hand, coherentists look for the possibility of making sense of justification purely as a relation between propositions, none of which is “basic” in the foundationalist sense, while avoiding vicious circularity. They claim that if a set of propositions are coherent then each member of this coherent set is justified, merely by virtue of the fact that they cohere with the rest of the set.

It is important at this point to recognize the distinction between truth and justification.[7] A theory of justification is a tool that enables one to determine which propositions are *more likely to be* true. By contrast, a theory of truth *defines*

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[7] For a detailed discussion of this point see Kirkham, 1992.

what it is to be true for a proposition without necessarily providing a means to identify such propositions.

Defending coherence theory of justification does not necessarily require acceptance of coherence theory of truth. Coherence theory of justification says that it is more likely that the members of a coherent set of propositions will be true. So it is compatible with other truth theories such as the correspondence theory of truth. Hereafter, I will use the terms ‘coherentism’ and ‘coherence theory’ only to refer to coherence theory of justification.

Another approach to justification is fallibilism. According to this view any proposition is fallible including the observational ones. Hence this approach does not claim the existence of a secure basis. However, it is distinguished from coherentism in that it still spares a special role for observational propositions. The source of justification is not mutual relations between propositions but the support that they receive from observational propositions even though they are revisable. Since in this fallibilist approach the status of the propositions are not equal I will call it non-egalitarian fallibilism.

I will now discuss coherentism and foundationalism in relation to justification of scientific theories. Then I will consider the difficulties associated with a fallibilist approach and argue that it is more beneficial to view the justification of scientific theories with a coherentist understanding of justification. I will, however, *not* focus on their success in providing a reply to the sceptical argument.

2.1 Coherence Theory of Justification

When one faces two rival sets of propositions, according to coherence theory of justification, it is more rational to prefer the set that is more coherent. Hence, a crucial element in a coherence theory is a measure that will enable one to determine which set is more coherent.

In his defence of coherentism Laurence Bonjour defines the degree of coherence of a set of beliefs in terms of five conditions that should be used to determine which set of beliefs is more coherent.[8] Bonjour's "second condition" for coherence is as follows: "A system of beliefs is coherent in proportion to its degree of probabilistic consistency."[9] Here probabilistic consistency is a measure of the likelihood of a given member of the belief set, when the others are assumed to be true.

The need for this condition is explained as follows:

> Suppose that my system of beliefs contains both the belief that P and also the belief that it is extremely improbable that P. Clearly such a system of beliefs may perfectly well be logically consistent. But it is equally clear from an intuitive standpoint that a system which contains two such beliefs is significantly less coherent than it would be without them.[10]

An example from the history of science may help understand this condition and the concept of "probabilistic consistency." When the maps of the two sides of the Atlantic were completed, it was realized that the continents have such a suitable shape that they would fit quite nicely if brought together. Even though this does not contradict with any of the beliefs held about the formation of the continents, some scientists began to look for a reason for this unlikely "coincidence." For, the observed structure of the continents was highly unlikely given the fact that they were created separately on their current locations. Bacon, for example, suggested that the continents were once a single whole and were broken by Noah's flood. The idea of continental drift—whether by Noah's flood or by other geological

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[8] Bonjour, 1985, pp. 95-99. Bonjour later changed his mind and opted for a form of foundationalism in his later writings. See, for example, Bonjour, 1999.

[9] Bonjour, 1985, p. 95. I will argue that other four conditions are just corollaries of the second condition. They will help clarify the concept of coherence, but are essentially contained in the second condition.

[10] Bonjour, 1985, p. 95.

means—surely eliminates the improbability; hence increases the probabilistic consistency of the set.

Concerning the justification of scientific theories, Bonjour's "fifth condition" provides some insights. The condition reads: "The coherence of a system of beliefs is decreased in proportion to the presence of unexplained anomalies in the believed content of the system." [11] An "unexplained anomaly" is nothing but the presence of a proposition in the system which is rendered highly improbable by the rest of the system. Therefore, this condition can be considered as a consequence of Bonjour's second condition.

The idea behind this condition can be clarified by an example from the history of science. When the observed orbit of Mercury turned out to be different from what was predicted by Newtonian physics, the coherence of the overall system was lowered. The propositions, other than the observation report (and the related propositions to the effect that the report was reliable), implied that the observation report has a very low probability. What saved the system from being contradictory was the additional proposition expressing the belief that "the discrepancy is only apparent and will dissolve in time by further research." Some suggestions to this effect were made, such as the postulation of a new planet (Vulcan), which will restore the coherence of the system. This is indeed the "normal science" activity that Kuhn describes. Discrepancies between the theoretical predictions and observation reports are not treated as problems of the theory but considered as incapability of the researcher. They are puzzles that should be given serious consideration but do not indicate a contradiction. Scientists attempt to solve such puzzles and hence increase the coherence of the system.

The first condition of Bonjour is simply the requirement that if a set is to be evaluated as coherent it must first be logically consistent. This quite obvious

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[11] Bonjour, 1985, p. 99.

condition is, however, nothing but a limiting case of the second condition. His third condition is as follows:

> The coherence of a system of beliefs is increased by the presence of inferential connections between its component beliefs and increased in proportion to the number and strength of such connections.[12]

This third condition looks like a statement that indicates the value of explanatory power. Indeed there have been philosophers of science who use the term "explanatory coherence" instead of explanatory power. Bonjour also admits that higher explanatory power indicates higher degree of coherence, but argues that the increase of the number and strength of inferential relations need not increase explanatory power. However, his very short discussion is superficial and his example is hardly convincing. In section 2.5, I will provide reasons why coherence is not reducible to explanatory power. In Chapter V, I will also discuss an actual historical case of underdetermination, where one of the rivals has more explanatory power whereas the other is more coherent. The fact that the choice of scientific community is squarely on the side of the theory that has higher coherence will support my claim that coherentism is a better way of understanding the justification of scientific theories. Moreover, ad-hoc modifications that are often condemned by philosophers of science generally increase explanatory power. However, they do not necessarily increase probabilistic consistency—hence, coherence. I will discuss the difficulties associated with ad-hoc modifications in sections 2.4.4 and 2.5.

Bonjour's fourth condition emphasizes another aspect of scientific research. The fourth condition is:

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[12] Bonjour, 1985, p. 98.

> The coherence of a system of beliefs is diminished to the extent to which it is divided into subsystems of beliefs which are relatively unconnected to each other by inferential connections.[13]

Since science deals with a wide range of phenomena, it is possible that the set of propositions expressing all of our scientific beliefs may contain relatively unconnected subsets. The set of propositions expressing quantum mechanical laws may be quite irrelevant to those of genetics. Note that this is not necessarily an indication of a serious problem in our set of scientific beliefs. To say that the subsets are relatively unconnected is different from saying that the whole set has a low degree of probabilistic consistency. If the subsets are distinct, one of them does *not* make the other *unlikely*. Nevertheless, it is quite obvious that if they were connected, and these parts support each other, the probabilistic consistency and so the coherence of the set will be increased. This is the underlying motivation for searching for unifications in science. Moreover, in some cases, seemingly irrelevant domains provide significant support to each other and play decisive role in theory choice. One such case is the discovery of radioactivity. By demonstrating that the earth contains a source of energy, physicists undermined Lord Kelvin's calculations of the age of the earth, providing enough time for evolution to take place.[14] The seemingly irrelevant domains—nuclear physics and theory of evolution—provide mutual support to each other.

There are two important points that need to be emphasised about Bonjour's "conditions." The first is that all of the conditions are a consequence of the second condition. Therefore, all but the second one are redundant. Indeed Bonjour's aim in listing them explicitly is to explicate the concept of coherence. Hence, in what follows, when aiming to compare two theories I will focus on the second condition only. The second point is that his conditions are only useful for comparison and

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[13] Bonjour, 1985, p. 98.

[14] For a popular discussion of the debate between Darwin and Kelvin see Gould, 1985, pp. 26-138.

not as a measure for some absolute evaluation of coherence. However, this will not pose problems for adopting a coherentist approach to the justification of scientific theories. As almost all philosophers of science agree, a theory is refuted only in the presence of a better one—even though what "better" means is debated. Moreover, when there is only a single theory, then modifications made on it can be evaluated on the basis of a comparison between the earlier and the later versions of the theory. So a tool for successful comparisons is sufficient.

After stating his conditions of coherence, Bonjour makes the following remark: "the progress of theoretical science may be plausibly viewed as a result of the search for greater coherence."[15] However, he says nothing further to elaborate this view and rather focuses on the truth conduciveness of coherence.[16]

I will discuss the advantages of describing scientific change as preference of more coherent set of beliefs. But first I will argue that despite its problems, coherentism is a tenable view of justification. Then I will evaluate some of the traditional problems of philosophy of science and argue how they will dissolve by a coherentist understanding.

2.2 Problems of Coherentism

Let me introduce major problems of the coherence theory. These problems may appear at first to be insurmountable. Nevertheless, I believe that there are good reasons to consider the coherence theory of justification as the most plausible approach. Indeed any other alternative suggested so far has its own problems. These alternatives can all be considered as the drafts of the correct theory of

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[15] Bonjour, 1985, p. 100.

[16] Actually, in a footnote attached to the above quotation Bonjour writes that this claim is elaborated in an article by Wilfrid Sellars, Sellars, 1963. Sellars's article focuses on the relation between theoretical and observational propositions and does not significantly distinguish explanatory power and coherence. So it is far from being an attempt to provide a coherentist approach to the justification of scientific theories.

justification. However, coherentism serves much better than its rivals in understanding science.

Coherent Lies: One can construct a coherent set of propositions none of which are true. This is a challenge indicating that coherence has no epistemic value, since coherence is not an indicator of truth.

A coherent set may be constructed out of propositions which are all false like in the case of a fairy-tale. If coherence alone is considered as a source of justification, then the propositions of a fairy-tale, none of which are true, may justify each other. When we read *Alice in Wonderland* we are told the adventures of a little girl and as far as the propositions of the text are concerned it is a coherent story. Yet we know—intuitively—that they are all unjustified. This supposedly implies that coherence is not an indication of truth. Since, intuitively unjustified propositions may have high degree of coherence.

Defenders of the coherence theory of justification developed several strategies to avoid this problem. Two of them are worthy of consideration. The first one is to argue that our set of beliefs must include a significant number of observation reports. This is supposed to avoid the problem by making one realize that his empirical beliefs are not coherent with the story of Alice. Unfortunately this strategy cannot work. First of all it is problematic to distinguish empirical and non-empirical beliefs. Secondly, it would not be in accordance with the egalitarian approach of coherentism to give a special role to some of our beliefs. Finally, cases like optical illusions indicate that having a special role for observational reports is unjustified.

The second strategy to avoid this problem is to argue that no matter how coherent a set is it should be our aim to enlarge the set. Such an epistemic duty will force us to include other novels, stories, scientific theories, daily experiences and whatever else is available to us. In that case it is hoped that the only way to make this set coherent is to include the belief that *Alice in Wonderland* is a fictitious story the character of which never lived.[17]

This strategy again is not found satisfactory by most of the critics. The counter argument is that there may be more than one coherent set possessing as many beliefs as one can imagine and yet these sets may be mutually incompatible. The counterpart of this problem in the philosophy of science is the problem of underdetermination by empirical evidence. In case we have two alternative theories about nature both of which lead to exactly the same predictions, we will be unable to determine which of them is true by empirical evidence. In such a case if one of them is the true theory, then the other will be a coherent lie just like the story of Alice.

There seems to be no solution to this problem especially if one aims to provide a conclusive answer for the sceptic. I will leave aside the sceptical worries about the issue I am dealing with and rather focus on how science actually works. So I will argue that coherence is a virtue in science even if it is "logically possible" for a coherent story to be false. However, as the discussion of case studies will indicate the problem of underdetermination is a more serious challenge to non-egalitarian approaches than it is to coherentism. In the actual practice of science, rival theories that are empirically equivalent never posed persistent problems of theory choice. This is indeed the major reason why traditionally, virtues other than empirical success are introduced by philosophers of science. In a coherentist approach a more natural way of explaining the choice among empirically equivalent theories will emerge.

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[17] This strategy is essential, since the trivial method of restoring coherence by eliminating some beliefs is certainly not likely to lead to a true set of propositions. Coherence may be truth conducive only if it is achieved among a large number of propositions.

So even though the lack of a foundation disables a coherentist to provide a satisfactory answer to a sceptic, it nevertheless provides a better understanding of how the problem of underdetermination is handled in science.

Inconsistency: 'Coherence' is a term which indicates a stronger relationship between propositions of a set than mere consistency. In other words, all coherent sets are consistent but a consistent set may not be coherent (at least not to any significant degree). The problem is that any individual probably possesses too many beliefs and when analysed, logical implications of some of them may turn out to be contradictory, rendering the whole set of beliefs unjustified. Inconsistency seems to be practically unavoidable—especially since one aims to keep the set of beliefs as large as possible. If this is the case then there can be no chance of achieving coherence and according to the coherence theory of justification none of our beliefs will be justified. Even the claim that our current scientific theories are true is contradictory. Quantum theory and general theory of relativity cannot both be true in their current form. If we look for coherence, how can two incompatible scientific theories be held simultaneously?

I certainly do not claim that current scientific claims are all coherent, since incompatibilities are obvious. But it should be noted that the *aim* of making our theories coherent does not require them to be currently coherent. Science, I will argue, progresses with attempts to increase coherence—sometimes by avoiding a contradiction.

The search for coherence is an invaluable guide, because even when theories have no empirical problems scientists can realize what problems are forthcoming and what kind of solutions should be developed by the help of this guide. To illustrate, Schrödinger anticipated the forthcoming problems due to a separation of classical and quantum realms about half a century in advance of any actual empirical difficulty and tried to remedy them. This point is also explored by Lakatos in his discussion of "positive heuristic" of a scientific research programme. He argues that the anomalies of a research programme can be anticipated in advance of their occurrence. The “protective belt” that will help us to preserve the “hard-core” of the programme is not constructed on a case by case base:

> But it should not be thought that yet unexplained anomalies—‘puzzles’ as Kuhn might call them—are taken in random order, and the protective belt built up in an eclectic fashion, without any preconceived order. The order is usually decided in the theoretician’s cabinet, independently of the *known* anomalies. Few theoretical scientists engaged in a research programme pay undue attention to ‘refutations’. They have a long-term research policy which anticipates these refutations.[18]

Measuring the Degree of Coherence: Clearly, when coherence is taken to be what justifies our beliefs, we need to be able to decide the degree of coherence of a set of propositions to see to what degree they are justified. Even if we lack an absolute measure we must at least be able to compare the degree of coherence of alternative sets of beliefs.

Coherence theory has long suffered from a lack of measure for coherence. The coherence of a set of propositions is generally described by vague terms like “mutual support” between those propositions or “hanging together” of the propositions. However, it is vital for a coherence theorist to describe a method to determine in a unique way which one among given sets of propositions is more coherent than the others.

In some artificial cases which set is more coherent can be intuitively obvious. But in actual cases a better criterion is required. Bonjour’s second criterion, which makes use of “probabilistic consistency”, is a good measure in cases where the sets that are compared share a large number of propositions. In the case studies that I will discuss, I will use Bonjour’s criteria to argue that coherentism is a better approach to understanding scientific practice than foundationalism and non-egalitarian fallibilism.

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[18] Lakatos, 1978, p. 49-50.

Truth Conduciveness: If one claims that a certain property of a proposition—such as coherence with some other propositions—provides epistemic justification for it, then one should be able to show that propositions having this property are more likely to be true than those that lack this property. In the case of the coherence theory of justification, the relation between truth and coherence is certainly not evident. This is the major criticism of coherentism and until now no satisfactory resolution of the problem is provided. Nevertheless there are several attempts to remedy this.[19]

However, I will not attempt to argue that coherence is truth conducive. Whether more coherent sets of propositions tend to be more likely to be true, is a question that lies outside the scope of this thesis.[20] I will only argue that development of scientific theories can be viewed as attempts towards increasing coherence, and cases of theory choice are based on a comparison of the coherence of the rival systems.[21]

2.3 Problems of Foundationalism

Among the several arguments against foundationalism the most severe one is what Haack named "too much to ask argument."[22] It points to the fact that a foundationalist aims at establishing a basis that is both not in need of propositional

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[19] An attempt to discuss the connection of truth and a specific sort of coherence is provided by Thagard (2007).

[20] There is indeed a very interesting argument to the effect that if we increase the coherence of a set by adding more propositions to it, the set will be less likely to be true (Klein and Warfield, 1994). If this argument is accepted and if again—as I will argue—science is a process of enlarging our belief system to make it more and more coherent, then scientific claims of today should be less likely to be true than those in the past. However, Klein and Warfield's paper is not conclusive. An excellent criticism is Shogenji, 1999.

[21] One may of course consider defending the coherence theory of justification by a naturalistic approach once it is accepted that more coherent theories are preferred in actual practice of science as implied in Thagard, 2007.

[22] Haack, 1995, p.25.

justification and also sufficient to justify most of our beliefs. Inclusion of too many propositions into the set of basic propositions runs the risk of error. Only a very limited number of propositions can reasonably be argued to be self-evident. However, as this set is kept small (such as limiting it only to propositions of logic) it is not clear how it can provide justification to a large variety of our beliefs.

The general tendency is to include some empirical propositions to the set of basic propositions. So that all other beliefs will be justified by them. However, it is not easy to avoid the possibility of error in case of empirical propositions.

There have been several attempts to establish a secure empirical basis that will be used to justify scientific theories. However, all such efforts failed and philosophers of science began to look for alternative approaches.

Below I will discuss the current dominant view in philosophy of science: non-egalitarian fallibilism. It is superior to foundationalism in that it recognizes the impossibility of finding a *secure* basis that is sufficiently large to justify our scientific theories. However, it still has some sever problems—due to the special role it devotes to observation reports—that a foundationalist approach will also suffer.

2.4 Problems of Non-Egalitarian Fallibilism

After the attempts to find a secure empirical basis failed, most philosophers of science adopted a fallibilist attitude. That is they considered observational reports as fallible. In this way they departed from foundationalism. However, they also avoided an egalitarian approach. Observational reports—though being fallible—are considered as having a privileged status. Moreover, for Lakatos and Popper the fallible observational reports are still the *only* elements that possess an epistemic value.

Lakatos claimed that it is not possible to discover a methodology for science, which will help scientists to make decisions when they are confronted with problems in the future. There exists no set of rules that list what the rational behaviour of a scientist in an actual case should be. However, he insisted that we can "reconstruct" the history of science, so that it will appear *as if* scientists followed a set of rules. Rationality can be found in science only when we view its history retrospectively.

Lakatos's observation that a methodology is not possible is based on a simple observation. An empirically adequate research programme may turn out to be rejected, or an empirically inadequate research programme may develop into a well established (and empirically successful) one after some slight modifications. Therefore, there can be no rational way to decide which research programme one should work on *before* a long time elapses. The amount of empirical support at a given time is not sufficient to justify or undermine a research programme, for the future performance of the programme cannot be foreseen.

Although this reasoning is simple and seems to be conclusive, it is based on an erroneous premise, namely, that the only rational reason to prefer one research programme over the other can be its empirical success in making predictions.

As Lakatos's own examples illustrate, predictive failures may simply be ignored at times when scientists have good reasons to think that the empirically unsuccessful programme is on the right track (so that it will in the future become empirically adequate). Similarly an empirically successful theory can be rejected (anticipating the occurrence of empirical problems in the future). In order to "rationally reconstruct" such historical cases, Lakatos introduced the idea of heuristic which somehow provides hints as to how to develop the theory and make it empirically successful.

Actually, what Lakatos aims is to introduce criteria other than empirical adequacy, but without assigning them any epistemic value. All the additional criteria that may influence the development of the theory are subsumed under the title "heuristic."

However, the justification is *only* provided by empirical adequacy. Heuristic of a research programme has no epistemic value in the Lakatosian scheme. This lack of epistemic support from the heuristic, as Thomas Nickles also argues, is a defect in Lakatos's view of science that should be remedied.[23] If the heuristic has no epistemic function one cannot decide which rival research programme is more rational to choose. Therefore, the rationality can only be argued after the preferred research programme is developed sufficiently and avoided most of its empirical problems.

It is of no use to list criteria other than empirical adequacy as means of preferring one theory over another, without arguing that they epistemicly justify the theory, if one is to preserve a realist attitude towards scientific theories. If the criteria of choice lack epistemic value, then our choice is not motivated by selecting the theory that is more likely to be true. The criteria can only be considered to have pragmatic value without any necessary connection with truth.

To illustrate, simplicity is often considered to be a good criteria of theory choice—at least when empirical evidence is inconclusive to choose among rival theories—that has pragmatic value. However, if one is unable to argue that, everything else being equal, a simpler theory is more likely to be true than its less simple rival, then the criterion of simplicity is of no use for a scientific realist.

I will now briefly describe the difficulties posed by a non-egalitarian attitude toward the justification of scientific theories. After stating these well-known problems, I will explain—by the help of historical examples—how they can be handled by a coherentist approach. I will argue that all these problems that seem to be persistent in the traditional understanding of science, either completely dissolve in a coherentist approach or else coherentism at least seems to be more promising towards their solution.

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[23] Nickles, 1987.

2.4.1 Problem of Theory-Ladenness

First of all, there is the difficulty of separating the observational from the theoretical. If one is to justify theoretical claims by testing their observational consequences, a working distinction between observational and theoretical is required. However, the impossibility of drawing this distinction led some philosophers even to argue that even very basic observation statements are contaminated by theories.[24]

An actual problem occurred in the early days of quantum mechanics related to this difficulty. Even though Heisenberg, Born and Jordan agreed that quantum theory should avoid reference to unobservable quantities they were not able to agree on what counts as an observable. Another case was Harvey's and Descartes's dissection experiments. Performing the same experiments they reached different "empirical results" supporting their own theory and refuting the others. It is evident that their views on the source of life influenced their observation reports.

This problem is quite trivially resolved if coherentism is adopted. Since the source of justification is coherence among accepted propositions, there is no reason to insist on a distinction between theoretical and observational propositions.

Note that this does not exclude the possibility that a theory is refuted on the grounds that it is "empirically inadequate." One may falsify the abiogenesis hypothesis based on empirical evidence such as the experiments of Pasteur. Nevertheless, what convinces us of the falsity of this hypothesis is not pure observation. The observational evidence may be interpreted in different ways and the decision as to which interpretation is superior largely depends on the accepted theories. To illustrate, fossils were not always considered as remnant organisms that once lived. Since many fossils observed were not similar to the living species, fossils were considered as either accidentally formed shapes on stones or else an

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[24] Hanson goes further arguing that what we "see" is dictated by the theory that we accept. Hanson, 1965, pp. 4-30.

intermediate step as the emergence of a living organism from inorganic world occurs, providing “empirical evidence” to abiogenesis. This last view was supported by Academy of Lynx, of which Galileo was a member.[25] What makes us accept that fossils are remnants of past organic creatures and the falsity of abiogenesis is not pure observation but also Darwinian theory of evolution in the light of which we interpret this evidence.

Single observation report that is not in accordance with the theory’s prediction will not be sufficient for its refutation. Its presence will be highly unlikely, hence, it will lower the probabilistic consistency. The accumulation of a large number of empirical problems, however, will lower the coherence of the system so much so that a search for a revision of the theory will be inevitable.

The reason for calling a theory empirically inadequate is not the presence of immutable empirical data that can be used to judge the success of our theories. Like any other propositions, observational reports are accepted or rejected on the basis of their coherence with the rest of the accepted propositions.

Notice that fallibilism is superior to foundationalism in that it enables revision of observational reports. However, it still insists on a distinction between theoretical propositions and observational propositions. Moreover, the weight of observational propositions is considered to be significantly larger than that of theoretical ones.

In the case of observing a pen partially submerged in a glass of water, one would say that the pen looks *as if* it were broken. Although by this we prefer a belief to what our sense organs dictate, hardly anyone will consider this as irrational. The most primitive observations made even without the use of complicated instruments can be dismissed as illusions. That the appearance of the pen is an illusion coheres

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[25] The name “Lynx” is chosen for this animal is known to have a great vision, symbolizing the emphasis of the academy on observation. For the studies of the academy and its views of natural history see Freedberg, 2002.

with our theories about optics, whereas the claim that it is actually broken demands a serious revision of many scientific theories.[26]

2.4.2 Problem of Underdetermination

Another persistent problem in the philosophy of science is the problem of underdetermination. Two different theories may produce identical predictions, making a choice based on empirical tests impossible. Such a case of underdetermination by empirical evidence occurred several times in the history of science. [27]

Maxwell's unification of electric and magnetic phenomena was highly problematic when it was first introduced. The theory seems to be unacceptable in the light of Galilean relativity. Since the velocity of an object is a relative property, observers that are in motion with respect to each other may experience magnetic and electrical phenomena in different ways. The rules for translating the occurrences in one frame to the other should preserve laws of nature. However, the translation rules provided by Galilean relativity do not preserve Maxwell's equations.

There appeared two alternative approaches to remedy the incompatibility of Galilean relativity and electromagnetic theory, none of which alters Maxwell's equations. The first of these was Lorentz's theory. He introduced a theory about aether which will alter length and time measurements in such a way that the incompatibility will be explained. On the other hand, Einstein's theory of relativity dispensed with the aether and introduced new transformation rules to replace

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[26] Similarly, it is hard to criticize anyone for questioning the reliability of an apparatus that has never been used before—such as Galileo's telescope. Galileo's observations required that the Aristotelian explanations of the motion of the heavenly bodies should be rejected. So, without an alternative explanation it is quite reasonable to reject the empirical evidence as being "made up" by the instrument itself.

[27] Among the several varieties of the underdetermination problem I am referring only to the version in which two theories have isomorphic mathematical structures so that all predictions whether tested or not are identical.

Galilean relativity. These two alternatives are empirically equivalent since Einstein's theory makes use of exactly the same set of equations which are even today called Lorentz transformations. However, Einstein's theory was quickly accepted even by Lorentz himself.

The theory choice cannot simply be based on the criteria of empirical adequacy. That there is a medium filling the universe is an idea that never had independent empirical support or refutation even though the often cited null result of Michelson-Morley experiment is generally considered as providing experimental data refuting the existence of aether. Indeed its existence or non-existence never became an issue of its own. Aether was needed to render wave theory of light tenable.

When light was considered to be a wave it immediately required a medium in which to propagate. This is because all waves that we can think of propagate in a medium and do not have an existence independent of it. Consider for example sound waves. They cannot exist without air, since sound is nothing but a propagating disturbance in air. Therefore when the wave theory of light was suggested, a medium was also made up to fill the space between the stars and our earth so that their light could reach us. That was the only reasonable explanation for how a wave can traverse that distance.

Of course this was not the only theory about light. The particle theory of light was also a reasonable suggestion not only because it dispenses with the aether but also became there were other phenomena that the wave theory was inadequate to account for. Indeed it turned out to be a real challenge for the scientists before the invention of quantum theory to decide about the nature of light. It seemed that light was either wave or particle but both views had serious empirical problems.

The difficulty was overcome when quantum mechanics made it evident that the alternatives wave and particle were not exhaustive—there is a third possibility. But by the time this was realized one strange property of light, namely that its speed is the same in vacuum for all observers was established and the aether theory was made unnecessary. Indeed nothing proved that it did not exist. Only we no longer needed aether to explain the propagation of light from the stars to our eyes.

Contrary to the common myth which elevates the status of the Michelson-Morley experiment to a crucial experiment about the existence of aether, the experiment did not in fact prove anything. Michelson won the Nobel Prize for his success in "designing optical instruments" not for his "conclusive proof."[28]

Especially the development of quantum mechanics made it possible to insist on the existence of aether. In an article published in 1954—almost half a century after Einstein's theory of relativity was introduced and several decades after quantum mechanics was established—the Nobel laureate Dirac demonstrated, by an ingenious proof, that the existence of aether is perfectly compatible with our scientific knowledge.[29] He even suggested that physicists should work on it. The reason that aether is not an interesting research area today is not the result of Michelson-Morley experiment.

The choice cannot be explained unless one resorts to criteria other than empirical adequacy. Even though many such criteria are suggested, there is neither a consensus as to how to determine them, nor what their hierarchy is. This makes it difficult to rationally explain the choices of scientists when they confront cases of underdetermination, especially because the scientific community reaches a consensus quite rapidly.

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[28] The presentational speech for Albert A. Michelson made by Professor K.B. Hasselberg explains why he was nominated for the 1907 Nobel Prize in Physics as follows: "… for his optical precision instruments and the research which he has carried out with their help in the fields of precision metrology and spectroscopy." Neither Einstein's name nor theories about aether was mentioned. (Downloaded on June 23, 2007, from http://nobelprize.org/nobel_prizes/physics/laureates/1907/press.html)

[29] Dirac, 1954.

However, as I will argue, comparison of the degrees of coherence of the rival theories dictates a decisive role. So a coherentist approach captures all those supplementary criteria in a more natural way. In the case of Lorentz's and Einstein's theories the decision can be explained by noting the low probabilistic consistency of Lorentz's theory. Even if one accepts the existence of aether and its influence on time and length measurements, there is absolutely no reason for these influences to be precisely at these amounts so as to preserve Maxwell's equations. Although there is no contradiction in Lorentz's theory, it incorporates a very unlikely coincidence reducing its coherence.[30]

2.4.3 Instrumentalist Critique of Scientific Realism

A powerful argument of instrumentalism is that a theory need not be true to perform its function as a generator of successful predictions. If that is the case we would not be able to justify our theories by arguing that they make successful predictions. It is quite clear that a false theory can make correct predictions. This is not just a hypothetical possibility but is something that is quite frequently encountered in the history of science. Newtonian physics still makes quite accurate predictions in some cases even though our current scientific knowledge indicates that Newtonian physics—strictly speaking—is not true. So even if it is reasonable to expect true theories to make correct predictions, there is room for dispute that correct predictions indicate truth.

If the *only* source of justification is left to predictive success, then, as Popper argued, only falsifying evidence may be decisive. However, such an attitude deprives our unfalsified theories of any justification. So any untested conjecture

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[30] It may be argued that this case can be explained by comparing explanatory powers of the underdetermined theories. Einstein's theory really has more explanatory power over Lorentz's theory. However, at this point all I want to show is that in this case the choice of the scientific community can *also* be understood rationally by a comparison of the degrees of coherence of the competing theories. In Chapter IV, I will present an argument to the effect that coherence and explanatory power are—although closely related—distinct virtues of a theory and show that there are cases where a more coherent set may have less explanatory power.

will be as justified—and so as reasonable to accept—as, say, theory of evolution. In the Schilpp volume Popper discusses the pragmatic reasons for using severely tested yet unfalsified theories for practical purposes and preferring them over other bold conjectures that are not tested yet. Since none of them are falsified the choice cannot be understood as a rational choice according to Popper. He argues as follows:

> [T]his choice is *not* 'rational' in the sense that it is based upon *good reasons in favour* of the expectation that it will in practice be a successful choice: there can be no good reasons in this sense…. On the contrary, even if our physical theories should be true, it is perfectly possible that the world as we know it, with all its pragmatically relevant regularities, may completely disintegrate in the next second….
>
> It is this kind of consideration which makes Hume's and my own negative reply so important…. More precisely, no theory of knowledge should attempt to explain why we are successful in our attempts to explain things.[31]

A coherentist approach turns out to be more satisfactory also in these cases. In the case of Newtonian physics—or any other falsified and rejected theory—one may simply argue in a falsificationist line to indicate that abundant contrary empirical evidence significantly lowered the degree of coherence of the theory, so that a rival with a higher degree of coherence is preferred. On the other hand, when an unfalsified theory is considered, successful predictions will be contributing towards the higher degree of coherence, avoiding the risk of levelling it to any untested conjecture.

2.4.4 Duhem-Quine Problem

As the attempts to distinguish the context of discovery and the context of justification failed, it became clear that the construction of a theory is an unending process, which takes place while the theory is also being tested. Therefore, even if a theory has false predictions in the early stages of its development, it might be

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[31] Popper, 1974, pp. 1026-1027.

rational to insist on developing it further. An interesting historical case is Klein-Gordon equation. Initially its predictions of the fine structure of the hydrogen atom were incorrect. Moreover, that was the only place it made any predictions at all. Yet further research provided great empirical support even from the case of hydrogen's fine structure.[32]

Such cases are encountered so often that many philosophers of science attempted to develop methods to handle this difficulty. Unlike a naïve falsificationist approach, one tries to specify conditions under which an empirical problem is sufficient to refute the theory.

Lakatos, for example, argues that it is sometimes clear from the heuristic of the research programme that a prediction will turn out to be false, even before it is actually tested. An example is Bohr's non-relativistic calculations made for the hydrogen atom. Knowing that the high speed of electrons will require a relativistic approach, Bohr already anticipated that his predictions will not be experimentally verified. This "saved" Bohr's approach from refutation since his research programme provided the means as to how the discrepancy between empirical data and theoretical prediction can be avoided. Likewise, Kuhn also rejected naïve falsificationism and considered such empirical problems as anomalies, which are individually not sufficient for the initiation of a revolution. Individual anomalies are dealt with during normal science periods without questioning the foundations of the paradigm.

Duhem-Quine problem also emphasises this difficulty. It indicates that when an empirical prediction fails it may still be rational to hold on to the theory. The auxiliary assumptions, that have been put to use to derive the prediction from the theory, may be altered so as to restore the match between the prediction and the observation. However, since this is possible for all cases, a scientist would not be

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[32] A popular discussion of this case with Dirac's views on scientific method can be found in Dirac, 1963.

able to refute any theory no matter how much empirical evidence builds up against it. This makes it seem as if empirical evidence is almost irrelevant to the refutation of a theory. Clearly this is not the case. The history of science is full of examples where theories are dismissed and empirical evidence plays a significant role in the process of theory choice.

The concept of “ad-hoc manoeuvres” is developed precisely to discriminate legitimate and illegitimate methods of preserving the theory in spite of its empirical failures. This is n option available only to fallibilists. Since they accept that observational reports are fallible they leave room for possible corrections of observational reports. They attempted to distinguish legitimate and illegitimate means of revising observational reports. The aim is to explain rationally the historical cases, where a clear empirical failure of the theory does not terminate research based on that theory. However, the so-called “ad-hoc manoeuvres” are sometimes occurring in the development of scientific theories and some of them are far too valuable for science to be dismissed as unscientific. Therefore, Duhem-Quine problem cannot be solved by introducing the idea of ad-hocness.

A famous ad-hoc move designed to save quantum theory is due to Pauli. He postulated an exclusion principle which brought him a Nobel Prize. The principle was designed to avoid the mismatch between quantum mechanics and observation reports. Behaviour of fermions and even the stability of atoms were a mystery before this principle was suggested. The principle basically forbids two fermions to be in the same state. At the time it was proposed the principle had no independent empirical content, made no novel prediction, and no reason as to why there should be such a rule is provided.[33]

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[33] The principle was proposed in 1924 and Pauli received the Nobel Prize for this work in 1945. The first possibilities of empirical tests, however, emerged by the development of quantum chromodynamics in the 1960’s. Moreover, the principle was introduced when no possible test was imaginable. For a detailed study of the postulation of Pauli Exclusion Principle, see, Massimi, 2005.

One may not, however, suggest that we should dismiss the rule from physics for being ad-hoc. It is not just a Nobel Prize winning idea, but is also one of the major components of quantum physics. Pauli's exclusion principle holds a place in quantum theory no less central than the more popular uncertainty principle of Heisenberg. This indicates that ad-hocness is not necessarily a sign of bad science or an irrational insistence on preserving a theory.

It is so difficult to discriminate legitimate and illegitimate manoeuvres in a way to faithfully explain history of science that Lakatos gave up the task altogether. Even though he talks about ad-hocness, no such move is forbidden and a research programme is likely to be dismissed only if it is degenerating.[34] By contrast, Popper, insisting on dismissing ad-hocness from science, made his position untenable in the face of the development of quantum mechanics. The wide-spread acceptance of the principle of complementarity, which is an ad-hoc principle according to Popper's own understanding of the term, was difficult to explain if resorting to ad-hoc manoeuvres is considered irrational. Popper tried to explain this by arguing that physicists failed to understand the ad-hocness of the principle. In *Conjectures and Refutations* he wrote:

> I do not believe that physicists would have accepted such an *ad-hoc* principle [Bohr's principle of complementarity] had they understood that it was *ad-hoc*, or that it was a philosophical principle—part of Bellarmino's and Berkeley's instrumentalist philosophy of physics.[35]

It would certainly be more plausible if we were to find an explanation as to why the principle of complementarity is accepted without attributing a lack of understanding or irrational decision to physicists as great as Bohr, Heisenberg, Pauli and many others.

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[34] To see how strongly this is tied to empirical adequacy of the theory see the quotation below to which footnote 80 is attached.

[35] Popper, 1989, p.101.

The discrimination between legitimate and illegitimate modifications is more easily recognizable in a coherentist approach. In a coherentist approach one does not simply evaluate an individual proposition to see whether it is legitimate or not.[36] In case of Pauli's postulation of the exclusion principle, the fact that he postulated a rule that cannot be independently tested is no more important than what he is trying to save. Without this principle quantum mechanics is untenable. Hence, the principle increased the internal relations between propositions which would seem highly improbable without its presence. Without Pauli Exclusion Principle, one would expect every electron of every atom to occupy the orbit with lowest energy. Hence, the known chemical properties of atoms should not exist. The principle is a rule that indicates how many electrons are allowed for a given energy level. Once a level is "full" other electrons should be placed in a higher level enabling the known chemical properties of atoms. Existence of atoms with the known chemical properties is an anomaly for quantum theory if Pauli Exclusion Principle is not accepted. Such a huge amount of "anomalies" is unbearable. The probabilistic consistency, and hence, the coherence of the system is severely diminished.

On the other hand, ad-hoc modifications that are claimed by Popper as inimical to scientific development are those which induce very unlikely coincidences. These modifications are not acceptable, not because they lack independent empirical content but because they lower the probabilistic consistency—hence coherence—of the system.

It may be argued that adding the criterion of explanatory power to an essentially foundationalist approach will solve this problem. After all, Pauli Exclusion Principle increases the explanatory power of quantum mechanics. However, even

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[36] Lakatos also attempted to avoid such a separate evaluation of propositions to figure out their value. One should, in his view, be aware of the positive heuristic of the research programme to decide if the given modification is in accordance with it (see, Lakatos, 1978). Nevertheless the decision as to whether a research programme is progressing or degenerating is based on its empirical adequacy not its coherence.

though explanatory connections generally tend to increase coherence, there is no guarantee that it will always be the case and there are several exceptions. I will discuss this issue in more detail in section 2.5.

2.4.5 Accidental Generalizations

Finally, some accidental generalizations may yield correct predictions but still we would not want to call them respectable scientific theories. Such cases are generally eliminated too quickly from the scientific literature that it seems this is only a logical possibility, posing no real difficulty. However, the case of Titius-Bode law is an excellent example indicating that it is too difficult—if not impossible—to discriminate accidental generalizations from causal laws with a foundationalist or a fallibilist approach towards the justification of laws. Even novel prediction is not by itself sufficient to praise regularities.[37] I will discuss Titius-Bode law in the next chapter. But to explicate how a coherentist approach works to distinguish accidental generalization from causal laws, I will briefly discuss a similar case, Balmer series, here.

Spectroscopic analysis of elements started much earlier than the development of quantum mechanics. This research did not lead to much progress and the data collected did not help the development of a theory, since the results were too complicated to obey a simple law. However, there was one exception. Hydrogen atom provided relatively simple data (now known to be due to its simple composition) and the wavelengths were successfully described by the following formula:

$$\lambda = \text{constant} \times n^2 / (n^2 - 2^2)$$

where, n is an integer larger than 2, and the numerical value of the constant is 3645.6 Å.

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[37] A detailed history and further developments of Titius-Bode law can be found in Nieto, 1972.

This formula, suggested by J.J. Balmer in 1885, was not considered to be anything more than an accidental generalization. The integer values n do not correspond to any physical quantity and for some unknown reason it was discrete. However, as quantum mechanics developed, this formula became part of scientific knowledge and certainly much more than an accidental regularity. As soon as the idea of quantization gained support, Balmer's series turned out to be an equation revealing the structure of the hydrogen atom.

Note that the difference is not in its empirical success or predictive power. With a coherentist approach we may argue that the acceptance of the formula as more than an accidental generalization is due to increased probabilistic consistency. Initially, although it was consistent with the known theories, it was highly unlikely for the spectrum to be discrete. However, with the development of quantum mechanics, one would expect the spectra to be discrete and the formula to depend on energy levels. So with the acceptance of the new theory, Balmer's formula became a coherent part of physics rather than an accidental generalization.

A foundationalist approach would certainly lack any means to explain the shift from the initial to the final status of the law expressed by Balmer. Therefore, criteria other than predictive power are invoked. One may try to argue, say, that the distinction between a law and an accidental generalization lies in their explanatory power.

2.5 Modifications of Fallibilism

The above mentioned problems of a non-egalitarian approach necessitated at least some additions to the simple justification by empirical evidence model. The most promising suggestion is, I believe, explanatory power. It is quite a sensible expectation that a theory should not only predict but also explain phenomena. It might be hoped that some of the problems mentioned above may be avoided by requiring that scientific theories—in addition to being empirically adequate—should also have explanatory power. For example, accidental generalizations seem to be easily recognizable by their lack of explanatory power. The statement "All gold spheres are less than a mile in diameter" is an accidental generalization since it has no accompanying "explanation" as to why this is the case. On the other hand "All uranium spheres are less than a mile in diameter" is not simply an accidental generalization, for we know that the radioactive nature of uranium ensures that such a large uranium sphere cannot exist.[38] The presence of an explanation justifies the second statement and permits one to rely on it for further predictions whereas the former statement is just an announcement of past experience and cannot be trusted for future cases.

The criterion of explanatory power may also be helpful in some cases of underdetermination where rival, but empirically equivalent theories differ in their explanatory power. This criterion actually acknowledges the importance of the mutual relations between the propositions of the theory. As Bonjour expressed in his third condition and the remarks following it, explanatory relations are those that increase the probabilistic consistency of the set. Therefore, the success of this criterion in some cases does not undermine, but rather supports the coherentist approach. Moreover, as I will argue, the demand for higher coherence is often but not always a demand for higher explanatory power.

To appreciate the relation between coherence and explanatory power we should also note that what requires explanation and what counts as a legitimate explanation depend on the theory at hand. There is no fixed set of phenomena that needs an explanation or a clear theory independent rule indicating what counts as a good explanation. Therefore, when there are rival theories in the same field, *some* of their problems and legitimate solutions may not overlap. To illustrate, the question "How can one explain the observed non-spreading of the wave-functions corresponding to classical objects?" appears only in Schrödinger's realist quantum theory but not in Bohr's instrumentalist approach that refuses to apply quantum

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[38] The examples by van Fraassen are quoted in Carroll, 2006.

mechanical equations to classical systems.[39] The observed stability of macroscopic objects needs explanation only if we accept Schrödinger's wave mechanics. Lack of an explanation will lower the explanatory power of wave-mechanics but will not affect the explanatory power of Bohr's approach even though they have an isomorphic mathematical structure. On the other hand, the question "How can one identify a system as classical?" is a legitimate question only in Bohr's approach. So, explanatory power of a theory is a parameter that should be evaluated depending on the questions that the theory raises. For this reason, as Bonjour argues, explanatory power is a measure of a special type of internal relations of the propositions of the theory.[40] Hence, its increase will generally add up to the coherence of the system.

However, the criterion of explanatory power has limitations. The most serious handicap is ad-hoc modifications. Such moves, often dismissed as unscientific, tend to increase the explanatory power as well as the predictive accuracy. Therefore, a foundationalist attitude, even when supplemented by the explanatory power criterion, will make it appear that ad-hoc moves are always desired.

It is true that sometimes ad-hoc manoeuvres are preferred by scientists, and they have turned out to be essential for the development of science. One successful ad-hoc manoeuvre is Pauli's postulation of neutrino to save the principle of conservation of energy from empirical refutation in the face of radioactive phenomena. The existence of neutrinos—though later empirically confirmed, by the realization of weak interactions—was postulated as objects that are *in principle* unobservable. This shows that it was the worst kind of an ad-hoc manoeuvre aiming only to save the theory, condemned by both Popper and Lakatos. Yet it turned out to be a spectacular success.

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[39] The question is in fact raised by Schrödinger himself. His attempts to find a solution led to coherent states that might be considered as a significant progress. Unfortunately, such states seem to be available only for harmonic oscillator potentials.

[40] For Bonjour's argument see, Bonjour, 1985, pp. 98-100.

Notice that ad-hoc modifications need not reduce the explanatory power of the theory. Pauli’s postulation provides explanation for some previously puzzling phenomena. Neutrinos are designed so as to explain why they are invisible—they lack mass, electric charge or whatever property that we are able to detect. But this time coherence also increases.

However, these are only special cases, and not any ad-hoc move is legitimate. Suppose one defender of the Aristotelian system argues that the data provided by a telescope are not reliable and that all our astronomical data are a result of optical illusions. Even if he manages to construct a system that attributes precisely the required properties to the telescope, his system will not be respected. It is empirically adequate, and does not lack explanatory power. However, it is very unlikely that our optical laws work properly for the sub-lunar world, but fail for the celestial world. Therefore, this suggestion will be incoherent with the rest of physics. An actual case is Lorentz’s theory of aether. The aether with the properties attributed to it by Lorentz, established the empirical adequacy of Maxwell’s equations and Galilean relativity. It certainly managed to explain why the speed of electromagnetic waves is the same in vacuum for all inertial observers, hence match with the null result of the Michelson-Morley experiment. This is not simply a reproduction of experimental data but Lorentz provided a mechanism indicating how the observed data occurred. Moreover it is not designed just for a single experimental set up. Lorentz’s theory is testable by other experiments. It suggested a mechanism which acts universally and the effects on a system can be predicted in advance.

Nevertheless, given our background knowledge, it is highly unlikely that a substance with exactly the properties Lorentz suggests exists. There were good reasons to accept that aether exists—especially for those defending a wave theory of light. However, there was no reason to attribute it the properties so delicately tailored for sweeping away the invariance of Maxwell’s equations under the Galilean transformations. It was a great coincidence which need not exist. Therefore his theory is much less coherent than Einstein’s theory. However, there is no reason to think that its explanatory power is any less. Moreover, they are empirically equivalent.[41]

The case of relativity theory indicates two things. First, coherence is not the same thing as explanatory power. Second, predictive accuracy supplemented with explanatory power, may still fail to rationally explain history of science. One needs to introduce other criteria such as simplicity to handle the case of relativity.

It is certainly possible to introduce several criteria each detecting a special type of internal relations among the propositions of a theory, so as to finally reach a system that is equivalent with a coherentist approach. However, not only this has never been done but will also be highly artificial. Moreover, within a non-egalitarian approach there is no explanation as to why simplicity or explanatory power is a source of justification. Another weakness of this type of modifications to fallibilism is that every case requires a different ordering of the criteria. Sometimes explanatory power and sometimes simplicity seems to guide the decision but no hierarchy can be shown among these criteria. Finally, fallibilism gives a special role to empirical adequacy. Therefore, one may not justify any case where a theory that is less empirically adequate is preferred to its superior rival. There are such cases in the history of science, however, such as the case of Titius-Bode law. If one reduces the importance of empirical adequacy and equalizes it to, say, simplicity, then all the attractiveness of non-egalitarian fallibilism will be lost.

I will briefly discuss, in Chapter IV, how Kuhn, Lakatos, and Laudan dealt with these problems. I will argue that they tend to modify fallibilism with essentially coherentist ideas to make their views compatible with the history of science. But before that, let me discuss three cases from the history of science, and compare how non-egalitarian fallibilism and coherentism handle each case.

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[41] Another actual case that I will discuss in Chapter V is the hidden variable approach to quantum mechanics. Here I only want to indicate that explanatory power is not sufficient.

CHAPTER III

CASE STUDIES

I will now discuss some historical cases to argue that a non-egalitarian approach—even if it is not a foundationalist one—fails to explain the history of science as a rational process. It needs to be supplemented with criteria other than empirical adequacy, whose value cannot be established by arguments other than a coherentist approach may provide. On the other hand, coherentism can handle all these cases, and the supplementary criteria needed for non-egalitarian fallibilism naturally arise in the coherentist approach. Moreover, it is also important to note that the criteria that enable non-egalitarian fallibilism to explain one case may be totally useless for another case, so almost each case requires it own special excuses for not fitting the fallibilist approach.

3.1 Titius-Bode Law

The discovery of Titius-Bode law constitutes one of the most interesting stories of plagiarism. While translating a text of astronomy written by Charles Bonnet, Daniel Titius inserted into the text a law and also a prediction that was a consequence of the law. The addition was made as if it also appeared in the original. In later additions of the translations Titius decided to indicate that it was his own contribution. However during this time Elert Bode had already published the law—even with the exact wording of Titius. Moreover, Titius later wrote that other scientists like Freyherr von Wolf thought of this relation for more than forty years earlier than himself.

Even though who should get credit for this discovery is debated, the law and its consequences are very clear. Indeed it is contrary to any post-positivist understanding of science. There is nothing that can be called a paradigm, research program or a research tradition. There is no debate on how to interpret the formula or its metaphysical implications. The "law" consists of just one formula:

$$r_n = 4 + 3 \times 2^n,$$

where r stands for the radius of a planet's orbit and n is an index that has the values $-\infty$ for Mercury, and 0, 1, 2, … for the other planets. When in 1776 Titius published the law as an insertion in Bonnet's book there were only six known planets.

It is certainly not too difficult to express some regularity that fits to only six cases. However, the success of Titius-Bode law was not its ability to correctly reproduce the distances of the six known planets. Indeed it worked only for the first four of them—with great accuracy—but deviated from the last two significantly when compared with von Wolf's measurements—best available data of the time. Titius (and later Bode, who reproduced Titius's law almost verbatim) was well aware of this miss-match and was quick to suggest a correction. Even when the law was expressed for the first time, Titius argued that there must be another planet between Mars and Jupiter for in that case the law would accurately reproduce the distances of all the planets. The values predicted by Titius-Bode law (after assuming that there is another planet between Mars and Jupiter) were significantly more precise than the predictions by Kepler's third law.[42]

Nevertheless, there was a great problem with the prediction of a new planet. After all, in the known history a new planet was never discovered. There have been arguments indicating that the number of planets must be six out of some considerations of perfection. The possibility of there being still another planet seemed so unlikely that even Titius refrained to call it a planet. He wrote:

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[42] A comparison of Kepler's predictions, Titius-Bode Law's predictions, and those of von Wolf's along with more recent measurements can be found in Nieto, 1972, p. 2.

> Let us confidently wager that, without doubt, this place belongs to the as yet still undiscovered satellites of Mars; let us add that perhaps Jupiter also has several around itself that until now have not been seen with any glass.[43]

Therefore the discovery of a new planet at the predicted location would be one of the best possible cases of confirmation of a theory that one could expect. It was not only novel but unexpected to the highest degree.

The law's prediction of a planet between Mars and Jupiter turned out to be successful. The newly discovered planet—called Ceres—was not the only great success of the law. There appeared to be yet another planet beyond the farthest known planet Saturn. The distance of this new planet, Uranus, was again correctly predicted by the Titius-Bode law. The prediction was made even when no one thought of the existence of a planet beyond Saturn.

It is not easy to dismiss Titius-Bode law as an accidental generalization. First, it is not just an expression of the past data in a confined way. It is a tedious yet straightforward task to find a polynomial of the fifth degree to generate the radii for the known planets to arbitrary precision. Moreover, such a polynomial would also have the benefit of more reasonably labelling the planets (it certainly looks awkward, to say the least, to number the first planet as $-\infty$ and then go on with 1, 2...). Such a polynomial could easily be dismissed as an accidental generalization since it would be designed to generate a known set of numbers correctly. On the other hand, Titius-Bode law *fails* to generate the correct values. It should have been considered as false, in the light of the empirical evidence, rather than a compact expression of the past data.

Secondly, the law made a novel prediction which turned out to be correct, and even when additional planets—beyond Saturn—are observed it remained correct. There has probably never been a successful prediction in the history of science as

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[43] Quoted and translated by Nieto in Nieto, 1972, p. 10.

unexpected as this. This is too great a success for what can be expected from an accidental generalization. But even this was not sufficient for accepting Titius-Bode law as a scientific theory. The reason is that no one came up with an idea as to why this law was successful. Empirical success is not by itself sufficient.

However, the surprising discoveries of the planets having the radii as predicted made many wonder whether there is a reason for why this relation—the so called law—holds. There has been extensive work on finding a reason.

If somehow it were possible to derive the relation from say Newton's laws, then of course the law will be considered on a par with, say, Kepler's Third law. To understand whether it was simply chance or whether the law is a consequence of physical necessity (perhaps for reasons of stability) scientists attempted to apply it to other similar systems. Initially the only available planetary system was the solar system, but still the law was tested on the satellite systems of planets. Similarly even today by means of computer simulations some physicists try to understand if there is a reason for the law to hold.

Titius-Bode law fails to relate with the rest of physics, since we expect—based on our background knowledge—that the distance of the planets from the sun should be related to their masses and velocities. However the law contains no such physical parameters. That both Kepler's and Titius-Bode's laws are called "law" could not have been more misleading. Kepler's law was a scientific theory whereas Titius-Bode law was a curiously successful regularity whose success was surprising given our background knowledge. The essential difference between the two is that the latter significantly lowers the coherence of physics. Let us compare Kepler's law and Titius-Bode law, and try to understand why one of them was considered as an accidental generalization.

Kepler's third law is a relation between the period and the size of the orbit. His own statement of the law is as follows:

> … it is absolutely certain and exact that the proportion between the periodic times of any two planets is precisely the sesquialterate proportion of their mean distances, that is of actual spheres....[44]

He illustrates the use of the law by comparing the Earth's and Saturn's period to distance ratios. In a more familiar form the law can be expressed as: "the ratio of the square of the period to the cube of the radii is the same for all orbits."

Why Titius-Bode law did not receive the prestige of, say, Kepler's laws cannot be explained simply by reference to empirical success. First of all, Titius-Bode law is much more precise in its predictions than Kepler's predictions.[45] Kepler's "absolutely certain and exact" relation seems to match with observations only remotely. Secondly, unlike Kepler's laws, Titius-Bode law made a novel prediction. Therefore, the superiority of Kepler's law cannot be explained by reference to the support it received from observational reports. The accepted set of observational reports favoured Titius-Bode law and Kepler's law. Therefore a foundationalist or a fallibilist approach cannot explain the decision of the scientific community unless they invoke criteria other than empirical adequacy. I will now compare Kepler's Third Law with Titius-Bode law with regard to Kuhn's values.[46]

Let me begin with the criterion of broadness of scope. Initially it seems to definitely favour Kepler. Kepler's third law not only applies to planets of the solar system but also to any other planetary system, such as satellites of planets and even to artificial satellites. All the efforts to modify Titius-Bode law to cover at least some of these other cases, however, constantly failed. Nevertheless, at a closer look the decision is more difficult to make. What causes one to say that Titius-Bode law does not apply to other planetary systems is a simple miss-match between empirical data and Titius-Bode law. We say that the law does not cover

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[44] Kepler, 1997, p. 411.

[45] For example, the distance between the sun and Saturn is predicted by Titius-Bode Law as 100, and by Kepler's Law as 65,4; whereas the observation was 95 (Nieto, 1972, p. 2.).

[46] Kuhn, 1977, pp. 321-322.

other systems in the sense that it does not yield correct predictions. However, if this is the sense in which “broadness of scope” is used, then it would be more reasonable to suggest that Titius-Bode law has a broader scope. After all it at least covers the solar system. Whereas Kepler’s law fails to predict the observation reports of even the solar system. One can, however, argue that the error margin of the Kepler’s law is close in every case, whereas Titius-Bode law’s error margin drastically increases when applied to any system other than the solar system. Only in this sense it might be reasonable to suggest that Kepler’s Third Law has a broader scope. So, the superiority of Kepler’s Law as to the criterion of broadness of scope, is only due to its significantly low predictive accuracy. It is hard to see how one may argue that this is a reason for preference of Kepler’s Law over Titius-Bode law, especially from an essentially foundationalist perspective.

As to fruitfulness, it is quite difficult to compare the two laws. We already know that neither of the laws led to new developments in the field of astronomy. Titius-Bode law was simply forgotten as an uninteresting regularity, whereas Kepler’s laws turned out to be corollaries of another theory. Nevertheless, unlike Kepler’s laws, there is still research, though limited, on Titius-Bode law. If somehow the success of the law be linked to an underlying stability law, then it will become a significant part of research in astronomy. So even if both of the laws are unlikely to be considered as fruitful to any significant degree, there might still be hope for Titius-Bode law. Whatever is the case, the fruitfulness criterion does not favour Kepler’s law.

Another virtue that Kuhn refers to is consistency. Unfortunately, this is again not of much help in our case, since the two laws that we are comparing were both internally and externally consistent with the then current theories.

Finally let me consider simplicity. It is not clear what one understands by simplicity of a theory. If by simplicity an ease of calculation is understood, then clearly Titius-Bode law is simpler. If, by simple, one means referring to fewer entities, then again Titius-Bode law is superior.[47] Understood in this sense the criterion is sometimes referred as principle of parsimony or the principle of Ockham's razor.[48] If on the other hand simplicity is considered as a curve fitting problem like Malcolm Forster argues, then it is almost the same as predictive accuracy and Titius-Bode law is superior to Kepler's geometric constructions.[49] Kuhn's own explication of simplicity is as follows: "bringing order to phenomena that in its absence would be individually isolated and, as a set, confused."[50] His suggestion is more like the often used criterion of explanatory power. There is no distinction between Kepler's law and Titius-Bode law in their explanatory power and how much order they bring to the phenomena that they describe. However, if Kepler's law is considered to have a much broader scope, then it may be said to "bring order" to more phenomena than its rival.

These comparisons I believe indicate that the only superiority of Kepler's law is a dubious superiority in broadness of scope. It is hardly reasonable to accept that much higher predictive accuracy, greater simplicity—at least according to some versions of it—and potentially higher fruitfulness failed to compensate this. There is simply no means of telling how much predictive accuracy or simplicity can be sacrificed for some increase in broadness of scope.

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[47] Lawrence Sklar argues that "ontological elimination" is one of the common themes in the development of theories (Sklar, 2000, pp. 11-40). Simplification, he argues, can be achieved by means of eliminating entities in a theory.

[48] Sober suggests the use of this criterion while choosing among empirically equivalent theories. He argues that unlike inductive or descriptive simplicity, parsimony can provide a choice which is not based on only pragmatic considerations.

[49] Forster, however, argues that there is still a possibility that simplicity may not guarantee predictive accuracy even when it is understood as a curve fitting problem. However, Titius-Bode law is so uncomplicated that his arguments do not apply in this case, see, Forster, 1998, http://philosophy.wisc.edu/forster/SciSimp.pdf.

[50] Kuhn, 1977, p. 322.

In the case I consider the predictive accuracy of Titius-Bode law over Kepler's third law is so overwhelming that it is simply not convincing that broadness of scope is sufficient to compensate for this difference. The worst match between the data and Titius-Bode law's prediction is in the case of Mars, where the discrepancy is less than 7%; in the case of Saturn it is 5% and all the other predictions match exactly with observations. The best prediction of Kepler's third law however is Mars with more than 15% of error, in the case of Saturn the error margin is way above 30%.[51]

Moreover a fallibilist, who spares the epistemic role to empirical propositions just like Popper or Lakatos, has no satisfactory argument as to why possessing any of the virtues other than empirical adequacy lends justification to a theory. There is no empirical proposition that would "justify" the claim that simpler theories are more likely to be true. The addition of criteria other than empirical adequacy, especially those that are rather a measure of the internal relations of the theory such as explanatory power or beauty, is rather a twist towards a coherentist approach. It is, therefore, more natural to begin with a coherentist approach rather than introducing such criteria in an unjustified way. Moreover, introduced in separation these criteria do not seem to work properly. Neither of the criteria suggested so far can explain why Titius-Bode law is not a fundamental law of nature.

On the other hand, a coherentist approach would easily handle the case of Titius-Bode law. Newtonian physics implies no rule as to the possible distances from the sun that can be occupied. That possible orbital distances are not quantized makes it impossible to predict which ones are actually occupied. However, for stability, the planet should assume a velocity appropriate to the orbit that it occupies. The period of the planet, i.e. the time that elapses for a full cycle, is again a function of the distance from the sun and the velocity of the planet. Therefore, if the period is

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[51] The orbital distance for Earth is excluded, since it is used as a calibration distance and hence, is exact for both.

known then the distance can be predicted.[52] Kepler's third law is an expression of this relation. Therefore, its presence is expected from Newtonian laws. The set of propositions making up the Newtonian physics and Kepler's third law has a high degree of probabilistic consistency. Conversely, Kepler's third law demands the stability of an orbit about a fixed source of force to be determined only by the distance from the source of force and the velocity of the planet. Therefore, a theory like Newtonian physics is (although not implied in any sense) expected from Kepler's third law.[53]

However, Titius-Bode law dictates the legitimate orbits, without needing to observe the velocities of the existing planets. Prediction of which orbits are actually occupied is not possible given the then current beliefs on the planetary systems. This is not because of an incompleteness of the theory but rather the background knowledge made it highly unlikely that some orbital distances are more likely to be occupied (or be different in any sense). It is highly unlikely that a rule, which does not refer to the velocity of the planets (either directly or indirectly such as by referring to the periods), would correctly predict the orbital distances of the planets. Therefore, Titius-Bode law had a low probabilistic consistency given the belief that the orbits are not quantized. Note that there is no contradiction between the two either. The case is similar to the unlikely structural similarities of continents without the idea of continental drift. Therefore, unlike Kepler's third law, Titius-Bode law causes incoherence but not inconsistency, since the possibility of quantization of the orbits is not forbidden. It is just that there is no reason for them to be quantized.

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[52] I explain the relation as if the orbits are circular. However, for elliptical orbits a bit more complicated explanation will give the same conclusion: the period and the velocity of a planet should be proportional, but without any information as to the values of one of these parameters the other cannot be predicted.

[53] Kepler even suggested (although for wrong reasons) that an inverse square law must be responsible for the motion of the planets just like Newton's law of gravitation.

The emergence of Newtonian mechanics only strengthened this case. Strictly speaking Titius-Bode law and Newtonian mechanics are consistent. However, it is very unlikely that they are both true. Hence jointly they have a low degree of coherence. On the other hand Kepler's law and Newtonian mechanics match so well that they support each other. They have a high degree of probabilistic consistency hence coherence.

Moreover, the empirical failures of Kepler's third law do not undermine, but on the contrary, increase its credibility. It is derived for a single mass revolving around another mass. Naturally if there are other massive objects in the region the orbit will deviate from the predictions of this simple model. In the solar system, other than the sun, there are several planets, their satellites and quite a large number of asteroids influencing the planets. Therefore, one anticipates the law to be only a first approximation and to sometimes significantly deviate from the observed radii. Even the conditions of deviations are available. With more closely spaced planets the deviations are expected to be larger.

So even though Titius-Bode law has more accurate predictions, it is dismissed as an uninteresting generalization. Newtonian physics, Kepler's third law, and observed radii of the planets constitute a much more coherent set than the one containing Titius-Bode law. Note that observation reports undermine Kepler's law when taken in isolation. However, they form a coherent set when Newton's laws are included, since these laws transform the empirical difficulties from anomalies to expected deviations, *increasing* the probabilistic consistency of the set formed by the observation reports and Kepler's third law.[54]

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[54] I do not simply say that by a more careful calculation including the effects of the other planets one may predict the orbits of the planets more accurately and so achieve a better match with observational reports. There is no way from Kepler's third law to include the effects of the other planets. What I emphasise here is that the errors of Kepler's Third Law turned out to be expected and not simply eliminated. The law holds perfectly only for an ideal planetary system and only as a first approximation for any real system.

A coherentist approach not only rationally explains why Titius-Bode law does not qualify as an important part of science, but also what kind of research it may initiate. Due to its great success one wonders if there is an underlying reason as to why the law works, so that its inclusion into our system will make Titius-Bode law and Newtonian mechanics coherent. For this to be the case Titius-Bode law should in some form or other be applicable to other planetary systems. Not only because of increasing the scope but also to make us see under what conditions the law fails and realize on which hidden physical parameters it depends. An ingenious suggestion to develop Titius-Bode law into a scientific theory is to introduce additional factors which are insignificant for the solar system but contribute to the orbital distance significantly for other systems or to demonstrate that the apparently constant numbers of the formula are actually a combination of the masses and/or periods of the planets of the system. If these were the case one would have good reasons to hold Titius-Bode law as more than an accidental generalization. Notice that discovering the constants to be variables depending on the masses and velocities of the planets will have no effect on the predictive power of the law. Yet, if this turns out to be the case, the law will be considered as equally scientific as Kepler's third law.

It can be argued that Titius-Bode law is an exceptional case in history of science. To some extent this is correct. However, there are several cases which were initially like Titius-Bode law, but later the success of the law was explained and so it became a part of science. Such cases encouraged the above claims that there was a chance that further development might have raised Titius-Bode law to a level of respected scientific theory. One such case is Balmer's series mentioned above. I will now discuss a few of these cases to show that not only the refusal of accidental generalizations from science but also acceptance of laws can be better understood in coherentist terms.

3.2 Ideal Gas Law

A similar regularity, the Ideal Gas Law, does not share the fate of Titius-Bode law. I will argue that the acceptance of the Ideal Gas Law cannot be explained simply by reference to its empirical adequacy.

The Ideal Gas Law is an historical case where a description of regularity is first considered as unscientific, and later qualified as the correct description of how gases behave. What makes it especially interesting is that throughout this transformation—from being an accidental generalization to a law—no new empirical evidence was provided. The evidence that was once neglected, was later considered as providing overwhelming confirmation.

The transformation is again caused by replacement of some of our beliefs that have very low probabilistic consistency with Ideal Gas Law, with beliefs that have a high probabilistic consistency with it. The theories that were accepted, when the law was first suggested, were not coherent with it. The Ideal Gas Law treats pressure as the effect, caused by the collision of gas molecules with the boundaries of the container. It also treats temperature as a measure of the average kinetic energy of those molecules. Therefore, to accept the Ideal Gas Law as something more than a mere accidental regularity, one should also accept that matter has an atomic structure. Ludwig Boltzmann's arch enemy Ernst Mach was arguing that atoms, since they cannot be observed even in principle, should not be part of scientific theories.

But after the view that matter is composed of atoms is established, with notable contribution by Einstein, the Ideal Gas Law became an obvious consequence of our understanding of matter. If matter is composed of atoms, then it is natural to expect that the interactions among these components play some role in the determination of the properties of matter.

The law was not suggested as a simple generalization of observational data like in the case of Titius-Bode law. The law was stated with an accompanying understanding of matter that would support the Ideal Gas Law. So it was indeed acceptance of this understanding of matter that really altered the status of the law.

Boltzmann's suicide in 1906 is generally considered to be caused by his exclusion from the scientific community. His views were not compatible with the dominant positivistic approach on the continent. And his critics were not just too numerous for him to handle but also included highly respected scientist like Mach. The disagreement was surely on more than whether or not the Ideal Gas Law holds. The law is a very simple relation:

$$P\,V = N\,k\,T$$

here 'P', 'V', 'N', 'k', and 'T' stand for pressure, volume, number of molecules, a constant number, and temperature, respectively.

Unlike the Titius-Bode Law this relation has some physical basis. It is a relation between physical quantities and founded on the assumption that gases are composed of tiny particles, which behave like Newtonian particles.

Today it seems to be easy to test this relation. However, as the name implies, this relation will certainly not hold for any real gas. It was derived under the assumptions that there are no forces acting between the molecules, and collisions between them are always perfectly elastic. This means that the relation can be used as a good approximation for those cases where the gas is very dilute, and also the molecules making up the gas are small.

Indeed neither Mach nor any other critique of Boltzmann argued that the relation is not a good approximation for dilute gases. They rather focused on the idea that the relation was based on—the kinetic theory of gases. According to this idea all physical phenomena were to be understood in terms of interactions between particles that obey the laws of Newtonian mechanics.

The proponents of the kinetic theory argued that heat was motion.[55] Their opponents, on the other hand, saw no reason to suppose that heat should be a phenomenon reducible to motion. Indeed there was none. Mach was perfectly right when he questioned the temptation to explain every phenomenon (such as heat) in terms of the interaction of Newtonian particles.

Not only the positivistic attitude ruled out the postulation of atoms but also there was no reason to claim that a good explanation is a mechanical explanation.[56]

As the view about the atomic composition of matter gained more support (especially by Einstein's work on Brownian motion), the kinetic theory of gases became coherent with the rest of our theories of matter. Therefore the Ideal Gas Law became an established law.

The difference between the Titius-Bode law and the Ideal Gas Law is striking. The former is more successful in its predictions, and moreover, it led to novel predictions as important as the discovery of new planets. By contrast, the latter is at best an approximation. Nevertheless Titius-Bode Law is rarely mentioned in physics books and only as a historically interesting story.

From an empiricist standpoint the development of the Ideal Gas Law is difficult to understand. The attitude towards the law did not change due to any modification of the law itself or by new observations. The difference is caused by a change in other theories. Hence, in this case a foundationalist or even a fallibilist approach can only be preserved by supplementing it with what Kuhn calls external consistency. Just like explanatory power, external consistency is also a measure of

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[55] Ironically this idea that was severely criticised by the nineteenth century physicists was defended as early as 1620 by Bacon. In his *Organon* he wrote: "what we have said about motion ... should not be taken to mean that heat generates motion or that motion generates heat ... but that actual heat itself, or the quiddity of heat, is motion and nothing else...." (Bacon 2000, Book II, aphorism XX.)

[56] A detailed discussion is provided in Sklar, 1993.

the relations between propositions of the system. In this case not only among the propositions of the theory but also a larger set that contains other accepted theories. Therefore, the increase of external consistency is an indication of an increase of the mutual relations and probabilistic consistency of the set that is under consideration. Whereas non-egalitarian fallibilism needs to be supplemented with an extra criterion in this case, from a coherentist standpoint only the evaluation of probabilistic consistency suffices.

Notice that the Ideal Gas Law fails to generate accurate predictions in most of the cases. However, just as in the case of Kepler's Third Law, when the whole system is considered, these empirical problems do not undermine but rather support the law. We not only know why the Ideal Gas Law should hold but also know under what conditions it should break down.

I will now consider one final case of a quite different sort. This time a well-established scientific theory with a significantly high predictive accuracy and explanatory power, but containing a very unlikely proposition that undermines its probabilistic consistency. This case will reveal how scientific theories develop even when no significant empirical problems are encountered and how serious consequences a low probabilistic consistency may have.

3.3 The Concept of Mass in Newtonian Mechanics

Newtonian physics has two different concepts of mass. However, they have a curious relationship that seems to be very unlikely. This relation, strictly speaking, causes no contradiction but significantly lowers the probabilistic consistency of the system. Hence, if my claim that a coherentist approach would explain the acceptance of scientific theories better is correct, one would expect this to cause dissatisfaction even when the theory was considered to be empirically adequate. Let me first state the two concepts and their problematic relation.

The famous second law of Newton is a relation between the force and the acceleration of the object described from an inertial reference frame. In Newton's words: "*a change in motion is proportional to the motive force immersed and takes place along the straight line in which that force is impressed*."[57] The proportionality constant in this relation depends on the object and is called its inertial mass. So, expressed as a formula (and neglecting the directions) in the familiar notation the law is:

$$F = m_I a$$

Where F is the force, a is the acceleration and m_I is the inertial mass (the proportionality constant depending on the object). Notice that the mass here is not a physical quantity that is expected to appear in our formula. The law is a relation between force and acceleration. The existence of a property called inertial mass is a consequence of the law. The function of this inertial mass can be considered to be to resist motion, since the greater the proportionality constant the less the acceleration of the object under the action of the same force.

There is, however, another mass of objects that need not have any relation with this inertial mass. The famous law of gravity is stated by Newton not as a single principle but it is rather built up in several steps each introducing one aspect of the law. What is important for us is only the part where a concept of mass which is distinct from the inertial mass is introduced: "*Gravity exists in all bodies universally and is proportional to the quantity of matter in each*."[58] The quantity of matter was defined at the very beginning of *Principia* as the density times the volume of an object.[59]

The quantity of matter appears here not as a proportionality constant but rather as a physical property that generates force. Unlike inertial mass, the quantity of matter

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[57] Newton, 1999, p. 416.

[58] Newton, 1999, p. 810.

[59] Newton, 1999, p. 403.

is a source of force. There is absolutely no reason why inertial mass and the quantity of matter, which is also called gravitational mass, should have the same value. This is no more likely than having the electric charge of an object to have the same value as its gravitational mass. Surprisingly, however, for any object the gravitational mass and the inertial mass of that object have precisely the same value.[60] This is a much unexpected coincidence. Certainly this does not reduce the empirical adequacy of the theory, for the theory does not predict that these values must be different. However, the theory suggests that these are two completely unrelated properties whose values match only accidentally. This might not be of much significance if this were the case only for some of the objects, but since it is true for every object one suspects a deeper reason for this. Again, this is an instance where an empirically adequate theory has a notable weakness in its probabilistic consistency.

That Newtonian mechanics was not refuted immediately is not a challenge for my view, since I already stated that a refutation can only take place in the presence of a better alternative. There is no absolute degree of coherence below which theories are discarded. However, as I emphasized before, the development of the only available theory is also an indicator of what counts as a source of justification. If theories are justified by successful predictions, then one aims to increase the predictive accuracy of a theory even when it has no rivals; similarly, if simplicity is a virtue then we aim to simplify a theory as much as possible even when it does not face any serious challenge.

When we view the development of Newtonian physics, we see that there have been attempts to increase its empirical adequacy. Some of these led to the discovery of a new planet (Neptune), some others led to the development of statistical methods. The main reason for the development of statistical methods is the difficulty of finding exact solutions for the many-body system. There have also

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[60] Strictly speaking, the two masses are proportional and the proportionality constant is the same for all objects. I simply assumed that the value of the proportionality constant is 1.

been attempts to increase its simplicity understood in the sense of Sklar's ontological elimination. Naturally there have also been attempts that failed but those that succeed provide sufficiently rich information as to what justifies our theories.

From a perspective that reserves the role of justification to empirical propositions, the presence of an improbable coincidence does not pose problems for the theory. However, we know that from the very beginning this coincidence disturbed scientists. To explain this, one has to resort to criteria other than empirical adequacy. On the other hand, a coherentist understanding immediately reveals why this coincidence is disturbing. Our belief system requires that it is extremely unlikely that two independent properties have the same value for all objects. Even though there is no contradiction in this set of beliefs, the probabilistic consistency is very low.

The attempts to find a reason for this coincidence led to the development of the general theory of relativity. As Zahar argues, Einstein,

> [r]efusing to believe in a simple coincidence… proposed to construct a theory which would explain the identity of gravitational and inertial masses, and which would thus unify gravitation and kinematics.[61]

Zahar's aim in that article is to argue that there is a "rational heuristic" that guides scientists towards new discoveries. However, if one insists that the only source of justification is a match between prediction and observation, then it is hard to see why heuristic is a useful guide for reaching more justified theories. In Zahar's understanding, heuristic includes "metaphysical elements" like correspondence principle or Meyerson's Identity Principle, none of which has a direct relation with empirical success.

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[61] Zahar, 1983, p. 252.

What Zahar correctly sees, I think, is the importance of the internal relations among the propositions of a theory even if some of these propositions are not testable. Unlike foundationalism, coherentism explains why such relations are important and play a role in the decisions of the scientific community.

The development of Newtonian mechanics indicates that a scientific theory that has a great empirical adequacy and explanatory power may still cause disturbance and initiate a search for alternative theories in case it include propositions that are unlikely given the rest of the propositions of the theory. This is in perfect accordance with what Bonjour says.[62] Hence, a coherentist approach successfully explains the disturbance among the scientific community towards a well established theory in case of Newtonian mechanics.

In the next chapter I will focus on what Zahar calls “metaphysical elements” present in the heuristic of a research programme. Many philosophers of science acknowledged their role in both the development of a theory and cases of theory choice. I will argue that since coherentism is not in need of making a distinction between theoretical and observational propositions, the role of metaphysical propositions requires no special treatment and a coherentist approach is far more satisfactory in understanding scientific change.

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[62] See the quotation to which footnote 10 is attached.

CHAPTER IV

METAPHYSICS AND SCIENCE

It is well-known that philosophy and science influenced each other. Both the developments in science had philosophical consequences and also some philosophical views shaped the development of scientific theories. The fact that some scientific theories are so evidently based on some philosophical views, led philosophers of science to consider the role of metaphysical views in science. That Robert Boyle adopted a mechanistic world view or that Ernest Mach was a positivist is well known. Moreover, there are quite a lot of studies on how scientific work influenced the evolution of philosophy. The discovery of Einstein's theory of relativity rendered many of Kant's views problematic. What I want to emphasize here is how philosophy shaped scientific theories. Therefore, rather than searching for philosophical consequences of a scientific theory, I will focus on the consequences of philosophical views on the development of science.

Let me illustrate why a consideration of the role of metaphysical propositions is inevitable for fully understanding theory choice decisions by an example from the history of science. Robert Boyle's conviction that all phenomena can be explained mechanistically is not an empirically testable claim. However, it guided Boyle's research. One may attempt to simply set aside this conviction as part of the context of discovery and irrelevant to the ultimate testing of Boyle's theories. This will surely be a foundationalist move, where the secure foundation is the observational propositions. However, if one is to supplement foundationalism with criteria other than empirical adequacy such as explanatory power, the standards as to what counts an explanation will surely contribute to the evaluation of the theory. In order not to deprive oneself of all sources of distinguishing accidental generalizations from causal laws, one should try to find the means to explain rationally the role played by such metaphysical propositions.

Fallibilists also acknowledge the role metaphysical propositions play in the development and evaluation of scientific theories. However, unlike the egalitarian approach of coherentists they undervalue the epistemic value of metaphysical propositions.

I will argue, in this chapter, that major philosophers of science such as Kuhn, Lakatos and Laudan, all resorted to tools that would more naturally arise in a coherentist approach, while they were considering the role of metaphysics in science. Moreover, their unwillingness to assign epistemic values to such propositions undermined their attempts to rationally explain scientific change. Fallibilism does no better than foundationalism in explaining scientific change unless it spares a significant role for criteria other than empirical adequacy in providing justification to scientific theories.

4.1 Paradigms

Noting that naïve falsificationism is untenable, Kuhn argued that one should consider a larger unit than theories to explain scientific change. Although what eventually changes a paradigm is still the accumulation of anomalies, a single empirical failure is not a real challenge for a paradigm. A few mismatches between prediction and data are rather puzzles for scientists. A paradigm is not just a set of formulas; it guides the researcher as to how to make research and dictates what the solution should be like. In an article discussing how scientists work during normal science periods according to Kuhn, Nickles argues that Kuhn's suggestions can be viewed as an attempt to solve the Meno Paradox. The paradigm provides the physicist not only with puzzles but also inform them about what the solution will look like.[63]

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[63] Nickles, 2003.

Kuhn refers to “metaphysical parts of paradigms” to remind us the presence of some of the shared commitments of the scientific community.[64] These metaphysical commitments are not things to be discovered or proved by scientific research but are the foundation of the scientific activity during the “normal science” period. And it is for this reason that scientists seem more concerned with philosophical issues during revolutions, which is the period when the foundations of the discipline (including its metaphysical commitments) are being replaced by new ones. Here it is evident that Kuhn not only discusses relations between some formulas and observational reports. Paradigms are meant to be structures that include even some metaphysical views.

As Kuhn stressed the presence of a single anomaly is not sufficient to demolish a paradigm. This is clearly not a naïve falsificationist view. However, Kuhn still defends a non-egalitarian position. What ultimately undermine a paradigm are neither its metaphysical commitments nor conceptual problems. The accumulation of too many anomalies—that is significantly lowered empirical adequacy—is the reason why revolutions in science occur.

Anomalies reduce the degree of coherence of a system. An anomaly is the presence of an accepted proposition that is considered to be extremely unlikely given our background knowledge. The observed orbit of Uranus constituted an anomaly within the Newtonian system since our theories predicted a significantly different orbit. However, this anomaly is not a refutation of the Newtonian physics because one may always hope to find an influence that has not yet been considered that will explain the deviation restoring the consistency of the system. Strictly speaking the observation report about the orbit of the Uranus was unlikely but not logically inconsistent with the Newtonian system of laws. The discovery of Neptune avoided the anomaly. This is because the coherence of the system is

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[64] Kuhn, 1970.

restored. With the influence of Neptune evaluated the observed orbit of Uranus is no more an unlikely observation report.

Therefore a system containing anomalies have a lower degree of probabilistic consistency, hence a lower degree of coherence, when compared with what it would be without those anomalies as Bonjour's "fifth condition" reminds us. Individually their effect can be counterbalanced by the strength of the relations among other propositions. However, the accumulation of anomalies significantly lowers the coherence of the system. During a "normal science" period the scientific research is guided by some metaphysical views, for coherence requirement is not limited to empirically testable propositions.

The values other than empirical adequacy that are introduced by Kuhn also focus on the internal relations of the system. The disruption of the metaphysical commitments of the paradigm will also alter the judgements as to which theory possesses these values to a greater extent. A new paradigm is not just a new set of formulas whose empirical success may or may not be better than the former. But the standards as to what counts as better may be modified.[65]

Nevertheless, Kuhn avoided an outright coherentist approach.[66] Because of this he failed to state why, say, broadness of scope is a source of justification for a paradigm. His values are suggested as reasons to prefer one theory over the other without necessarily providing epistemic support to the preferred theory. This motivates some of his critiques to consider Kuhn's approach to science as depicting scientific claims to be without any justification and having their force

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[65] Kuhn later weakened his views on incommensurability. However, the essential point is unaltered. He emphasised the importance of the coherence of a system, and the inability to test the success of a paradigm simply by empirical evidence. Kuhn, 2000.

[66] In a recent article Kuukkanen argued that the major reason for Kuhn's avoidance is his conflation of coherentist theory of justification with a coherence theory of truth. He also argues that despite what Kuhn says "[Kuhn's] philosophy can be incorporated into a coherentist epistemology" (Kuukkanen, 2007, p. 555).

basically due to the existence of large number of believers. Such marginal conclusions from his views, especially based on his incommensurability thesis, are criticised by Kuhn himself.[67] However, if the values are not useful to indicate that a theory possessing them is more likely to be true than a rival lacking them, it is hard to see how one may defend their use within a scientific realist position.[68]

Without a secure basis to justify our beliefs, the only reasonable alternative is coherentism. By rejecting both, Kuhn motivated sceptical challenges towards science, even though he does not share these views.

4.2 Research Programmes

When Lakatos argued that heuristic plays an important part in science, he actually meant that within an already existing research programme, heuristic guides the scientists in their attempts to develop the theory when it encounters empirical problems. As Zahar argued, heuristic can be used in a broader sense so that it can also be used to guide scientists for further developments, even in the absence of empirical difficulties.[69]

For Lakatos and Zahar, metaphysical principles are far too important to be neglected as mere psychological factors. To illustrate this, Lakatos argues that it is

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[67] Kuhn, 2000.

[68] Wesley C. Salmon distinguishes three different sorts of virtues: "*informational virtues, confirmational virtues,* and *economic virtues*" (Salmon, 1990, p. 196). As long as one insists that the only confirmational virtue is empirical adequacy or event that it is the most important one, the addition of other non-confirmational virtues will not make his position an egalitarian coherentist position. However, it would also not be reasonable to consider such a view as a traditional foundationalism as long as it accepts the fallibility of observational reports. Such views may be considered as attempts towards avoiding the serious problems of foundationalism. What I argue in this thesis is that they are insufficient and a real alternative is coherentism which explains why we are guided towards theories that are more likely to be true by relying such so-called non-confirmational virtues.

[69] For a detailed study of the emergence of general theory of relativity from Newtonian mechanics by, what Zahar calls almost deductive steps, see Zahar, 1989. Zahar broadens the heuristic so much that even the emergence of a new research programme can be explained by it.

the "positive heuristic" of the research programme that enables scientists to proceed in the presence of empirical refutation. Precisely because he allows working on a refuted theory, he considers his account as superior (in the sense that it describes the actual historical cases more accurately) to naïve falsificationism.[70] For Zahar, some metaphysical propositions have a prescriptive role and their recognition will show that the process of discovery "rests largely on deductive arguments."[71] Nevertheless, heuristics of a particular theory is generally difficult to express for both of them.

Lakatos says "if the positive heuristic is clearly spelt out, the difficulties of the programme are mathematical rather than empirical."[72] What led Lakatos to this conclusion is his attitude to treat heuristic as mere rules of thumb. One of the examples that he provides as a heuristic of Newtonian mechanics is the following: "the planets are essentially gravitating spinning-tops of roughly spherical shape."[73] This is nothing but an idealization. At several points he emphasises that researchers know in advance that these assumptions are incorrect. They will get better agreement with experimental data as such assumptions are relaxed. Therefore, even when experimental data seem to refute their theory, they stick to their research programme. Lakatos's own examples includes Bohr's research programme now known as the old quantum theory. When the initial predictions are derived from the theory no one expected the results to match with empirical evidence. The calculations did not take into account the relativistic effects but the significantly large speeds of the electrons ensured that relativistic correction would not be ignorable.

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[70] Lakatos, 1978, p.51.

[71] Zahar, 1983, p.245.

[72] Lakatos, 1978, p. 51.

[73] Lakatos, 1978, p.51.

This example also lets us realize his conviction that, when clearly stated, heuristic expresses just a demand for more careful mathematical calculations. Naturally what once was a part of heuristic may be dropped from the research programme when the mathematical techniques are developed well enough to drop an idealization.

Lakatos's suggestion implies that scientists make use of approximations and idealisations in order to derive empirical predictions from their theories. This is certainly correct. However, if we expect heuristic to guide research, it should enable scientists to predict which idealizations severely alter the predictions. Treating the solar system as a non-relativistic system will not lead to significant deviations of the calculations. However, ignoring the satellites of a planet may lead to a significant deviation.

The assumption of the Newtonian research programme that "planets have no interactions among each other but move only under the influence of the sun" should be relaxed at some stage of the development of the theory if it is to be empirically adequate to any significant degree.

However, after this assumption is dropped it would not be acceptable to explain the remaining discrepancy by reference to the possibility that the combined effect of the objects in a distant galaxy might prove significant. When Newtonian mechanics failed to describe the orbit of Mercury no scientist attempted to calculate the influence of distant galaxies. Although all the calculations are made assuming that "only the masses in the solar system exert force" we would still not be satisfied with the possibility that relaxing this assumption would improve empirical success. The reason is that what guides scientists is not just the knowledge of what idealizations are made. But they are also equipped with the knowledge of which of these idealizations are more significant that the others and how much increase in precision will they provide if they are to be relaxed.

Moreover, Lakatos's account falls short of explaining the cases where researchers makes progress even when there is a yet undiscovered parameter that is disrupting the empirical adequacy of the research programme. The Klein-Gordon equation was empirically inadequate because it was not taking into account the spin of the particle. However, when the equation was first suggested, that particles have spin was not known. So what led scientists to believe that further developments will eliminate empirical discrepancies is not their realisation of an idealization. They had absolutely no idea that they were making an approximation. That there was an idealization can only be attributed by a later generation of scientists.

Zahar attempted to remedy these problems by modifying Lakatos's concept of positive heuristic. He suggests that heuristic contains the metaphysical commitments of the research programme.[74] However, when he talks about the "metaphysical character of the hard core" he mentions the principle of correspondence, and when he explains how discovery is guided by metaphysics, he refers to Emile Meyerson's Identity principle. Such general principles might play a role in science; however they are not specific to a research programme. But every research programme has its own philosophical commitments, which are not essential for science in general—and therefore may not exist in another research programme.

As opposed to Lakatos, Zahar avoids distinguishing the hard core and the heuristic of a research programme sharply, since in his account they are not flexible. He notes that scientific decisions (or for that matter everyday decisions) are based on some metaphysical principles. But they are too general and apply to *all* research programmes. So they cannot be altered. "Such principles are stable in the sense that they preceded the birth of science proper and have since remained largely

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[74] Zahar, 1983. Zahar argues that the hard core and the heuristic are not really distinct.

constant; they could be innate and even possess a genetic basis."[75] His examples include correspondence principle and Emile Meyerson's identity principle.

However, he also notes that when kept so general it is not possible to see how heuristic guides the development of the theory. Moreover, all such principles need not be (and probably are not) consistent.

> [S]uch principles are mostly non-technical, perhaps even vague. If they were all made precise at once and then conjoined, they might well entail contradictions…. The heuristic of a research programme is determined by the *coherent choice* it makes among these principles and by the more or less sharp formulation it gives to each of them.[76]

Therefore, Zahar argues that a research programme selects some possible metaphysical principles which are then sharply formulated to yield a prescriptive import, and finally this prescription leads to a formulation of a "meta-statement" or a rule that is technical enough to be directly relevant in restricting the form of the theory.

Contrary to his reference to extremely general principles that might even be genetically hard-wired in human thinking, he also provides very specific principles for some theories. Let me consider one of them:

> Special Relativity is based on the metaphysical proposition that no privileged inertial frames exist. This leads to the prescription that all theories should assume the same form in all inertial frames; the corresponding meta-statement is that all laws of nature are Lorentz-covariant.[77]

When understood in this more restricted sense (as opposed to genetically hard wired principles of human thought) Zahar's account is much more helpful for

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[75] Zahar, 1983, p. 260.

[76] Zahar, 1983, p. 260.

[77] Zahar, 1983, p. 249.

understanding some cases in the history of science than Lakatos's account. Nevertheless, Zahar tried to use heuristics only to give an account of discovery as a largely deductive process and avoid making use of them in theory choice. On this matter he manifestly sided with Popper:

> Yet Popper is perfectly right in distinguishing between the context of discovery and that of justification. In the methodological appraisal of theories it is not the heuristic devices as such, but certain logical considerations which play a significant role.[78]

Zahar never considered the possibility that the metaphysical principles that form the heuristic are inconsistent. He simply assumed that a heuristic of an actual scientific theory is a coherent subset of all possible such principles. Moreover, since he treats the principles as part of human thinking, the question "what if a particular scientist dissent from these principles?" did not occur.

Whatever the case, the only place in Lakatos's system to introduce the metaphysical commitments of a research programme are positive heuristics. Just like Kuhn, Lakatos also realized the difficulties of maintaining a foundationalist attitude towards the justification of scientific theories, while at the same time arguing that theories are actually justified. Lakatos, recognizing the possibility of empirically successful theories to be false, introduced essentially coherentist elements in his methodology such as allowing ad-hoc manoeuvres to be sometimes legitimate. To illustrate, while discussing ad-hocness he wrote: "the successive modifications of the protective belt must be in the spirit of the heuristic."[79] Although not clarified further, "being in the spirit of heuristic" seems to be a measure of internal relations of the propositions of the research programme. That is not to say that all ad-hoc manoeuvres are legitimate but some of them are. Moreover, these relations are so important that a modification that is "not in the

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[78] Zahar, 1983, p. 246.

[79] Lakatos, 1978, p. 179.

spirit of the heuristic" is a sign of bad science even though it establishes the empirical adequacy of the programme.

Nevertheless, like Kuhn, Lakatos also refrained from adopting a coherentist approach and insisted that the only source of justification is empirical success. While discussing the acceptability of theories, Lakatos makes a distinction between theoretical and empirical progress as follows:

> Let us say that such a series of theories is *theoretically progressive*… if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also *empirically progressive*… if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*. Finally, let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not. [80]

Note that the theoretical here means "not actually tested." It is rather a potential of being empirically progressive. When later in his article he replaces "series of theories" with "research programmes" he preserves this distinction of being progressive and degenerating. So what ever role is provided to any other value is ultimately irrelevant to the epistemic justification of the programme. What matters is only its empirical adequacy.

Lakatos ended up saying that one cannot provide guidelines to scientists; the rationality of science is something that can only be constructed by a selective approach to the history of science.

A coherentist approach will more easily handle cases of ad-hocness, as I have argued earlier. Moreover, Nickles's criticisms will also be avoided.[81] Heuristic will provide epistemic support to a research programme by being a part of the set of

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[80] Lakatos, 1978, pp. 33-34.

[81] Nickles, 1987.

propositions building the research programme. The modifications that are not "in the spirit of the heuristic" will be those that are rendered highly improbable by the heuristic and therefore reduce the probabilistic consistency of the programme.

4.3 Research Traditions

Research traditions as defined by Laudan is a modified version of Lakatos's research programmes. A research tradition is again built around metaphysical and methodological commitments. However, Laudan also emphasizes the importance of conceptual problems along with the empirical ones. This is again a move to incorporate essentially coherentist elements within his system. In Laudan's approach problems such as the miraculous coincidence of the values of gravitational and inertial masses deserve special attention, even though they are neither empirical difficulties nor contradictions. As I have discussed in section 3.3, such cases are handled naturally in a coherentist approach.

Laudan also allows the possibility that a research tradition's metaphysical commitments can be modified in time. The hard core or the heuristic of a research programme is, according to Laudan, too rigid to reflect the evolution of actual research.[82] Laudan argues that as much as the metaphysical commitments shape the research tradition and how the theory and data will be interpreted, there is also a reciprocal relation. The empirical evidence or other theoretical considerations may modify the metaphysical commitments. Unlike Kuhn or Lakatos, he does not view these modifications as revolutions. The metaphysical commitments do not define the research tradition, and one may change them without actually changing the research tradition.

His realization of the possibility of modifying metaphysical commitments is again in accordance with coherentism. If the philosophical elements were to have a privileged status within a research programme, it would be contrary to the

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[82] Laudan, 1977, p.78.

egalitarian approach that coherentism demands. I have already argued that one of the virtues of adopting a coherentist approach is to avoid the need to distinguish between observational and theoretical propositions. Therefore, it would not be appropriate to impose a condition to the effect that some propositions have a privileged status. To restore the coherence of the system, one may alter any proposition, whether it is an observation report or a metaphysical commitment. Nevertheless, Kuhn's claim that these will be questioned only during times of revolution is generally correct, since a change in a metaphysical commitment will generally change the degree of coherence of the set significantly and require a compensating modification of many other beliefs.[83]

There are, however, cases where a relatively minor change took place and even though a metaphysical commitment is discarded it is not acceptable to consider the change as a change of paradigm or research programme. One such example is Newton's discovery of gravitational attraction. He did not cease to be working in the Newtonian research programme, when he dropped his commitment to explain all phenomena by repulsive forces. The philosophical parts can also be altered in the light of empirical evidence. As Newton argued there were so many phenomena explained by the help of this attractive force that its sacrifice only on the basis of a presupposition about what counts as a legitimate explanation was not grounded.

In this example empirical evidence and the existence of an attractive force form a very coherent set. Newton demonstrated that all the known laws of kinematics, mostly due to Galileo, and Kepler's laws follow from this attractive force. Moreover, Kepler's laws indicated that an inverse square law should be responsible for these relations. Therefore, the set of propositions formed by the attractive force and the empirical evidence has a high degree of probabilistic consistency. The expectation that only repulsive forces exists seems to be highly

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[83] This is similar to the holistic view of Quine. Some propositions are more central and hence harder to modify. However, this does not make them immutable and if need be any proposition will be discarded.

unlikely, given this set.[84] Even Leibniz who insisted that all forces are repulsive was compelled to find an explanation for how repulsive forces, organized appropriately, may result in an effect that will look like an attractive force.[85]

As this example indicates, even if some elements are harder to replace nevertheless the aim is to achieve coherence and no proposition is “basic” or “immutable” in the foundationalist sense. The observation reports are an integral part of a system whose coherence is considered. It is natural that a theory whose predictions constantly fail will be rejected. Not because the observation reports provide a secure basis for the ultimate test of the theory but because the acceptance of these observation reports made it highly unlikely that the theory is also true, hence, the system will have a very low coherence.

A notable drawback of Laudan’s approach is the extreme generality of a research tradition. To illustrate, one of the research traditions he named is “quantum theory.” This not only includes all alternative interpretations of quantum mechanics, but also includes quantum field theory, and might even include research on quantum gravity.[86] However, within what Laudan calls a single research tradition, there are more than one set of metaphysical commitments that are mutually incompatible.

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[84] This, however, turned out to be disturbing since the attractive force appeared to have properties that no mechanical cause may have. Therefore, accepting the existence of an attractive force seems to be incompatible with the mechanistic world view. A significant attempt to totally avoid this incoherence came from Kant. Kant, 2004.

[85] Leibniz introduced several tools to explain these phenomena in terms of repulsive forces. Some of these are his idea of conspiring motion and aether with a theory of vortices. Even though they together may restore empirical adequacy and preserve the metaphysical commitment, they induce numerous unlikely elements (Newton argued that the hypothesis of vortices is not compatible with celestial phenomena). Moreover, Leibniz held so many suspicions against Newtonian mechanics that even if these modifications were accepted he would still be against the theory.

[86] Whether or not he considers quantum gravity as part of “quantum research tradition” is not explicitly stated, but from the discussion in his book *Progress and Its Problems* there is no reason to exclude it. Laudan, 1977.

I have tried to show that major philosophers of science were not only compelled to depart from a foundationalist understanding of how scientific theories are justified and adopt a fallibilist position but they also realized the need to invoke criteria other than empirical adequacy to explain the process of theory choice. Moreover, the modifications that they suggest were basically trying to capture the importance of the strength of the internal relations among the propositions of a theory. However, an insistence on the privileged status of empirical propositions undermined their projects. A better alternative is a coherentist approach. Notice that the fact that empirical adequacy provides justification to our theories is not ignored in a coherentist approach. However, empirical data lose their privileged status as the primary source of justification.

CHAPTER V

QUANTUM MECHANICS AND THE PROBLEM OF UNDERDETERMINATION

In this chapter, I will try to show how the coherentist approach explains rationally the rejection of hidden variable theories, whereas the non-egalitarian approach fails to handle this case even when it is supplemented with criteria other than empirical adequacy. The case of quantum mechanics is ideal for this discussion for several reasons. First, being emerged recently its development is well-documented. Secondly, there are at least three different quantum theories that share a completely isomorphic mathematical structure. This ensures that the theories are completely underdetermined by empirical evidence.[87] Thirdly, unlike many other cases of underdetermination, the criterion of explanatory power is of no help to explain the actual choice by the scientific community. So this case will also enhance my claim that higher degree of coherence is not necessarily the same thing as higher degree of explanatory power. Fourthly, any other criterion other than empirical adequacy suggested so far either fails to discriminate between the alternatives or favours the theory that is actually rejected. Another reason is that it displays clearly the role of metaphysical propositions in the development of science and the difficulties attached to classifying propositions as theoretical or observational. Finally, from the perspective of a coherentist approach the case is easily explained by arguing that the hidden variable theory of Bohm is rejected

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[87] Strictly speaking, the underdetermination is limited. Copenhagen interpretation suggests that for classical objects classical physics should be used making it possible to make a different prediction than the other interpretations. However, this possibility has only been recently explored and cannot be used to explain the reactions towards these theories before 1970's.

because it has a low degree of probabilistic consistency when compared with its rivals.

Before starting to evaluate the different versions of the theory I will first start by some general remarks.

Quantum mechanics is the only major scientific theory that has no popular myth about its "discoverer." For some reason it is well understood that this theory was developed by the contributions of several scientists. Nevertheless, anyone interested in the field will notice frequent references to two articles as the initial work. One of them is Max Planck's article published in 1900 and the other is Einstein's paper of 1905. Both of these scientists are also famous for their resistance to the further development of quantum theory.

Kuhn explains Planck's attitude by arguing that he was indeed an important figure in the classical tradition and not as great a revolutionist as generally depicted. Yet his work was misinterpreted (not altogether accidentally) by others, who actually took the revolutionary steps that irritated a classicist like Planck.[88]

The case of Einstein is quite different, for he was not really a member of the classical tradition. However, his case is much studied thanks to his popularity. There is a general agreement, supported by Einstein's own comments on the issue, that his later resistance to quantum mechanics was due to its indeterministic character.

After his long efforts to find a way to reject (by means of thought experiments) the indeterminacy principle failed, he opted for an argument that aims to show the incompleteness of the theory.[89] According to him the missing parts of the theory—

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[88] Kuhn, 1978.

[89] Einstein, Podolsky, Rosen, 1935.

parts that will make the theory deterministic—will in the future be discovered and a complete theory would be developed.

On the other hand, Schrödinger was unhappy with quantum theory not due to its indeterministic character but rather because of its instrumentalist nature. So he tried to develop his own version which is still indeterministic but dispenses with Bohr's claims that one may only find a good tool to generate predictions and should avoid making claims as to the truth of quantum theory.

While Einstein was trying to demonstrate that the theory was false or it was at least incomplete, and Schrödinger working on an alternative version, the theory was gaining more and more empirical support. During the 1920's and 1930's it was so successful that even those who supported Einstein and looked for a deterministic version of the theory were imposing the condition that any alternative should be empirically equivalent to the then current version. In his book *The God Delusion*, Richard Dawkins describes the empirical success of quantum theory by reference to a vivid example of a famous physicist: "Richard Feynman compared its precision to predicting a distance as great as the width of North America to an accuracy of one human hair's breadth."[90]

As I will argue below, it is not this great empirical adequacy that marginalized Bohm's and Schrödinger's approaches. They are empirically equivalent to Copenhagen interpretation.

Let me now explain the role played by the positivistic inclinations of the physicists during the development of the theory. It will be an important part of our evaluation as to why some interpretations of the theory failed to receive as much attention as its empirically equivalent rivals.

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[90] Dawkins, 2006, p. 365.

5.1 Positivism and Quantum Theory

The fact that quantum theory is indeterministic was not the only reason why it met with resistance. Indeed there was an intense controversy among scientists about its positivistic foundations. To understand how the commitment to positivism influenced the development of quantum theory, let me summarize how Heisenberg derived his celebrated principle of indeterminacy.

In the matrix formalism of quantum mechanics, which is due to Heisenberg, there are mathematical objects (matrices) corresponding to physical quantities (like momentum, energy, spatial location, etc.) but these do not commute under some operations.[91] The lack of commutativity of matrices makes their interpretation difficult. Naturally, one can look for a classical analogue to understand the phenomenon better. There is indeed such an analogue, namely classical rotations, which so perfectly suits the case that almost all textbooks on quantum mechanics make use of it to introduce the idea.

If you apply two rotations to a three dimensional object successively—some special cases aside—the final position of the object will depend on the order in which the rotations are made. So rotations are not commutative.

In the classical case the non-commutativity is explained by the fact that the rotations are "actions" and not simply the readings of a measurement device. Since they are actions they have time dependence and it is not surprising that the order of application in time is influencing the final outcome.

Physicists adopted the same strategy to explain the non-commutativity of matrices in quantum physics. The matrices are viewed as actions performed on a physical system. These actions are considered as measurements. But the fact that measurements are sometimes non-commutative implied that there are cases when

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[91] Ordinary numbers commute under, say, multiplication, meaning that a × b will always be equal to b × a.

two different measurements cannot be performed simultaneously just like two rotations cannot be applied to a single object simultaneously. Note that this is not a technical difficulty that one may hope to overcome in time by technological improvements. Non-commutativity of measurements means that for a physical system it is impossible, even in principle, to measure some of the properties simultaneously.[92]

Since measurement is the only way to acquire knowledge of the system, this means that the value of some physical quantities cannot be known, even in principle. Heisenberg derived the principle mathematically and then attempted to provide an interpretation for his result. There are more than one and even incompatible interpretations in his paper.

The one that most clearly demonstrates his positivist attitude is where he describes a thought experiment aiming to measure the momentum and position of an electron simultaneously. In the experiment he designed, a photon is sent to a stationary and point-like electron, the location of which will be determined by determining the direction to which the photon is reflected. It is obvious that the photon will transfer some energy to the electron and so its momentum will be altered. One may try to minimize this influence on its momentum by increasing the wavelength of the photon but in that case the precision of the position measurement will diminish.

After arguing that a precise measurement of both variables simultaneously is not possible, he concluded that it is meaningless to talk about their values simultaneously. So if an electron has a value for its momentum at a given time it is

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[92] The uncertainty principle is a limitation on the precision of measurements of conjugate parameters. The term ‘measurement’ above should be understood as precise measurements.

meaningless to ask its position at that time. It is not that simply we cannot know it, but rather there is no such thing to be known.[93]

Even though one may reject this reasoning, it is not easy to argue that this is only a limitation on our knowledge. The matrix formulation contains no parameters as to the properties of the system other than their measurements. Therefore, even when we have performed an actual measurement and obtain a value for a single variable, it is difficult to argue that the system possessed that before or after the measurement.

As a consequence Heisenberg's matrix formulation denied the possibility of a legitimate reference to properties that are not *actually* measured. The theory seemed to suggest that reality is constructed by our measurements. Yet the theory also allowed one to make and test some counterfactual propositions about the unmeasured properties of quantum mechanical entities. Quantum mechanics *sometimes* enable one to predict the outcome of a particular measurement with certainty. At least in such cases, one should be able to refer to the properties of the unmeasured system without violating any principle. This is a serious conceptual difficulty that made Heisenberg's version untenable.

I will now describe briefly the different theories sharing the common name "quantum theory" and possessing an isomorphic mathematical structure. The first one is a modification of matrix mechanics so as to avoid the aforementioned conceptual problem. The other two interpret the uncertainty principle in a totally different way. Notice that these are all non-relativistic theories.

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[93] His conclusion based on this thought experiment is strange, since the experiment starts by assuming a stationary and localized electron, which, according to the conclusion, is impossible.

5.2 Copenhagen Interpretation

An interpretation of quantum mechanics is a solution to the measurement problem.[94] The aim is to suggest a link between our "classical" observations and the "counter-intuitive" properties of quantum systems.

Niels Bohr's famous Copenhagen interpretation provides a trivial, ad-hoc yet working model. He kept Heisenberg's mathematical structure intact but limited it to quantum systems that can never be observed by humans.

Bohr's instrumentalism is most clearly expressed in his following remark: "There is no quantum world… only an abstract quantum description."[95] By adopting an instrumentalist approach, Bohr dismissed all questions as to the truth of quantum theory. Moreover, the question as to whether the measurement result is possessed by the quantum system after the measurement does not emerge in his interpretation. The Copenhagen interpretation only predicts the readings of the measurement devices and not the properties of the quantum mechanical systems.

Therefore, the link between quantum properties and classical measurement results ceases to be a problem. The theory only predicts the appearance of macroscopic objects (such as the location of a pointer of a measurement device) and avoids referring to quantum systems themselves as possessing some value.

Bohr's argument is that we are capable of observing only macroscopic objects. Our language is shaped in a classical environment. We may not hope to alter this situation since, even if we want to measure, say, the momentum of an electron what we do is to generate a value on a measurement device that can be observed by naked eye. This makes it inevitable for us to use a language developed for classical world, even when we try to describe quantum world. Our language being

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[94] For a review of interpretations suggested so far see Omnès, 1994.

[95] Kosso, 1998, p. 158.

unsuitable for this task denies us a true description of the quantum world. We may, however, describe it in complementary ways, none of which are superior to the others, yet cannot be used simultaneously.[96] What we actually do is to describe the behaviour of measurement devices, and the impossibility of measuring some properties simultaneously is nothing more than that of preparing two mutually exclusive experimental designs simultaneously.

Indeterminacy relations of Heisenberg are understood to be a limitation on preparing measurement conditions. When the experimental set up is constructed to measure one of the parameters of a system, constructing a set up—without ruining the first one—that will enable the measurement of the other parameter that do not commute with the first one became impossible. The set of parameters corresponding to describing these mutually exclusive constructions are complementary in the sense that whenever one of them is used the use of the other is illegitimate.[97]

This complementarity principle of Bohr is the heart of Copenhagen interpretation.[98] The theory would lack any explanatory power or probabilistic consistency if the principle were discarded. Hence, contrary to Popper's suggestion that it *should* have been condemned by the scientific community for being ad-hoc, it became an essential part of quantum mechanics.[99] The acceptance of the principle was quite rapid not because physicists failed to realize that it is ad-

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[96] The effects of ordinary language in our understanding of quantum world and Bohr's related arguments are discussed in Bergstein 1972.

[97] Note that this is not similar to the incompleteness suggested by Einstein, Podolsky and Rosen. There is no "more complete" description to be found in Bohr's interpretation.

[98] Bergstein wrote: "the notion of complementarity is so essential to this theory [Copenhagen Interpretation of quantum mechanics] that it might equally well be called the theory of complementarity." Bergstein, 1972, p. 26.

[99] For Popper's claim that the principle is ad-hoc, see the quotation to which footnote 35 is attached.

hoc but because it significantly improved the probabilistic consistency of Copenhagen interpretation.

However, Bohr's interpretation forced him to introduce an arbitrary distinction between the quantum realm, where there are superpositions, limitations due to uncertainty, non-locality etc., and the classical realm, where Newtonian physics rules and ordinary language is sufficient. It would have been acceptable if what distinguishes a quantum system from a classical one could be stated in an objective and non-circular way. But no such criterion exists.

5.3 Hidden Variables

An alternative way to avoid the conceptual difficulties is the famous hidden-variables approach, which was supported by Einstein's incompleteness claim. The history of hidden variables theories is interesting in itself, for several proofs of their non-existence have been suggested and later turned out to be inconclusive. This shows that physicists, who were disturbed by the positivist or instrumentalist views, searched for an alternative even in the absence of empirical difficulties.

The search for hidden variables is not a fashionable research area today. The reason for this is not the impossibility proofs. Such proofs by Von Neumann, or John Bell rule out only a special type of hidden variable theories. Initially, hidden variables were nothing but a hope expressed in the famous article of Einstein, Rosen, and Podolsky.[100] There was not an actual theory that can be tested or in any way compared with Copenhagen version. The early suggestions were also easily refuted due to their poor agreement with empirical evidence.[101] Indeed hidden variable theories were not just interpretations of the theory but rather rival theories

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[100] Einstein, Podolsky and Rosen, 1935.

[101] For a review of several different hidden variable theories, some of which are empirically tested and refuted, see Belinfante, 1973.

since they altered the mathematical structure of the theory and were not empirically equivalent to it.

However, David Bohm came up with an ingenious idea and managed to formulate a theory that is empirically equivalent to standard quantum mechanics and also deterministic.[102] Instead of suggesting that there are parameters which are yet unknown to physicists and will be discovered in the future, he suggested that all parameters are known but not being given full treatment due to the uncertainty principle. Since the uncertainty principle for coordinate and momentum prevents one to attribute both of these qualities to a quantum system in Copenhagen interpretation, whenever one of them is attributed to a system the other plays the role of the hidden variable that is being searched by Einstein and his followers. By this trick Bohm avoided any empirical refutation of the theory, but to avoid the impossibility proof of Bell he kept the theory non-local.[103]

However, the hidden variable theory of Bohm did not receive much support from scientific community. The main reason is that such theories are suggested to avoid the strangeness of quantum mechanics. A hidden variable theory not only aims to establish that laws of nature are deterministic, but also imposes other limitations on how nature should be, including locality. By dropping the locality condition Bohm managed to avoid the impossibility proofs and achieve an empirically equivalent theory but rendered his hidden variable theory totally uninteresting.

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[102] Bohm, 1952

[103] Among the several impossibility proofs of hidden variables, Bell's version is the only one considered as plausible today. Bell demonstrated that any *local* hidden variables theory will make predictions that are different from the Copenhagen interpretation. Moreover the difference can be tested without knowing the specifics of the hidden variable theory. For Bell's views on hidden variables and his proof of different predictions see, Bell 1996. The actual testing of the predictions of a "possible" hidden variable theory and quantum mechanics revealed that local hidden variables cannot be empirically adequate. For one of the early experimental tests see Aspect et. al. 1982. However a non-local hidden variable theory like Bohm's may still be empirically equivalent or even more accurate than the standard version. Moreover, Bohm argues that Bell's proof and the related experiments are not conclusive, see Bohm and Hiley, 1993, especially chapter 7.

5.4 Wave Mechanics

Schrödinger was working on an alternative interpretation of the theory.[104] Contrary to Bohr's instrumentalist approach he argued that quantum mechanics is universally applicable. And against Einstein he considered the theory to be complete. Moreover, Schrödinger was no less committed to positivism than Heisenberg.[105] However, Schrödinger was more careful in his formulation. He realized that with the impossibility of attributing simultaneous momentum and coordinate values to a physical system, the concept of trajectory also loses its meaning. But there can be no particle in the ordinary sense that does not have a trajectory. Therefore, instead of determining when it is legitimate to talk about a property of a particle and when it is not, he considered his waves as the basic entities. The popular "wave-particle duality" is a consequence of Bohr's approach (they are complementary descriptions) and it does not emerge in Schrödinger's system. However, these "waves" are not waves in the classical sense. The naming is somewhat misleading, since unlike classical waves the existence of these so-called waves does not depend on a medium for existence. Schrödinger considers this special type of waves as the correct and complete description of any physical system, whether it is macroscopic or microscopic and irrespective of whether or not any actual measurement is performed on the system. These waves, however, have no classical analogue.

In Schrödinger's formulation a quantum mechanical entity, say, an electron is not a localized object but rather an electric charge smeared over space. Moreover, he argued that any physical system is a quantum mechanical system.[106] His theory

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[104] Although Schrödinger developed wave mechanics as an alternative to matrix mechanics and did not initially expect it to be empirically equivalent to Heisenberg's system, he later demonstrated that the two versions are empirically equivalent due to their isomorphic mathematical structure.

[105] See, Bitbol, 1996.

[106] His popular thought experiment known as Schrödinger's cat is designed to demonstrate that if a system (such as a radioactive atom) obeys quantum mechanical laws then any other system

shares the same mathematical structure but since it suggests that even the macroscopic objects should be considered as quantum mechanical systems, it had a much broader scope. However, the theory at its early stages did not possess the necessary tools to generate correct predictions for this larger scope. As can be expected, Schrödinger's major focus was to develop the tools, such as coherent states, to handle macroscopic systems.

Quantum theory appeared to attribute multiple values of a parameter to a single particle. This "superposition" of different values was avoided by Bohr through limiting the theory to the readings of the measurement devices (which are never observed to be in superposition states). In Schrödinger's formulation, since one dispenses with the concept of particle, multiple locations or momentum cease to be problematic. However, one should then explain why such superpositions are never observed at a macroscopic level. Bohr quickly applied his approach to Schrödinger's wave mechanics and suggested that whenever a quantum system is measured by a classical apparatus its wave function "collapses" to a single value. This move saved wave mechanics from being refuted by macroscopic observations but introduced a distinction between ordinary interactions and a measuring interaction; a quantum system and a classical system. The distinction, however, is not clear. When I compare different versions of quantum mechanics below, the term "wave mechanics" will be reserved for Schrödinger's programme and not Bohr's transformation of it to Copenhagen interpretation.

5.5 Development of the Theory

As I argued before, the discovery and justification of a theory are inseparable processes. The formulation of quantum mechanics and its interpretation is still an uncompleted task. However, there are several things that can be said about the way quantum theory developed and how it will develop in the future.

(even if it is a macroscopic object like a cat) that interacts with it, should also be treated as a quantum system. Therefore, Bohr's instrumentalist approach is untenable.

Initially, Bohr's Copenhagen interpretation won the day. It allows one to use either matrix formulation or wave formulation on quantum systems and accurately predict the experimental results. Since many-body systems were and still are too complicated to handle, macroscopic objects were not given much attention. Nevertheless, there has been some discussion concerning how to deal with macroscopic objects. As further developments enabled more delicate experimental tests, scientists are convinced that macroscopic objects are no different from microscopic ones in any essential way requiring different laws.

Hidden variables, on the other hand, never became much popular. There is a general consensus that the fundamental laws are indeterministic. Note that this consensus is reached when an empirically equivalent deterministic version is available. Even though there is a general consensus, some scientists and philosophers of science argue that hidden variables deserve more attention.[107]

Another general tendency among the scientists is to treat macroscopic objects as quantum mechanical systems. Especially by the progress achieved after 1970's in experimental set ups, it became possible to create "Schrödinger's cat states" (i.e. macroscopic systems that are in a superposition state).

The major line of development of the theory is therefore along the suggestions of Schrödinger. The final theory is supposed to be able to attribute properties to actual physical objects; it should be indeterministic; it should be universally applicable to any physical system.

In wave mechanics one may consider the wave function of the whole universe and discuss how it will evolve under the laws of quantum mechanics. In 1957 Hugh Everett discussed the consequences of treating the whole universe as a quantum mechanical system.[108] Since there is nothing outside the universe, such a treatment

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[107] A notable example for recent defence of hidden variables is Norris, 2000.

[108] Everett, 1957.

would not be possible within Bohr's interpretation, which requires that a quantum system must be observed by an external measuring device that obeys the laws of classical physics.

Today any interpretation that does not aim to alter the mathematical structure of the theory and also does not distinguish quantum and classical realms is called an Everett type interpretation.[109] Finally, it can safely be argued that positivism is still an essential part of the theory. One of the recent interpretations of the theory developed along Schrödinger's suggestions is the consistent histories interpretation. The following quotation from one of the leading physicists defending this interpretation indicates the positivist tradition that it belongs to:

> If $PQ \neq QP$, the question "Does the system have property P or does it have property Q?" makes no sense if understood in a way which requires a comparison of these two incompatible properties. Thus one answer might be, "The system has property P but it does not have property Q". This is equivalent to affirming the truth of P and the falsity of Q, so that P and $\tilde{Q}$ are simultaneously true. But since $P\tilde{Q} \neq \tilde{Q}P$, this makes no sense. Another answer might be that "The system has both properties P and Q", but the assertion that P and Q are simultaneously true also does not make sense. And a question to which one cannot give a meaningful answer is not a meaningful question.[110]

Here P and Q represent any properties that can be measured, but just cannot be simultaneously measured with precision (that $PQ \neq QP$ means that they are non-commutative); such as position and momentum. This is exactly the same interpretation of uncertainty as that provided by Heisenberg.

As to the future of quantum mechanics one may also expect a challenge from general theory of relativity. Since quantum mechanics and general theory of

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[109] For a good classification of what the possible ways of interpreting quantum mechanics are, see Elby, 1998.

[110] Griffiths, 2002, p. 63.

relativity are inconsistent in their current form, scientists are expecting that at least one of them will be modified.

The field of physics that focuses on the relation between quantum theory and gravity is one of the major research areas in theoretical physics. The task is to find a theory which explains both quantum mechanical interactions and gravitational interactions. One of the difficulties of this field is that there are almost no empirically testable differences among the alternative theories of quantum gravity.[111] Hence, the decisions are based on criteria other than empirical adequacy. Currently both kinds of phenomena are quite well understood and we have theories that have great empirical power but it seems that this is not considered as sufficient by scientist to quit looking for an alternative to these theories.

The major problem is that these interactions seem to be of different type. The three of the four known types of interactions—electromagnetic, weak and strong interactions—are explained by an exchange of quanta between the interacting objects.[112] To illustrate, the repulsion of two electrons is considered to be an event that is due to an exchange of a photon between the electrons. On the other hand, gravitational interactions involve no such exchange if we accept the general theory of relativity. General relativity assumes gravitation to be a result of geometry and not a direct interaction between two massive objects.

There are alternative research programmes suggested by different physicists. Therefore it would not be acceptable to talk about quantum gravity as if it is a well understood and agreed upon project. One research programme looks for a quantum

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[111] This is, however, a technical difficulty and not due to an isomorphism in the mathematical structures of the theories.

[112] Although these interactions are understood to have a similar structure their unification is not completed. This is another interesting area of theoretical physics. Especially the fact that the strength of strong interactions increases with distance seems to be a serious challenge.

theory of gravity in the sense that gravity is explained by exchange of a quantum—named, graviton—between the masses and would be completed if such an explanation is found. Another one is the superstring programme motivated by the previous success of unification of weak and electromagnetic forces.[113]

The problem is considered extremely important by many physicists. To illustrate, Mendel Sachs starts his book on this topic by writing: "theoretical physics is presently at a very exciting time in the history of scientific discovery."[114] Sachs argues that general theory of relativity will be preserved intact and it is quantum mechanics that will be significantly modified. He suggests that the inconsistency between the two theories is essentially due to their metaphysical commitments. Whereas quantum mechanics is a theory developed in accordance with positivistic ideas, relativity is developed in accordance with realism.

The interesting thing is that the issue can be completely ignored for all practical purposes. There is absolutely no case where both quantum effects and gravitational effects can be considered and also where we can make calculations and observations precise enough to detect any difference.[115]

5.6 Evaluation of the Development

In this section I will discuss how non-egalitarian fallibilism and coherentism explain the development of quantum theory. By using the empirical adequacy criteria the initial success of Copenhagen interpretation is easy to explain. It was

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[113] For a detailed discussion of alternative research programmes and their motivations see Butterfield and Isham, 1999.

[114] Sachs, 2004, p. vii.

[115] One of the greatest contributions of Hawking is his work on quantum effects in black holes. Now known as the Hawking radiation, he predicted that we may in principle observe a radiation from a black hole due to effects of quantum field theory. However, this again is only possible "in principle" since devices precise enough to detect such radiation are far beyond our technical capacities and possibly will never be built. For a good review of quantum theory of black holes, see Kiefer, 1998.

the theory providing the best match with empirical evidence. Hidden variables were just a hope and wave mechanics failed to generate correct predictions for macroscopic objects.[116] Moreover, some of the further developments may also be understood with an essentially foundationalist approach. As it became possible to create Schrödinger's cat states, the Copenhagen interpretation lost some of its empirical adequacy. Macroscopic objects turned out to be quantum mechanical systems unlike what Bohr suggested. Wave mechanics on the other hand significantly improves its empirical adequacy by these new developments. However, much earlier than the actual realization of Schrödinger's cat states experimentally, Schrödinger's theory had a significant support. Moreover, even today it is far from making specific predictions for macroscopic objects. A macroscopic object is far too complicated to apply the laws of quantum mechanics and derive predictions.[117] The only superiority—in respect to their empirical adequacy—of Schrödinger's approach to Bohr's approach is its general conviction that all systems are quantum mechanical systems. All that has been experimentally shown is that not all macroscopic objects are non-quantum systems. This, even though it undermines Bohr's position, does not significantly increase the predictive accuracy of Schrödinger's theory.

On the other hand, relying only on the criterion of empirical adequacy fails to explain why Bohm's hidden variable theory is out-fashioned today. Bohm's theory is empirically equivalent to wave mechanics. Let me consider if the addition of criteria other than empirical adequacy would improve the fallibilist position.

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[116] Although wave mechanics and Copenhagen interpretation share the same mathematical structure, Copenhagen interpretation did not make any predictions for macroscopic objects, discarding them as out of the scope of the theory.

[117] That the laws governing a system are known does not necessarily mean that we are capable of applying the laws to the system. Moreover, this may not be simply a consequence of technical inability. There might be other factors yet undiscovered but effective in the interactions of many body systems.

To begin let us consider broadness of scope. It is clear that wave mechanics and Bohm's theory have a broader scope than Copenhagen interpretation in the sense that they have a larger domain of applicability. This criterion not only fails to distinguish between Bohm's theory and wave mechanics but also makes it difficult to explain the initial success of Copenhagen interpretation over the other two.

Secondly, considerations of simplicity, whether understood as parsimony or as bringing order to diverse phenomena, would again demand a rejection of the Copenhagen interpretation but fail to explain the negligence towards hidden variables. The Copenhagen interpretation suggested the existence of mechanisms such as "wave function collapse" and attributes special status to "measurement." It fails to "bring order to diverse phenomena" just from the start by excluding the classical realm from its domain of applicability. On the other hand, early versions of hidden variables had the drawback of suggesting extra entities to restore determinism and therefore were less parsimonious. Bohm's theory, however, suffers no such difficulties, since the function expected from a "hidden variable" is served by the already existing ones in Bohm's theory. Therefore, Bohm's theory is simpler than Copenhagen interpretation.

Thirdly, all theories are internally consistent but have problems with the theory of relativity. Even though their being non-relativistic is a serious problem, this is not a ground on which the choice can be based, since all of them are equally undermined with this problem.

Fourthly, let me consider the criterion of fruitfulness. Understood as leading to new research and disclosing new relations, Copenhagen interpretation is the weakest of the three. Its structure is to delimit and strictly separate several types of phenomena. Not only it demarcates classical and quantum realms but also introduces many other such artificial separations. For example a measurement is not an ordinary interaction; so it should be treated separately. Such strict demarcations prevent further developments of the theory by discarding any unexplained issue outside the scope of the theory.

On the other hand, for both Schrödinger and Bohm every physical system is a quantum mechanical system, and every interaction—including those that can be considered as measurement—should be treated equally. Even though these initially rendered wave mechanics and hidden variable theory of Bohm empirically more problematic, they significantly increase their scope and initiate further research. Both wave mechanics and Bohm's hidden variables theory have a clear programme for further developments. Some of these further researches, especially those suggested by Schrödinger, have already been carried out.

If Copenhagen interpretation was the choice of the scientific community, it would have been possible to explain the choice only by the empirical adequacy criterion. However, since Schrödinger's wave mechanics is preferred one has to find a distinction between Bohm's version and Schrödinger's version of quantum mechanics. As to fruitfulness Bohm's theory seems to be superior to wave mechanics. Unlike wave mechanics it is possible to modify Bohm's theory—not necessarily by introducing new entities that will lower its simplicity—in such a way that it will make predictions that are forbidden by the uncertainty principle. Therefore, the theory may also prove to be significantly more empirically adequate than any of its rivals. Such a path of development is not available to wave mechanics or Copenhagen interpretation.

Hence, hidden variable theory of Bohm is superior to Copenhagen interpretation not only with respect to empirical adequacy but also with respect to simplicity, broadness of scope and fruitfulness. Moreover, none of these criteria suggest that wave mechanics is superior to Bohm's theory. They have a common domain of applicability and neither contains entities or mechanisms that the other did not introduce. However, Bohm's theory has more potential than wave mechanics. Hidden variables left room for further developments that will enable it to make predictions beyond the precision allowed by uncertainty relation and provide a complete description in the sense expected by Einstein. Therefore, hidden variable theory has more prospects of development than its rivals. The traditional approaches to the evaluation of rival quantum theories made it appear as an irrational decision on the part of the scientific community to dismiss hidden variables as a serious alternative.[118]

From a coherentist perspective the initial success of Copenhagen interpretation is explained by relying on empirical adequacy. The abundant accepted empirical propositions about the macroscopic world and Schrödinger's claim that macroscopic objects are quantum mechanical systems are not mutually supportive. It is highly unlikely that the spread of the wave function of each constituent of a macroscopic object cancels out leaving a localized object due to some curious coincidence. Therefore, Schrödinger's theory requires an unlikely coincidence which significantly lowers the probabilistic consistency. However, as Schrödinger and his followers provide possible methods to deal with macroscopic objects, the probabilistic consistency begins to increase. It is shown by Schrödinger himself that in harmonic oscillator potential, the spread of different parts cancels out. More recent studies revealed some mechanism that might be responsible for this.[119] These are not ad-hoc sugesstions aiming to save the theory. The Schrödinger cat states are obtained by purposefully lowering the dissipation in a system (by means of cooling). Technical developments such as superconductors now testify the existence of mechanisms that wash away the superpositions in ordinary macroscopic systems. The realization of these mechanisms increases the probabilistic consistency of wave mechanics significantly. The case is similar to the realization of continental drift to explain the structural similarities of continents.

However, even today it is not possible to provide a full quantum mechanical treatment of say, a table, but still Schrödinger's approach has a relatively higher degree of probabilistic consistency. The major factor that lowers the probabilistic

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[118] That the ignorance of hidden variables is not based on good reasons is argued explicitly in Norris, 2000.

[119] For a review of possible mechanisms of decoherence, such as interaction with environment or stochastic collapse models, see Giulini et. al., 1996.

consistency of Copenhagen interpretation is its instrumentalist attitude. It is difficult to accept that an object obeys a totally different set of laws than its constituents. Moreover, the requirement that a measurement is a special type of interaction is hard to believe. Our belief system made it highly unlikely that the result of an interaction depends on whether the result is recorded or not.

Hence, Copenhagen interpretation has a low degree of probabilistic consistency whereas wave mechanics has an increasing one. Schrödinger's theory seems to have only a single problem. That is its being non-relativistic. However, as I said before, this is a common problem for all three versions of the theory that I consider here.

Unlike non-egalitarian approaches with supplementary criteria other than empirical adequacy, coherentism manages to explain why Schrödinger's wave mechanics is preferred to hidden variable theory of Bohm. Schrödinger proposed a theory that is non-classical. The positivist commitments of the new theory enabled him to exclude reference to unobservable entities. The uncertainty relations are not difficulties that he should overcome. Since values corresponding to non-commuting operators cannot be measured simultaneously, even in principle, they cannot be attributed to a system simultaneously. He made no attempt to avoid these relations but simply developed his ideas in accordance with them. Being unable to attribute trajectories, he discarded all reference to particles. Hence, his theory was in accordance with its metaphysical commitments.

On the other hand, this cannot be said for Bohm's theory. His development of a new interpretation will clearly show us that it is essentially a classicist programme. Because of the requirement of keeping his theory empirically equivalent to the standard version, he started with Schrödinger's equation. After rearranging its terms, he obtained an equation that is the classical Hamilton-Jacobi equation plus a term starting with $\hbar^2$. So the equation will reduce to the equation of classical physics if Planck's constant were zero. The small value of Planck's constant made the equation appear as a small correction to classical physics. The new interpretation starts with his method to absorb this extra term into the classical part by defining a quantum potential.[120] Hence, the project is to show that classical physics with just a minor modification is sufficient to predict quantum phenomena.

As one may expect, a classical theory has no room for uncertainty relations or any other form of indeterminacy. This led Bohm to search for possibilities to violate uncertainty relations. That his theory has potential of eliminating uncertainty relations, by further developments, is considered as a great virtue of the theory by Bohm. Unfortunately, his theory is non-local and contextual. Such properties cannot be accepted by any classical physicist. The programme of preserving classical physics with minor corrections on the equations dramatically failed when a non-local theory is shown to be unsuitable for the task. Bohm's theory is significantly less coherent than its non-classical rivals since it claims that "classical physics with its concept of particles, forces and even equations of motion are essentially correct" and "laws of nature are non-local" are highly unlikely to be simultaneously true. Note that being non-local is not by itself a problem. Schrödinger's wave mechanics or Bohm's Copenhagen interpretation are also non-local. What causes a problem in case of Bohm is that non-locality is incoherent with the aims of the researchers that demand a hidden variable theory. In their famous paper, where the hope for a complete theory is first expressed, Einstein, Rosen and Podolsky made use of the locality principle to argue that quantum mechanics is incomplete. Hence, a non-local theory is not satisfactory for them.

Finally, let me consider the criterion of explanatory power. I will argue that if this criterion is invoked it will at best be insufficient to discriminate between the alternative versions of quantum theory that I considered above. This will not only support my view that non-egalitarian fallibilism, even with the help of criteria

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[120] Bohm, 1952.

other than empirical adequacy, is inappropriate to understand scientific change, but also illustrate that explanatory power is not by itself a measure of coherence.

Explanatory power is a measure of internal relations of a theory. The phenomena that need to be explained may be different for rival theories of the same domain. To illustrate, theories of motion aim to provide explanation for deviations from the natural motion. However, what natural motion is, is not defined independent from the theory. Whereas zero acceleration is natural for Newtonian physics, free fall is a natural state of motion in general theory of relativity. Since free fall is an accelerated motion it is explained by Newton by reference to a force, namely, gravity. The fact that there is no gravitational force in general theory of relativity, however, does not lower its explanatory power, since free fall is not a phenomenon that needs explanation.

Moreover, there are some questions that are legitimate only within a theory and are not applicable to others. A major reason for resistance towards Galileo's views on planets was his inability to explain their motion in terms of the laws governing celestial objects. It was an immediate problem for Newton and the unification of celestial mechanics and terrestrial mechanics was completed only when he managed to derive Kepler's laws form Galilean kinematics plus gravitational force that applies to *all* massive objects. On the other hand, the question how to derive the laws for heavenly bodies from the laws that terrestrial objects obey, was not raised in the Aristotelian system. The question is legitimate only if the relevant theory assumes that planets are composed of similar stuff with that of earth.

So to evaluate the degree of explanatory power of a theory, one needs to be able to specify the phenomena that need to be explained. Moreover, what counts as a legitimate explanation is again not specified independently of the theory. For Boyle only mechanical explanations were allowed:

> He that cannot by the mechanical affections of the parts of the universal matter explicate a phenomenon will not be much helped to understand how the effect is produced by being told that nature

> did it, so, if he can explain it mechanically, he has no more need to think or (unless for brevity's sake) to say that nature brought it to pass than he that observes the motions of a clock has to say that it is not the engine, but it is art, that shows the hour.[121]

Hence, his theory's explanatory power should be judged on the basis of this view. Any phenomenon that lacks a mechanical explanation will be considered as unexplained. However, this is not true for all scientific research. Not only in the texts of ancient philosophers that Boyle ridiculed but also in current theories of physics mechanical explanations are not essential. An attempt to evaluate the explanatory power of a scientific theory with a criterion external to the theory would not do justice to it. Therefore, just like coherence, explanatory power is a measure of internal relations of a theory.

Nevertheless, when it comes to rival versions of quantum mechanics, it can be seen that one theory may be superior to the others on the basis of a comparison of their explanatory powers, yet fail to be more coherent. The reason is that not all relations that need to be considered for coherence are explanatory relations.

Copenhagen interpretation turns out to be clearly inferior to any of its rivals on a comparison of explanatory power. First of all phenomena such as superconductivity are simply impossible in Bohr's theory. Moreover, it provides no explanation as to what distinguishes a measurement from a non-measuring interaction. Another major difficulty of the theory is its artificial distinction of classical and quantum realms the separation of which seems to be made separately for each different experimental set up. It fails to explain how objects that obey a set of laws are combined to form larger objects that are governed by a totally different set of laws. Both Schrödinger's theory and Bohm's theory are superior to Copenhagen interpretation. So I will now compare the other two.

If we try to evaluate the explanatory power of hidden variable theory of Bohm we will see that it does not face any difficulty in explaining empirical evidence that

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[121] Boyle, 1996, p. 35.

Schrödinger's wave mechanics does not face. Moreover their problems are overlapping. Just like Schrödinger, Bohm considered macroscopic objects as quantum mechanical systems. So, he has the task of explaining the emergence of classical laws as a consequence of underlying quantum laws. Macroscopic systems that display manifest quantum behaviour are also well understood and even predicted by these theories. Since they suggest that decoherence emerges as a result of interaction of a large number of elements, systems where interactions are greatly suppressed (such as extremely low temperatures) are expected to behave like quantum mechanical systems despite being macroscopic.

There is simply no ground to suggest that Schrödinger's theory has any more explanatory power than Bohm's theory. Therefore, the lack of attention to Bohm's theory cannot be based on its relatively low explanatory power.

It may even be argued that Bohm's theory has a greater explanatory power. The theory also manages to explain why the theory is indeterministic. This is considered by Bohm to be one of the major advantages of the theory over its rivals. Since it has potential for developing into a deterministic theory, one may argue that the theory only seems to be indeterministic due to its incompleteness. Even if there is not a way to find a complete version, still Bohm's theory explains the indeterminacy by referring to the quantum potential. This potential having the factor $\hbar^2$ is so tiny that for everyday macroscopic objects it is insignificant, causing the impression that the laws are deterministic at macro level. For micro objects or for systems where interactions are successfully suppressed, the effect of this potential becomes dominant.

The non-locality of the theory is not, however, something that needs to be explained by Bohm's theory, any more than by Schrödinger's theory. However, the motivation of Bohm's theory is to find an essentially classical theory. Therefore, it fails the task that is set to it. Even though its explanatory power is no less than its rivals, it contains propositions that are unlikely to be true simultaneously. The motivations of the theory and its non-local character lower its probabilistic consistency.

As the above discussion indicates, from a non-egalitarian standpoint Bohm’s theory is not only superior to Copenhagen interpretation, but it is also superior to the accepted wave mechanics suggested by Schrödinger, and its modified versions. Hence, its rejection seems to be somewhat irrational as Norris argued. But from a coherentist approach Bohm’s theory, despite its advantages, has a low degree of coherence when compared to wave mechanics, justifying the general conviction of the scientific community.

CONCLUSION

If science is a rational activity, the decisions of the scientific community can be explained rationally. If the scientific theories are considered as correctly revealing the actual laws of nature rather than mere devices for correct predictions, they should be epistemically justified. Philosophers of science, who defend scientific realism, mainly looked for criteria that will provide justification to scientific theories and then try to explain scientific change on the basis of comparing theories with these criteria.

As I have argued above, the obvious choice of “empirical adequacy” is insufficient. Realising this, other supplementary criteria are also introduced. Nevertheless, many historical cases indicate that the decisions of the scientific community cannot rationally be understood even by means of these additional criteria.

Nevertheless, the motivation of these additional criteria is to capture the internal relations of the propositions that make up the theory and with our background knowledge. Hence, I argue, that a coherentists approach to the justification of scientific theories will be more adequate to explain the history of science.

One can see that almost all attempts to improve our understanding of science as a rational process tend to emphasise the holistic character of justification (such as Kuhn’s paradigms or Laudan’s research traditions) and the mutual support of the beliefs that make up the theory (such as the emphasis on explanatory power or internal and external consistency). Why these modifications emerged and actually successfully handled several cases is best explained, I believe, due to the fact that the justification of scientific theories lies in their coherence.

A coherentist approach is more adequate for understanding scientific change than the non-egalitarian approaches. Being able to dispense with the distinction between observational and theoretical propositions, many persistent problems of philosophy of science, such as dealing with accidental generalizations or underdetermination, are handled quite easily. The advantages of adopting a coherentist approach are displayed by several examples above.

Finally, from a foundationalist or a non-egalitarian fallibilist standpoint one cannot provide good reasons as to why supplementary criteria such as simplicity or explanatory power are virtues of a theory. On the other hand, these criteria—as long as they indicate the mutual support of the beliefs that make up the theory—are understood as providing justification in a coherentist approach since they increase the probabilistic consistency of the theory.

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APPENDICES

APPENDIX A

ÖZET

Bilimsel gerçekçilik görüşünün savunucuları, bilimsel kuramlarımızın doğru olduklarını ya da en azından yaklaşık olarak doğru olduklarını öne sürer. Ancak bir kuramın doğru olduğunu göstermenin güçlükleri bilimsel gerçekçiliğin en önemli sorunudur. Bir kuramın doğruluğu gösterilemese bile, söz konusu kurama inanmak için gerekçelerimiz olduğunu gösterebilmek bilimsel gerçekçiliği savunabilmek için önemlidir. Olası gerekçeler arasında ilk akla gelen kuramın başarılı öngörülerde bulunabilmesidir. Bir kuram eğer başarılı öngörülerde bulunabiliyorsa onun bu başarısının doğru olmasından kaynaklandığını düşünebiliriz.

Ne var ki başarılı öngörülerde bulunmak kendi başına yeterli değildir. Yanlış kuramların başarılı öngörülerde bulunabiliyor olması bir yana, öngörüleri başarısız olan pek çok kuramın bilim toplumunca dikkate değer bulunduğunu bilim tarihinde gözlemleyebiliyoruz. Yani kuramın gözlemsel verilerle uyuşmaması durumunda, bu uyuşmazlık geçici bir süre için görmezden gelinebilmektedir. Öyleyse bilimsel kuramlarımıza inanmamızın nedeni yalnızca başarılı öngörüleri olamaz.

Pek çok bilim felsefecisi bu durumu dikkate alarak bilimsel kuramlarda aradığımız nitelikleri belirlemeye çalışmışlardır. Örneğin Thomas Kuhn; basitlik, geniş kapsamlılık, verimlilik ve tutarlılığı önemli “değerler” olarak görmektedir. Ancak bu liste tam olmadığı gibi bu değerler kısmen örtüşmektedir. Konuya ilişkin yayınlarda sıkça başka değerler de anılmaktadır. Bunlardan en önemlisi açıklama gücüdür. Gerçekten de bilim tarihine baktığımızda kuramlarımızdan yalnızca olayları öngörebilmelerini değil aynı zamanda onları açıklayabilmelerini de beklediğimiz anlaşılacaktır. Tutumluluk, alçakgönüllülük ve hatta tutuculuk ve güzellik gibi başka pek çok değer de dile getirilmiştir.

Tüm bu değerlerin ortaya konulmasındaki amaç bilim tarihini rasyonel bir süreç olarak açıklayabilmektir. Bir bilimsel kuramın dışlanması ve başka bir kuramın kabul edilmesi sırasındaki seçimimizi hangi kriterlere dayanarak yaptığımızı belirlemek ve sonra bu kriterlerin uzun dönemde bizi doğru kuramlara götüreceğini söyleyebilmek isteriz.

Ortaya atılmış bu "değerlerle" ilgili üç temel soun vardır. İlki bunların kesin bir biçimde tanımlanamamasıdır. Örneğin basitliğin önemli bir değer olduğunu savunan iki ayrı bilim felsefecisi, rakip iki kuramdan hangisinin bu değere sahip olduğunu belirlemek gerektiğinde farklı seçimlerde bulunabilmektedir. İkinci olarak bu değerler arasında bir sıralama yapmak olanaklı görülmemektedir. Biri daha basit diğeri daha verimli iki kuramdan hangisini seçmenin rasyonel olacağını söyleyemiyoruz. Son olarak bu niteliklere sahip olmanın neden önemli olduğu sorusu yanıtlaması güç bir sorudur. Öyleyse kuram seçiminde kullandığımız ölçütler nelerdir ve bunları kullanmak neden rasyoneldir soruları bilimsel gerçekçilik için önemlerini koruyan, tartışmalı sorulardır.

Doğruya ulaşmayı amaçlayan bir etkinlikte kullanılacak seçim ölçütlerinin (ya da Kuhn'un söylediği biçimiyle "değerlerin") rasyonel olması, bu değere sahip olan kurama inanmak için iyi gerekçemiz olması ile olanaklıdır. O halde öncelikle, bir önermeye inanmanın nasıl gerekçelendirilebileceğine ilişkin tartışmaya bakalım.

Kuşkuculuk, en aşırı biçimiyle hiçbir önermemize inanmak için rasyonel gerekçemiz bulunmadığını söyler. Bunun için ortaya koyduğu argüman söyledir: her önerme ancak başka önermeler tarafından gerekçelendirilebilir. Gerekçelendirmenin gerçekleştiğini söyleyebilmek için işlemin sonlu aşamada bitmesi gerekir. Öyleyse gerekçelendirmemiz ya kendileri gerekçelendirilmemiş önermelerde sonlanacaktır ya da döngüsel olacaktır. Her iki durumda da önermelerimizin doğruluğuna inanmak için rasyonel gerekçemiz olduğunu söylemek uygun olmaz.

Bu argümanı çürütmenin bir yolu "temeldencilik" olarak anılan bir gerekçelendirme yaklaşımına yol açar. Buna göre bazı önermelerin gerekçelendirilmesi için başka önermelere gerek yoktur. Bu tür önermeler diğer tüm önermeleri gerekçelendirmekte kullanılacak bir "temel önermeler kümesi" oluştururlar. İkinci bir seçenek pek çok önermenin birbirini desteklemesi durumunun bir kısır döngü yaratmayacağını söylemektir. Buna göre bir önermeler kümesindeki önermelerin doğru olduğuna bizi ikna eden unsur, bu önermelerin arasındaki uyumdur. "Uyumculuk" olarak adlandıracağımız bu görüşe göre gerekçelendirme, önermeler arası ilişkinin bir sonucudur.

Uyumculuk yaklaşımının en önemli sorunu bir önermeler kümesinin uyumlu olması ile ne anlatılmak istendiğinin açıkça ortaya konulamamış olmasıdır. "Karşılıklı olarak destekleme" ya da "birbirine tutunma" gibi sezgisel anlatımlar "uyum" kavramını büyük ölçüde belirsiz bırakmaktadır. Kavramı anlamamıza yardımcı olacak en açık anlatım Laurence Bonjour'un 1985 tarihli kitabında sunulmuştur. Bonjour'un sunduğu açıklamanın temeli bir olasılık hesabına dayanır. Buna göre bir kümedeki bir önermenin doğru olma olasılığı, kümedeki diğer önermeler ışığında düşük ise bu kümenin "uyumu"—bu önermenin olmadığı duruma göre—azdır. Dikkat edilirse burada yalnızca karşılaştırmalı bir ölçüt sunulmuştur. Önermelerinden çoğunun özdeş olduğu iki kümeden hangisinin daha uyumlu olduğunu belirlemeye yarayabilecek bu ölçüt mutlak bir uyum cetveli oluşturmayı sağlamaz.

Bununla birlikte bu tezin amaçları açısından Bonjour'un açıklamaları yeterlidir. Bu tezde bilimsel kuramların seçilmesinde hangi kuramın daha uyumlu olduğunun en önemli ölçüt olduğunu savunacağım. Başarılı öngörülerde bulunabilme ölçütü ancak kabul edilen önermeler kümesinin uyumunu arttırdığı için ve bunu yapabildiği sürece dikkate alınmaktadır. Benzer biçimde sıkça dile getirilen basitlik, verimlilik ve hatta güzellik gibi ölçütler de kabul edilen önermeler kümesinin uyumunu belirlemeye yöneliktir. Kuramlarımızın bu niteliklere sahip olmasını istememizin nedeni bunların kendi değerleri değil uyumlu olmaya yaptıkları katkıdır. Bu nedenle sunduğum bilim tarihinden örnekler bu ölçütlerin bazen dikkate alınmadığını gösterir. Bu değerlere sahip bir kuramın herhangi başka bir nedenle uyumunun az olması durumunda dışlanabildiğini gösteren tarihsel örnekler vardır.

Uyumculuk yaklaşımında önermeleri benimsememize yol açan, önermeler arası uyum olduğundan farklı türden önermeleri ayırmak gerekli değildir. Tüm önermeler eş değerdedir. Buna karşın temeldencilik yaklaşımı bazı önermeleri "başka önermelerden bağımsız olarak gerekçelendirilenler" olarak ayırır. Bu "temel" önermeler arasında hangi önermelerin olacağı konusu tartışmalıdır. Ancak eğer bilimsel kuramlarımızın gerekçelendirilmelerini temeldencilik yaklaşımıyla açıklamak isteseydik temel önermelerin bir kısmının gözlemsel önermeler olmasını isterdik. Çünkü salt mantıksal önermeler yardımıyla hangi bilimsel kuramı benimsememiz gerektiğini belirleyemeyiz. Tutarlı olan her kuram aynı derecede gerekçelendirilmiş olur.

Ne var ki gözlemsel önermeler, temeldencilik yaklaşımının amaçlarına uymazlar. Gözlemsel önermelerimiz kimi zaman yanlış çıkabilmekte bu nedenle de bize güvenli bir temel sunamamaktadır. Bilim felsefecileri bu nedenle temeldencilik yaklaşımını benimsememişlerdir. Onun yerine gözlemsel önermelerinin yanlış olabileceğini ve değiştirilmeleri gerekebileceğini dikkate alan "yanılabilirlik" olarak adlandırılan görüş yaygınlaşmıştır.

Yanılabilirlik görüşünün savunucuları hiçbir önermenin temel olmadığını kabul etmekle birlikte gözlemsel önermelere özel bir yer ayırırlar. Buna göre her ne kadar gözlemsel önermeler yanlış olabilirlerse de, önermelerimizin gerekçelendirilmesi başka kuramsal önermelerle ilişkilerinden çok, gözlem önermelerine dayandırılabilirliklerinden kaynaklanmaktadır.

Özellikle Lakatos'un "tarihin rasyonel olarak yeniden kurulması" görüşü durumu çok iyi açıklamaktadır. Lakatos kuram seçimimizde rol oynadığını bildiğimiz ölçütlere epistemik bir değer vermemektedir. Bu da bu ölçütlerin araştırmacılara yol gösteren ölçütler olarak kullanılmasını engellemektedir. Bunlardan söz etmek için önce bilimsel araştırmanın gelişimi beklenmelidir. Kabul edilmiş bir araştırma programı başarılı öngörülerde bulunabilir duruma geldiğinde ve rakiplerinden belirgin biçimde daha iyi olduğunda, eğer tüm gelişimi gözlemsel başarıları ile açıklanamıyorsa tarihsel gelişimini açıklamak için bu ölçütler kullanılabilir. Ancak şu anda gözlemsel olarak eş güçte olan iki kuramdan hangisini seçmek gerektiği sorusu yanıtsız kalacaktır.

Oysa bilim tarihinden örnekler göstermektedir ki bilimciler, pek çok kez gözlemsel olarak denk iki kuramdan birini seçmek durumunda kalmışlardır. Uyumculuk yaklaşımı, Lakatos'un epistemik değer vermekten kaçındığı ölçütlerin neden uzun erimde bizi doğru kuramlara götürebileceğini açıklar. Böylece yalnızca geçmişe bakıp tarihi yeniden yazarken değil, günümüz kuramlarını da değerlendirme olanağı sunar.

Uyumculuk yaklaşımı bilim felsefesinin pek çok geleneksel sorununun üstesinden başarıyla gelebilmektedir. Ayrıca bilim tarihini de rasyonel bir süreç olarak açıklamakta alternatiflerinden çok daha başarılıdır.

APPENDIX B

CURRICULUM VITAE

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