Phenomenological Realism, Superconductivity and Quantum Mechanics

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Abstract

The central aim of this thesis is to present a new kind of realism that is driven not from the traditional realism/anti-realism debate but from the practice of physicists. The usual debate focuses on discussions about the truth of theories and how they relate with nature, while the real practices of the scientists are forgotten. The position I shall defend is called "phenomenological realism".

The realist doctrine was recently undermined by the argument from pessimistic meta-induction, also known as the argument from scientific revolutions. I argue that phenomenological realism is a new kind of scientific realism that can overcome the problem generated by the pessimistic meta-induction, and which reflects scientific practice. The realist has tried to overcome the pessimistic meta-induction by suggesting various types of theory dichotomy. I claim that the different types of dichotomy normally presented by realists do not overcome the problem, for these dichotomies cut through theory vertically. I argue for a different kind of dichotomy, one that cuts horizontally, between high-level and low-level theoretical representations. I claim that theoretical forms in physics have two distinct types depending on the way they are built. These are theoretical models that are built depending on a top-down approach and phenomenological models that are built depending on a bottom-up approach. I argue that for the most part only phenomenological models are the vehicles of accurate representation.

I present two case studies. The first case study is from superconductivity, where I contrast the BCS model of superconductivity with the phenomenological model of Landau and Ginzburg. The other case study is a fresh look at the Bohr-Einstein debate.

To Aseer with love

Contents

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Preface

A long time ago I read a passage from the Italian philosopher Antonio Gramsci in his prison notebooks, which I think, still holds. I will quote the full paragraph here:

> Engels' expression that 'the materiality of the world is demonstrated by long and laborious development of philosophy and the natural sciences' needs to be analysed and made precise. By science does he mean the theoretical or the practical-experimental activity of the scientists or the synthesis of the two activities? In this we could be said to have the typical unitary process of reality, in the experimental activity of the scientist which is first *model of dialectical mediation* between man and nature, the elementary historical cell by which man, putting himself into relation with nature through technology, knows it and controls it. Undoubtedly, the promulgation of the experimental method separates two worlds of history, two epochs, and begins the process of dissolution of theology and metaphysics and the development of modern thought, whose crowning is Marxism. Scientific method is the first cell of new method of production, of the new form *of active union between man and nature*. The scientist-experimenter is also a worker, not a pure thinker, and his thought is continually controlled by practice and vice versa, *up to the point where a perfect unity of theory and practice is formed.* (Gramsci 1980, p107 my italics)

This passage was part of a critique of the Russian scholastic type of Marxism that transformed Marx's ideas into a mechanistic materialism strange to his dialectical approach. The basic two points that are relevant to my work here are: the unity between practice and theory and the conception of scientific practical activity as a mediator between man and nature.

Two questions are important in this context. These are: what is the relation between the experimental and the theoretical and how can the theoretical represent nature? In my MSc dissertation I focused on thought experiments and real experiments in quantum mechanics. At that stage I was not sure that a thought experiment would have the same elements as the real experiment and would have the same effect on scientific discourse. A deep look into the Bohr-Einstein debate gave a definite positive answer.

The French philosopher of science Gaston Bachelard has been another source of inspiration. His idea of phenomeno-technology helped me to understand the deep embodiment of the human technological and scientific activity in nature and also helped in highlighting the concept of 'perfect unity between man and nature'. Also, his idea of merging different philosophical positions which are apparently in conflict led me to look into the relation between the realist position and the anti-realist from a different angle, and to try to develop 'the best of both worlds'.

Nevertheless, the realist position was the dominant. I want to hold onto it. Usually, realists try to hold that scientific theories are the vehicles of representation. Such a position has been challenged, philosophically and empirically, for most high-level theoretical representations. Nonetheless, the low-level theoretical representations prove to be able to withstand the challenge.

When I came to study at the LSE, I saw in Nancy Cartwright's[1] work on models the perfect soil to grow a realist position that holds onto its realist routes while accepting some of the anti-realist critique to high-level theories.

In this dissertation I try to present such a position: phenomenological realism. I am not arguing against realism. So, in my debate with structural realism I am not contrasting their position with that of anti-realists, but I am contrasting their position with practice in physics. Because of that I rely on arguments and discussions in the physics literature rather than that in the

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[1] See her 1983, 1989, 1994a, 1994b and 1995.

philosophical literature. For example, in chapter two, in the discussion related to the correspondence principle I present the types of formal correspondence accepted in physics. Of course, the philosophical literature is full of papers and discussions on the correspondence principle but I do not approach the discussion from that angle.

In chapter one I argue against structural realism.[2] By far, the most successful current scientific realist position is that of structural realism. Structural realism advocates a structure-content dichotomy, asserting that the structure represents the underlying relations between real objects while the content is mere interpretation and might change through theory change. By doing so, structural realism claims that it has been able to have both the no-miracle argument and the argument from scientific revolutions on its side.

The important device, which secures the structural realists' claim of having the argument from scientific revolution on their side, is the correspondence principle. In chapter two I argue that the structural realists' definition of the correspondence principle does not fit physics. Furthermore, I claim that because of the many ways that formal correspondence can be applied in physics it is not as important as has been suggested in the philosophical discourse.

In chapter three I argue for the division between low-level and high-level theoretical representations. I argue that such a division is exemplified by the theoretical models for the latter and phenomenological models for the former. In chapter four I illustrate the advantages of this new division by means of a case study from superconductivity. There I contrast the BCS model of superconductivity, the model that was accepted for a long period of time as *'the theory'* of superconductivity, but failed to account for all types of superconductors, with the Landau and Ginzburg phenomenological model, which proved a more plausible model to be a representative of

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[2] There is more than one version of structural realism, here I discuss only one (that of John Worrall and Elie Zahar) as a way to contrast it with my own version of realism.

superconductivity, and which was able to even account for the new high temperature superconductivity.

These advantages give strong support for phenomenological realism. In chapter five I argue for phenomenological realism. I claim that the pessimistic meta-induction is not an argument against phenomenological models in the same way in which it is against high-level theories. Because of the way they are built, phenomenological models have a good chance of surviving through theory change. I claim also that the no-miracle argument can work in favour of phenomenological models. Hence, there exists a distinction within scientific practice that would provide a basis for being a scientific realist and which would overcome the pessimistic meta-induction: realism about phenomenological models but not about theories.

Third, I illustrate the difference between structural realism and phenomenological realism by means of the Bohr-Einstein debate. In the last chapter I take a fresh look at that debate in the light of my distinction between the two types of theoretical forms in physics.

Chapter One

Structural Realism

1:1 Introduction

In this chapter I argue that major replies of scientific realism to the pessimistic meta-induction argument depend on introducing a dichotomy on the theoretical level. I looked particularly at structural realism. In this case the dichotomy is between structure and content. I argue that this dichotomy does not help: In order for structural realism to have the argument from scientific revolution on its side, it needs to adopt a restricted concept of correspondence principle. In chapter two I argue that such a concept does not fit physics.

Recent attacks on realism have succeeded in presenting a serious challenge to it. This challenge eventually affected the beliefs of even a deep believer in realism like Hilary Putnam (1990), who changed his position under the influence of such critiques. Hitherto, the core point in the realist position was that theories are true representations of nature, or approximately so. The strongest argument for realism is that suggested by Poincaré and re-affirmed in Putnam's early work: the no-miracle argument. As Putnam puts it: "the positive argument for realism is that it is the only philosophy that does not make the success of science a miracle" (Putnam 1975, 73).

Realists do not accept that all theories are true (or approximately true) representations of nature, just the successful ones are. However, realists do not agree on the criterion of what to accept as a true or successful or approximately true theory. In his *Smoke and Mirrors,* James Robert Brown presents one definition of successful theories:

> ...By calling these theories successful I chiefly mean that: 1) they are able to organise and unify a great variety of known phenomena; 2) this ability to systematize the empirical data is more extensive now than it was for previous theories; and 3) a statistically significant number of novel predictions pan out, i.e. our theories

> get more predictions right than mere guessing would allow (Brown 1994, 4).

Contrary to Putnam, Brown does not accept that the critiques of realism are good. He is a Platonist realist who believes that our theories reveal the true properties of nature.

The major recent attacks on realism come from pessimistic meta-induction on the one hand and new versions of empiricist arguments on the other. These positions are associated with Larry Laudan[1] (the former) and Bas van Fraassen[2] (the latter) -- van Fraassen defines his position as "constructive empiricism". Laudan relies on the history of science to claim that the realists' explanation of the successes of science does not hold. He argues that the success of theories cannot offer grounds to accept that these theories are true (or even approximately true). He presents a list of theories that have been successful and yet are now acknowledged to be false. Hence, he concludes, depending on our previous experience with scientific revolutions the only reasonable induction would be that it is highly probable that our current successful theories will turn out to be false. Van Fraassen claims that despite the success of theories at accounting for phenomena (their empirical adequacy), there could never be any grounds for believing any claims beyond those about what is observable.

In reply to these attacks many realists have suggested that the way forward is not in accepting the theories as a whole as correct representations of nature, nor is it in rejecting them as a whole, but rather in adopting a kind of division between different parts of the theory. That is to say that some parts might be accepted as correct representations of nature while the rest as, e.g. in Philip Kitcher's case, presuppositional posits. These kinds of dichotomy divide the theoretical forms vertically, cutting through low-level as well as high-level theoretical forms. I claim that such vertical divisions do

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[1] See for instant his 1981 and 1984.

[2] See for instant his 1980 and 1985.

not solve the problems realists' face. In Chapter three I will suggest an alternative division, not in terms of accepting theories as real representations of nature, but by claiming that there are two kinds of theoretical forms in physics. One takes a top-down approach toward accommodating experiments; the other is phenomenological and takes a bottom-up approach. I claim that the theoretical outcomes that result on the phenomenal level, which I call "phenomenological models", often hold true representations of nature, while the high-level theoretical forms, which I label "theoretical models", seldom are true representations of nature. This division is horizontal: it divides between low-level and high-level theoretical forms.

As for now, I will try to show how a particular form of realism fails to overcome the difficulties generated by the critiques of Laudan and van Fraassen and also fails to reflect the real practice in physics. Among the realists the particular form of scientific realism I have chosen to discuss is the structural realism programme. This is because I think it is the most defensible of the current positions in realism, and because it takes into consideration the attacks on realism launched by different anti-realists during the last three decades. I start by laying down the bases of structural realism. Structural realism aims to present a plausible position that would hold the two arguments from realism and from scientific revolutions to its side. It suggests that the type of division that realism ought to make within theories is that of dividing the theory into structure and content, where it accepts that the structure (by which they mean the mathematical structure) represents nature while the content can be filled indifferently and changed (sections 1:2).

Other examples of such a division within theory include Philip Kitcher's distinction between two different posits: "presuppositional Posits" vs. "working posits". The former are the non-referring terms in a theory which "have to exist if the instances of the schemata are to be true" (Kitcher 1993,149), while the latter are the referring terms which feature in a successful explanatory schemata. Stathis Psillos suggests another distinction as I will show below. The problems for structural realism, as well as for Kitcher and Psillos, are depicted by the questions: why should we accept some parts of theory to be true representations of reality while refusing this status to other bits of the same theory? And, how would we be able to draw the line between these two parts? (Section 1:3) I suspect that a deep study into the history of science would reveal, most of the time, that there is at least one counter example to each of these divisions.

In sections 1:4 and 1:5 I will show that the kernel of the structural realism programme is the correspondence principle. Structural realism depends on an important heuristic device in order to secure an 'optimistic' interpretation of scientific revolutions. This heuristic device is the correspondence principle. Although structural realism accepts that there are different kinds of correspondence between old theories and new ones, I believe that only one special kind of formal correspondence can best express their concept of structural continuity.

1:2 Structural Realism

The attacks against realism deploy a well-defended argument. That is the pessimistic meta-induction. John Worrall puts it as follow:

> Revolutionary changes have occurred in accepted scientific theories, changes in which the old theory could be said to "approximate" the new only by stretching the admittedly vague and therefore elastic notion of 'approximation' beyond breaking point (Worrall 1989, 107).

So, in an attempt to overcome the difficulties facing realism, John Worrall and Elie Zahar suggest that realists should focus on the form or structure of theories rather than on content. They trace this position to Poincaré, Zahar also traces this view to Duhem. The core idea, as Worrall puts it, is to have the best of both worlds: realism and instrumentalism. There are two main arguments in the realism/anti-realism debate, or so -- at least -- Worrall claims. The chief argument for realism, as it is accepted by most if not all realists, is the no-miracle argument. The one against realism is the argument from scientific revolutions, which gives a pessimistic interpretation of theory change: if we look at the history of science we would conclude that even the most well supported theories turn out to be wrong.

Structural realism suggests that if we consider only the highly supported mathematical structure of the old theories, then it is possible to present a good argument that would hold the evidence from scientific revolutions to the side of realism. This argument runs as follows. If a given, and highly supported mathematical form that accounts for a set of phenomena $\{P_i\}$ is preserved through theory change, either as it is or via the correspondence principle, then it is highly probable that it represents, or at least approximately represents, the true underlying structure of nature.

> Hence, this neglected position of Poincaré's, can offer the only hopeful way to have the best of both worlds: to give the argument from scientific revolution its full weight and yet still adopt some sort of realist attitude toward presently accepted theories. (Worrall 1989, 99).

Worrall states that there are *no essentially new arguments* in the current realism/anti-realism debate. For him, Poincaré and Duhem put forward the two chief arguments in that debate. Worrall puts the no-miracle argument as follows. It is impossible for highly empirically successful theories (i.e. theories that are explanatory and predictive), to be true

> without what the theory says about the fundamental structure of the universe being correct or 'essentially' or 'basically' correct. (Worrall 1989, 101)

Zahar takes a more elaborate position on the matter. He puts the argument as follows:

> It is highly unlikely that organically compact hypotheses should either explain the facts in a systematic and non ad hoc way or should establish hitherto unsuspected connection between these facts, without simultaneously being at least approximately true; i.e. without reflecting, in a way more or less approximate, real connections between individuals. This is because the various components of a unified system are so closely knit together that a considerable departure of any one of them from the truth would have repercussions throughout the system, hence lead to a refutation somewhere in its empirical domain (Zahar 1994a, 1).

This way of putting the argument concentrates on the structural form of the theory and its hypotheses, and the fact that this structure is "closely knit" in a unified system.

Due to his belief that these are the two chief arguments, Worrall claims that only a position that can have both arguments on its side can be satisfactory. He thinks that structural realism is such position, i.e. a position that

> both [underwrites] the 'no miracles' argument and [accepts] an accurate account of the extent of theory change in science (Worrall 1989, 117).

So, what is structural realism, and how it can have both arguments on its side?

Zahar suggests a rank of realism. He begins with metaphysical realism[3] and methodological realism[4], which do not bear centrally on my concerns here. Structural realism, according to Zahar, is a kind of scientific realism that adds a further thesis

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[3] Metaphysical realism claims that there is one structure of the universe of which our mind is a part, so that the mind, while trying to reveal this structure, will be governed by the same laws which it wants to reveal.

[4] Methodological realism adds a further assumption to metaphysical realism that the structure of reality is an intelligible representation of that reality.

> that successful theories, i.e. those unified systems which explain the data without ad hoc assumptions, are approximately true. That is: such systems reflect, in one way or another, the order of things as they are in themselves. Successful hypotheses model and in some sense 'correspond' to real structure of the world (Zahar 1994a, 20).

Structural realism differs from other kinds of scientific realism in accepting that the only part in the theory that generally should be interpreted realistically is the mathematical structure which is said to convey the real structure of nature, or at least to approximate that structure. Hence, structure gets preserved while content changes. That gives structural realism the ability to claim that a theory, despite being a false theory, has the power *to predict*: it is the mathematical structure that gives this power to the theory. But what does it mean to say that the mathematical structure has the power to predict?

Consider, for example, a mathematical structure of a certain phenomenon given by a function of the fourth order ($F(x) = Ax^4 + \dots + Dx + E$). This function has two positive peaks. Now, if the experimental data give us information about the first peak, but do not provide any data about the second peak, then the mathematical structure provides the power to predict that there would be another peak. To illustrate this point, I will take Worrall's example, which I will discuss below. In Fresnel's theory, the mathematical equations predict that light incident on a plane surface from a direction away from the normal will be reflected according to the light's state of polarisation. It interprets the light to be mechanical vibrations propagating in an ether medium. In Maxwell's new interpretation of light propagation, he accepts that light will be reflected according to its state of polarisation, but he states that light is an electromagnetic wave propagating in an electromagnetic field. In both cases the phenomenon of the light (reflection or refraction) is dependent on its state of polarisation. And the same mathematical equations were used. Here structural realism will claim that such an example shows that while the mathematical structure continued to hold, the content changed.

The predictive power, as structural realism claims, is contained in the ability of the mathematical equations to give certain results whenever input numbers are provided; whether these numbers are interpreted as 'cats' or as 'lions' is another matter. This is what leads Worrall to argue that the structure is what holds a real representation of nature. He thinks that the content of the theory can be changed with scientific revolutions, i.e. that the interpretation of the variables as cats or lions might change with theory-change while the mathematical structure ought to be preserved. Hence, after describing the realists' claims, such as those of Putnam and Boyd, about theories in science and how new theories should resemble the old ones, Worrall says that the realist's

> intuitions are better captured in a rather different position which might be called structural or syntactic realism (Worrall 1989, 112).

1:2:1 Fresnel's theory

As I mentioned, 'structure' - in structural realism - refers to the mathematical structure of a theory. The idea is that the highly confirmed theories of mature science, those that have the power to predict and unify, have a mathematical structure that is capable of surviving scientific revolutions.

To illustrate his position, Worrall gives an example from optics: Fresnel's theory. Fresnel's theory of diffraction states that light consists of vibrations that propagate through a mechanical medium (ether). It states that any unpolarised light could be analysed into two components: vertical and horizontal. And it gives the reflection and transmission (refraction) coefficients of the light polarised in the plane of incidence to be:

$$R_{II} = \frac{\tan(\theta_2-\theta_1)}{\tan(\theta_2+\theta_1)}$$

$$T_{II} = \frac{4\sin\theta_2\cos\theta_1}{\sin 2\theta_2+\sin 2\theta_1}$$

While the coefficients of the light polarised perpendicular to the plane of incidence are

$$R_{\perp} = \frac{\sin(\theta_2-\theta_1)}{\sin(\theta_2+\theta_1)}$$

$$T_{\perp} = \frac{2\sin\theta_2\cos\theta_1}{\sin(\theta_2+\theta_1)}$$

Where θ_2 is the angel of reflection or refraction and θ_1 is the angle of the incidence[5].

This theory was 'falsified' by Maxwell's theory of optics. The new theory assumes that the medium is the field rather than the ether[6]. Worrall claims that the mathematical equations (structure) of Fresnel's theory can be derived from Maxwell's theory. The important point for Worrall is that the structure of Fresnel's theory is preserved in the new theory. Hence, Fresnel's mathematical equations are still an accurate basis for calculation. This success in preserving the structure leads Worrall to conclude:

> Thus if we restrict ourselves to the level of mathematical equations - not notice the phenomenal level - there is in fact complete continuity between Fresnel's and Maxwell's theories (Worrall 1989, 119).

In this example Worrall accepts that even if Fresnel's theory is built on the false assumption that there exists an ether medium, its mathematical structure is 'right'. Now under the new theory – Maxwell's - the same structure continues to capture the phenomenon. The new theory needs only, (due to the fact that the content of Fresnel's theory was found to be wrong,) to reinterpret the symbols of the structure in a different way; i.e. to suggest a new content. Fresnel's theory asserts that the vibrations, which are

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[5] See Smith and Thomson 1982.

[6] It is more accurate to say that Maxwell's theory rejected the idea of the light's need for an ether medium and assumed a totally different theoretical frame, that of saying that the light waves are electromagnetic waves that propagate through a field.

responsible for the way in which the theory represents the coefficients, are vibrations in the ether medium. Maxwell's theory replaced this identification of the vibration (with the ether medium) with another identification related to the electromagnetic field. Worrall claims that if we "do not notice the phenomenal level" we can see that the mathematical form of Maxwell's theory resembles that of Fresnel's. So, Worrall thinks the mathematical structure was capable of surviving through scientific revolutions while the content of Fresnel's theory was replaced with another content.

The main idea here is that if the old mathematical structure continues to yield correct results, and if the new theory retains the mathematical equations of the old theory, then this is a very good reason (in structural realism's term), to be optimistic in suggesting that this mathematical structure reflects the underlying structure of the universe.

Structural realism ignores all the elements that constitute the theory except the mathematical equations. The rest of the theory elements, according to Worrall, would be on what he calls the 'phenomenal level'. But any theoretical model (or even a phenomenological model for that matter) consists, as I will argue in the next chapter, of: the mathematical equations, a description of the phenomenon under study including its environmental set-up, and a story that usually connects the symbols of the mathematical structure with the real entities of the world.

In Fresnel's case we do not have a fundamental theory like that of Newton's or Einstein's which is associated with different types of phenomena; we have a theoretical representation related to the reflection and refraction of light. Fresnel's 'theory' represents a kind of theoretical model. It gives a story telling us that light propagates through an ether medium. It associates certain properties of the phenomena with the symbols in the mathematical equation, such as associating the properties of the vibration of light with the refraction (T) and reflection (R) phenomena. It is also important to mention that the only measurements that take place are the angles of refraction and reflection[7]. So, from Fresnel's perspective the ontology in which the phenomenon occurs is the ether medium. The new theory associates the phenomenon with a different picture that would prove that the previous understanding of the phenomenon is false. Therefore, it needs to present another story.

I think structural realism would face a problem here. The new theory has a different understanding of the phenomenal set-up than the old one, in the sense that it assigns to it a different ontology. It claims that 'light is an electromagnetic wave that propagates in an electromagnetic field' rather than 'light is a wave that propagates in an ether medium'. Now, if a person wants to accept that theories represent nature then they should not rule out the importance of the ontological bases in both theories: ether versus fields -- there exists one nature and either it is a nature with fields or a nature with ether. Structural realists claim that the only aspect of reality we have good reason to believe that we are describing correctly, is its structure. As Poincaré puts it:

> These are merely names of the images we substituted between these real objects which Nature will hide from us for ever from our eyes. The true relations between these real objects are the only reality we can attain. (quoted in Worrall 1989, 118)

These relations are given by the mathematical structure even if we do not know what these entities are. They claim that we can change our understanding of these entities when a change of evidence occurs, while the mathematical structure that relates these entities will not change. But, the structure is not the only thing that will survive scientific revolutions; and not all structures will be retained.

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[7] Some times these laws are stated using the concepts of refraction and reflection indexes. It will make no difference on calculations because these two indexes are defined using the refraction, reflection and incidence angels, nevertheless I prefer using the angles, because they are directly related to the phenomena.

Psillos (1995a, 15-46) challenges the structural realist claim -- that the new theories preserve only structure but not content -- by saying that in the new theories, some of the physical interpretation of the mathematical structure is also retained. He analyses Worrall's example of Fresnel's law and shows that some non-structural features of Fresnel's interpretation of the wave-equation, for example:

> transversality, ability to sustain potential and kinetic energy, finite velocity of propagation and others (Psillos 1995a, 19)

were retained in Maxwell's theory. Although this claim might be true for Worrall's (1989) position it might not hold against Zahar's (1994) position. Zahar agrees that some of the content of older theories might get retained in the new ones. But the important point for him is that only structure is what is regularly retained in the new theory, in cases where it takes over the predictive successes of the old. Any necessary change would be applied to the content. If some of the content is preserved after that, then that is fine but it ought not be a presupposition that the new theory would preserve any content. But before I go into Psillos critiques of structural realism let me discuss how structural realism thinks it answers the pessimistic meta-induction critique and how it thinks it can hold the argument from scientific revolutions on its side.

1:3 Structural Realism and the pessimistic meta-induction

Structural realism tries to overcome the difficulties suggested by the fact that even the highly successful theories, like Newton's, had turned out to be false due to new discoveries. As I mentioned earlier, the debate about realism was overshadowed by two main arguments: the no-miracle argument and the pessimistic meta-induction. The pessimistic meta-induction argument against realism has two parts:

1) The empirical evidence supporting a theory does not give enough warrant that the theory is true or even approximately true. The history of science shows that many theories that had good empirical support have proved to be false. Current scientific theories are not in a better status than their ancestors are and it is highly probable that they too will turn out to be false. Hence, there are no grounds to believe that our current successful scientific theories do represent the world.
2) Even if some of the new scientific theories are using the same terminology as the old theories, there is a discontinuity of what these terms mean, as well as a discontinuity at the structural level.

Structural realism answers these objections mainly by two 'steps':

1) The history of science shows that mature scientific theories with a high empirical support often have a structure that can survive scientific revolutions. So, yes, some of the scientific theories cannot be true or even approximately true unless we want to stretch the meaning of approximately true 'beyond the breaking point' (Worrall 1989, 107); but mature theories often have mathematical structure which can 'mirror reality'. Everything else in the scientific theory is likely to be false, and if some of the content of a theory survived through theory change, then that is a bonus. Through theory change there is continuity on the structural level and not necessarily on the content level.
2) In the case of structural change, the structure of the new scientific theory should yield that of the old theory at a certain limit. The correspondence principle is the key device that ensures scientific continuity and realism.

By using such an argument, structural realism secures the no-miracle argument by claiming that it is no miracle that scientific theories work because the structures of these theories represent true relations in nature, or at least approximately do so. Nevertheless, the argument from scientific revolutions still holds, because sometimes, as structural realism would argue, to say that a theory (with its structure and content) is (even) an approximately true theory cannot hold except by taking the concept of approximately true beyond its breaking point.

So, according to structural realism, the argument from scientific revolutions is right on one part of theories, but wrong on the other. This argument is right if the content of a theory is accepted to be as the representative part of that theory. But it is not right if the realist take only the structure to be the representative part of the theory.

Other scientific realists argue that there might be a way out for scientific realism from the pessimistic meta-induction, without giving in to it. Psillos disagrees with structural realism because it differentiates between the structure of the theory and its content. He claims that the mathematical equations of scientific theories are not semantically free. He says:

> a scientific realist can explain the fact that mathematical equations are retained in theory-change, on the grounds that they form an integral part of well-supported and (approximately) true theories. But she would not claim that all that is retained is empirical content and (un-interpreted) mathematical equations. Nor would she claim that there is a dichotomy between the structure and the content of physical process. (Psillos 1995a, 19)

He claims that scientific realism can offer a proper reply to the pessimistic meta-induction, without the concessions that structural realism make. He claims that if the forms that are preserved from the abandoned theories are those responsible for the empirical success of these abandoned theories, then scientific realism can be defended in face of the pessimistic meta-induction.

To overcome the pessimistic meta-induction Psillos suggests another type of theory division. Let us first look at his general claims about scientific realism. Psillos (1994, 1995a, 1995b, 1996) claims that scientific realism offers the following theses:

1) Through theory-change, not only the structure is retained but also "some of the properties incorporated into the physical interpretation of the mathematical equation."
2) Scientific knowledge is cumulative: the new 'full' physical content of new theories, which incorporated some of the old content, is "better supported by current evidence". New theories can interpret the mathematical equations better than their ancestors can because the evidence we have supports them more than it supports the interpretation of old theories.
3) Due to the fact that new theories enjoy better support than old theories, we should be optimistic rather than pessimistic about the development of science: New theories are more likely to be true than false.

Psillos divides Laudan's list of 'successful-yet-false' theories into two categories: Theories that were taken to be merely speculations, while others were "taken to be rather firmly supported by the evidence and entrenched background beliefs" (Psillos 1995b, 7). Psillos accepts only two theories from Laudan's list to be in the second category. These are "the caloric theory of heat and the whole family of ether-theories" (Psillos 1995b, 9). However, Psillos argues that in order to overcome Laudan's attack, the realist ought to prove that the central terms of the theories from the second category refer. This is because these theories are firmly supported by evidence, and because realists believe in the no-miracle argument; therefore, the limited success of these theories means that they capture real aspects of nature and this success by itself is not a mere miracle. Psillos shows, in two case studies, that what would be considered as central terms in these theories had been retained in the new theories.

The move that Psillos introduces is what he calls "divide et impera" (Psillos 1996, 1): scientific theory can be divided into two parts, one consists of the claims that contributed to successes in science: *working postulates*; and second consists of the *idle components*. This move, he says,

> suggests that if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of scientific realism can still be defended (Psillos 1996, 3).

Here, his strategy is similar to that of structural realism in that we ought to divide the theories -- although they disagree on where the division lies. According to structural realism, the division is clear: structure versus content. Psillos poses two questions: 1) what justifies the claim that not all components of a theory contribute equally or equally well to successes in science? And 2) is the division really in agreement with the development in science?

Psillos wants to focus on 'specific' successes of old theories. Like Worrall, he considers Fresnel's theory of diffraction as an example. He says:

> if an opaque disk intercepts rays emitted by light source, a bright spot will appear at the centre of its shadow (Psillos 1996, 5).

He claims that the hypotheses essentially involved in generating this successful prediction, i.e. that the light-waves have a transversal character, were "carried over" to the new theory of light.

But what Psillos claims is persevered here, is not a hypothesis of a theory, even if the theory will deploy such a fact as a hypothesis. Elsewhere, (Psillos 1996, pp 34-35) Psillos admits that Fresnel reached his original hypothesis from his experimental observations (with Arago). He quoted Fresnel saying:

> we [i.e. Fresnel and Arago] both felt that these facts would be explained very simply, if the vibrations (oscillatory movements) of the polarised waves took place in the plane itself of these waves [i.e. if they are transversal] (Psillos 1996, p 34).

He also admits that Fresnel did not use mechanics in his hypothesis. This theoretical hypothesis correlated with the experimental observation led Fresnel to believe that a bright spot would appear at the centre of the shadow.

But later this fact, i.e. that a bright spot would appear at the centre of the shadow, was proved through experimental verification to be a phenomenal fact, i.e. a fact that is observed experimentally and is very well described independently of the high-level theoretical representation of Fresnel's theory. I am not speaking here about the mere empirical observation of a bright spot at the centre of the shadow. I am speaking about the phenomenal level, that is the empirical observation with the description of the set-up and of the movement of the light. I.e. the simple description deployed by Fresnel and Arago plus the experimental verification and the experimental set-up which led to this verification[8], without the assumption that light propagates in an ether medium. At that level, it is a fact that the bright spot would appear at the centre of the shadow and this fact is very well described by the movement of light in relation to the set-up. This is regardless of the high-level theory that suggests that the light movement is propagation in an ether medium or in a field.

Now, the new theory would of course carry over such a hypothesis exactly because it is experimentally verified, and hence it is from the point of view of the new theory part of the empirical and phenomenal basis of that

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[8] This kind of theoretical explanation is called phenomenological model. This will be discussed in details in chapter three.

new theory. What I mean is that this fact is not a hypothesis from the point of view of the new theory anymore. This is because when Fresnel suggested such a hypothesis, sets of experiments were performed in order to verify the hypothesis. The mere fact that it was found to be true, is what renders it as a phenomenal fact. Hence, and after such verification, the new theory cannot proclaim it as a hypothesis.

In the cases Psillos cites, it is easy to show that: these theoretical forms are a straightforward theoretical account of the phenomenal/ experimental facts and are called, as I will argue in chapter three, phenomenological models. In these cases there is a lot of theorising going on but that does not lead to the conclusion that the theories, which the models take their tools from, are to be taken as true as the phenomenological model would be. The reason lies in the way these models are constructed and when and why the theoretical description and explanation come into place (as in the case of Fresnel and Arago's simple description). High-level theoretical representations, which are expressed by theoretical models, contain more than this descriptive move. Theoretical models have an unaccepted explanation of the phenomenon, claiming that this explanation is 'the real' cause that 'explains' why the phenomenon exists. Some examples of these models would be: Cooper pairs in superconductivity, mesons in high energy physics, strings in super strings theory and Fermi surface or Fermi sea in solid state physics. These kinds of models don't have straightforward experimental support, while the phenomenological models do have such support.

A simple example of the description associated with low-level theoretical models would be the case of electrons. In a chemical electrical circuit we know that the number of negatively charged particles on the cathode should rise, whenever the circuit is closed, until the anode is ionised completely. Now our description that there 'exist negatively charged particles called electrons' is supported by the observation of the rise of the negative charge detection at the cathode. This account is not enough to claim that electrons are real, because up to this point, we did not test this existence in a different experimental context to verify this claim. A further move is needed: if we can, after accepting this description, develop another experimental set-up, in which we would use the properties of electrons and we succeeded then the claim that electrons exist and they are real entities has good empirical and phenomenological support. Such a move might be the development of electrical equipment[9].

The problem for Psillos' position would be captured by the question: how would we be able to define accurately which parts of the theory we ought to accept as real representatives of nature from the parts which are merely a working postulate which might turn out to be wrong?

One might say that a careful look at the failed theories, in relation to what we accept as good theories at the current moment, might give us the information to decide which parts of the old theories still hold. This is a redundant exercise, because even if we know now what parts of the failed theories were retained, that would not give us any information about the current theories and their parts, which should be accepted as real representations of nature from those parts which are not. It appears that there is little in common between the parts that have been retained in the past. Sometimes it is the structure; sometimes it is this or that postulate related to the content. Therefore, we would not be able to assert which parts of the current theories would have the same fate. I think that finding a way out from such a dilemma would not be possible by merely looking for common grounds in theoretical forms. Rather there is a need to look at the processes in which these theoretical forms were constructed and which ones were retained in relation to these processes.

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[9] Ian Hacking gives another instance where we "manipulate" electrons: the electron microscope. Nevertheless, Hacking confines himself to accept only the entity realism but not any theoretical description associated with the entity. See Hacking 1983 for details.

So, while structural realism gives us a clear-cut account of what is to be interpreted as a real representation of nature -- the mathematical structure --, Psillos fails to clarify this distinction in the case of "divide et impera", other than by pointing to the properties which are related to phenomenological models. If these are all that we can accept from the old theories then phenomenological realism seems the way to go.

1:4 Mathematics and scientific discoveries

In this section I want to explore more aspects of structural realism's view on structure and its importance for scientific discovery. For structural realism the structure is the essential part of any scientific theory which would be preserved through theory change. A realistic interpretation of the mathematical terms would help, according to structural realism, in presenting a coherent, and physically meaningful structure. Here I will only state the structural realist's view, and I will leave discussing it to chapter two.

So, how could a realistic interpretation of the mathematical equations serve science from the point of view of structural realism? Zahar answers this question in two ways. First, by interpreting the mathematical terms realistically they can become

> physically meaningful, hence testable, at least in principle. The theory remains syntactically unchanged, but its observational content, i.e. its content with 'observable' reality, is extended (Zahar 1980, 8).

Second, it leads to new discoveries. Zahar claims that a realistic interpretation of the theory (mathematical equations) would lead to the discovery of new relations between elements of nature, relations not yet known. Zahar states that if a hypothesis H in a previous theory needs to be modified because it is in disagreement with a well-confirmed set of laws, a hypothesis H*(t) would be suggested as a substitution of H. Now t can be given an interpretation that subsumes it under a certain category obeying certain laws. Suppose H*(t) violates these laws. Then H*(t) should be modified yet again. A real

> breakthrough is achieved when H*(t) is modified into H' (t) which conforms to the laws in question (Zahar 1980, 8).

Zahar claims that the set of procedures that is taken in modifying H into H' (t) would seem purely deductive (on a metalevel).

Zahar concludes from the claim about the role of interpreting the mathematical equation realistically in new discoveries, a more general one, which goes from the mathematical structure of a given physical theory to the possible mathematical structures. He says:

> The mathematical form of a successful theory models and hence reveals the structure of a universe which remains largely inaccessible to our senses. Therefore, by speculating about the possible mathematical structures of our physical laws, we may be able to devise fruitful heuristic strategies (Zahar 1994a, 28).

Such a belief in the mathematical structure as revealing the inaccessible structure of the universe is justified by structural realism's account of scientific discovery. Zahar stresses:

> Our knowledge of the external world is thus coextensive with that of the syntax or of mathematical structure of empirically successful theories. I.e. the syntax of the unified theories, which predict novel facts or establish unsuspected connections between disparate data, must be taken to reflect the *ontological order of things* (my Italics. Zahar 1994a, 13).

Thus the mathematical structures, as the structural realist will claim, are the best structures we have that can *reflect* the structure of an *ontological order of things of an otherwise inaccessible universe*. Thus structural realism claims that a realistic interpretation of the mathematical equations can serve the development of science.

One should not fail to note that experiments constrain the interpretation of the mathematical equations, that it is the phenomena that give the experimental data, and that the mathematical representation of these experimental data is provided, most of the time, after the data. So, the relations between the elements and properties of the phenomena are provided by experimentation. Therefore, what physicists' face is not a case of having a vague mathematical equation presented to them by God and all they need is to interpret its symbols. What physicists' face is a set of relations provided by what they can apply of their knowledge to test the phenomenon.

Let me give an example. In deriving the laws of uniformly accelerated motion, we prepare a set of experiments to discover the relations between distance and time, acceleration and time, and so on. From these experiments we obtain a large amount of experimental data that can provide reasonable plots from which we can infer mathematical forms. The set of relations and the story, as I will show later in chapter three, provide the bases in which a mathematical form is suggested as a part of a representative model of the phenomena under study. As I will argue, there are two distinct types of theoretical forms in physics. The main difference between them is in the way they are built. One depends on a top-down approach and the other on a bottom-up approach. Both types of theoretical forms require some sort of a story to link the mathematical equation with the properties of the phenomena. The difference between these two types of story is that one depends entirely on elements drawn from the set of experimental activities, while the other depends on the theoretical coherency with the theoretical frame. In the high-level theoretical representations the story would depend on other theoretical models that are, mostly, questionable. Structural realism suggests that in a new theory the terms of the mathematical equation be reinterpreted. This means that the underlying approach toward the theoretical forms is that of a top-down approach.

1:5 Structural realism and the correspondence principle

We need one final ingredient to understand structural realism: the use of the correspondence principle. I shall introduce the correspondence principle here for completeness, but delay a critical discussion until the next chapter. In certain cases, the new theory presents a structure which does not resemble the old, well-supported structure. Take Einstein's theory for example. It presents a "totally" new structure that differs from Newton's theory. In such cases, in order for structural realism to claim that there is a justified mathematical connection between the mathematical structure of the new theory and that of the old, it needs a type of correspondence between the two theories. As Zahar says, the relation between new theories and old ones is described by the *Correspondence Principle*, along with secondary requirements like Lorentz-covariance, derivability from a principle of least action or variation principle and the possibility of a Hamiltonian formulation of the laws to be constructed (Zahar 1994a, 4). This position is essential for structural realists. Mathematics is a deductive science, and, they argue, if there is to be a mathematical continuity between the mature old and highly confirmed theory and a new one, then the mathematical structure of the new theory should yield the 'old' one at a certain limit. And due to the importance of the mathematical structure, from structural realism point-of-view, the type of correspondence needs to be formal.

So, how does structural realism state the Correspondence Principle? Suppose we have a highly confirmed and logically consistent theory T (i.e. mature) that gives a well confirmed law φ which accounts for phenomena {pi} in the domain D. Suppose also that some new experimental evidence leads to the breakdown of φ outside the domain D (i.e. a refutation of T). A theory T' is suggested. If T' has constructed a new equation Ω that accounts for the same phenomena {pi} and avoids the refutation of φ, then structural realism suggests that Ω ought to yield φ at a certain limit. That is if Ω is a function, let us say of λ, then $\Omega \rightarrow \varphi$ as $\lambda \rightarrow 0$, when λ is an appropriate parameter of Ω (such as v/c) that picks out in some way the old domain D in which T was successful.

Zahar gives the relation between Planck's version of the second law of motion and that of Newton's as an example of a correspondence between a new and an 'old theory'. Planck suggested that

$$\vec{f} = \frac{d}{dt}\left(\frac{m\vec{v}}{\sqrt{1-v^2/c^2}}\right).$$

Now the correspondence between this structure and that of Newton can be achieved by taking v/c → 0

$$\vec{f}_{v/c\rightarrow 0} = \frac{d}{dt}(m\vec{v}).$$

(I will discuss this example in detail in the next chapter.) Hence, the new theory yielded the old one in a certain limit.

One other thing is important here: that the propositions of the old theory are approximately preserved through revolutions. Zahar claims:

> There ought to be a translation of the old system into the new one, such that:
>
> a) all observational functions and predicates remain unchanged, and
>
> b) the old axioms are transformed into theorems, or into limited cases of theorems of the new theory (Zahar 1994a, 18).

1:6 Conclusions

In this section I will outline the basic elements of this chapter. Structural realism is a type of scientific realism that asserts a distinction between the structure and the content of a theory. This is where the mathematical structure is the true representation of the structure of nature, while the content of the theory is related to the interpretation of the terms. Through a scientific revolution, the structure can be passed on to the new theory, either without any change or as a limiting case of the new structure, while the content can be reinterpreted. The key element in this continuity between the old and the new theory, according to structural realism, is the correspondence principle. Due to the fact that the most important element in the theory is the mathematical structure, it is claimed that this correspondence between the new and old theory is a formal one.

Hence, structural realism states that it is highly unlikely that the mathematical structure

> should either explain the facts in a systematic and non ad hoc way or should establish hitherto unsuspected connections between these facts, without simultaneously being at least approximately true; i.e. without reflecting, in a way more or less approximate, real connections between individuals (Zahar 1994a, 1)

(i.e. the no-miracle argument). It also states that through theory-change this mathematical structure is retained (i.e. an optimistic interpretation of scientific revolutions).

Such a position appears to accommodate the two main arguments in the realism/anti-realism debate on its side. That is, by differentiating the structure of the theory from its content, it succeeds in holding onto a realist position which claims that the structure of the theory capture the structure of reality. But it also accepts the argument from scientific revolutions by assuming that although the structure survives scientific revolutions through the correspondence principle, the content is nevertheless reinterpreted. But can such a position be mapped onto scientific practice?

First: the position, in a way, goes too far in putting the emphasis on the mathematical structure. This does not leave any weight for the interpretation of that mathematical structure. But theories need this interpretation to provide any kind of relation to reality. Hence, the mathematical structure is not sufficient to represent nature. Strong emphasis on the mathematical structure leads to loose relations to nature. In addition this is not what happens in physics. In most cases in physics the theoretical forms give a story that gives an account of why the mathematical structure ought to be accepted. This story is important in judging the acceptability of any model as a real representation of nature.

Second, to secure structural continuity between the old and the new theory, at least in a certain limit, structural realism relies on the correspondence principle. The correspondence principle acts as a heuristic devise to make sure that the well-confirmed structures are preserved. But, being totally formal, the structural realists' definition of the correspondence principle does not comply with physics[10]. The aim of next chapter will be to further illustrate and support this point.

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[10] Many philosophical discussions had concentrated on the importance of a generalised correspondence principle, (See for instant Radder 1991 and 1997, French (ed.) 1991), I do not pursue the same line of argumentation. I will draw my conclusions out of the correspondence principle as applied and understood in physics.

Chapter Two

The Correspondence Principle

2:1 Introduction

In this chapter I will show that the structural realist understanding of the correspondence principle does not fit physics. I will put forward two theses:

1) If we take the cases where structural realism claims that structural continuity is preserved via the type of correspondence its authors describe, we will find that some of these cases are not in agreement with the claims of structural realism. I illustrate this point by means of an example from quantum mechanics in section 2:3.
2) I will also show that structural realism tailors its definition of the formal correspondence principle to suit the needs of structural continuity. The types of "formal" correspondence that are accepted in physics would not support their claims. As a result, the structural realist definition of the correspondence principle makes too much and too little of correspondence. Too much because in order to secure what it sees as a defensible kind of realism, it demands a very restricted type of correspondence which often does not obtain. And too little because it downplays the importance of other kinds of correspondence that have been centrally employed in science.

2:2 Zahar and the Correspondence Principle as a meta-statement

Zahar asserts that until recently there was a sharp distinction in the philosophy of science between the 'context of discovery' and the 'context of justification', where only the second lies in the

> domain of methodology, whose proper task is to evaluate theories supposed to be laid on the table, i.e. supposed to have already been constructed. (Zahar1983a, 243).

He argues that this distinction does not hold any more and it is possible to speak about the context of discovery on a methodological level. His idea is

> both to underline the continuity between science and common sense knowledge and to remove some of the mystery surrounding the notion of heuristics by showing that, at any rate in some programmes, they operate purely deductively (at the metalevel) (Zahar 1983a, 245).

Accordingly, he wants to develop a general method of constructing theories by examining the methods actually used in constructing them. In doing so, Zahar is trying to answer two questions: "is there in physical science such a thing as rational heuristics, and if so, how does it operate?" He replies by confirming that there are "rational heuristics", which include "The Correspondence Principle".

Zahar has a very restricted account of the correspondence principle: he thinks that it works from the old theory to the new one. He says: "yet in all that [ways of correspondence], we remain strictly within the boundaries of deductive logic" (Zahar 1983a, 247). He interprets Poincaré's position on the correspondence principle as saying

> if an old hypothesis H turns out to have been systematically 'convenient' throughout some domain D, it is improbable that this should be due to pure chance; it can be assumed that H reveals true relations which ought to reappear perhaps in a slightly modified form, within a new theory T. In other words T must tend to H whenever certain parameters, by tending to zero, restrict T to the domain D (Zahar 1983b, 163).

Zahar claims that by using the correspondence principle the new theory's mathematical form can be seen as derived from the old theory. He acknowledges the fact that the correspondence principle was applied in physics long before Bohr introduced it in quantum mechanics. It is helpful to quote one of Zahar's examples in full. He says:

> As an important illustration of the correspondence principle let us examine Planck's *modification* of Newton's second law of motion. Planck used the equation:
>
> $$\vec{f} = \tfrac{d}{dt}(m\vec{v}) \qquad (2.1)$$
>
> *in order to derive*
>
> $$\vec{f} = \tfrac{d}{dt}\left(\frac{m\vec{v}}{\sqrt{1-v^2/c^2}}\right) \qquad (2.2)$$
>
> which is in general incompatible with classical dynamics. Planck proceeded as follows: taking stock of Einstein's approach, he demanded that Newton's second law
>
> $$\vec{f} - m\vec{a} = \vec{f} - \tfrac{d}{dt}(m\vec{v}) = 0 \qquad (2.3)$$
>
> be replaced by a Lorentz-covariant equation $\psi = 0$. Planck knew that the Lorentz transformation tends to the Galilean one when $v/c \rightarrow 0$. He thus required that $\psi \rightarrow (\vec{f} - m\vec{a})$ when $v/c \rightarrow 0$. It follows by continuity that $\psi_0 = (\vec{f} - m\vec{a})$, where ψ_0 denotes the value of ψ for $v = 0$. He had then to find a particular case where both some force and its transformation law are known independently of the motion caused by the force. Lorentz, Einstein and Poincaré had already determined the transformation rules for the electromagnetic field and, and long before that, Lorentz had found the force acting on an electron to be equal to $e(\vec{E} + \frac{v}{c} \times \vec{H})$; ... Planck chose a moving inertial frame in which the

> electron is instantaneously at rest; he applied the law $\vec{f}'-m\vec{a}'=0$ to the immobile electron, thus obtaining $e\vec{E}'-m\vec{a}'=0$... he finally applied an inverse Lorentz transformation which took him back to the stationary frame and yielded the equation
>
> $$e(\vec{E}+\tfrac{\vec{v}}{c}\times\vec{H})=\tfrac{d}{dt}\left(\frac{m\vec{v}}{\sqrt{1-v^2/c^2}}\right) \qquad (2.4)$$
>
> Since the left-hand side is the so-called Lorentz-force, Planck equated the force with the dynamical vector
>
> $$\tfrac{d}{dt}\left(\frac{m\vec{v}}{\sqrt{1-v^2/c^2}}\right) \qquad (2.5)$$
>
> *Thus he overthrew Newton's law of motion*
>
> $$\vec{f}=\tfrac{d}{dt}(m\vec{v}) \qquad (2.6)$$
>
> *which had hitherto been regarded as the foundation stone of the whole of physics, by a proof involving the Correspondence Principle applied to that same law* (my italics, Zahar 1983a, 247-248).

Zahar thinks of the correspondence principle as a 'rational heuristics' which guides scientific discovery. This heuristic rule can operate in a deductive way on the meta-level, given a rich enough set of other assumptions. Zahar claims that such a rule helps to present a reconstruction of a presumed logical deduction of the new theory out of the old. He assumes that Planck used equation 2.1 in order to derive equation 2.2. This goes as follows. On Zahar's account, Planck required that the new theory ψ ought to retrieve Newton's law of motion at a certain limit. Then he was able, using this heuristic rule and using other premises like the transformation rules, to derive ψ logically. So, according to Zahar the structure of Planck's derivation is: if the old theory is O, the correspondence principle will guide us to accept that any new theory N ought to yield O at certain limit. So, we try to find N by assuming O with some other scientifically plausible premises

(P), such that (O and P) $\vdash$ N. Hence Zahar's final remark "thus we overthrew Newton's law of motion ... by a proof involving the correspondence principle applied to that same law."

Because structural realists think of structural continuity as a continuity at the mathematical level, they need only to make sure that, in theory-change, the mathematical equations of the old and highly successful theory are preserved at least under limiting conditions. In their view, this preservation of the mathematical structure secures the empirical success of the old theory, since the mathematical structure represents nature. Hence, by securing mathematical or structural continuity between the old theory and the new one, the empirical success of the new theory, at least in the old domain, is supposed to be guaranteed. This kind of correspondence is known as formal correspondence. Other kinds of correspondence such as numerical correspondence and conceptual correspondence do not interest structural realism. This is simply because the conceptual continuity between the old theory and the new one is merely a bonus, and the numerical continuity is preserved via the structural continuity, or this is at least what structural realists might claim[1].

So, structural realism makes the following assumptions about the correspondence principle:

1) The correspondence principle is, as Zahar puts it:

   Let $\varphi = 0$ denote a known law which is to be modified; the correspondence principle could read as follows: 'The new law is of the form $\psi = 0$, where ψ is a function of a quantity λ such that $\psi \rightarrow \varphi$ as $\lambda \rightarrow 0$' (λ may be some known parameter like v/c) (Zahar 1983a, 249)

---

[1] It is important in this context to acknowledge that in certain cases with the development of the technical equipment and with the change of the theoretical understanding of certain observations the numerical value of the new form would not resemble the old value.

2) The correspondence principle is a formal device to secure structural continuity.
3) Structural continuity is required if the new structure is to retain the old successful and well-confirmed structure.
4) Hence, the correspondence principle works on the meta-level from the old theory to the new theory.

Let us therefore concentrate on formal correspondence, to see whether the structural realism definition and understanding of the correspondence principle fits physics.

2:3 Formal correspondence

Consider quantum mechanics and its correspondence to classical mechanics. The leading figures who introduced this notion of the relation between the emerging new theory in physics and its counterpart in the traditional perception of physics were Planck and Bohr. Planck stated in 1906 that by taking the limit of Planck's constant as it goes to zero, such a connection could be maintained. Bohr introduced another limit, that of the quantum number becoming very large to the extent that the quantum system would not anymore be counted as quantum and the classical picture would emerge. Nevertheless, in the current status of the quantum theory we would find four kinds of formal correspondence between quantum and classical mechanics.

2:3:1 The old correspondence principle

Planck formulated the correspondence principle for the first time in 1906:

$$\lim_{h\to 0}[\mathit{Quantum\ physics}] = [\mathit{Classical\ physics}]$$

He demonstrated that the radiation law for the energy density at frequency v:

$$u(\nu) = \frac{8\pi\, h\nu^3}{c^3(e^{h\nu/kT} - 1)} \qquad (2.7)$$

corresponds in the limit $h \to 0$ to the classical Rayliegh-Jeans law:

$$u(\nu) = \frac{8\pi\, kT\nu^2}{c^3} \qquad (2.8)$$

where k is Boltzmann's constant, T is the temperature and c is the speed of light. This kind of correspondence entails that the new theory should resemble the old one not just at the mathematical level but also at the conceptual level as well. Let us remember that this kind of correspondence was important for Planck to assert the relation between his 'radical' assumption of discrete energy levels that are proportional to the frequency, and the classical theory. Hence, Planck insists that the terms in the new equation refer to the very same classical properties.

The old correspondence principle secures continuity at the conceptual level as well. Hence, it is not a paradigm case for structural realism because structural realism maintains that such approximation between the new and old theory at the conceptual level can generally only be accepted "by stretching the admittedly vague and therefore elastic notion of 'approximation' beyond breaking point" (Worrall 1989, 107).

2:3:2 Configuration correspondence principle

This claims that the quantum laws correspond to the classical laws when the probability density of the quantum state coincides with the classical probability density. Take, for example, a harmonic oscillator that has a classical probability density:

$$P_C(x) = 1/\left(\pi\sqrt{x_0^2 - x^2}\right) \qquad (2.9)$$

where x is the displacement. Now if we superimpose the plot of this probability with that of the quantum probability density $|\psi_n|^2$ of the eigenstates of the system and take (quantum number) n → ∞, we will obtain figure 1 below. As Richard Liboff, a leading expert in the field, notes

> Clearly, the classical probability density P_C does not follow the quantum probability density $|\psi_n|^2$. Instead, we see that it follows the local average in the limit of large quantum numbers n:
>
> $$P_C(x) = \langle P_Q(x) \rangle = \langle |\psi_n|^2 \rangle = \frac{1}{2\varepsilon} \int_{x-\varepsilon}^{x+\varepsilon} |\psi_n(y)|^2 \, dy \tag{2.10}$$
>
> the interval ε decreases with increasing quantum number n (Liboff 1984, 52).

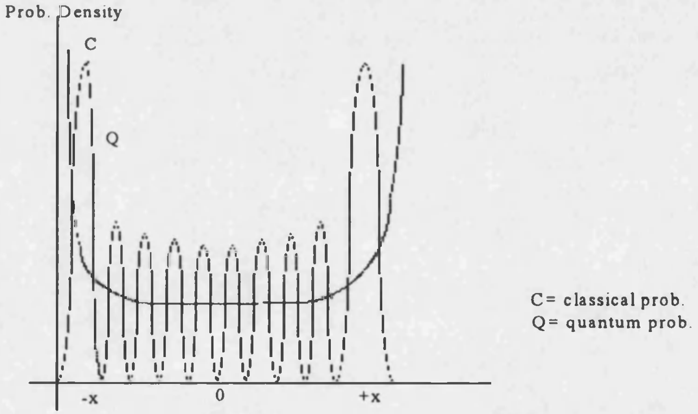

Figure 1

Structural realism argues that by the correspondence principle, the new theory ought to retain the structure of the old theory. But in the case of the configuration correspondence principle, the new equation does not retain the exact structure of the old. As it can be seen from equations 2:9 and 2:10 the average value of P in the new theory coincides with the value of P in the old theory, i.e. the structure of the old theory was not retained.

Hence, this kind of correspondence will not serve the purposes of structural realism. Let us remember that structural realism concentrates on the mathematical forms rather than on the numerical values. In this case, the average of the probability density in the limit of large quantum number coincides with the classical probability. This coincidence is merely numerical. If the old and well-supported form is not preserved via correspondence then this kind of correspondence is no good for structural realism. For that we would need to show that the limit of quantum probability density is the classical probability and not that the limit of the average of quantum probability coincides with the classical probability.

2:3:3 The frequency correspondence principle

The third type of correspondence is that known in quantum mechanics' books as the Bohr Correspondence Principle (or Frequency correspondence principle)[2]. This claims that the classical results should emerge as a limiting case of the quantum results in the limits n → ∞ (the quantum number) and h → 0 (Planck's constant). Then in the case of frequency the quantum value should be equal to the classical value, e.g. $\upsilon_Q = \upsilon_C$. In most cases in quantum mechanics we find that the quantum frequency would coalesce with the classical frequency in the limit n → ∞ and h → 0.

This kind of correspondence might also generate problems for structural realism. One obvious problem relates to the assumption that Planck's constant goes to zero. What is the meaning of saying that 'a constant goes to zero'?[3] A constant is a number which has the same value at all times and having it as zero is contradictory, unless it is zero. Zahar might reply by saying that in correspondence we ought to take the real limiting

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[2] Oliver Darrigol claims that this kind of correspondence is not what Bohr had in mind when he suggested the correspondence principle (Darrigol 1992 and 1995).

[3] Of course this point can count also against the old correspondence principle of Planck (the first in our list) because it is build on the assumption that the limit is of Planck constant going to zero.

value and not the abstract one. In the case of relativity the limit "c goes to infinity" is an abstract one and the real limit should be "v/c goes to zero". In the case of quantum mechanics to classical mechanics one might say that we ought to take the limit $n \rightarrow \infty$ as a better one than $h \rightarrow 0$. The point here is that values like c and h are constants and would not tend to go to zero or to infinity, but n and v/c are variables - $n = (0, 1, 2, 3\ldots)$ and v/c varies between 0 when $v = 0$ to 1 when $v = c$. This point will not help structural realism because it needs both limits to yield the classical equations, as I will argue below.

Another objection might be that the value of h should be neglected because even if we considered it, it would not affect the outcome. It is important to notice here that the choice of the limits is related to our previous perception of the relation between the new and the old theory. In the case of quantum and classical it tends to say that in the large quantum number the quantum system can no longer be perceived as a quantum system. The other limit is that of assuming Planck's constant to go to zero because at that limit when Planck's constant would not affect the system then the system is not quantum anymore. Nevertheless, this kind of correspondence would continue to have a problem, because in some cases the limit $n \rightarrow \infty$, as we will see below, would not resemble classical mechanics.

Structural realism would need to take that $n \rightarrow \infty$ *and* $h \rightarrow 0$ as universally equivalent, which means that the results obtained from limiting n to infinity should be the same as the results obtained from limiting Planck's constant to zero[4]. This is so because the limits are applied to the same structure and this structure would need to resemble the same classical

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[4] Let us remember here that Zahar says that 'various components of a unified system are so closely knit together that a considerable departure of any one of them from the truth would have repercussions throughout the system' (1994a, 1).

structure in order to accept that this classical structure represents true relations in nature.

But n → ∞ and h → 0 are not universally equivalent, because at least in the case of the quantum frequency (Liboff 1975 and 1984), the limit n → ∞ does not yield the classical one, while the limit h → 0 does. What occurs here is that, in certain quantum systems, when we take the limit of a system when h→0 and take the limit of the same system when n → ∞, we will find that the two results are not universally equivalent. In some cases in quantum mechanics, like a particle trapped in a cubical box, the frequency in the high quantum number domain turns out to be displaced as:

$$v_q^{n+1} = v_q^n + h / 2md$$

where m is the particle's mass and d is the length of the box. Such a spectrum does not collapse toward the classical frequency in the limit of large quantum numbers, while the spectrum of the particle does go over to the classical continuum in the limit h → 0.

In the case at hand, although the quantum frequency resembles the classical frequency in the classical limits in most of the cases, it does not in some cases because the empirical set-up of the case would mean that a correction term is needed to present the quantum frequency. This correction term contains Planck's constant, but is not related to the quantum number. Hence in such cases the two limits will not yield the same result.

In general if we apply the limit h → 0 to the equation $v_q^{n+1} = v_q^n + h / 2md$, then the correction term h/2md would vanish and the quantum frequency will yield the classical frequency. But if we apply the limit n → ∞ then the correction term h/2md would not vanish. Hence, the outcome structure of applying the two limits is not the same structure.

The mere fact that the frequency correspondence fails to account for some cases between quantum mechanics and classical mechanics would rule it out of the structural realism account of correspondence. Structural realism asserts that the role of the correspondence principle is to show the connection between the new structure and the old structure of the two successful theories in their domains of application. The fact that the two limits, $n \rightarrow \infty$ and $h \rightarrow 0$ turn out not to be universally equivalent means that the new structure would not retain the old structure in both limits. Therefore, it fails to correspond two "real" structures to each other. Hence, one of the two highly successful structures is bound to be not even an approximately true representation of nature. This would not be in agreement with what structural realism wants of the correspondence principle. That is because: 1) the new structure should yield the old one at the limit(s), and 2) the structure expresses real relations between objects, therefore 3) if the two limits do not give the same structure, then there exists more than one real relation between the same objects $\Rightarrow$ (contradiction).

So, if we want to think about frequency correspondence from a structural realism point-of-view, it runs into two sorts of problems. First the acceptance of a constant to vanish, and second the disagreement between limiting the same structure to the old one by using two different accepted limits, i.e. $n \rightarrow \infty$ and $h \rightarrow 0$ are not universally equivalent. Hence it would not support a realist interpretation of the structure as a representation of the underlying blue print of nature.

2:3:4 Form correspondence principle

The last type of correspondence is the Form Correspondence Principle, which claims that we can obtain correspondence if the functional (mathematical) form of the new theory is the same as the old theory. This kind of correspondence is especially fruitful in particular cases where other kinds of correspondence do not apply. Let us take the example used in frequency correspondence (quantum frequency). As we saw, in the case of the particle in a cubical box the outcome of n → ∞ does not coincide with the outcome of h → 0. Hence the two limits fail to achieve the same result. In cases like this form correspondence might overcome the difficulties facing frequency correspondence. The aim of form correspondence is to prove that the classical frequency and the quantum frequency have the same form. So,

> if υ_Q denotes quantum frequency, υ_C classical frequency, and E energy, then form correspondence is satisfied if $\upsilon_C(E)$ has the same functional form as $\upsilon_Q(E)$ (Liboff 1984, p 52).

In the special case of the particles in a box, Liboff (1975) shows, by using a dipole approximation, that the quantum transition between state s + n and state s where s >> n, gives the relation:

$$\upsilon_Q^n(E) \approx n(E_s / 2ma^2)^{1/2}. \qquad (2.11)$$

He also noticed that if we treat the same system classically (particles of energy E in a cubical box), the calculation of the radiated power in the n-th vibrational mode is given by the expression:

$$\upsilon_C^n(E) = n(E / 2ma^2)^{1/2}. \qquad (2.12)$$

Both frequencies have the same form, even if one is characterising quantum frequency while the other the classical, and even if their experimental treatment differs. Hence, the Form Correspondence Principle is satisfied.

Despite the fact that this seems to be a strong case for structural realism because of similar equations, there are deep problems even here. An important objection that would affect the idea of correspondence as represented by structural realism is that in the classical case E denotes the average energy value of an ensemble of nth harmonic frequency, while in the quantum case it denotes the eigenenergy of that level. Also, in the quantum case the energy is discrete, and the only way to assert that the quantum frequency yields the classical one is by saying that when the quantum number is very big the number of points which coincide with the classical frequency will increase, using the dipole approximation, which asserts that the distance between the points in the quantum case is assumed small. Hence the quantum case does not resemble the classical case as such but it coincides with the average of an ensemble of classical cases.

Let us look next at another interesting case of form correspondence which complies with the structural realism criteria (that the domain of the new theory is an extended domain of the old one), but which seems to be in conflict with the structural realism understanding of the correspondence principle. This case is from corresponding quantum chaos to classical chaos. The argument runs as follows. Classical chaos exists. If quantum mechanics is to be counted as a complete theory in describing nature, then it ought to have a notion which corresponds to classical chaos. That notion can be called quantum chaos. What theoretical physicists did at this stage was to think about ways that resemble a chaotic behaviour in quantum systems. They came to accept that certain systems behave chaotically. Now, it turns out that, according to our knowledge of classical chaos and of the new quantum formalism of the so called quantum chaotic systems, a direct correspondence between the notion of chaos in quantum mechanics and that of classical mechanics does not exist.

The way out was to accept a kind of form correspondence. How? Because it was impossible to find a parameter in the quantum chaotic system such that when limiting it to zero (or to infinity), the results resemble the form of classical chaos, the physicists tried to find a first approximation type of correspondence. They tried to limit both systems to a third and mutual form. The classical chaos goes in a certain limit to the form ϕ, and quantum chaos goes also to the same form at the same limit:

$$\lim_{n\to\infty} \textit{classical chaos} = \phi$$

$$\lim_{n\to\infty} \textit{quantum chaos} = \phi$$

Hence because both chaotic systems, the classical and the quantum, tend to the same form at the same limit then they correspond: a certain kind of form correspondence is satisfied. This is a form correspondence between both classical and quantum to a third form, but because we only have classical and quantum theories, the physicists involved argue, then the correspondence is from one to the other[5].

Taking these points into consideration, this kind of correspondence does not support the structural realism correspondence either. Because:

1) The correspondence is between a form in quantum mechanics with a classical form which expresses an ensemble of the nth harmonic frequency as a function of average system energy;
2) In the case of quantum chaos the form correspondence is not between an old theory and the new one but between both the old and the new with a third equation.

Structural realists might say that the third equation is part of a future theory that will accommodate both quantum chaos and classical chaos as limited cases. But such a claim is speculative and would not affect my type of argumentation because I accept only what is accepted already in physics.

Having criticised the use of form correspondence by structural realism, I need to stress that form correspondence can be a very useful heuristic device in constructing new models. What I have in mind is cases where the mathematical form of a certain phenomenon gets transformed into a phenomenon of a different domain. I will try to examine in some detail a

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[5] For details about the correspondence between the quantum chaos and classical chaos see: Belot and Earman, forthcoming.

case in superconductivity and show how a form correspondence principle applies to this case.

2:4 Superconductivity and the form correspondence

The main thrust of form correspondence, in the case of superconductivity, is the claim that it is possible to put a quantum formula into classical equations if we can change the quantum formula in the limit into a form where it looks similar to a classical form[6]. I will here discuss the key link between quantum and classical descriptions in superconductivity: Josephson junctions, which are an important factor in building SQUIDs (superconducting quantum interference devices).

First, a necessary requirement for a system in superconductivity to count as a quantum mechanical system on a macroscopic level is that the temperature T should be low enough to satisfy the inequality

$$k_B T \ll \hbar \omega_0 \qquad \text{(A)},$$

where k_B is Boltzmann's constant and ω_0 is the classical frequency of the system. If (A) is not satisfied the thermal disorder would blur the quantum mechanical effect. Such systems occur in superconductivity where the superconductor is placed in a very low temperature[7]. Then the magnetic flux of a LC-circuit satisfies the Schrödinger equation; i.e. the magnetic flux is quantized. But even then it would be impossible to detect that the magnetic flux satisfies the Schrödinger equation. The reason lies in the form correspondence principle which

> guarantees that whenever the characteristic 'scale' V_0 of the potential energy is large compared to the spacing of energy levels,

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[6] See Leggett 1980, 1984 and 1985.

[7] I will discuss in details the conditions under which the superconductivity phenomenon occurs in chapter three.

> which in turn is approximately h times the characteristic frequency of classical motion ω_o, then the predictions of quantum mechanics for quantities such as the mean position reduce to those of classical mechanics. (Leggett 1986, 592)

So, in order to detect a macroscopic quantum behaviour we need to go beyond the correspondence limit[8]. The key to the problem is to look at how superconductivity treats the Josephson junction.

A Josephson junction consists of two superconductors that are separated by a thin film insulator, as in figure 2a. (The weak link in a SQUID ring (as in figure 2b) can also be treated as a Josephson junction in a similar way.) Because the insulator is a thin film, we can expect that the superconducting current, which can be thought of as a fluid of pairs of electrons, can tunnel through it. According to Feynman's analysis of the junction, if Ψ_1 is the amplitude of finding an electron on one side, and Ψ_2 the amplitude of finding it on the other, the two amplitudes would be related as follows:

$$\begin{aligned} i\hbar\frac{\partial\psi_1}{\partial t} &= \hbar T\psi_2 \\ i\hbar\frac{\partial\psi_2}{\partial t} &= \hbar T\psi_1 . \end{aligned} \qquad (2.13)$$

where $\hbar T$ represents the effect of the electron-pair transfer interaction across the insulator. T is a measure of how many electrons might penetrate the insulator. If the insulator is thick, T will be equal to zero.

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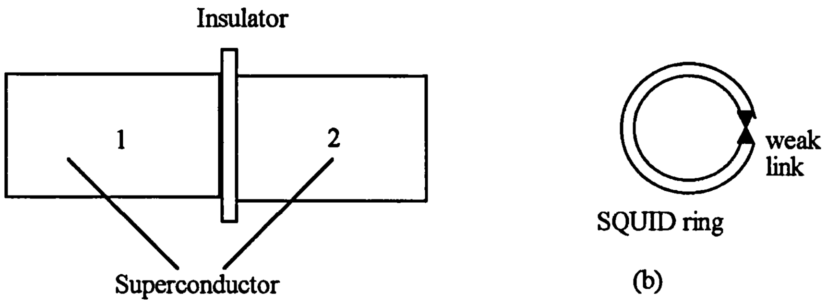
[8] Leggett claims that the situation in SQUIDs is a place where the quantum formalism is applied on a macroscopic level. He claims that in the case of macroscopic quantum tunnelling the experimental set-up can provide a way of testing quantum interference without being affected by the correspondence limit. He goes on to saying that the only way to do so is by looking into SQUIDs where he claims that the quantum mechanical effect can be detected. As I will show below this claim is questionable. Moreover, the new experiments in SQUIDs show that the superposition of the two currents manifests itself as a classical current outside the ring and we detect only one of the two currents at any time inside the ring (Cartwright and Shomar, forthcoming).



(a) (b)

Fig. 2.2

By assuming that $\psi_1 = n_1^{1/2} e^{i\theta_1}$ and $\psi_2 = n_2^{1/2} e^{i\theta_2}$, where n_1 and n_2 are the density of electrons on the two sides of the junction, we can prove that the flow of the current probability density from 1 to 2 will depend only on the phase difference $\delta = \theta_2 - \theta_1$:

$$\mathbf{J} = J_0 \sin\delta = J_0 \sin(\theta_2 - \theta_1) \qquad (2.14)$$

where J_0 is proportional to T. The important point I want to make here has to do with form correspondence. Equation 2.14 can be accepted as corresponding in form to a classical equation, $I = I_0 \sin\delta$, which is how current is treated in a classical circuit. Once we have done so, we no longer treat the Josephson junction as a quantum device, but as a part of a classical circuit. The Josephson junction (or alternatively the SQUID ring) can now be presented as a circuit with an equivalent classical electrical circuit (figure 2.3).

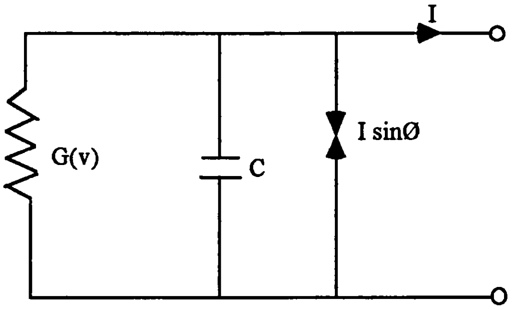


Fig. 2.3

Another form correspondence would be applied at this stage. In a classical circuit, as that of figure 3, when an external current (or external magnetic flux) is applied through the circuit, the total current 'I' is equal to the sum of the currents in the three branches:

$$I = I_c \sin\delta + \frac{V}{R} + C\frac{dV}{dt} \qquad (2.15)$$

where R is the resistance, C is the capacitance, and V is the voltage.

Now, as Josephson proved, the relation between the phase difference and the voltage is given by $\frac{\partial\delta}{\partial t} = \frac{2e}{\hbar}V$, i.e. the voltage $V = \frac{\hbar}{2e}\frac{\partial\delta}{\partial t}$. Then by asserting that the Josephson junction would behave as a classical circuit (using the form correspondence), then the total current would be:

$$I = I_c \sin\delta + \frac{\hbar}{2\mathrm{Re}}\frac{d\delta}{dt} + \frac{\hbar C}{2e}\frac{d^2\delta}{dt^2} \qquad (2.16)$$

At this stage we have an equation that relates the current with the phase difference only without any direct reference to the voltage[9]. The analysis continues to use another analogy that employs form correspondence. That is of comparing this form (2.16) with a form in mechanics. This would help us to understand more about the mechanism of the junction. Equation 2.16 is analogous to an equation of a pendulum in a situation as that in figure 4. The total torque τ on pendulum would be:

$$\tau = \tau_0 \sin\theta + D\frac{d\theta}{dt} + M\frac{d^2\theta}{dt^2} \qquad (2.17)$$

where M is the moment of inertia, D is the viscous damping and τ is the applied torque.

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[9] As Anderson emphasises, the phase difference in this case has become a classical variable, and in any future experiment is interpreted as a classical one (Anderson 1987).

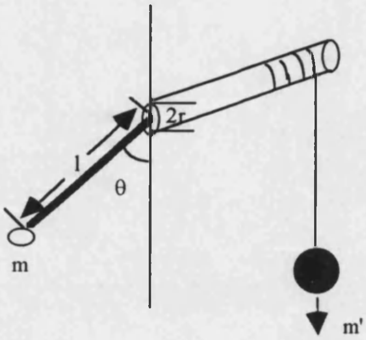

Fig 1.4

Both these equations have the general mathematical form:

$$Y = Y_0 \sin x + B\tfrac{dx}{dt} + A\tfrac{d^2x}{dt^2} \qquad (2.18)$$

Now, from this accumulation of form correspondences it was possible to draw a quasi-classical picture of a quantum system (Josephson junction). Also we could anticipate a new limit in Josephson junctions, namely the critical current. The idea goes as follows. In the pendulum case there is a critical value that the applied torque has, after which the pendulum collapses. Similarly, in the Josephson junction case, if the current is higher than the value of the critical current, the system will not exhibit any tunnelling effect.

These two simple examples are among many similar treatments where form correspondence is applied in superconductivity and in SQUIDs[10]. One might say that such a treatment is no more than a way to treat a phenomenon with any tools that can give us an accurate phenomenological model to represent it, whether it is classical or quantum.

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[10] As we can see here Leggett's criterion for when we might be able to detect a macroscopic quantum effect would be questionable. This is because in the case of magnetic flux the equation of total flux would have a similar form, as that of the total current and the same analogy with the classical pendulum will be valid. Hence, the more complex representation of the weak link that is presented by the Leggett programme would not have the same weight that Leggett tried to attribute to. The quasi-classical representation would have the same form as that of the classical formalism. Thus why would we accept Leggett's suggestion that such a system is a quantum one on a macroscopic level that does not have a classical treatment?

Of course, this kind of correspondence is different from the concept of correspondence employed in structural realism for several reasons. First, in the case of Josephson junctions, form correspondence allows a density current to be mapped into a current equation, and hence the quantum mechanical density current is substituted by a classical current. Second, in the case of corresponding the current equation with the torque equation, form correspondence does not relate a new theory with an old theory where both theories have the same domain of application (with perhaps an extended domain of the new theory). Rather the correspondence is between the form of a new theory with a well-known form from an old theory whose domain has no relation with the phenomena under study. So in effect the form correspondence depends deeply on the idea of analogy between various parts of physics. This analogy is related solely to the type of behaviour the system under study might exhibit in fact.

Let me clarify this point. As Zahar declares, the new theory N had to explain the empirical success of the old theory O. Now assume O is successful in a domain D, and breaks down outside D. N is successful in the extended domain D + D*. N ought to resemble O when limiting a certain parameter in N to zero. Here both N and O are related to the same domain D and its extension D*. In the cases of form correspondence I have just discussed, the form F1 is a successful form in a model related to a domain D1; the form F2 is related to the phenomenon understudy in a different domain D2. There is no intersection between D1 and D2; i.e. D2 is not an extended domain of D1. For structural realism, although the terms in N might be interpreted differently than their interpretation in O, N and O are still related to the same empirical successes. F1 and F2, however, would have totally different empirical successes. Hence, it would no longer be considered as representing the underlying structure of nature. Therefore, it would not agree with the structural realist claims.

Form correspondence seems a suitable candidate for the kind of correspondence advocated by structural realism, since its main concern is with the form regardless of the content. Structural realism might accept a weaker version of the correspondence principle, such as form correspondence, since it still depends on the mathematical structure. But I do not think that they will accept that this kind of analogy can exemplify the important kind of correspondence that, in their view, supports realism. This is because the two parts of physics that the same form operates in are remotely related. We are not corresponding, let us say, quantum mechanics to classical mechanics, but in some cases we are relating current with torque. Now if they restricted the use of form correspondence to the related domains of applicability, then they would stand a better chance in fitting their correspondence with the cases in physics.

2:5 Conclusions

Structural realism presented its version of the correspondence principle having in mind the aim of presenting an optimistic interpretation of scientific revolutions, that is to present a heuristic device which can secure mathematical continuity between the well supported structural forms of the old theories in the new ones. Now in the cases we have seen from the types of formal correspondence applied in physics, we see that there is a factor in each of them which is similar to factors in the structural realist definition of correspondence. Nonetheless, in all of the kinds of correspondence employed in physics we would find elements that would not support the kind of realism advocated by structural realism.

Now what we have here is a correspondence principle which is built precisely to match a certain type of understanding of realism. This sort of correspondence might succeed in explaining certain types of structural continuity in a way that would support the type of realism advocated by structural realism. But, as we saw, it would not match most of the types of correspondence used in physics. Furthermore, if we wanted to restrict ourselves to the cases where a structural realist correspondence might be applicable to accept the forms as representative of nature, we would find that we have very few cases.

Scientific practice might need different kinds of correspondence to achieve new relations and to relate certain domains of applicability to other domains. But is it a necessary claim for all kinds of realism to account for the developments in science? I do not think so. For I take theories as merely tools to construct phenomenological models that are capable of representing nature. In that case whether these theories correspond to each other in some limit or not will be irrelevant. Correspondence of theories concerns realists who think that fundamental theories represent nature and approximate its blueprint.

Chapter Three

Theoretical Dichotomy

3:1 Introduction

In this chapter I introduce a new kind of dichotomy at the theoretical level. The motivation for such a dichotomy is to present a defensible position in scientific realism that can overcome the pessimistic meta-induction. This dichotomy is a horizontal one that divides low-level from high-level theoretical representations. It depends on the process by which different kinds of models are constructed. This process is related to the physicists' practices.

Fundamental theories, like Maxwell's theory, cannot usually be directly 'applied' to a certain phenomenon. Generally, a theoretical model is constructed out of the first principles of the theory. In Maxwell's theory the four basic equations (first principles) are:

$$\begin{aligned} &1)\quad div\,\mathbf{D} = \rho_f \\ &2)\quad div\,\mathbf{B} = 0 \\ &3)\quad curl\,\mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} \\ &4)\quad curl\,\mathbf{H} = \mathbf{j}_f + \frac{\partial \mathbf{D}}{\partial t} \end{aligned} \qquad 3{:}1$$

where D is the electric displacement vector of a field, B is the magnetic field, E is the electric field, H is the magnetic intensity vector, ρ is the charge density and j is the current density. Then there will be a process of 'concretization' in order for such a model to apply to a set of boundary conditions.

Take a typical textbook case, as presented in the book by I. S. Grant and W. R. Phillips. They do not take Maxwell's equations from the beginning and try to apply them to the various problems. This is nearly the last thing they do in the book (it is in chapter 10). This would not show anything if after learning these equations the student knew how to solve the problems by directly applying them. But there is a lot of fiddling to do in order to find the solutions to the equations which “fit the boundary conditions and distributions of charge and current appropriate to different problems.” (Grant and Phillips 1982, 344) At that point the authors remind us that

> Often one’s physical insight enables approximate solutions to be found which illustrate the most important features of a particular situation. (Ibid., 344)

This is exactly what one needs to know how to do: one needs to develop ‘physical insight’.

Earlier in the book, when the electric displacement vector was first introduced, it was made clear that it was not a physical entity. The authors say:

> unlike the electric field E (which is the force acting on unit charge) or the polarisation P (the dipole moment per unit volume), the electric displacement D has no clear physical meaning. The only reason for introducing it is that it enables one to calculate fields in the presence of dielectrics without first having to know the distribution of polarisation charges (Ibid., p 73)

This clear separation between the parts of a theoretical representation which have physical meaning from those who are merely tools for calculations is not a feature unique to these authors. This is a trend we are constantly being reminded of throughout typical physics courses.

Maxwell’s equations are the basis of his theory of electromagnetism. This theory does not apply directly. It needs models to relate it with the particular cases under study. Even when these models are produced at a derivational level as solutions to Maxwell’s equations, they usually do not fit real physical problems except, sometimes, as an approximation. Additional factors will be employed to construct a proper model for the particular physical situation. The first set of solutions that is derived directly from Maxwell’s equations under certain hypothetical boundary conditions is what might be called 'theoretical models'. While the other models produced with the additional factors related to the physical situation might be called 'phenomenological models'.

Models, in my opinion, act as mediators between the high theories and: either hypothetical situations or real concrete situations[1]. Models in physics, as I use the term, all (theoretical or phenomenological) have three parts:

1) They have a set of mathematical equations.
2) They describe the environmental set-up and/or the boundary conditions in which the phenomenon under study can exhibit itself. In doing so there will be a referential assertion between the data acquired (or expected -- if any), and some of the mathematical symbols. This will depend on an implicit or explicit reference to a body of antecedent knowledge that provides meaning for these symbols independent of any further meaning they acquire by relation they will be given to features of the situation being modelled.
3) They present a story.

The third point needs further attention. Usually, a story:

a) presents a coherent account of why a certain phenomenon behaves in such and such a way. In doing so, the story might either adhere to the need of theory coherency (in the case of theoretical models), or it might adhere to the physical coherency in relation to the phenomenal facts (in the case of phenomenological models).
b) needs, in order to present such a coherent account, to relate the mathematical symbols with the real features of nature (or the hypothetical situation). This might be done also in two ways either

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[1] In this thesis I accept models as mediators between theory and experiments in a special way as dialectical mediators See Gramsci 1980 and Bachelard 1984a. The development of this idea I follow is not Margaret Morrison's idea of mediator models (1990a, 1990b and forthcoming), although I agree with her, but of Gaston Bachelard's idea.

to stick to already agreed terminology in relation to a certain theoretical framework (theoretical models), or to associate the symbols with certain properties exhibited by the phenomenon without strictly accepting the existing terminology (phenomenological models).

c) gives an account of how the model can represent the known properties of the phenomenon under study.

It should be noted that all models I consider in this thesis are theoretical in character. The kinds of models we are considering all use concepts whose criteria of application are not exhausted by measurement procedures but include constraints taken from a body of formerly presupposed theories. Consider acceleration. We can measure the time, distance and the change in distance with respect to the change in time. These are our data. At that level alone there is very little theorisation. But as soon as we introduce even the concept of acceleration or speed or force, there will be.

Usually, fundamental theories are used in constructing models, either by derivation (theoretical models) or merely as tools[2] (phenomenological models). Although I insist that the models which can be constructed by applying the bottom-up approach -- even in the cases where only one fundamental theory is used as a tool in such construction and even if the fundamental theory used in that construction is itself the theory which could produce the same mathematical equations of the model by derivability -- are phenomenological models, they are not the only kind of phenomenological models.

In a lot of situations in physics we do not have a fundamental theory that provides a set of theoretical models. Rather we are left with a set of experimental observations and phenomenal observations of which we have

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[2] See Cartwright, Shomar and Suárez 1995, also see forward chapter four for the use of fundamental theories as tools in constructing a phenomenological model.

little understanding. In these cases we also try to build models that represent the studied phenomenon. In most cases, the model needs to employ more than one fundamental theory. I shall call these models, too, 'phenomenological'. Better: most of what intuitively seem to be phenomenological models in physics are built in this way. Let us now look into the specifications of each type of theoretical forms.

3:2 Two types of theoretical forms

As I said, in general two distinct types of theoretical forms can be found in physics. The main difference between them is the process by which they are built. One starts from a theoretical framework to accommodate experimental observation and the other goes from observation to theoretical representation. These two types can be labelled as theoretical models and phenomenological models.

In spite of the general properties (mentioned above) of both theoretical and phenomenological models, each type has distinct properties of its own related to these general ones. I will characterise the distinction between these two types of theoretical forms, but this distinction need not be a clear 100% one. It is not so important to come up with a precise definition rather than a rough characterisation of the difference, since my thesis is not that models fall into two natural kinds - theoretical and phenomenological - and only the second kind can represent accurately or will survive scientific revolutions. Rather I claim that the ones that are constructed in a bottom-up approach are more likely to. So it is not a matter of deciding beforehand that these characteristics sum up all those of all models. Let us now see the difference between them in accordance with the general definition of models.

3:2:1 The mathematical element

In physics all models have a set of mathematical equations. In the case of theoretical models, the mathematical equations need to be derived from the mathematical bases of the fundamental theories: they try to solve the mathematical equations of the fundamental theories in relation to a set of hypothetical boundary conditions. By contrast in the case of phenomenological models they stem from the phenomenological level and use fundamental theories as tools in constructing their mathematical equations.

I differentiate between the mathematical basis of a 'fundamental theory'[3] (like Maxwell's equations) and the equations of the theoretical models produced by derivations from this mathematical basis which accounts for different situations.

Let me illustrate my point by means of a rather simple example from electromagnetism. The displacement current equation (1 in 3:1) is the sum of the electric field **E** and the polarisation **P**:

$$div\ \mathbf{D} = \varepsilon_0 div\ \mathbf{E} + div\ \mathbf{P} = \rho_f \qquad (3.2)$$

(or, $\mathbf{D} = \varepsilon_0 \mathbf{E} + \mathbf{P}$). This is not a realistic description, simply because by definition the displacement current is not as real as the two parameters **E** and **P**. One might say that if I accept that both **E** and **P** are real then a logical consequence of such assertion is to accept that **D** is real. I do not see this as a straightforward conclusion. In practice we use this law because it is difficult to find the distribution of the polarisation charges, so it is easier to assume the displacement current, which we find by using the integral form of equation (3.2):

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[3] It is important to say that the expression 'fundamental theories' is widely used to express a cluster of theories that are highly abstract and give mathematical generalisations that might be used in different concrete situations.

$$\int_S D.dS = \int_V \rho_f d\tau \qquad (3.3)$$

In effect, we are assuming that the flux of D that is going out of an imaginary surface (just as in Gauss' law)[4] is equal to the total free charge enclosed in that surface. So, in concrete physical situations we know that only E and P are occurring but we assume D for simplifying the calculations. Hence, D is a mathematical tool to arrive at simple solutions. In effect physicists know, when they apply the law of the displacement current, that such a current is a mere assumption to help them to find approximately the real values which are difficult to measure. This statement is also true of Gauss' law. Models related to the displacement current and Gauss' surface are, despite their importance in calculating the expected value of the real parameters, theoretical models that express elements which do not have any counterpart in reality.

If we go a step down in this hierarchy we find two parameters E and P. Take the electrical field. A general model can represent it[5]:

$$\mathrm{E}(r) = \frac{1}{4\pi\varepsilon_0} \int_{all\ space} \frac{(r-r')\rho(r')d\tau'}{|r-r'|^3} + \frac{1}{4\pi\varepsilon_0} \int_{all\ surfaces} \frac{(r-r')\sigma(r')ds'}{|r-r'|^3} \qquad (3.4)$$

where r is the distance from the charged surface, r' is the distance of the charge from the centre of the axis, ρ is the volume charge density and σ is the surface charge density.

This model is also a description for the electrical field and can be applied in any hypothetical electrical field situation whether it is an electrical field surrounding a charged particle or a charged surface or a charged volume. In each practical phenomenon the theoretical model will be reduced to a specific model which deals with the boundary conditions of the phenomenon. But in most cases the surface would not be smooth and easy to

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[4] Some texts accept this form as the generalised Gauss' law.

[5] It is important to clarify what I mean by a model. It is the equation with the relevant description and story, in accordance with my definition above. Nevertheless, and for simplicity only, I use here the model's equations as a pointer to that model.

deal with in terms of finding the centre of the axis and the type of charge distribution over the surface or the volume. A further assumption is employed. This is: any surface can be contained in another symmetrical shape that will contain all the charge distribution on the surface (Gauss' Law). So in the case of an ionised sodium atom with a charge +e, we assume a spherical surface of radius R centred on the ion and large enough to contain the atom. Then the electrical field on the surface of the sphere would be:

$$E(r) = \frac{e}{4\pi\varepsilon_0 R^2} \qquad (3.5)$$

Although this equation is similar to the E(r) general equation, it is not derived from it but through introducing Gauss' law.

We can think of this example in two ways. First that we have a fundamental theory that can give us all the models that are needed in the practical situations related to the theory. In this case the 'idealised' description given by the theory, equations 3.2 and 3.4, is de-idealised and a model of the specific situation is produced, equation 3.5. So in this case the way we solve the problem is by going from a theoretical description to a real situation; i.e. on a top-down approach. Alternatively, we may start from a real situation in which we have an ionised sodium atom and we want to find the electrical field for it. We know the general description of an electrical field but we do not know the shape of the atom. If we do not want to assume that it is a point particle then we need another assumption to find the electrical field. This is found in Gauss' law. Then a model will be constructed. This is a bottom-up approach.

It is important to say that even if the phenomenological model was constructed by using a theoretical tool with an assumption which is known not to be strictly true (Gauss' law) the output model does not have any symbols that do not refer to the real situation (see equation 3.5). Basically the model tells us that all equation (3.5)' symbols refer: the atom has a charge +e, this atom can be contained in a spherical space of radius R and E is the electric field within that space. In this particular case, it happened that by using the top-down approach or the bottom-up approach we end up with the same mathematical equation (3.5), because we are dealing with a simple situation taken in separation of any outside effect.

Such approaches are choices adopted by physicists, although, historically, most of the time, the theoretical representation follows the empirical findings. Even in the case at hand the electrical field equation was developed and adopted on empirical grounds long before Maxwell suggested his theory. Hence, it was adopted by using a bottom-up approach. It would be safe to say for these kinds of models, even if they were later incorporated in a theory, if the theory turn out to be wrong at any point in the future, they will probably continue to hold.

The important point I want to highlight here is that even when a model is incorporated into a fundamental theory or its mathematical equations are derivable from a fundamental theory, it is still the case that there are two ways of thinking of such a model. One is asserting that it is a model derivable from the fundamental theory; i.e. from a top-down approach; while the other is starting from experimental and phenomenal assertions to arrive, with the help of the fundamental theory, to the model; i.e. from a bottom-up approach.

Although these two types of theoretical forms can overlap and produce the same mathematical part of the model[6], nevertheless that does not mean that the theoretical model is the same as the phenomenological model. Two questions can be raised here: why is the way the model is arrived at relevant to the appraisal of the model? And, is the problem a matter of complexity? It is an important element in any model to give not only the mathematical equation, but also a story that associates the

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[6] I must say here that some times the fundamental theory will incorporate the phenomenological findings into its structure in a way that leads automatically to such overlap.

mathematical symbols with the real physical situation. This story, as I will argue below, is different if the standpoint of the model is theoretical than if it is phenomenological. In the theoretical model the story might give a set of assumptions in order to fit the mathematical coherency of the theory, while in the case of phenomenological models physical coherency takes priority. The more complex the model, the easier it is to see the theoretical standpoint of the theoretical model in contrast with the phenomenal standpoint of the phenomenological model. Because of these different standpoints, the process by which the model is arrived at is important. It is the standpoint of the model that makes it a theoretical or a phenomenological model.

As I said earlier, the bulk of phenomenological models exist in fields that do not have a 'fundamental theory'. A simple description of what physicists do will illustrate the building of such phenomenological models. If a new phenomenon is discovered (or built), physicists will try to understand it. A group of usual procedures is applied. The first of these procedures is to know the conditions in which the phenomenon occurs. Then, the use of different experimental activities will provide a set of data. At this point our knowledge of the great variety of theories available in physics will be important. Physicists will search within all possible theoretical schemes to find a mathematical structure that might represent the data. They do this by, so to speak, "plotting the data"; they then begin thinking about which of the known mathematical forms used in physics can account for something similar to the plotted data.

These mathematical forms need not be forms related to the field of study of the phenomena. Now, it would not be enough to find a mathematical form that expresses the data pattern formally. The physicists' "physical intuition" would lead them to identify the mathematical form's symbols with the properties of the phenomena. Hence, the model would need a descriptive body that would identify the symbols in the mathematical form with properties related to the phenomena.

However, 'physical intuition' does not mean relating the data to an existing theory already agreed to cover the phenomenon, but possibly changing a tool from an understood theory, possibly in another domain, to adopt it to what is studied. Hence, 'physical intuition' plays a role in presenting a story that can relate the data and the ontological world of the phenomenon with some fairly well understood mathematical structure.

Now, the physicists would have a mathematical form and a set of possible identifications between the symbols and properties. However, there is no formal justification for this identification. They would start to relate the different information they have about the phenomenon and to see if they could present a consistent story about it. If the outcome story succeeds in relating the properties to the symbols, then the mathematical form together with the description of the experimental set-up and the story will constitute a phenomenological model.

It is important to say that the mathematical structure, which might be used to summarise the data, will not provide enough information to constitute a model. I do not think, for example, that a phenomenological model is merely a mathematical structure. At the same time, it isn't naked data. There is more to it. The story, which provides the basis for how to deal with the mathematics in relation with the phenomenon, is as crucial as finding a mathematical structure.

A phenomenological model is a type of theoretical representation which stems from the phenomenological level, gives a description of the environmental set-up of a phenomenon and which aims to give a mathematical structure of the relations related to it with a story that presents a coherent account of the relation between such mathematical structure and the phenomenon under study. However, the theoretical account provided by a phenomenological model is not as abstract as the one provided by a theoretical model.

3:2:2 The environmental/boundary conditions

The second element in a model, as I said in the definition, is that every model has to describe the environmental set-up and/or the boundary conditions under which the phenomenon will exhibit itself. The main difference at this level is that the theoretical models represent the idealised description of the phenomenon in so far as that is related to the set of boundary conditions and mathematical bases of the fundamental theory. While the phenomenological models describe the environmental set-up of the phenomena, including the factors that would allow them to exhibit themselves.

In the simple example I gave in the last sub-section, there is very little difference between the two models. Both models accept the same associate meaning of E or R or ε_0 (in equation 3.5), both accept that in the cases where the surface is not very well defined they need a further assumption of a surface which might contain all the charge on that surface.

But in more complex cases, like the case of superconductivity, the theoretical model gives general boundary conditions such as that the superconductor ought to be under a certain transition temperature and ought to consist of this and that kind of alloys or substance. Nevertheless, it would not be able to specify the exact transition temperature of a certain superconductor or the exact chemical combination that allows a certain substance to be a superconductor. By contrast, the phenomenological model will depend on the experimental results and the 'rules of thumb[7]' to specify these quantities and it will plug them into the model, and it defines them in accordance with such information.

One might say that the theoretical model might state that certain elements in the model are experimentally given, but then the theoretical model cannot be accepted as 'explaining' these certain elements. In the case

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[7] See chapter five section one for details.

of superconductivity, the BCS authors claim that their model explains the transition temperature and predicts that the highest possible transition temperature would be in the range of 30° K. This was verified as a false prediction. The Landau and Ginzburg model presents a different approach, it accepts that some of the experimentally verified information need not be explained, as long as the model can represent an accurate account of the phenomenon.

The phenomenological model gives a detailed description of the environmental set-up. But what does this means? A lot of phenomena in nature do not exhibit themselves without human intervention. There are two types of natural phenomena: one is related to the objective world out there without our intervention such as thunder and lightning, earthquakes and so on; the second is phenomeno-technology[8]. The idea here is: objects in nature have the ability to exhibit certain phenomena, our rational activity can provide the conditions that allow these phenomena to occur. The important difference between these two kinds is that the first exist in nature without any special environmental set-up while the latter exist in nature by virtue of human interventions which manipulate a special environment which allow the phenomenon to exhibit itself. In the case of superconductivity, for example, the phenomenon of superconductivity itself does not occur unless we put the superconducting material into a special environment where the temperature can be reduced to the limit of the transition temperature. This is done by putting the superconductor in liquid Helium or liquid Nitrogen.

What I want to highlight here is that nature by itself cannot produce half of the phenomena studied by physics. A great many are the products of our realisation of our experience, i.e. there is nature by itself and *humanised nature*.

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[8] Bachelard 1984, p 19.

3:2:3 The story

The third element in any model is the story. In the case of theoretical models the story relates the mathematical symbols to the properties of the phenomenon. Now, this story should present a coherent account of why the phenomena behave the way they do. This coherent account is related to the fundamental theory which the model was based on. The story must be able, at least, to provide a physical basis for the theoretical model that can provide, presumably, good representations of the properties in the field of application.

Next chapter I illustrate the case of a theoretical model by looking at the model of the electromagnetic properties of superconductivity, which is a part of what is known as the 'BCS theory' (theoretical model, here forward BCS) of superconductivity. The BCS is a theoretical model derived from the quantum theory. It provides a theoretical basis that relates the phenomenon of superconductivity with quantum mechanics. The BCS consists of other theoretical models that are related to the properties of superconductivity. Some of these models are highly abstract and are driven by the need to correlate the model with quantum mechanics. Examples include the theoretical model of Cooper pairs, which is essential in relating the BCS with quantum mechanics, the theoretical model of the electromagnetic properties of superconductors, and the theoretical model of the energy gap. In the case of the electromagnetic model the story legitimatises using this or that equation from quantum mechanics or the importance of accepting this or that approximation which would allow the final result to represent the properties of the phenomenon.

The 'BCS theory aims' to present, as the author tell us, a microscopic theory of superconductivity that 'substitutes' for the different 'phenomenological theories' suggested up to that point. Its standpoint is that of quantum mechanics. This dependence on a microscopic theory and the success in deriving a mathematical structure which will yield approximate results to those obtained by empirical findings led the authors to claim that they had succeeded in deriving a microscopic (fundamental) theory (BCS, 1957).

The BCS story goes as follows. First it assumes a theoretical constraint on the way it ought to describe the electrons in superconductors: it assumes that 'in first approximation' each electron moves independently in 'some sort of self-consistent field'. By such an assumption they develop the Sommerfeld-Bloch individual particle model -- which is a successful model in describing normal metals -- to present a wave function 'of the metal as a whole'. Hence, the normal state is described by the Sommerfeld-Bloch individual particle model, but the ground state wave function of the superconductors is a linear combination of a many-normal-state configuration in which Bloch states are 'virtually occupied in pairs' (the Cooper pair model). This second assumption also depends on purely theoretical grounds.

Now the important piece after these two assumptions is to base their theory on an idealised model in order to neglect certain aspects which if incorporated into their theory would effect the coherency they are seeking. The BCS authors say: 'Our theory is based on a rather idealised model in which anisotropic effects are neglected' (BCS, 1957, p 1178). The line of assumptions depending on the microscopic understanding of superconductors would continue on and on and at each new assumption they would add a further theoretical constraint.

By contrast the story presented by the phenomenological model is not as abstract as the story presented by the theoretical model. Phenomenological models need not give us an explanation, of why certain phenomena behave the way they do, related to 'fundamental theories'. It is sufficient that they give an account of the behaviour of the phenomena, and give good predictions. This account is related only to the phenomenal facts associated with the particular phenomenon under study.

The story of the Landau and Ginzburg phenomenological model of superconductivity goes as follow. The Landau and Ginzburg model starts with a well-accepted description of superconductors as having thermal energy. The idea that superconductivity is related to thermodynamics is already an established empirical fact: resistance depends on thermal fluctuation. So by assuming that the superconducting state is a state where the electrons transform from disorder to order, Landau and Ginzburg were able to suggest that the transition between the normal and superconducting state is a second-order transition. This with the help of their 'physical intuition' led Landau and Ginzburg to suggest an equation to express the free energy of a thermal system as a function of an order parameter. They also associated the order parameter with the wave function. Then they were able to derive a set of equations that can account for the known properties of superconductivity as well as to predict new properties.

Both stories use a lot of assumptions to justify their moves, but the important difference between the two kinds of justification is that one depends on issues raised by theoretical constraints while the other depends far more heavily on empirical and phenomenological constraints.

3:3 Between the experimental and the phenomenological

My discussion in the last section concentrated on theoretical representation and the two different types of such representation. Let me now turn to the other side of the usual dichotomy between theory and experiment: the experimental activity in physics. Ian Hacking (1983) argues that experiments have a life of their own. Many other historians and philosophers have presented similar arguments and have pointed out examples of how experiments are developed in physics independent of theory. Peter Gallison's

How Experiments End, Bruno Latours' *Laboratory Life* and Gooding's *Experiment and the Making of Meaning* are all examples of such research. Such works highlight the importance of the experimental side of scientific practice.

I am interested in a different type of scientific practice. Physics provides us with a set of theoretical tools and a set of experimental and observational facts. A dialectical interaction between the two sets provides our usual best candidates for true representations of nature. I think that phenomenological models constitute these best candidates for true representations of nature.

For the scope of this thesis, I adopt a simple correspondence notion of truth, i.e. that the terms in a theoretical representation correspond to real properties of the phenomenon under study. For example, we say "The gravitational force acting on a falling body is equal to the mass of that body multiplied by the earth gravity (g)". In such a theoretical description 'mass' is suppose to refer to the a property the falling body has: its mass; 'gravitational force' to the gravitational force of the earth, etc, Then the claim is true if the force is equal to the mass times gravity. Having said that, I do not think that any of the other usual notions of truth would challenge my position.

Instead of the usual distinction between theoretical and experimental practice, I am interested in phenomenological practice, the practice which benefits from the interaction between the theoretical and the experimental. Physicists have started to recognise this activity as independent of the other two, because they want to recognise the important input of both theory and experiment in building certain types of models in physics. In high-energy physics as well as in superconductivity and polymers, the term "phenomenologist" is now widely used to denote people working in constructing a specific type of theoretical understanding. This type of theoretical understanding departs from the empirical findings related to certain phenomena and using whatever theoretical means they have without caring much about whether these theoretical means belong to a single "accepted" theory.

Whether these physicists want to differentiate themselves from theoreticians for merely sociological reasons is something to be explored. I think there is more to their attitude than that. Their attitude is supported by the complexity and the lack of clarity of the new abstract theories, and the failure of some of these theories to give good predictions whereas the phenomenological models do provide accurate representations in the field.

As early as 1943 scientists were concerned with the relation between theory and experiment. It is, of course, accepted that there is a lot of influence from each activity on the other. Max Born, for example, ended a public lecture in 1943 on "Experiment and Theory in Physics" with the following statement:

> I believe that there is no philosophical highroad in science, with epistemological signposts. No, we are in a jungle and find our way by trial and error, building our road behind us as we proceed. We do not find signposts at cross-roads, but our own scouts erect them, to help the rest. ... My advice to those who wish to learn the art of scientific prophecy is *not to rely on abstract reason, but to decipher the secret language of Nature from Nature's documents, the facts of experience* (Born 1943, p 44, emphases added).

The major theme in his lecture was to address the extremists on both sides of the theory-experiment dichotomy: On the one hand the extreme German experimentalist school which renounced theory altogether; and, on the other hand, the claim made by many theorists that

> to the mind well trained in mathematics and epistemology the laws of Nature are manifest without appeal to experiment (Born 1943, 1).

He addressed the types of problem that can emerge from such extremes. The experimental trend on its own often fails to introduce new experiments and to predict new phenomena. The theoretical one, on the other hand, might easily, using first principles alone, lead to two contradictory outcomes. Born discusses in some detail the Milne and Eddington case. In that case both Milne and Eddington claim to have succeeded in building a theory using a priori principles, and yet when we examine these two theories we see that they are "widely different and contradictory"(Born 1943, 44). Born's message is clear: theory without experiment is empty and experiment without theory is blind.

Phenomenological activity had its supporters even before it was recognised as an independent practice. Lev Landau, the famous Soviet scientist, thought that any theoretical physicist ought to equip him/herself with minimal knowledge of important bases in all fields of physics. This minimal theoretical knowledge would help the theorist to find appropriate solutions for the problems that the experimentalist faces. Landau used to teach his students that it is the main job of the theorist to solve the problems the experimentalist faces. He himself used to start his day by going to the labs and discussing any problems the experimentalists had before going to his office[9].

Another example is Heinz London, the co-author with Fritz London of the London and London model of superconductivity. In his case he was an experimental physicist who was interested in doing theoretical physics. For example he was the first to suggest the 'two fluid model' and the 'penetration depth' on the surface of the superconductor[10]. In neither case, though, did he publish his findings in theoretical journals because he was not keen on publishing any work that he had not thoroughly tested in his laboratory. His ideas and the experimental tests he did were crucial for the

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[9] For details see Abrikosov 1973, Ginzburg 1989 and Khalatnikov 1989.

[10] I will describe these terms in the next chapter.

construction of the London and London model of superconductivity. In fact that model would play a very important role in the history of superconductivity[11].

Nevertheless, the recognition of there being a special kind of activity in which theory and experiment interact is in itself something worth exploring[12]. I think that this is due to recognition of a problematic development in both activities, especially at the theoretical level.

My attempt to present a new kind of realism depending on phenomenological representation relies on such recognition. The phenomenological level is not just empirical and it is definitely not just a collection of mathematical equations – it is the inter-relation between the observable and the reasonable.

3:4 An example from physics

Let us take the usual example of theory development: Newton's theory vs. Einstein's general theory of relativity. While most of us agree that Newton's theory is false, we still use some of its models. No engineer, when building a bridge, will ever apply Einstein's general theory of gravity; they will prefer to use Newton's theory. But if you give a problem about the shift of light coming from a comet to a scientist, they will think directly about Einstein's theory.

Let us take a careful look at Newton's theory. We claim now that Newton's theory is false because we have good reasons to believe that the theoretical frame in which its models where embedded (that is the frame of absolute space and absolute time) is not a good one. Newton's theory

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[11] For some details about Heinz London see Gavroglu 1995.

[12] Of course interact here does not refer to the old sense where the experiment interacts with the theory in order to test it and verify its correctness. Here it means a positive interaction where each contributes in constructing a type of theoretical representation that departs from the experimental facts.

suggests two main concepts: the concept of continuous force and the concept of universal gravitation. By these two concepts Newton aimed to unify the two mechanical models, of Kepler and Galileo. He succeeded in presenting a frame in which the Newtonian particle is at the basis; a material particle which exists in a system that has the Euclidean geometrical dimensions and possess mechanical properties like kinetic energy, potential energy, momentum, ...etc. Now this theoretical frame presents a story which employs a number of elements that are not derived or derivable from the phenomenological level. These elements include concepts such as absolute space, absolute time, action-at-a-distance, universal gravitation and inertia. The story was presented in his *Principia* and is very well known. Nevertheless, the simple models that deal with the motion of a body from point (a) to a point (b) are not affected by such a frame. The importance of the frame and its concepts was to present a coherent story to connect the laws of universal movement with the laws of motion; to unite the motion of light with the motion of wheels.

Einstein's theory presented a different account, an account which provides another kind of unification, that between energy and mass, a story which replaces the concept of action-at-a-distance with the concept of fields, which gives answers to the anomalies facing the old theory. But also it replaces a bunch of theoretical concepts with another bunch aiming to keep coherency and to add, as Einstein would say, simplicity and beauty. These theoretical concepts include space-time curvature and fields.

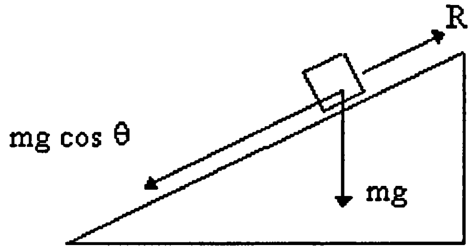

Figure 3.1

But after such a shift on the high theoretical level, we still find that the low-level theoretical models were not affected by any of these points. Let me give an example. A box of mass (m) is situated on top of a slope that has an angle θ with the x-axis. If the slope can, due to friction, resist the movement of the box by a force R, then the total force which will move the box toward the bottom of the slope is given by (see figure 3:1):

$$F = mg\cos\theta - R = ma \qquad (3.6)$$

where (g) is the constant of gravity and (a) is the acceleration of the box. Now in this example we did not mention any of the concepts of absolute space or absolute time. Here the mathematical outcome expresses a series of observations on moving bodies over a slope with a known friction. It depends on our previous knowledge of trigonometry and of a generalised model, 'Newton's second law, $F = ma$', which might also be inductively inferred from direct observations and mathematical knowledge. This model is still true, whether we accept an absolute space and time or we accept a space-time curvature. It depends only on the phenomenological level.

So, what really happens in low-level representation is that the phenomenon will almost dictate the mathematical form via the observed data. This is a general practice in physics, as Einstein says:

> Nobody who has really gone deeply into the matter will deny that in practice the world of phenomena unambiguously determines the theoretical system, in spite of the fact that there is no logical bridge between phenomena and their theoretical principles (Einstein 1935, p 126).

3:5 Building a phenomenological model

Looking at the practice of physics we can see how it is possible to build a scientific structure, on phenomenological grounds, without the need for a unified theoretical frame. I can give many examples from different domains in physics. As I have said, in chapter four I will study in detail how the Landau and Ginzburg phenomenological model of superconductivity was constructed. But for now I will restrict myself to one simple example.

Let us see how the phenomenological model of capacitance was constructed. Certain materials can, if an external pressure influences them, produce some kind of electrical potential difference between their sides, if they are under the effect of a flowing current. Now, if we put one of those materials between two charged plates then that material will have the ability to store charge. This material with the two charged plates is what is known as a capacitor. The charge can be stored in the capacitor due to the potential difference between its plates, and for every type of capacitor the capacitance is a constant value. If we want to discharge a capacitor we connect the capacitor to a resistor.

When capacitors were discovered, two important theories were known in physics: mechanics and electromagnetism. The obvious thing for physicists to do under these circumstances is to try to use these two theories to understand the new phenomenon. The idea is to use these as tools to build a model. The important point at this stage is to see the way the physicists will depart from different phenomenological facts and how they will deploy at each stage another tool from the well-known theories.

The capacitor is a material; thus the particles (electrons and atoms) can be treated as if they constitute a mechanical system which through its movement transmits the electrical current from one side to the other. Also, capacitors store charge; hence they must be treated as an electrical system. So, what kinds of tools can be used to express these two properties of the capacitor?

First, on the mechanical side, the electrons have to move from one plate of the capacitor to the other; that will justify the use of Newton's second law. But this is not the whole story. The electrons during their move through the lattice will be affected by friction and by the chemical bond between the atoms (which can be modelled as a kind of oscillation). So, here we have three mechanical parts:

1) the movement of particles from one plate to the other
2) frictional force and
3) the chemical interaction (oscillation).

There are three mathematical forms that express these three properties.

Now, to build a model which will map the mechanical movements of the electrons through the capacitor, the most sensible thing to do, from the 'physicists intuitions' point of view, is to add these three mathematical forms to each other to form the new equation. The outcome force equation is:

$$F = m\frac{d^2x}{dt^2} + f\frac{dx}{dt} + kx \qquad (3.7).$$

Here the first term refers to Newton's second law, giving the force due to acceleration of the particles inside the lattice; the second term is the force due to friction, while the third is the force due to the oscillation of the particles in the lattice. The model would not be accepted at this stage, for it is important before the acceptance of any model to verify its accuracy with the experimental results. If there were an agreement between the two sides, then physicists would accept the model as representative of the studied phenomenon. But in this case there is another part to the phenomenon.

The other side is due to the electrical properties. A power source of potential V produces a current *I*, which is equal to the dq/dt where q denotes charge; when this current run through the capacitor it will charge the capacitor. The capacity in this case is equal to q/V. When the electrical circuit is opened the capacitor will start to discharge and produce an induced current from the capacitor, i.e. the capacitor will behave as a power source. Hence, there will be a movement of electrons from one side of the capacitor to the other. This is important for unifying the relation between the mechanical model and the electrical model. Putting this information to use will give the potential through a capacitor:

$$V = L\frac{d^2q}{dt^2} + R\frac{dq}{dt} + \frac{q}{C} \qquad (3.8)$$

where the first term refers to the induced voltage, the second to the voltage from resistance, while the third term is due to the capacitance. We can see the similarity in the mathematical form between the electrical and the mechanical equation. This similarity speaks about the way of presenting the data which happen to have the same shape of the plotted carve.

Let us remember the criteria for a phenomenological model: a phenomenon is studied and some empirical facts are revealed about it; a mathematical equation can express the plotted data; a story should be told about how this mathematical equation can represent the phenomenon. Now if the model produced captures the empirical facts and can predict the behaviour of a similar set-up then we may say that the model is a successful one. In our case the model uses tools from Newton's theory and from electromagnetism. Up to the point where we stopped earlier, it seems that there isn't anything more than just applying the best theories we have to a special case. Yes, but the model doesn't stop here. It should give us the relation between the two sides, because as we saw, when the capacitor is discharging it will generate a mechanical movement inside it. Hence such a movement ought to be captured by the model. Here the departure from the straightforward application of old theories can be seen: we have two equations that represent a certain phenomenon from two different perspectives. One uses the effect of forces and concentrates on the experimental verification of 'distance x'. So the main variable in the equation is (x). The second equation studies potential and the major variable that all parts depend on is the charge (q), which is also experimentally verifiable. If there is a relation between the two parts then there ought to be some kind of a second order relation between the two variables, the distance and the charge. One simple mathematical way to do so is by adding to each of the two equations a term that can represent its effect on the second variable; that is to add a charge term to the first equation and a distance term to the second equation. So, the model which represents the capacitance, with the relation between the mechanical and the electrical behaviour of the phenomenon, would be:

$$\begin{cases} F = m\frac{d^2x}{dt^2} + f\frac{dx}{dt} + kx + Aq \\ V = L\frac{d^2q}{dt^2} + R\frac{dq}{dt} + \frac{q}{C} + Ax \end{cases} \tag{3.9}$$

where A is an arbitrary constant. It might be said that nowadays we look at the problem from a different perspective and what is represented by the model can be fully explained by the fundamental theories of today. But a careful look into the textbooks in physics will show us that even if a better tool is used the model produced is the same model with little change.

3:6 Conclusion

In this chapter I suggested that there exist two kind of theoretical constructions in physics, and claimed that one of them is highly abstract while the other is more concrete and a better candidate for being a real representative of nature. By this kind of distinction I will try to motivate a realist position which can accept the no-miracle argument and still overcome the difficulties generated by the argument from scientific revolutions. Structural realism tried to accommodate the anti-realist attacks by restricting itself to a structure-content dichotomy. In this spirit, I have suggested an alternative distinction: between phenomenological models and theoretical models. This kind of distinction between the two types of theoretical construction form the basis for the realist position which I advance and defend in chapter five. For now let me turn my attention to a case study in superconductivity which exemplifies the theoretical dichotomy I have suggested here.

Chapter Four

The Case Of Superconductivity

4:1 Introduction[1]

Last chapter I claimed that two main kinds of theoretical forms can be found in physics. Theoretical models: that is, models built on a top-down strategy, the other kind of theoretical form, I claim, built on a bottom-up strategy, are phenomenological models. In this chapter I will illustrate such a division in the case of superconductivity. By looking at the key points in the history of superconductivity I will show that the BCS theoretical model of superconductivity, was constructed by a top-down approach while the Landau and Ginzburg model was adopted on a bottom-up approach.

It is important to understand the key historical points in the history of superconductivity in order to fully understand the impact of both the 'BCS theory' (1957) and the Landau and Ginzburg model (1950). The Dutch physicist H. K. Onnes first discovered the property of superconductivity in 1911. Of course superconductivity would not have occurred if Onnes had not invented a way to condense Helium. Due to that it is fair to say that superconductivity is a kind of phenomeno-technology (see chapter five). Onnes thereafter detected that metals when cooled to a very low temperature, inside liquid Helium (under 4°K), exhibit a strange phenomenon: the total disappearance of resistance under a critical transition temperature T_c. Later in 1933 W. Meissner and R. Ochsenfeld discovered that the magnetic field is expelled inside the superconductor under a certain transition magnetic field $\mathbf{H}_c$ (The Meissner Effect).

The first successful attempt to construct a model of superconductivity was at the hands of Fritz and Heinz London in 1935 (section 4:2). Later, in 1950, Landau and Ginzburg developed "an extension of the London phenomenological" model "to take into account a space

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[1] The information in this chapter and in chapter five are collected from studying a lot of books and papers on superconductivity of which some are not directly referred to in these chapters, nevertheless I included them in the bibliography.

variation of the order parameter" (Bardeen 1956, 369). They suggested the following phenomenological equations[2]:

$$\alpha\psi + \beta|\psi|^2\psi + \frac{1}{2m}\left(-i\hbar\nabla - \frac{2e\mathbf{A}}{c}\right)^2\psi = 0$$
$$\mathbf{j} = -\frac{ei\hbar}{2m}\left(\psi^*\nabla\psi - \psi\nabla\psi^*\right) - \frac{e^2}{mc}\psi^*\psi\mathbf{A} \qquad (4{:}1)$$

where α and β are experimental constants, ψ is a pseudo wave function and **A** is the local vector potential (section 4:3).

Bardeen, Cooper and Schrieffer suggested in 1957 a microscopic 'theory' (theoretical model) of superconductivity (from now on BCS), which was set up to provide a fundamental understanding of the phenomenon of superconductivity. Although they were not the first to introduce microscopic analyses of superconductivity, their theoretical model was the first coherent and reasonably successful microscopic theoretical model designed to understand the phenomenon of superconductivity (section 4:4). I think that the difference between these two types of theoretical forms is an exemplar of the theoretical division presented in chapter three.

4:2 The London and London Model

In an attempt to present an explanation to the paradoxical findings of Meissner, London and London suggested in 1935 that diamagnetism is a property of ideal superconductors. This led them to construct a phenomenological model: the Londons' model. This model has two main equations:

$$c\,\mathit{curl}\Lambda\mathbf{j}_s + \mathbf{H} = 0$$
$$\frac{\partial}{\partial t}(\Lambda\mathbf{j}_s) - \mathbf{E} = 0 \qquad (4{:}2)$$

---

[2] This is known as the MQM (macroscopic quantum model).

where j_s is the superconducting current density, **H** is the magnetic field, **E** is the electrical field, c is the speed of light and Λ is an experimental constant equal to m/ne^2, where m is the mass of the electron, n is the number of electrons and e is the electron's charge.

Superconductors differ from other kinds of conductors by two things: the property of no resistance and the expulsion of the magnetic field. Superconductors are not "perfect conductors", although they exhibit perfect conductivity[3]; this is due to the magnetic expulsion[4].

In a previous paper, "The Tool Box of Science", co-authored with N. Cartwright and M. Suárez, we argued that the London equations[5] were constructed on phenomenological bases rather than theoretical. Fritz and Heinz London were aware that Maxwell's electromagnetic equations could give the following equation for the relation between the density current and the magnetic field:

$$curl \Lambda \dot{\mathbf{J}} = -\tfrac{1}{c}\dot{\mathbf{H}} \qquad (4{:}3)$$

When they integrated with respect to time they obtained a non-homogeneous equation for **H**:

$$\Lambda c^2 \nabla^2 (\mathbf{H} - \mathbf{H}_0) = \mathbf{H} - \mathbf{H}_0 \qquad (4{:}4)$$

where $\mathbf{H}_0$ denotes the magnetic field at time zero. This means that if the superconductor was inside a magnetic field when it was shifted into the superconducting phase then the magnetic field should freeze in the superconductor. Meissner's experiments obviously contradict this conclusion. So, the London brothers thought that they might accept that a single homogeneous solution of equation (4:3) might be considered as the

---

[3] Perfect conductors are a kind of conductors with a neglected resistance, but they do not expel the magnetic field from it. I.e. when they are exposed to a magnetic field they lose some of their perfect conductivity. This does not happen in superconductors, due to the magnetic expulsion.

[4] See Burns 1992, p 10.

[5] A full discussion of the London equations and its philosophical implication is presented in Mauricio Suárez 1997.

fundamental law for the superconducting state upon the experimental confirmation for such claim. They say:

> One should not use a differential equation like [(4:3)] which contains too many possibilities, as it gives nature more freedom than it wants. If in reality $\mathbf{H}_0$ is always confined to the value zero, then this means that
>
> $$\Lambda c^2 \nabla^2 \mathbf{H} = \mathbf{H} \qquad (4:5)$$
>
> is to be considered as a fundamental law and not to be treated as a particular integral of a differential equation. (London and London 1935, 73)

This new "fundamental" law can give us the following relation:

$$curl \Lambda \mathbf{J} = -\tfrac{1}{c}\mathbf{H} \qquad (4:6)$$

which obviously does not give us the same information as equation (4:3). It is important to see that the move from (4:3) to (4:6) is not derivational but is an inference that depends on the experimental facts.

If we want to play the theoretical game of finding a ground in fundamental theories for a phenomenological model through some kind of derivation, we find that two ways were suggested for the London equations: The first way[6] is a direct analogy: derivation from diamagnetism. The second is the BCS treatment, which depends on a quantum field theoretical derivation using Green functions and Hartree-Fock approximation (I will give the result of the derivation in the section 4:4).

Let us start with the first treatment. Consider a long superconducting solenoid, as in fig 4.1, where a particle of charge e is fixed to the axis of the solenoid by a spring. The solenoid has a current *i* running through it. When we raise the solenoid's temperature above its transition point the current and magnetic field caused by it will die off. At this stage an induced EMF will accelerate the particle ε around the route C giving the particle an angular

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[6] See Hall, 1981, p 147.

momentum *mvr*, where v is the velocity of the electron and r is the radius. From conservation of momentum we assume that there is an electromagnetic momentum which is reserved in the charge particle at rest[7]. The angular momentum during the decay of the field is

$$mvr = er\int \mathbf{E}\,dt \qquad (4{:}7)$$

But from Faraday's law:

$$\mathbf{E} = -\frac{1}{2\pi r}\frac{d\phi}{dt} \qquad (4{:}8)$$

where f is the magnetic flux, E is the electrical field. So

$$mvr = \frac{e\phi}{2\pi} \qquad (4{:}9)$$

And since

$$\phi = 2\pi r\mathbf{A} \qquad (4{:}10).$$

Then

$$mv = e\mathbf{A} \qquad (4{:}11).$$

We redefine the total momentum **P** as

$$\mathbf{P} = mv + e\mathbf{A} \qquad (4{:}12).$$

where mv is the induced momentum and eA is the electromagnetic momentum in the initial rest state of the particle.

Now electromagnetism implies that the current density **j** is

$$\mathbf{j} = ne\mathbf{v} = \frac{ne}{m}(\mathbf{P} - e\mathbf{A}) \qquad (4{:}13).$$

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[7] It is important to mention that this treatment is analogous to the classical treatment of kinematic momentum and the dynamical momentum. Feynman gives a good account of the example in his chapter on superconductivity: *The Feynman Lectures in Physics*, Vol. 3, chapter 21.

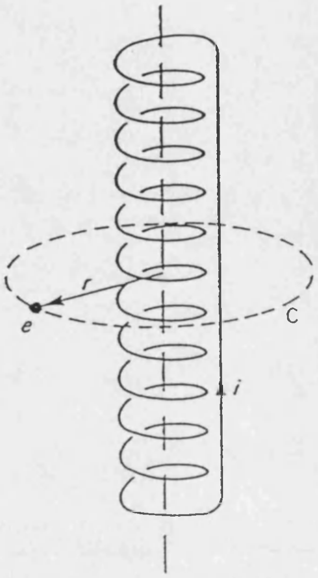

Fig. 4.1 A charged particle accelerated by a decaying magnetic field.

At this stage to be able to derive the first London equation, we can go with F. London's (1950, 120) suggestion that the current density is divided into normal and superconducting currents:

$$\mathbf{j} = \mathbf{j}_s + \mathbf{j}_n$$
$$\mathbf{j}_s = -\frac{ne^2}{m}\mathbf{A} \qquad (4{:}14).$$

Here the idea of the two fluid model[9] is important to explain this partition, which is a method used to derive the thermal effect of superconductivity. The idea is that the superconducting material consists of two kinds of fluids, one is the normal part of the superconductor while the second is the

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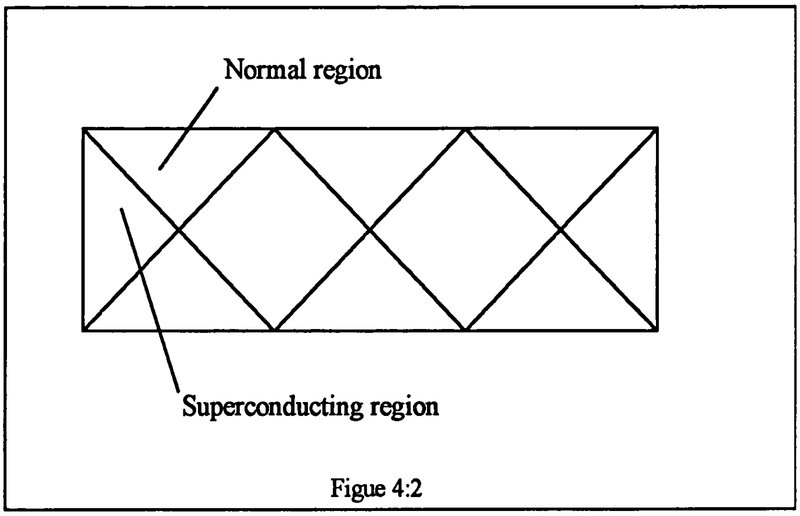
[9] The idea appeared first in 1934 by Gorter and Casimir, but as Kostas Gavroglu shows in his book *Fritz London: A Scientific Biography* the idea was first suggested by Heinz London. See Gavroglu 1995, p 109.

superconducting part. A figure suggested by F. London, in the (1949) paper, can give an example of what is meant by a two fluid model.

These phenomenological models[9] were not questioned by any later theoretical work. Even in the BCS, the two current densities $\mathbf{j}_n$ and $\mathbf{j}_s$ were interpreted as $\mathbf{j}_p$, which is called the paramagnetic current, and $\mathbf{j}_d$, which is the diamagnetic current. This interpretation can give a reason to accept the first term as a normal and not effective current, while ($\mathbf{j}_d$) is the effective current density of the superconductor. Eventually that means that the superconducting phenomenon is related just to the diamagnetic term and not to the paramagnetic, whereas the Meissner effect is related to the paramagnetic term, with the assumption of an energy gap. This idea was important for the derivation of the BCS, as we will see in the next section.



Figue 4:2

Another thing can be done here. If we take both terms and accept the quantum mechanical substitution of the momentum **P** by $i\hbar\nabla$, and take n, the

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[9] These models are phenomenological in my sense because, for example, the two fluid model was built on a bottom-up approach, has a mathematical equation ($j = j_s + j_n$), a description and a story.

number density, to be the probability density $\psi^*\psi$, we can derive an equation similar to Landau and Ginzburg's[10] phenomenological equation:

$$\mathbf{j} = -\frac{i\hbar e^*}{2m^*}(\psi^*\nabla\psi + \psi\nabla\psi^*) - \frac{e^{*2}}{m^*c}\psi^*\psi\ \mathbf{A} \qquad (4{:}15).$$

where $e^* = 2e$ and $m^* = 2m_e$[11].

One might say that the first deduction, which applies the idea of superconductor as a huge solenoid, is legitimate and mathematically true, so we must accept it as a derivation of the London and London phenomenological model of superconductivity from fundamental theory. The points that must be raised here against such a claim are:

1) The derivation from diamagnetism is for a specific case where the particle is attached to the solenoid by a field. This means that the particle is external, although not free. On the other hand the superconducting state is a state where the phenomenon emerges within the metal itself. Hence, the superconducting electrons are internal.
2) If we can assume that the momentum can be divided into two terms, one the intrinsic electromagnetic momentum of the particle and the other an induced momentum, then we can accept that one of them could be zero and the other not. This cannot be the case in interpreting the current

---

[10] From Michael Tinkham, 1974, p. 111.

[11] For such an argument see A. Rose-Innes and E. Rhoderick (1988), p 102. Also it is important to state that London & London wrote in their 1935 paper, that from their equations:

$$\Lambda c\mathbf{J} = -\mathbf{A}$$

$$\Lambda c^2\rho = -\phi$$

> "One is very strongly reminded of Gordon's Formulae for the electrical current and charge in his relativistic formation of Schrödinger theory:
>
> $$\mathbf{J} = \frac{he}{4\pi im}(\psi grad\psi^* - \psi^* grad\psi) - \frac{e^2}{mc}\psi\psi^*\mathbf{A}$$
>
> $$\rho = \frac{he}{4\pi imc^2}(\psi^*\frac{\partial\psi}{\partial t} - \psi\frac{\partial\psi^*}{\partial t}) - \frac{e^2}{mc^2}\psi\psi^*\phi$$ "

density in superconductors, because both the paramagnetic and the diamagnetic currents are functioning at the same time with the same set of electrons.

When London & London suggested their equations to model the Meissner effect, a number of experimental activities tested the correctness of their model. The result of these experiments show that London's equations could give good predictions for the superconducting current, if the following conditions are satisfied:

1) $\mathbf{H} \ll \mathbf{H}_C$. Otherwise at the boundary zone where **H** is near the critical magnetic field the model failed to predict what happens experimentally.
2) The superconducting electron density is constant.
3) The penetration depth is less than the thickness of the sample.

Hence, when these conditions are not satisfied, a new model should be introduced.

So, the difficulties of the Londons' equations are: first, when the temperature $T \approx T_C$ the penetration depth becomes larger and the thickness of the "walls" separating the normal and the superconducting states becomes larger, too. Experimentally the relation between the penetration depth and the thickness of the walls was calculated to be roughly

$$= x\left[\frac{T_0}{(T_0 - T)}\right]^{\frac{1}{2}} \qquad (4:16).$$

The second difficulty for the London equations is related to the change of the free energy between the normal and superconducting states. As Ginzburg pointed out

> if we restrict ourselves to the case of a steady field, then [London's equations], together with Maxwell's equations, are sufficient for determining the density $\mathbf{j}_s$ of the superconducting

> current and the field **H** in the superconductor. (Ginzburg 1956, 589)

So, if we want to have a broader understanding of the change from the normal to the superconducting state, the London approach is not sufficient. Any new theory or model should also aim to account, in addition to the Meissner effect, for additional empirical facts:

1) A second order phase transition.[12]
2) The transition temperature is proportional to the isotopic mass M of the metal nuclei (the *isotope effect*)[13].
3) The thermal vibrations of the atoms are the principal cause of electrical resistance in metals at ordinary temperature[14], while the superconducting state has infinite conductivity.
4) The energy gap.

As Landau and Ginzburg pointed out in the introduction to their paper:

> The existing phenomenological theory of superconductivity is unsatisfactory since it does not allow us to determine the surface tension at the boundary between the normal and the superconducting phase and does not allow for the possibility to describe correctly the destruction of superconductivity by a magnetic field or current. (Landau and Ginzburg 1950, 546)

To overcome these difficulties Landau & Ginzburg tried to formulate what they saw as a generalisation of London's equation[15]. This is the topic of the next section.

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[12] Laughlin (1988) 525.

[13] Sproull and Phillips 1976. p 380.

[14] Ibid., pp 348.

[15] A. B. Pippard had also suggested a generalised version of London's equations in 1953. I will not discuss his suggestion here because it is not used any more. It is important to say that BCS authors had arrived at a similar equation to that of Pippard:

$$j(r) = -\frac{3}{4\pi c\Lambda\xi_o}\int\frac{R[R.A(r')]e^{-R/\xi_0}}{R^4}dr'$$

4:3 The Landau and Ginzburg model

In 1935 when London and London suggested their solution for the Meissner effect in superconductivity, they ended their paper by stating that a more general solution for superconductivity might be inspired by studying Gordon's formulae for the electrical current and charge in the relativistic formation of Schrödinger theory:

$$\begin{aligned} \mathbf{J} &= \frac{he}{4\pi im}(\psi\nabla\psi^* - \psi^*\nabla\psi) - \frac{e^2}{mc}\psi\psi^*\mathbf{A} \\ \rho &= \frac{he}{4\pi imc^2}(\psi^*\frac{\partial\psi}{\partial t} - \psi\frac{\partial\psi^*}{\partial t}) - \frac{e^2}{mc^2}\psi\psi^*\phi \end{aligned} \qquad (4{:}17)$$

Where ψ here is the electrons wave function. Also, as I mentioned earlier, if we substitute $i\hbar\nabla$ instead of P in equation (4:14) and take n, the number density, to be the probability density $\psi^*\psi$, we will arrive to the equation (4:15):

$$\mathbf{j} = -\frac{i\hbar e^*}{2m^*}(\psi^*\nabla\psi + \psi\nabla\psi^*) - \frac{e^{*2}}{m^*c}\psi^*\psi\ \mathbf{A} \qquad (4{:}15)$$

which is the same as the first equation of (4:17). Nevertheless, at that time a direct connection between a quantum mechanical description and superconductivity was not proved.

However, Landau and Ginzburg started in fact from this remark and from the experimental evidence that showed that Londons' equations cannot give an accurate account of superconductivity. The idea was to try to find the correct tools from the existing theories to bring about some kind of deduction that will end up with a similar equation to that of Gordon's, but which can be directly related to the field of superconductivity.

It is also important to mention that Ginzburg had also pointed out in 1955 that Pippard's model is a limiting case of Landau and Ginzburg model.
For further information consult: A. B. Pippard, 1953 and Ginzburg 1955

At that time it was an experimental fact that superconductivity exhibited some kind of thermal fluctuation. As I have said, the thermal vibration is known to be the cause of electrical resistance. Hence the region of no-resistance should be related to what could happen for such thermal vibration.

Landau had been working on a theory for the phase transition in the solid state. He thought that it could be of aid in their derivation. He suggested that the transition from the normal to the superconducting state is a second order transition (as in the transition from the ferromagnetic to the paramagnetic)[16]. This means that the thermal vibration of the electrons which cause the electrical resistance will be ordered, under the transition from the normal to the superconducting state; no thermal vibration will occur any more, which implies that no resistance will occur as well. To relate this kind of transition to thermodynamics he offered, a *'guess* ' for the free energy

$$\int F(\Delta; T, \mathrm{E}) d^3 r, \text{ [17]} \qquad (4{:}18)$$

where Δ is an order parameter which is function of r. Expanding this function in terms of power series in Δ^2 we find that

$$F = -\mathrm{E}\Delta + g_0 + \frac{1}{2} g_2 \Delta^2 + \frac{1}{4} g_4 \Delta^4 + \ldots.. \qquad (4{:}19).$$

The standard text by Tilley and Tilley describe this saying

> Landau's general theory of second-order phase transitions is based on the idea that a phase transition could be characterised by some kind of order parameter, and a simple postulated form for the dependence of the free energy on the order parameter. (Tillery and Tillery 1986, 294)

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[16] As Landau and Ginzburg put it: "In the general theory of such transition there always enters some parameter [Δ] which differs from zero in the ordered phase and which equals to zero in the disordered phase." (1950, p 548)

[17] J. Schrieffer 1964, p. 19.

Consider the non-vanishing terms. Because it is a second order phase transition, the term g_4 is positive, and the higher order terms can be neglected[18]. Think about the transition from the normal to the superconducting states as a transition from disorder to order, and assume that the normal free energy has the same form as in thermodynamics, which is:

$$F = U - T\Sigma \qquad (4{:}20)$$

where U is the initial energy. Then in the case of a fixed magnetic field, the relation between the superconducting free energy and the order parameter Δ for a cubic crystal can be written as follows:

$$F = F_s = F_n + a\Delta^2 + \frac{b}{2}\Delta^4 + C\left[\left|\frac{\partial\Delta}{\partial x}\right|^2 + \left|\frac{\partial\Delta}{\partial y}\right|^2 + \left|\frac{\partial\Delta}{\partial z}\right|^2\right] \qquad (4{:}21)$$

where A, B and C are arbitrary constants, F_s is the superconducting free energy, F_n is the free energy in the normal state.

Now, in the case of a variable magnetic field, equation (4:21) must be completed by adding another vector potential **A** which is related to the field **B** (**B** = Curl A).[19] Thus, in order to keep the invariance of the free energy F, equation (4:21) must be changed into:

$$F = F_n + a\Delta^2 + \frac{b}{2}\Delta^4 + C\left|\left(-i\nabla - \frac{2e\mathbf{A}}{\hbar c}\right)\Delta\right|^2 + \frac{\mathbf{B}^2}{8\pi} \qquad (4{:}22)$$

Tillery and Tillery continue: the next "crucial insight in Landau and Ginzburg was that for a superconductor the order parameter **must** be identified with the macroscopic wave function Ψ"[20]. Landau and Ginzburg want to arrive at a mathematical expression which is similar to those of the current in quantum

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[18] Second order transition means that g_4 is positive in contrast with the first order transition where g_4 is negative and we can not neglect the rest of the terms.

[19] It should be pointed that $\mathbf{B}_C \equiv \mathbf{H}_C$ under the CGS system and $\mathbf{B}_C/\mu_0 \equiv \mathbf{H}_C$ under IS system·

[20] Tillery & Tillery, p 294.

mechanics. To get their equation to correspond to that, they ought to identify a parameter in their equation with the wave function. The only parameter which can be thought of as conveying the same properties as Ψ is the order parameter. So, in this sense it was essential to them to identify the order parameter with the macroscopic wave function Ψ. Landau and Ginzburg consider such a wave as an effective wave function. They say:

> In the phenomenon of superconductivity, in which it is the superconducting phase that is ordered, we shall use Ψ to denote this characteristic parameter. For temperature above T_c, Ψ = zero in the state of thermodynamic equilibrium, while for temperature below T_c, $\Psi \neq$ zero. We shall start from the idea that Ψ represents some 'effective' wave function of the 'superconducting electrons'. Consequently Ψ may be precisely determined only apart from a phase constant. Thus all observable quantities must depend on Ψ and Ψ^* in such a way that they are unchanged when Ψ is multiplied by a constant of the type e^{ia}. We may note also that since the quantum mechanical connection between Ψ and the observable quantities has not yet been determined we may normalise Ψ in an arbitrary manner. (Landau and Ginzburg 1950, 548)

They assert that the order parameter is related to the local density of superconducting electrons n_S[21] where $n_s = \frac{N_s}{v} = |\psi(r)|^2$. Then by setting the correct values of the coefficients, depending on the experimental results we get:

$$F = F_n + \alpha\psi^2 + \frac{\beta}{2}\psi^4 + \frac{1}{2m}\left|\left(-i\hbar\nabla - \frac{2e\mathbf{A}}{c}\right)\psi\right|^2 + \frac{\mathbf{B}^2}{8\pi} \qquad (4:23)$$

---

[21] Burns, p 18.

It may seem that F has just been modified from equation (4:21) to equation (4:23), but this is far from right. Two features of the construction illustrate my thesis about the use of theories as a tool for constructing phenomenological models: First, equation (4:21) itself had been constructed using tools from different theoretical models which are not connected to superconductivity. These tools are the phase transition, the assumption that the free energy is a function of the order parameter and quantum mechanics. Second, equation (4:23) is not derived from quantum mechanics, rather a non-quantum mechanical equation (4:21) is reformed in (4:23). This illustrates a theoretical influence that functions merely as a way to express the experimental results. The physicists see that

> this construction of equation [(4:23)], independent of any detailed theory of the superconducting state, represented a *tour de force* of physical intuition. (De Gennes and Pincus 1966, 176)

Now from the modification of the free energy in equation (4:23) Landau & Ginzburg set ΔF = 0, to obtain the following equations:

$$\begin{aligned} &\alpha\psi + \beta|\psi|^2\psi + \frac{1}{2m}\left(-i\hbar\nabla - \frac{2e\mathbf{A}}{c}\right)^2\psi = 0 \\ &\mathbf{j} = -\frac{e^* i\hbar}{2m^*}\left(\psi^*\nabla\psi - \psi\nabla\psi^*\right) - \frac{e^{*2}}{m^* c}\psi^*\psi\mathbf{A} \end{aligned} \qquad (4{:}24)$$

As we can see these equation have the same form as Gordon's equations even if there is no direct relation in a deductive sense. Gordon's equations are formulated from the relativistic Schrödinger equation, and as we have seen Landau and Ginzburg's equations were the result of reasoning about the phase transition in fluids in relation to the free energy.

Therefore, the Landau and Ginzburg model is a theoretical form that is directly related to the phenomena at hand, and would be described as a phenomenological model because:

1) It uses the existing theories as tools in constructing a set of mathematical equations that can be associated with the known experimental results in the field of superconductivity. In this case, although is not essential for phenomenological models, these mathematical equations are similar in form to previous mathematical forms; the similarity between the Landau and Ginzburg's equations of [4:23] with those of Gordon.
2) It gives a detailed description of the environmental set-up in which the phenomenon of superconductivity occurs. That is, the specific factors that would allow special types of material to exhibit the phenomenon of superconductivity. Some of these factors are the critical temperature, the critical magnetic field and the types of materials and alloys.
3) It gives a story that: a) Does not care about the coherency with the high-level theoretical forms as long as it presents a coherent and consistent account in relation with the facts at the phenomenal level. b) Gives a set of identifications that relates the mathematical symbols in the mathematical equation with the properties of superconductivity. For example it relates the parameters in the mathematical form, like α and β, with properties in superconductivity like the coherence length and the penetration depth; it relates different properties of superconductivity to each other; and it describes how these properties manifest themselves in relation with the particular conditions of the system. One other important example of the identification of the mathematical with the physical is that of identifying the order parameter with the pseudo wave function.

Furthermore the Landau and Ginzburg model had been constructed by departing from the phenomenon.

The importance of the Landau and Ginzburg model was not appreciated in the west at the time it was suggested[22]. At that time, there was

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[22] There is a long explanation for such lack of appreciation of the western scientists to the Soviet scientists; I will not go into its details. But basically after the Second World War the centre of the scientific community was shifted from Europe to America. There the

no acceptable microscopic theoretical model for superconductivity stemming from a fundamental theory, i.e. quantum mechanics, (as I said earlier the first one was the BCS). So, when they thought of introducing the wave function in their equation there was no derivational justification for such an attempt. The only justification was the rationalisation of experimental result. However, when the BCS was suggested in 1957, it drew a direct connection, as we will see below, between quantum mechanics and superconductivity. In 1959 Gor'kov, a physicist from the Soviet Union, proved that the order parameter wave function in Landau & Ginzburg is proportional to the energy gap between the condensed state and the first excited state in the electrical distributions of the fluids. This energy gap is also proportional to kT (where k is Boltzmann constant). Using this argument he went on to prove that the Landau & Ginzburg model was in agreement with the BCS at least for that limited case.

As I said earlier Landau's attitude toward doing theoretical physics was very distinctive. He thought that the theoretical physicists ought to solve the problems that face the experimental physicists. His idea is to give a rational generalisation of the evidence provided on the phenomenological level. In his opinion all theoretical physicists ought to equip themselves with a broad knowledge of the fundamental theories in physics in order to be able to use any of their formulas to solve problems raised out of experiments and phenomena. This basic knowledge that the theorists ought to acquire used to be called the 'theoretical/technical minimum' course[23].

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dominant approach was that of relating models to fundamental theories. Nevertheless, a lot of physicists felt uneasy with this approach to mention but one: Richard Feynman. Another factor was the lack of communication and trust during the cold war between the Soviet scientists and the western scientists. Gavroglu 1995 gives an instance of this difference see pp 180- 266.

[23] See Khalatnikov 1989, pp 34-41.

This spirit was also the motor behind his inspirational model for the phenomenon of superfluidity[24]. In 1941 Landau suggested a phenomenological theory of superfluidity. He developed this model in several papers (Landau, 1941, 1944, 1947 and 1949)[25]. This model turns out to be very successful and was able to predict properties which were not discovered experimentally until years later. The irony in this was Fritz London's reaction to Landau's model. Although his own early model was driven by phenomenological considerations, he did not apply the same attitude to Landau's attempt[26]. Kostas Gavroglu discusses this in his book *Fritz London; a Scientific Biography*. He thinks that 'London was convinced that Landau's approach had serious theoretical deficiencies' (Gavroglu 1995, p 202). These deficiencies circulate around the way Landau built his theoretical representation. Gavroglu emphasises that:

> In all his writing and correspondence, London expressed the belief that Landau's theory was a rationalization of Kapitza's[27] experiments together with the insight provided by Tisza's[28] first paper on the two-fluid model. (Gavroglu 1995, 201).

This rationalisation of the experimental is what London disapproved of, although he himself as we already saw had adopted such a rationalisation. Gavroglu shows that London's attitude toward the way theoretical physics ought to be done changed dramatically after he lived in the United States. He says:

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[24] Superfluidity is the sister phenomenon of superconductivity. Superfluidity is a phenomenon which appears in liquid Helium, when under a transition temperature the liquid starts to flow without viscosity.

[25] Even if the case of superfluidity is a further support to my claim that phenomenological models are a better representative of nature than theoretical models, I will not discuss this issue here.

[26] Gavroglu thinks that there was a shift in London theoretical stands. He starts to change his stands to the 'American way' after he left Europe to the states. See Gavroglu 1995, pp 180- 266.

[27] An experimentalist from the Soviet Union.

[28] A Polish physicist worked with London in Paris during 1937 and was the first to suggest that the idea of two fluid model can work for superfluidity.

> It is rather ironic that Landau's style was now closer to London's own style in his work in quantum chemistry, and London's approach in superfluidity was more reminiscent of the American's style in quantum chemistry that Heilter and London were critical about. (Gavroglu 1995, 189)

At the end of that period London started to doubt his own old style. He asserted that

> Any macroscopic theory has in general to go beyond the strictly phenomenological data - the same is for instance the case in my 'macroscopic' theory of superconductivity which also was suggested by certain molecular ideas, but, of course, not based on them. (in Gavroglu 1995, 203)

However, I think that such a style is not only a characteristic of Landau and the European physicists, but it is also a division between two theoretical forms, as I argued in chapter three. Even in America there have been people like Feynman who value such a style of doing theoretical physics.

4:4 The BCS model

In 1957 the BCS model was constructed starting from the basis of quantum field theory, aiming to incorporate the empirical facts stated in section 4:2[29] and to build a 'microscopic theory of superconductivity'. In contrast Landau and Ginzburg's basis was the experimental facts and its aim was to build a theoretical representation. The belief that quantum field theory could establish a basis for understanding the properties of superconductivity came

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[29] The BCS paper starts by saying: "The main facts which a theory of superconductivity must explain are 1) a second order phase transition at the critical temperature, T_C, 2) an electronic specific heat varying as exp $(-T_0/T)$ near $T=0°K$ and other evidence for an energy gap for individual particle-like excitations, 3) the Meissner-Ochsenfeld effect $(B=0)$, 4) effects associated with infinite conductivity $(E=0)$, and 5) the dependence of T_C on isotopic mass, $T_C\sqrt{M} = \text{const.}$", (BCS 1957, 1175).

into place after an accumulation of microscopic quantum models accounting for this or that aspect of superconductivity. Hence, the basis for BCS was a theoretical one; whereas the basis of Landau and Ginzburg was phenomenological.

BCS, like Landau and Ginzburg, recognise that any acceptable model of superconductivity must account for the following phenomenological aspects: 1) the energy gap; 2) the Meissner effect; 3) infinite conductivity; 4) the isotopic effect and 5) the second-order phase transition at the critical temperature. Their aim was to construct a model from first principles that would incorporate at least these facts.

The main theoretical obstacle in the way of building a microscopic theoretical model of superconductivity was the fact that the Sommerfeld-Bloch model (1928) gives a good description of the normal state but not of the superconducting state. In order to overcome this difficulty, BCS assumed that as a first approximation it is possible to neglect the correlations between the positions of the electrons. Also it is assumed that the electrons float in a field determined by the conducting electrons and the ions.

Another assumption is essential to construct the model. Cooper was working on correlations between electrons brought about by the interaction between the electrons and the vibrations in the lattice. This type of interaction is known as the electron-phonon interaction, and is said to be the vital element in superconductivity. This type of electron-phonon interaction leads to electron pairing (Cooper pairs). By making these assumptions, BCS were able to present a story that can justify why the superconductors do not comply with Sommerfeld-Bloch model. BCS say

> The electron-phonon interaction gives a scattering from a Bloch state defined by the wave vector k to $k' = k \pm \kappa$ by absorption or emission of a phonon of wave vector κ. It is this interaction which is responsible for the thermal scattering. Its contribution to energy can be estimated by making a canonical transformation

> which eliminates the linear electron-phonon interaction terms from the Hamiltonian. (The BCS 1957, 1176)

Having justified the use of a modified Sommerfeld-Bloch model and the proper approximations which can incorporate any mismatch with what is expected of it, BCS turn to how their model in general (as well as their model of electromagnetic properties) might benefit from such a modification:

> In the theory, the normal state is described by the Bloch individual-particle model. The ground state wave function of a superconductor is formed by taking a linear combinations in which the Bloch states are virtually occupied in pairs of opposite spin and momentum. (BCS 1957, 1176)

Now, the pairing of electrons would allow more than one electron to occupy the same state. That is because by such pairing, electrons are shifted from being Fermions to being quasi-Bosons. The importance of this point is obvious once we recall that electrons are Fermions, which means they are unlikely to coexist in the same quantum state. But we can model a pair of electrons as quasi-bosons. This assumption is the most important one, because, by claiming that the electrons can pair, BCS were able to present a Bloch-like model for superconductors.

Let us now turn our attention to BCS's model of just the electromagnetic properties of superconductivity. The BCS authors accept that the phenomenological models present equations that can fit the data. Nevertheless, they insist that these equations have a poor origin. They want to present a clear and straightforward derivation of them from a fundamental theory: quantum field theory. As we saw earlier, in the Londons' equations of current density and magnetic field, the connection with a fundamental theory, i.e. the derivation from diamagnetism, had two problems: the artificial division of the current and the separation between the electrons and the heavy mass of the ions. So, BCS first set out what needed to be done: to give a physical account of the partition of the current density. BCS suggested that such a separation can be interpreted as a separation of diamagnetism and paramagnetism:

$$\mathbf{j} = \mathbf{j}_p + \mathbf{j}_d \qquad (4{:}25).$$

So how can they derive this kind of equation using the quantum mechanical approach?

BCS at this stage needed a large number of theoretical tools to build their model. These tools were:

1) Previous physical theories such as quantum field theory, electrodynamics, diamagnetism, Maxwell's equations and thermodynamics. These theories are what legitimises the mathematical forms constructed by BCS to account for superconductivity. If BCS had failed to provide a good and tidy derivation using these accepted theories their model would not have qualified as a legitimate theoretical model stemming from a fundamental theory. Though this may not be the only criterion for being an acceptable theoretical model, it is a necessary one.
2) Mathematical tools: The theory uses the quantum field theoretical technique of second quantization, the Green function, and different types of special approximations such as Hartree-Fock type approximations. These are abstract tools that can help to manipulate the existing facts so that they can be seen as similar to a given part of an existing fundamental theory.

At this stage the model would need a story to justify the way the tools are used, and to connect the mathematical parts of the theory to the natural phenomena. An analogy here might be helpful. While the story the phenomenological model presents is strongly associated with empirical and phenomenological findings, the story the theoretical model gives is like a sculpture or a painting which the artist meant to be realistic but turned out to be surrealist or even abstract. The point here is that the story given by the theoretical model uses other accepted theoretical models that might be justified only if we accept the underlying concepts and principles from the fundamental theories.

Although generally physically motivated, the identification of the mathematical terms with different sorts of real features in the superconducting phenomena does not usually tend to give a good description of the phenomena. This point is important because it is this identification that gives the mathematical equations of the model a physical meaning.

The BCS model depends on a group of other theoretical models that can associate superconductivity with quantum field theory. Firstly, the Cooper-pairs (that the electrons in the superconducting state occur in correlated pairs that have the same quantum state). These pairs of electrons can be created through the electron-phonon interaction -- a second theoretical model -- which says that electrons interact with a lattice producing phonons. This idea was greatly supported when the isotopic effect was observed. Materials in nature are a combination of many isotopes, and the isotopic effect is the finding that the transition temperature depends on the isotopic nuclear mass. The idea that electron-phonon interactions are "primarily responsible for superconductivity" seems reasonable, because it implies that the vibrational motion of heavy nuclei plays an essential role in the formation of pairs of electrons. One must remember that the relation between the thermal vibration and conductivity was an established fact by that time.

The BCS model also tried to incorporate the idea of the two fluid model (a phenomenological model). As we have seen, this model assumes that we can imagine the superconducting material to consist of two kinds of fluids overlapping one of which is responsible for the normal state and the other for the superconducting state. The BCS model claimed that such a division can be obtained if we accept that superconductors exhibit both paramagnetic and diamagnetic current densities at the same time (equation

4:14 was modified into equation 4:25). By such an assumption they would be able to deal with each property separately.

This model can also help, in addition to the Cooper-pairs, in understanding the use of another theoretical model: that of the Fermi surface. This is an imaginary surface in k-space (spin-vector space) that separates the occupied energy levels from unoccupied energy levels and will define the first empty level.

The next step for BCS is to derive the relation between the current density and both the potential and the momentum. From these two equations all the other known mathematical descriptions of the superconductors' properties can be derived. For this derivation, using a quantum field theoretical framework, BCS needed to employ all the mentioned models and tools from fundamental theories.

But in order to complete the model they need to present a story. They start by suggesting a Hamiltonian for the electrons in the superconducting state. Then they add the isotopic mass, $M_{S,}$ and its relation to the phonon-electron interaction to see its effect on the non-diagonalised terms in the Hamiltonian. Then the diagonalised part was re-normalised using "Bloch energies". Introducing the idea of the Fermi surface allowed them to define the occupied states from the first free state. After the model employed these elements, the possibility of finding a straightforward derivation from quantum field theory is now in hand. The model started by using annihilation and creation operators, then Hartree-Fock like approximations and so on, and so forth. It arrived at a special kind of wave function which, by defining the correct Hamiltonian and accepting a certain gauge where $\nabla \cdot \mathbf{A} = 0$, in which $\mathbf{A} = 0$ if $\mathbf{H} = 0$, gave us a derivation for the paramagnetic and diamagnetic current densities:

$$\mathbf{j} = \mathbf{j}_p + \mathbf{j}_{d(}$$

$$\mathbf{j}_p(r) = \begin{cases} -\dfrac{e^2\hbar^2(2\pi)^{\frac{3}{2}}}{2m^2c\Omega^2} \sum_{i\neq 0} \sum_{k,q,\sigma} \sum_{k',q',\sigma'} (2\mathbf{k}+\mathbf{q})k' \bullet \\ \left[\begin{array}{l} a(\mathbf{q}')e^{-iq\cdot r}(\psi_0(T) \left| c^*_{k'+q',\sigma'} c_{k',\sigma'} \right| \psi_i(t)) \times (\psi_i(T) \left| c^*_{k+q,\sigma} c_{k,\sigma} \right| \psi_0(T)) \dfrac{1}{W_0 - W_i} \\ + \mathit{complex\ conjugate} \end{array} \right], \end{cases}$$

and

$$\mathbf{j}_d(\mathbf{r}) = -(\frac{ne^2}{mc})\mathbf{A}(\mathbf{r}), \qquad (4:25)$$

This, of course, can be accepted as a straightforward derivation from fundamental theory, namely quantum field theory. But BCS admit that their task would not have been possible had they not made special assumptions, particularly that of the formation of Cooper pairs, that came to be the cornerstone of their theory.

It should be stated at this point that both the BCS and the Landau and Ginzburg models can account for the most important properties of superconductors: i) the penetration depth; ii) the coherence length, which is "a measure of the distance within which the superconducting electron concentration cannot change drastically in a spatially-varying magnetic field" (Kittel 1986, 336) and iii) the energy gap. In the case of the BCS model, however, it needs a series of approximations to account for each of these three properties. The BCS model gives an account of these properties as a consequence of the current density equation 4:26. The results are:

1) The coherence length is $\xi = a\frac{\hbar v_0}{kT_c}$.

2) The penetration depth is $\lambda_L(T) = ({}^{4\pi}/_{\Lambda_T c^2})$.

3) And the energy gap is $\Delta(0) = 2\hbar\omega e^{-1/N_0 V_0}$.

Hence, the BCS model fulfils the criteria I suggested in chapter three. It is consistent with and derivable from one single fundamental theory (quantum field theory), gives a coherent story about type one superconductors, and can give an account of the existing empirical findings.

In this chapter I have presented an illustration of the theoretical dichotomy suggested in Chapter Three. This case study is very important to clarify the points raised in Chapter Three. Firstly, it gives a clear example of the horizontal division between high-level and low-level theoretical representations. Secondly, it shows that all models, whether theoretical or phenomenological, have common elements. Both kinds of model have a set of mathematical equations. They both present a description of either the boundary conditions or the environmental set-up. They both present a story that claims to account for the properties of superconductivity (at the time). And they both make use of previous fundamental theories (as tools or by derivation). Thirdly, and most importantly, it shows that the crucial difference between the two types of theoretical forms is their point of departure. While the theoretical models take fundamental theories as their basis, the basis for phenomenological models is the experimental and phenomenological facts. I will turn my attention now to the philosophical implications of such a case study.

Chapter Five

Phenomenological Realism

5:1 Introduction:
5:2 Superconductivity and Models
5:3 Phenomenological Realism
5:4 Bachelard: the epistemological terrain
5:5 Summary

> So, only too often, the philosophy of science remains corralled in the two extremes of knowledge: in the study of philosophers of principles which are too general and in the study of scientists of results which are too particular.[1]
>
> Gaston Bachelard

5:1 Introduction:

In this chapter I argue, using the information we have on superconductivity, that it is unlikely that any theoretical model can represent all kinds of superconductors. In contrast, the phenomenological model can, due to its flexibility, represent the behaviour of all superconducting materials. Hence, in this typical case in applied physics the phenomenological model is a better representative of nature than any theoretical models (section 5:2).

I use the features illustrated in this example to argue that phenomenological models are generally the best vehicles of representation of nature. With such a position I claim to be able to appeal to the no-miracle argument as an argument for realism, and also to be able to overcome the major criticism toward realism from the argument from scientific revolutions. This is the topic of section 5:3.

Section 5:4 will cast more light on the philosophical grounds of phenomenological realism and in what ways it is different from other kinds of scientific realism.

5:2 Superconductivity and Models

In the previous chapter I argued that Landau and Ginzburg model was built using a bottom-up approach, while the BCS model was built using a top-

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[1] From Bachelard 1968, p 5.

down approach. The standpoint of the Landau and Ginzburg model was the experimental and phenomenological facts; the standpoint of the BCS model was quantum field theory. The beauty of this case study is that both models aim to arrive at the same known mathematical expression, i.e. that suggested by London and London (Gordon's equations). They both admit that there is a need for plugging in some empirical results into the mathematical derivation in order to get the result they want, although they disagree on which of these elements to accept and the way they take these elements to affect the theoretical outcome. They both can account for the elementary properties of superconductivity (at the time). And they both make use of previous fundamental theories. Nevertheless, the BCS gives a totally different story than that given by the Landau and Ginzburg model, and the two overall models are distinctly different. Let me here discuss the differences between these two approaches.

Bardeen stated, back in 1956, that:

> Anything approaching a rigorous deduction of superconductivity from the basic equations of quantum theory is a truly formidable task. The energy difference between normal and superconducting phases at absolute zero is only of the order of 10-8 eV per atom. This is far smaller than errors involved in the most exacting calculations of the energy of either phase. One must neglect terms or make approximations which introduce errors which are many orders of magnitude larger than the small energy difference one is looking for. One can only hope to isolate the physically significant factors which distinguish the two phases. For this, considerable reliance must be placed on experimental findings and the inductive approach. (Bardeen, 1956, 276)

So Bardeen, who with Cooper and Schrieffer put forward the BCS model, himself admits that any theory departing from quantum theory would need to "neglect terms or make approximations which introduce errors which are many orders of magnitude larger than" the quantities one is looking for. The BCS model needs exactly these approximations to be able to account for practical situations.

By contrast, Landau and Ginzburg's model, relying on "experimental findings and the inductive approach", is able to present a mathematical structure that can be consistent with a representation of the phenomena, trying to relate different bits and pieces from the shattered information which were provided through years of experimentation.

The most important factor in counting BCS as a theoretical model related to a fundamental theory, is its use of quantum field theory, i.e. its use of a microscopic base for understanding a macroscopic phenomena. On the basis of that derivation the BCS model succeeded in achieving the following:

1) The interaction between electrons can lead to the separation between the ground state and the first exited state by an energy gap. This can give an explanation of the critical field, the electromagnetic properties and the thermal properties as a result of the energy gap.
2) The experimental quantity of the energy gap matches that of one measured from the electron-lattice interaction, which leads to electron-electron interaction.
3) The coherence length and the penetration depth are consequences of the BCS formation.
4) Also, as I have shown in the previous section, the London equations and so the central phenomena of superconductivity -- no resistance and the Meissner effect -- are explained.
5) The quantization of the magnetic flux in the superconducting ring, with a charge equal to 2e rather than e, which could be interpreted as a consequence of the pairing technique.

It should be stated here that the Landau and Ginzburg phenomenological model also depended partly on microscopic factors, and also they employed their knowledge of fundamental theories to construct their model. Yet nobody counted their model as properly related to a 'fundamental theory'. That was primarily because their derivation did not give a clear reason for taking the order parameter to be a wave function; and also because their derivation was not seen as a straightforward derivation from a previous fundamental theory. This attitude toward the two derivations can give us a clear idea of the difference between these two types of theoretical activity.

Interestingly, the Landau and Ginzburg model has proved to be capable of adapting to new properties of superconductivity whereas BCS has failed to account for these new discoveries, as we shall see next. In this sense I think that Landau and Ginzburg can give an example of the way phenomenological models can prove more fruitful than theoretical models.

So far so good, But why did the BCS model in spite of all its success fail to maintain its position as the accepted model of superconductivity? Up to a certain point the BCS model can be a reliable model in its predictions about superconductivity. This is especially the case if we are dealing with type one superconductors; that is the type of superconductors that has just two phases: normal and superconducting, with a perfect expulsion of the magnetic field (Meissner effect), and where the materials are simple metals. The problems facing the original BCS model started with the discovery of type two superconductors in 1960.

In 1957 the Russian physicists A. A. Abrikosov[2] published a paper saying that accepting the Landau and Ginzburg model entails that there might be another type of superconductor where the superconducting state can exhibit some kind of magnetic penetration through the superconductor. This was verified experimentally three years later. We now understand that the

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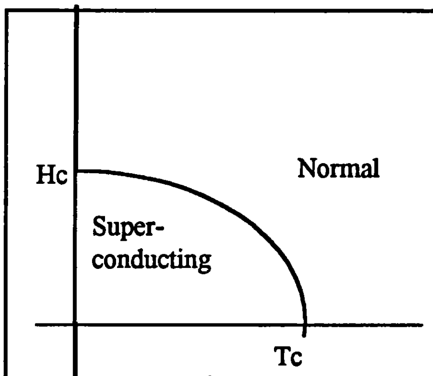
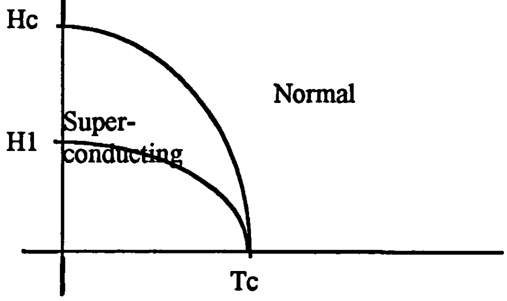
[2] Earlier in 1955 Abrikosov published another paper, also depending on the Landau and Ginzburg model. That paper had been forgotten for a long time. In it he claimed that some superconductors might carry enormous current values. This paper turns out to be very important in the generalised Landau and Ginzburg model that can account for high temperature superconductors.

superconducting state in type two superconductors is split into a perfect Meissner effect region under a certain magnetic field H_l and an ordered penetration of the magnetic field in some kind of vortex lines when the magnetic field is between H_l and H_c. (See figure 5:1)[3]



a) Type 1 Superconductor with perfect Meissner effect.



b) Type 2 Superconductor.

Figure 5:1

The BCS model managed, using further assumptions, to account for type two superconductors. But other kinds of superconductors, especially high temperature superconductors, which were discovered in 1986 by G. Bednorz and A. Müller, prove more problematic. It is important here to say that in all the interpretations of the BCS model concerning the critical temperature, the most optimistic one suggests 30°K to be the highest

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[3] For a simple discussion of this point, consult Bishop, Gammel and Huse 1993, p 28.

possible critical temperature. Now we have superconductors with (125°K) T_C[4]. So the BCS model cannot be seen as valid for all kinds of superconductors.

In a discussion between P. Anderson and R. Schrieffer (1991, p 54) on the difficulties facing a theory for high temperature superconductivity, Anderson says:

> I think few people realize that we now know of at least six different classes of electron superconductors, and two other BCS fluids as well. Out of these only one obeys the so called conventional theory -- that is, BCS with phonons that fit unmodified versions of Eliashberg's equations. (Anderson and R. Schrieffer 1991, 54)

These superconductors are:

1) Free-electron-like (s-p and lower d-band) metals. These all fit the theory and can be predicted.
2) Strong-coupling, 'bad actor,' old-fashioned 'high T_c' materials such as Nb_3Sn and $Pb(Mo_6S_8)$. These seem to have phonons, but they have many unusual properties in both their normal and superconducting states, and it would be rash to assume they fit simple theory. They have, for instance, peculiar magnetic properties in the normal states.
3) Organic superconductors. These are still almost a complete mystery.
4) Heavy-electron superconductors. These are now proven to be BCS-like but anisotropic-so-called d-wave superconductors, perhaps. No phonon mechanism is proposed.
5) $BaBiO_3$-based superconductors. These have phonons but cannot fit simple theory because their electron density is too low, Coulomb repulsion seems nearly absent and they have their highest T_c's at doping where

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[4] A good historical account of the developments in the field of superconductivity and high temperature superconductivity can be found in:
B. Schechter, (1989) and G. Vidali (1993).

conventional superconductors become normal, that is, at the metal-insulator transition.

6) High-T_C cuprates. In addition to their abnormal T_C's, these materials have very abnormal normal-state properties.

Anderson continues by saying that it is 'crazy' to think that the new high temperature superconductors can fit the BCS theory since even most of the simpler ones do not fit. He says:

> Back in the 1960s we may have created the abomination, a theory that has become 'nonfalsifiable' in the Popperian sense in that people insist on inventing more and more ingenious ways to make it fit any anomaly! (Anderson and R. Schrieffer 1991, 54)

In fact that was quite right on the theoretical level. Even a great physicist like A. Pippard said in 1964 about the success of the BCS model:

> This success is so remarkable that I almost believe you would forgive me if I were to say there now remain no problems in superconductivity. (Quoted by Vidali, p 99)

Nevertheless, most of the physicists of superconductivity were reluctant to use the BCS in practice, especially after they found that the Landau and Ginzburg phenomenological model could give them the same predictions with simpler mathematics. A survey of the textbooks on superconductivity can tell us about the role of the BCS. One of the most read textbooks was Michael Tinkham's book *Introduction to Superconductivity*. He writes, in 1974 (three years after the BCS authors got the Nobel Prize in physics for their work in superconductivity), that in his book

> The emphasis is on the rich array of phenomena and how they may be understood in the simplest possible way. Consequently, the use of thermal Green Functions has been completely avoided, despite their fashionability and undeniable power in the hands of skilled theorists. Rather the power of phenomenological theory in giving insight is emphasized, and microscopic theory is often

> narrowly directed to the task of computing the coefficients in phenomenological equations. (Tinkham 1974, the introduction)

In 1986 after the new discovery of high temperature superconductors, the BCS model was questioned and other theoretical models where suggested to give an explanation for the phenomena of superconductivity. So what did these theoretical models achieve?

Many theorists still want to accept the BCS model claiming that it is possible to develop a more general model that depends on the BCS assumptions. We saw that the BCS model needs certain assumptions to be consistent with quantum field theory and to be able to derive the needed mathematical form. I.e. the assumption that electrons occur in pairs in the superconducting state, that a derivation from quantum field of the paramagnetic and diamagnetic currents in superconductors is possible, and that the electron-phonon interaction is responsible for superconductivity. As I said all these assumptions are being challenged. In response, some theorists claim that some of the experimental observations can support these assumptions. These are:

1) It is an experimental fact that most superconductors, even those of high temperature T_C, have a “fundamental” charge of 2e. That can be a confirmation for the pairing technique suggested by BCS.
2) Some of the experiments conducted on high T_C superconductors indicate that there is an energy gap in the superconducting state.
3) Many of the new superconductors have the same properties as conventional superconductors, in particular: Josephson tunnelling and the vertex structure of type two superconductors.
4) The measurement for the penetration depth of the new superconductors agrees with the theoretical calculations using BCS.

At this stage it will be essential to specify what the protagonists in the debate mean by the BCS model. One of its authors, R. Schrieffer, suggests that the BCS is

> a microscopic field theoretic framework for treating a fermion system in which an effective attractive interaction brings about a phase-coherent pair condensate, with strong spatial overlap of fermion pairs. The energy of a single pair drifting relative to the condensate is discontinuously increased by the action of the Pauli principle. (Anderson and Schrieffer 1991, 56)

So, in Schrieffer's view, the BCS is reduced to just its structural relation and derivability from the microscopic field theory with a prime assumption of the effective interaction that brings about pairs of electrons. He wants then to argue that the BCS does not include all the assumptions (the energy gap, the Fermi surface, the interpretation of the two fluid model as a paramagnetic/diamagnetic currents, etc.) as a part of the theoretical model. All these assumptions go into the different models that can be constructed using the theoretical frame. He says

> I would submit that while many models are required to account for these widely different systems, in fact *a single* (his italics) BCS theory underlies the physics of **all** (my emphasis) the apparently distinct phenomena. (Ibid. 56)

In effect Schrieffer claims that the underlying theory for all kinds of superconductors is the quantum field theory. I cannot see how someone could believe in this claim unless he believes that the BCS model is merely its mathematical equations. If I am to be a structural realist I might be happy about such a claim; it is the mathematical structure that survives through scientific revolutions. To the contrary I think that a physical model needs to be questioned on its mathematical level and on its physical assumptions. I think that the BCS model, contrary to what Schrieffer claims, fails on both levels.

The BCS model cannot be accepted as a successful theoretical model unless we associate with it at least[5]: the Fermi liquid and Fermi surface assumptions, the electron pairs (Cooper pairs), the isotropic effect, and the single band assumption. Without these assumptions the derivation from the quantum field theoretical frame would not be possible and without which a successful explanation for the properties of superconductivity is not possible. The BCS authors wove these factors into the model through the 'story'. Each of these points can be questioned. In fact, the new theories depend on questioning this or that aspect of the BCS model, if not all the model.

Schrieffer himself is developing what he calls the 'spin-bag theory' of superconductivity where he tries to start from the same grounds as the BCS model. Nevertheless, the new evidence against the BCS would make it even harder for such an approach to succeed. This evidence includes:

1) Treating the Cooper pairs as entities that can be treated as quasi-Bosons gives rise to a deep theoretical discussion about the possibility of there being pairs of electrons the way BCS suggests[6]. And the experimental evidence for the pairing techniques is too complicated and can be interpreted in different ways, particularly in the case of high temperature superconductors where the materials are highly complicated and their structure is yet to be fully understood. Let us remember that Cooper pairs are essential ingredients of any BCS-like 'theory'.
2) The BCS model is concerned mainly with the superconducting state, while for the new superconductors it is their normal state that is more puzzling. So it is important for an acceptable theory of superconductivity to account for the properties of superconductors in the normal state as well as in the superconducting state. BCS fails to do this.

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[5] Schrieffer might have ignored this fact because he accepts that these factors are part of the quantum field theoretical frame. Even if this is the case, the model would not be its mathematical equations alone, and it would not be able to account for new discoveries.

[6] See for example: Y. Chen, F. Wilczek, E. Witten and B. Halperin, (1989).

3) The Cu-O materials have an anisotropic chemical structure; that means that the BCS assumption of neglecting the anisotropic effect and dealing with idealised materials[7] is essentially a wrong thing to do. Let us remember Bardeen comments:

> One must neglect terms or make approximation which introduce errors which are many orders of magnitude larger than the small energy difference one is looking for. One can hope only to isolate the physically significant factors which distinguish the two phases [normal / superconducting]. (Bardeen 1956, 276)

It seems that the BCS made the wrong choice by this idealisation.

1) The extremely high value of T_C, whereas the BCS model predicted the highest of 30°K.
2) Very small coherence length in high temperature superconductors, in comparison with the accepted coherence length limits in the BCS model.
3) A "close proximity of anti-ferromagnetic phases"(Burns, p 3), which is not consistent with the BCS.
4) The new high T_C materials do not fit Fermi liquid and Fermi sea assumptions.
5) The new high T_C materials must be treated as at least a two dimensional system. The BCS treats superconductors as a one-dimensional system.
6) High value for the energy gap that lies in the range $3.5kT_C$ to $8kT_C$ which is larger than the isotropic BCS value of maximum $3.5kT_C$.
7) More importantly, the chemical structure of the new high T_C materials is highly important in understanding and in measuring the properties of these materials. Also it is an essential factor of superconductivity. The BCS claims that superconductivity is not dependent on the structure. (BCS 1957, 1178)

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[7] BCS, p. 1178, "Our theory is based on a rather idealized model in which anisotropic effects are neglected."

Along with this long list of why BCS theory is not accepted any more, there are also other experimental constraints that have emerged from dealing with new high T_C materials. In an article by P. Anderson (1992, pp 1526-1531), he discuss exactly these experimental constraints on having a theory for high T_C superconductivity.

It is clear, then, that the BCS model fails to account for superconductivity. Also it is clear that the standpoint of the BCS model is the departure from the quantum mechanical theoretical frame, so the BCS model is a theoretical model as I have already characterised in chapter three. While BCS failed to account for the new experimental evidence, the Landau and Ginzburg model was able to adopt and to incorporate all the evidence within its generalised form.

I think that the major factor in such success is that the Landau and Ginzburg model, counter to the BCS, takes the experimental and phenomenological facts as its standpoint. The association of the phenomenological model with the experimental evidence, and the liberty the model leaves for some parameters to be measured experimentally puts it in a better position to represent new kinds of superconductors. Hence, the phenomenological model proved to be more able to represent nature than the theoretical model of superconductivity. Moreover, the phenomenological model is able to adapt with the new evidence in the field.

There are many suggestions for a revival of some kind of theoretical model for superconductivity[8]. By far the most important theoretical models in the field now are:

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[8] There is a huge industry of papers and books on how to ground a theory of superconductivity. This work was suggested by the discovery of high temperature superconductivity.

1) The spin-bag model which was suggested by Schrieffer and his group. This model can be seen as a modification of a BCS type model[9].
2) The resonating-valence-bond model, suggested by Anderson, which does not accept the Fermi liquid argument, and which does not take the BCS to be a base[10].
3) The Anyon superconductivity model which questions the presupposition of Fermi surfaces and Fermi liquids made by conventional theoretical models of superconductivity, and works from outside the quantum field theoretical framework[11].

The striking thing about superconductivity is that it is a phenomenon that has two distinguishing properties, zero-resistance and the Meissner effect (or magnetic vertex penetration), but these two do not have the same known origin. There are, as I already said six known types of superconductors and each of them has different normal state properties. The chemical properties of some are very complicated and give rise to many contradictory results. Theoretical models have so far failed to give a single generalised account for these different kinds of superconductivity. Many factors have been investigated in an attempt to account for superconductivity, but until now all of these factors appear to have experimental evidence against them (Anderson 1992).

Of course it is important for any theoretical model not to contradict any of the experimental observations that cannot be accepted as exceptions. Anderson urged this kind of position in addressing the BCS assumption that all superconductors are Fermi liquid type materials:

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[9] J. R. Schrieffer, X. Wen and S. Zhang, (1989) and J. R. Schrieffer and A. Kampf, Phys. (1990a) and (1990b).

[10] P. Anderson (1987), (1990); and P. Anderson, G. Baskaran, Z. Zou and T. Hsu, Phys. (1987).

[11] A collection of the important papers can be fond in: F. Wilczer (1990).

> Here I must appeal to a point of logic. The common response, when one makes a firm statement that all of these materials are not Fermi liquids because of one or another observation, is to say that the observation encounters exceptions among these many materials. But that is not the point: if they are all at the same fixed point -and they clearly are- it will be non-Fermi liquid for all if it is not for any one: it is necessary only to prove the negative in one instance. Exceptions are logically irrelevant. (Anderson 1992, 1527)

There is no one generalised theoretical model for superconductivity. Physicists in the field still use the generalised Landau and Ginzburg phenomenological model of superconductivity. This model can equip physicists with effective mathematical techniques to predict the behaviour of superconducting material or design superconducting devices. Also, it is possible to predict new properties out of the model (prediction of type two superconductors, prediction of high field superconductors, prediction of high current superconductors, etc....). The major factor that makes the phenomenological models so powerful is the fact that they are a first level abstraction departing from the experimental level.

The experimental observations now seem to indicate that it is improbable that we will be able to arrive at a theoretical model of superconductivity that departs from existing fundamental theories. This is because the essential assumptions for the candidate theoretical models have proven to be in contradiction with experiments involving this or that kind of superconductors. That leads us, if we want to continue to search for a theoretical model derivable from fundamental theories, to one of two options.

The first option is to say that the candidate theoretical models are no good for superconductivity but some other theoretical model will emerge that can account for all the aspects of superconductivity. Of course such a point of view does not tell us a lot, because theoretical models should, by definition, be derivable from previous fundamental theories. Hence, the predictions of the new theory should not be in contradiction with the well-confirmed predictions of the previous theoretical models. So this option will require a whole new theoretical approach, not just for superconductivity but for other domains as well.

The other option is to say that there is more than one theoretical model for superconductivity: one for conventional superconductors, one for high temperature superconductors, one for organic superconductors, etc.

I do not see either of these two options as necessary. I think if we accept that phenomenological models are the representatives of nature, then our theories will eventually be merely tools to help us in constructing new theoretical tools and new phenomenological models. I think that science and scientists will have more freedom by doing that, and that will help them to go beyond the theoretical limitations. After all, if all the physicists in superconductivity accepted the BCS model, we would have never been able to achieve high temperature superconductors.

The story of the physicist Bernd Matthias is in place here. Matthias was a German experimental physicist and chemist who had a special approach to how physicists should work. This approach was highly dependent on the experimental observations. He taught his students that approach saying:

> Let us look at so many instances of one given phenomenon that at least we can get a ... feeling for what the crucial conditions are. If we do this, then relying on the correctness of these conditions, we can make predictions. This is what I did in superconductivity.... . And the fact that these compounds become superconducting is a justification for this approach[12].

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[12] From Schechter, p49.

Some of his highly theoretical colleagues thought of his empirical approach as alchemy[13]. Nevertheless, his 'alchemy' proved fruitful and a school of this type of practice developed.

Matthias was known for his work on superconductivity. He was a very important experimentalist who discovered a large number of superconducting materials. He started a school of experimental work that put the emphasis on phenomenological ways of discovering whether certain combinations of chemical compounds can be superconductors. For this work he was described as the Mendeleev of superconductivity (Clogoston, Geballe and Hulm 1981, p 84). He did not believe that theories gave a true representation of nature. His disbelief in the use of theories in superconductivity, including the BCS, was based on a simple fact: 'No theorist has actually predicted a new superconductor, let alone its transition temperature' (Schechter, p. 57).

In describing the ways of finding materials with higher transition temperature, he rightly claims that all the increases in T_c were the result of experimental or semi-experimental approaches. Matthias accepts the phenomenological approach toward theoretical forms. Using this approach he developed several models.

One of these models was used to identify compounds that would have high transition temperatures. As mentioned earlier, the BCS model has failed to put forward any clear understanding of why certain materials could be superconductors, and, as a result, has failed to predict which material could exhibit superconductivity. Alternatively, Matthias and his colleagues[14] were able to apply the phenomenological approach to construct a model to find higher transition temperature superconductors. Matthias noticed that within the transition elements the ratio of the number of electrons to the area of the atom (e/a) is a primary parameter and for superconducting transition

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[13] Matthias was working in America where his kind of approach was not common.

[14] See Hulm, Kunzler and Matthias 1981.

elements would be between 4.5 and 5. Matthias was able to discover that Nb and V compounds would have high transition temperature (at least to what was known at that time), Nb_3Sn with $T_c = 18$ °K using this simple assertion. This assertion depends on experimental evidence and some theoretical assumptions like the isotope effect and its relation to superconductivity,

Matthias was a major rebel against the acceptance of the BCS model as the main model of superconductivity. He insisted that the claim made by the BCS model of the impossibility of finding any superconductors with a transition temperature more than 25 °K was simply wrong. He kept trying to find a combination of chemical substances that would disprove the BCS claim, but died in 1980 before finding one. Just before his death he predicted that oxide superconductors would provide a new type of superconductors. He was proved right. His prediction was examined by one of his students, Paul Chu, a Chinese Professor who studied under Matthias at the University of California, San Diego, and by two other experimental physicists at IBM Labs (Bednorz and Müller) who arrived at the first known high-temperature superconductors in 1986.

Matthias' attitude toward theorists can be summarised by this quotation from a lecture in 1974. He says that: theorists and their predictions:

> clutter up the literature, they confuse the mind, and they give all of us a bad image. Because if they predict, basing it on something, and then fail, we have only two choices. Either they [theorists] are stupid, which they aren't, or they predicted on the basis of something that isn't true. Now which of the two choices would you choose? (In Schechter 1989, p 47).

This attitude is exactly what allowed Matthias to continue his investigation to find new kinds of superconductors. If it were not for people like him many aspects of superconductivity would not have been discovered.

To conclude: we have looked at a theoretical model (the BCS) which had been accepted, as a legitimate straightforward derivation from a fundamental theory. We saw that this theoretical model failed first of all to predict whether a material can be a superconductor or not, not to mention that it failed to account for the new experimental evidence.

Alternatively, we have seen that a phenomenological model (Landau and Ginzburg) was able to account for all the known properties in superconductivity. It was able to give good grounds to develop predictions that proved to be right. It was able to account for the new experimental evidence. Although the model fails to give what can be accepted as a straightforward derivation from fundamental theory, this did not render it false.

5:3 Phenomenological Realism

> Yet it is to some such conclusion that we shall have to come if we wish to define the philosophy of scientific knowledge as an open philosophy, as the consciousness of a mind which constitutes itself by working upon the unknown, by seeking within reality that which contradicts anterior knowledge.[15]
>
> Gaston Bachelard

Up until now I have argued that a different kind of dichotomy on the theoretical level can be inferred from observation based on the practice in physics. I urged that such a dichotomy cut horizontally between low-level and high-level theoretical representations. Here, I want to argue that there is a space for a new kind of realism. In what follows I will not put forward arguments against realism. I believe that theoretical representations refer to real relations between objects, and that the no-miracle argument is the strongest argument in support of realism.

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[15] From Bachelard 1968, p 9.

Most realists accept that to overcome the pessimistic meta-induction a kind of dichotomy is needed on the theoretical level. In accordance with my suggestion of a horizontal dichotomy on the theoretical level, I accept that low-level theoretical representations do often survive and that they have a better chance to do so than high-level one. I call such a position phenomenological realism.[16]

The overall strategy of phenomenological realism could be described as being a realist at the empirical and phenomenological levels only. It might be cast as a midway approach between instrumentalism, which asserts that theories are mere instruments, and realism which claims that theories are representative of nature, or approximately so.

Although theoretical models share with phenomenological models some of their overall characterisation, the phenomenological models have better stakes for being accurate representation of nature. This contrasts with the theoretical models which, due to their need to present a neat and tidy derivation out of 'fundamental theories', have bad stakes.

Although I take the mathematical bases of the so-called 'fundamental theories' as tools to construct models (theoretical and phenomenological), my position is not an instrumentalist one. This is because: it accepts low-level theoretical models, which I call 'phenomenological' models, as a real representation of nature, which implies that it accepts that some theoretical descriptions do refer. Instrumentalism usually asserts that theoretical representations at all levels are mere instruments.

I agree with Worrall that there are two major arguments in the realism/anti realism debate: the no-miracle and the pessimistic meta-induction. So, in order to defend my position I ought to show the ways it deals with these two arguments.

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[16] It is important to say here that I am using the physicists' notion of 'phenomenological' not that of the German philosophical tradition.

Let us start with the no-miracle argument. Usually, realists would say that it is impossible for our highly empirically successful theories to be true 'without what theory says about the fundamental structure of the universe being correct or 'essentially' or 'basically' correct'. (Worrall 1989, p 101) I put it in a different way. I would say that it is impossible that the low-level theoretical representations to be true without what these representations say about nature being correct.

Phenomenological models, as characterised in chapter three, are generally the best vehicles of representation. These models, as we saw, are highly related to the empirical findings. So, it is more probable that what they say about nature is correct. It must be said, that even if the phenomenological models, when constructed, take the phenomenal facts as their starting point. They should, nevertheless, encompass more than just the facts they start off with. For they ought to be able to predict unknown properties of the phenomenon. This is an important point, because it is obvious that a theoretical representation can get an empirical fact right if it was constructed taking this empirical fact as a presupposition. So, in order to give the no-miracle argument a weight in favour of phenomenological models, it is important that these models are able to predict properties of the phenomenon not yet known.

As we saw in the case of Landau and Ginzburg's phenomenological model, it was able to predict, before it was observed, the existence of type two superconductors. So, in this case it would be impossible that the Landau and Ginzburg's model was able to predict yet unknown properties if what it says about nature is not basically correct. This argument is less problematic because it is the positive argument for realism.

Let me take the other argument: the pessimistic meta-induction. This argument says that deep changes in accepted scientific theories make the assertion that the old theories approximate the new only possible by stretching the concept of approximation to beyond its breaking point. This argument is a strong objection to the realist doctrine. But it is aimed more at the high-level theoretical assumption rather than the low-level ones.

In the example given chapter three (3:4), we saw that the change on the high-level theoretical assumptions, between accepting the three dimensional absolute space and absolute time and accepting the space-time curvature, did not affect the truth status of the low-level theoretical representation. Physicists still use the phenomenological models associated with Newton's theory because each of these models is related to a specific phenomenon. These phenomena are still the same.

Scientific revolutions affect deeply, most of the time, the high-level theoretical concepts, relations and basic point of view. In the Newton-Einstein case, the programmes that the two theories aim to fulfil are totally different. Newton wanted a unification of Kepler-style and Galileo-style mechanical models, while Einstein was concerned with the unification between energy and mass. On a high-level theorisation, this change can affect the whole theoretical frame. Another factor on that level is the mathematical information. During the eighteenth and nineteenth centuries the mathematical language developed rapidly and new concepts were suggested. These concepts were employed in constructing the new Einsteinian framework.

I claim that a change in theory should be seen as a change in the tools that are used in constructing models. Let me clarify this point. If a phenomenological model is known to represent a certain situation, like the movement of a box over a rough surface, then any latter theory should aim to produce a model out of its first principles to represent this movement if it falls in its domain. That is, to produce a theoretical model which approximate the same basic known equation. In the case of Newton's theory, this would be, in a way, by a straightforward application of Newton's second law. In Einstein's case, this is done by first making some kind of correspondence to correspond the new laws to those of the old. Then we are able to claim that the Einsteinian theory captures the basic equations of the known model.

Now, looking at this point from phenomenological-model-construction point of view: let us assume that we do not have a model to represent the movement of a box on a rough surface, and we have two theories, that of Newton and of Einstein. Then we can see that there is more than one tool to be used in the construction of a model; but, at the end of the day, it is not important whether we arrived at the model using the first tool or the second tool. The end phenomenological model is the same. Let me give an analogy:

Building a radio (transmitter and receiver) can be done in two ways: using simple tools in a school laboratory: board, transistors, capacitors, resistors, microphone, speaker, ...etc.; and with a simple explanation of the use of such tools, one can build a radio. The outcome will not be a brilliant manufacture but it will enable us to demonstrate the idea of sending and receiving a signal without wires up to 500 metres. The second way is by using the latest technology to manufacture a radio. It turns out that many of the previous tools are redundant in favour of IC (integrated circuits), and the human agent might not be needed directly -- the computer robot will do the job. Of course the outcome of such manufacture is superior to the laboratory radio. But still the outcome in both cases is a radio, and both will demonstrate the same phenomenon of transmitting and receiving signals without wires. The point that I want to make is that whether we used the more advanced tool or the simple one the outcome radio will demonstrate the same phenomena.

A phenomenological model, as I mentioned in chapter three, ought to give a description of the environmental set-up of a phenomenon, and to present a story that gives an explanation of the relations between the different elements. This story is also important in relating the mathematical structure of the model with the phenomenon. In the cases related to a moving body on a rough surface or building a bridge, the environmental set-up does not change (even if it is not the same elements that are involved. I mean here that the material used in building bridges changes because our knowledge of the physical properties of materials has developed. But that need not change the overall model that the bridge building depends on).

One might say that Newton's laws, like F = ma, were not about a certain restricted domain of applicability, but they were "for *all* material objects moving with *any* velocity you like" (Worrall 1989, 104). This cannot be an objection to the phenomenological models associated with Newton's laws. This is because in these models the domain is restricted by the description of the environmental set-up of the phenomenon. These models are local. So, phenomenological models associated with Newton's laws are not for *all* material moving with *any* velocity. This locality of a phenomenological model is an essential aspect of it.

In addition, it is highly probable that a phenomenological model can be modified to account for new findings. The flexibility that the phenomenal standpoint gives to the phenomenological model helps it to change in accordance with the available evidence. But this flexibility does not make the model a mere data model. The associated theoretical description elevates it from a mere data-model to a low-level theoretical representation. But why are phenomenological models flexible?

Phenomenological models give a simple theoretical representation of the empirical evidence. I claim, they present a correct picture of nature. Also, they have elements that are plugged into the mathematical part from empirical findings. These elements can vary depending on which factors are crucial to the system under study. Now if new evidence appeared, it might be incorporated into the empirical level and then plugged into the model. Sometimes this cannot be done. In this case a further modification to the model might be needed. It is highly probable that these modifications will not alter the basic assumptions of the model.

Of course, I cannot give a general claim that all phenomenological models would be able to incorporate new evidence, but I might say that successful phenomenological models do. In the case of the Landau and Ginzburg model, the physicists were able to modify the model to account for the new findings in the field of superconductivity without the need to change its basic assumptions.

Let us take another point. As I explained in the introduction of chapter three, physicists have a reasonably good understanding of what they can accept as having a clear physical meaning. In the case of the displacement current there is a widespread agreement between physicists that both the electrical field and polarisation have a physical meaning, but they also know that the displacement current is a mere mathematical tool that helps them to simplify the calculations. The 'physicists intuition' and the 'physical insight' are but some of the techniques that are used by physicists to insure that any concepts, without 'clear physical meaning', would not be mixed with true representative concepts.

One might say that even simple concepts might not have a 'clear physical meaning'. The 'mass' concept, to take but one important example, has changed dramatically from the Newtonian view to the modern view. How is such a concept viewed from a phenomenological realism point of view? The phenomenological models that use the concept of 'mass' view it from a phenomenal perspective. That is to say that in each phenomenon, like a box sliding on a rough surface, the object (box) has a mass.

Our accumulative experience of the concept of mass will no doubt be part of the phenomenological model's intuitive concept of mass. Our experience started with the early observations of human kind: that 'the big' is not necessarily the 'massive'; that weighing an object can give a more 'objective' observation of its 'mass'; that the relation between weighing an object and the angle of weighing is important and that there is a relation between the shape and density of an object and its 'mass'. All these elements will be intuitively part of the concept of 'mass' employed in the phenomenological model of the box sliding on a rough surface. Nevertheless, in all this accumulative experience there is no place for the Newtonian theoretical concept of 'abstract mass'.

One might argue in response that when we use a mathematical equation, even a simple one, e.g. one that specifies that $[F = mg\cos\theta - R = ma]$, we are then speaking of an abstract mass. The reason, I take it, is because the equation $mg\cos\theta$ has been obtained due to our assumption that the box has a centre of mass, and there is a weight trajectory from that centre of mass toward the earth = *mg*. But it is an equally plausible account that says that the box mass, whether it is the centre of mass or not, has a presumed weight trajectory = *mg*. The important point from a phenomenological point of view is that the box on the rough surface has a mass, this mass has a weight trajectory toward earth equal to *mg*, and the effective weight that helps in the movement of the box on the rough surface is equal to $mg\cos\theta$. Here we are using 'theoretical' information that is not related to any cohesive picture of the universe. Whether there is a negative mass, or an abstract mass, or the mass of the box is pictured as concentrated in a point called 'the centre of mass', these are abstract theoretical assumptions that go beyond those needed by the phenomenological model.

Another objection is in place here. The structural realist, and most realists, might say: if you accept that the low-level theoretical representation, which uses concepts like **E** and **P**, is a representation that holds true about nature, then why can't we extend this truth to the high-level theoretical representation? Why, if phenomenological realism accepts that the low-level models are the vehicles of representation, should we deny this to theoretical models?

First, the pessimistic meta-induction shows us that a lot of theoretical forms that we thought had been verified turned out to be false. It is by no means possible to accept concepts such as absolute space, absolute time and action-at-a-distance as any approximation to the space-time curvature. And to accept that the high-level theoretical forms can represent or approximately represent nature, the new entities ought to approximate, at least, the old theoretical concepts. In the superconductivity case study, theoretical models like Cooper pairs or the Fermi sea are questionable. This leads us to reject the high-level theoretical forms as a true representative of nature.

Second, concepts like **E** and **P** appear in phenomenological models because they are, if not measured empirically, easily verified out of the empirical measurements. As we saw in the example in the model of the electrical field, **D** did not appear in the mathematical part of the model even if we accept that it might be used to arrive at that model.

Third, the process by which the low-level theoretical representation is developed is totally related to the phenomenal level and gets its representative weight out of this association. The high-level theoretical forms, most of the time, function to keep the coherence of the theory; they help in simplifying the mathematical calculations of certain properties which are otherwise difficult to attain.

To take another point, it might be said that scientific revolutions depend on a conceptual shift between the old and new theories. In the case of light the shift was from accepting that light consists of particles to saying that light is a wave. But a model or a theory that does not make any assertion about this matter would not be susceptible to the argument from pessimistic meta-induction. The structural realist dichotomy between structure and content would be a sufficient reply to such argument. It does not matter if the light is composed of particles or of waves, as long as the structure of the old theory is preserved.

But, as we saw the structural realist reply has problems with the real test field: physics. As Worrall himself indicated, although the idea of the composition of light was changing, "there was a steady basically cumulative development in the captured and systematised empirical content of optics" (Worrall 1989, 108). Without any direct impact from doctrines about the constitution of light, the scientific models were dealing successfully with refraction, diffraction, interference, reflection, polarisation and the photoelectric effect.

Yes, the theories of the time played an important role in building these various models, but, at the end of the day, the models did not use the abstract theoretical explanation in the models. These models are not mere empirical models. The cumulative development was not associated only with the empirical findings. The development on the theoretical level between the Newtonian picture of light to Fresnel's theory to Maxwell's electromagnetism to Einstein's photons to de-Broglie's wave-particle duality, was associated with, if not generated by, new experimental evidence.

At each stage the scientific community was able to produce an effective model to deal with the new properties of light. At each of these stages some of the tools of the theory of the day were employed. These models are qualified to be phenomenological models. In these models the theoretical input is important, but it is not as crucial as the effect of new experimental evidence. And most importantly in my differentiation between phenomenological and theoretical model, the theoretical input to the phenomenological model is not as crucial as that to the theoretical models.

Worrall argues that the important factor in the success is due to the structure of the theories, which captured the real relations between things, or, to put it in his terminology, that they "must somehow or other have latched onto the 'universal blueprint'." (Worrall 1989, 101) Nevertheless, the success depended on more than the structure. The possibility, in the cases that were not yet observed, of building an experimental set-up that can capture the predicted phenomenon contributed to such success. The possibility of associating some of the theoretical concepts with real properties, i.e. content, was also important. Worrall himself admits, although in a footnote, that the theories were "also *guided* by what was already known empirically about light" (Worrall 1989, 115 my italics). Therefore, the success cannot be reduced to one factor: the structure.

In accordance with the points raised in this section I conclude that: in the case study discussed in chapter four, the Landau and Ginzburg model is an exemplar of phenomenological models. It illustrates various points discussed here. First, it allows us to deploy the no-miracle argument: it is no miracle that the Landau and Ginzburg model is able to predict unknown properties of superconductivity because it is a real representative of nature. Second, the Landau and Ginzburg model is a phenomenological model of a typical phenomeno-technology type of phenomena. Superconductivity needs a special environment, that of low temperature (either under liquid Helium or liquid Nitrogen). Also new kinds of high-temperature superconductors need a lot of technical work before they would be able to exhibit superconducting properties, to the limit that the same technical procedure might produce a superconductor and might not. This might depend, according to one interpretation, on the number of electrons in the specimen. Third, the Landau and Ginzburg's model, because of its low-level theoretical frame, survived through new discoveries.

Let me now turn my attention to a different issue. I think that Bachelard's philosophy is an important source of inspiration.

5:4 Bachelard: the epistemological terrain

In the last section I argued that, in most of the cases at least, the models which are still in use, and which were constructed by using tools out of 'false' theories, are the phenomenological models. In general these models play the role of mediation between theory and experiment. In this section I will turn my attention to draw the lines of similarity between my project and that of the French philosopher of science Gaston Bachelard. Bachelard's aim was to reconcile rationalism with empiricism. This is in a way analogical to my project of reconciling instrumentalism with realism.

I will start from the last point raised in the last section. Bachelard thinks that scientific thinking is essentially a dialectical interaction between the mathematical and the experimental. As he puts it in his poetic language:

> To think scientifically is to place oneself in the epistemological terrain which mediates between theory and practice, between mathematics and experiments. To know a natural law scientifically is to know it as a phenomenon and a noumenon at one and the same time (Bachelard 1968, 6).

To my mind there is no noumenon behind the phenomenon: the phenomenon is the noumenon. So by saying that it is important to know the noumenon and the phenomenon at the one and the same time we in fact are saying, if we take the physicists' understanding of the phenomenon, that the phenomenon is all there is. The whole business of searching for the 'structure' (or the blue print) of the noumenon is not important. Phenomenological realism is about the things that exists out there with and without our intervention - not only what exists without our intervention. This is of course a point of disagreement between my realism and that of structural realism. Structural realism claims that we cannot access the relata, Zahar says:

> In a unified and highly confirmed theory, the basic relations mirror reality while the relata themselves remain inaccessible to us (Zahar 1994, 1)

The only reality, in his sense, is the reality of relations between unknown objects. But in the scientific practice we, as humans, sculpt the object. Let me give an example of how the physicists construct a superconducting quantum interference device (SQUID). I will not go into the details but I will give the general strategy. First the physicists decide which materials they want to use. This depends on the kind of SQUID they want to build, its transition temperature, its magnetic properties and so on. They then smash these materials and heat the compound to 1500 degrees. Then they reduce the heat gradually in a very carefully monitored way so that the temperature goes down by 150 degrees every hour. At 900 degrees they dig a hole inside the specimen. And then they continue to reduce the temperature until it is back to room temperature. The SQUID would work after that by putting it in liquid helium or liquid nitrogen (and connecting it with an electrical circuit). In this case we know the structure (I mean here the mathematical structure, in accordance with what structural realism would accept as structure) of the mathematics governing the making of SQUIDs (remember that to construct a SQUID you need a model of such construction). Also we know how to calculate their magnetic and current specifications through our knowledge of superconductivity. But also we know the content related to such structure, the objects themselves, because we construct them and we know how to manipulate them.

Let me now turn to another point. In practice, we do not restrict ourselves to a coherent nature, which forces a coherent science so that every law of science must be part of a harmonic unified theory. We try to present, using the best of our descriptive abilities, local models and pictures that can provide us with understanding. Much of the real development in modern science was not related to the huge effort delegated for the unification of all theories in physics, but was related to technological developments and to phenomenological developments in sciences. I do not want to undermine the role of theoretical physics. But the role that I think that theoretical physics can play is not that of finding the theory of everything. This was and is still a dream for many physicists. But the facts are that, whenever a major question in physics is answered, thousands of other questions will occur. The conflict between human and nature is one of no end. Our rational activity affects nature as much as nature affects our rational activity.

I do not mean that the rational construction of science is all that we want to know about. Scientific activity is related to both rational and experimental activity. Most philosophers of science do not deny this. But the important thing here is to see on what we should put the emphasis. For me, the emphasis is on the latter. Also scientific theories are theories about nature. Now scientific practice is an active practice toward nature. The dialectics between our rational observation and nature itself is what leads us toward a theoretical structure of which it is reasonable to say that it goes some way toward providing the laws which govern nature.

I agree with Bachelard that there is a dialectical relation between the rational and the experimental. Bachelard supports a reconciliation tendency between the empirical and the rational. In his view this is a way of bringing together empiricism and rationalism:

> So, if one could translate into philosophical terms the double movement which at present animates scientific thought one would perceive that there has to be alternation between a priori and a posteriori, that empiricism and rationalism in scientific thought are bound together by a strange bond, as strong as the bond which joins pleasure and pain. Indeed the *one triumphs by assenting to the other:* empiricism needs to be understood; rationalism needs to be applied (Bachelard 1968, 6).

The important point for Bachelard is to find reconciliation between mathematics (or better: mathematical physics) and experiment. He says that the relation between mathematics and experiment developed through extended solidarity. When the experiment comes with a new message about a new phenomenon, the theoretician starts to modify the current theory, in the hope that it will provide an explanation to understand the new phenomenon. Through this modification - which is a posteriori indeed - the mathematician provides a theory that, after it is partially modified, explains the newness (Bachelard, 1981, p28).

On the other hand also the experimental physicists will try to search for the correctness of a theoretical prediction when what the theory predicts has not been tested yet. So the practice of science depends on both sides of scientific activity at the same time, without saying that the theory will lead experiment or that the empirical findings are all that matters. Bachelard summarises the correlation between the theory and experiment in two questions: Under what conditions can we explain an accurate phenomenon? (Accurate here is an important word, because the mind is essential for accuracy.) And under what conditions can we provide real evidence to support the validity of a mathematical system for a physical experimentation? So the new scientific spirit needs two kinds of certainty. First the certainty that the rational is associated with reality in a way that allows the rational science to deserve the name scientific realism. Second, the certainty that rational arguments, which explain the experiments, ought to be already part of our experience. Bachelard wants of his position (applied rationality) to assert that there is no rationality in vacuum, and no disjoint experience (Bachelard 1981, p31-32).

I also agree with Bachelard in rejecting conventional realism because of its static frame. On the contrary he believes that:

> The experimental as well as the mathematical conditions of scientific knowledge change with such rapidity that the problems confronting the philosopher are posed differently every day. To follow scientific thought, one must reform the rational frameworks and accept new realities (Bachelard 1968, 42).

Bachelard thinks that conventional realism puts the realism of laws above the realism of things and of facts. He thinks that there are, as an addition to the realism of things and of facts, two faces for reality. On the one hand it is realising the rational, i.e. trying to realise a predicted phenomena out of a theoretical representation. This is practised in science through phenomeno-technology; these phenomena if they were not suggested by the scientific activity, would not be experienced. And on the other hand there is the technological realism (Bachelard 1984, p 5), which reveals properties of nature even without our fully understanding those properties, and which leads us to search for new models and new structures. So we are not revealing the underlying structure of the world, as structural realism claims, because the phenomeno-technology which we are realising through our science would not demonstrate itself without our intervention and therefore its reality is not independent of our intervention. This would not mean, as some might think, that Bachelard's position is an anti-realist one. Because he thinks that there is another level of reality: the level of realisation. Our intervention is not something out of reality; it is in reality and hence a part of that reality. This leads him to the notion of objectivity. He says:

> One does not point to the real, one demonstrates it. This is true particularly in cases involving an organic phenomenon of some kind. When the object under study takes the form of a complex system of relations, then it can only be apprehended by adopting an appropriate variety of methods. Objectivity cannot be separated from the social aspects of proof. The only way to achieve objectivity is to set forth, in a discursive and detailed manner, a method of objectification (Bachelard 1984, 12).

Of course there is the well-known interpretation of Bachelard which concentrates on the 'social aspects of proof', that is the social constructivist's interpretation. According to that view, the real challenge to the realist position is captured in the questions.

> How can experimental or experimentally tested knowledge, including its theoretical explanations, "be about an independent reality," if the phenomena on which it is based (the experimental processes and results) do not occur "naturally" but have to be artificially produced and maintained in special laboratories? If not only the knowledge of experimental processes and results but even their existence is essentially dependent on the work done by

> human beings, how can we refers to human-independent reality? (Radder 1996, p 77).

This type of interpretation concentrates only on the social aspect of the phenomeno-technology. Bachelard, as I read him, does not challenge the reality of the phenomenon as part of nature. Instead he challenges the claim that it would have been possible (even in principle) for such a phenomenon to be produced in nature without human agency. Reality for Bachelard is the reality that puts the human in the centre of it, not as a passive observer but as an active agent who helps in pushing the potentialities of nature to its limits. Bachelard asserts that any phenomeno-technology would not be possible in principle unless the environmental set-up, which is a human-dependent set-up, occurs. For example, neither electrical networks nor the electrical currents running through them, either in America or in South Africa, would have been possible except via human activity. This is because in all cases when you want to build an electrical network and to get an electrical current to run through it you need generators, which are essential human-dependent technological equipment.

I think that a realist position can accommodate Bachelard's point of view. How? Well, by accepting that human activity itself is part of nature; that is to say that human-dependent nature is part of reality. This is a simple answer to a very complex issue, but I do not see why, our activity which is embedded in nature and which is totally related to nature ought to be thought of as unreal. Hence, the reality of phenomena includes the reality of the human agent that helped the potentialities in nature to exhibit themselves, these potentialities which would not have exhibited themselves without human intervention. Bachelard characterised the development in scientific discovery as follows:

> The essence of scientific psychology would then lie in the reflection whereby experimental laws are transformed into rules for discovering new facts. This is how laws become co-ordinated

> with one another and how deductive thinking is introduced into inductive science. Scientific knowledge accumulates, one might say, without taking up additional room in the mind, and this is one difference between scientific knowledge and empirical erudition: Science uses tested methods to filter facts (Bachelard 1984, 136).

Bachelard's main interest is in saying that the real practice of science is to build a mathematical model which captures the phenomena, and is able to predict new phenomenon, new reality through experience (experiment). There is no simple reality studied by scientists, whether it is object, phenomenon or event. The scientific reality is a coupling between a mathematical model and a technological structure.

5:5 Summary

In this chapter I argued for a new kind of realism, departing from practice in physics, which is able to retain the no-miracle argument and to overcome the argument from pessimistic meta-induction. Phenomenological realism asserts that phenomenological models hold a true representation of nature while high-level theoretical forms fail to do so.

Chapter six

Bohr and his Debate with Einstein

6:1 Introduction

The Bohr-Einstein debate is one of the most important and the most highlighted debates in the history of physics. Although the points of dispute are very important, it might be said that the crucial factor that makes the debate so important is the fact that it is between the two individuals that we associate with modern physics: Einstein for his general and special theory of relativity and Bohr for his association with quantum mechanics and the atomic theory. In spite of all the attention given to this debate, some puzzling points are yet to be understood.

For one, why, in spite of its clarity, was Bohr's starting remark on Einstein's thought experiments: 'I cannot understand it'? Bohr had a very good idea about Einstein's complaints and yet it seems he could not understand Einstein's simple presentations! Another point is related to the great effort Bohr made in restructuring Einstein's thought experiments. Why was it important to Bohr to restructure the experiments? Had Einstein missed any point in his presentation or was there another important factor? These questions and the type of realism that Bohr might accept are the topic of this chapter.

In the Bohr-Einstein debate, Einstein tried to highlight the theoretical difficulties of the quantum theory. When the matrix formulation was introduced in 1927 and Born suggested his probabilistic rules, Einstein was deeply worried about the probabilistic nature of quantum mechanics. "God doesn't play dice" was his famous reaction.

The Bohr-Einstein debate started officially in 1927. The first round was conducted during the period between 1927-1930[1]. There they discussed the following points: the uncertainty principle, wave-particle duality and the complementarity principle. The second round took place in 1935 when

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[1] An earlier round of debate took place during the early twenties. I will not comment on it here See Pais (1993).

Einstein with two other collaborators (Podolsky and Rosen) wrote their famous paper: "Can Quantum-Mechanical Description of Physical Reality Be Considered Complete?" which is known as the EPR paper.

The second round has influenced and activated a major part of the philosophical and physical debate about the foundations of quantum mechanics. It was accepted as a Bohr victory over Einstein. Later, Bell's papers[2] on EPR opened the possibilities for many real experiments in quantum optics and quantum mechanics. EPR -- especially in Bohm's version of it (1951, 1952 and 1958) -- touched very sensitive points: the formulation of quantum mechanics, locality, causality and the completeness of quantum mechanics. When I wrote my masters' thesis, the EPR debate helped me to articulate an important question which is still at the base of my studies, maybe in a more complex form. That is of the relation between theory and experiment in physics and the relation between our knowledge about the world and the world in itself. At that time I looked into one angle of this relation, that of the relation between thought experiments and real experiments. I compared the EPR thought experiment with the realisation of the EPR-B[3] by Aspect & al.

In the previous chapters I took that point to a new horizon: the relation between both theoretical and experimental work and the phenomena in nature. The main idea, which I will argue for in this chapter, runs as follows. Einstein and Bohr are theoretical physicists who express two types of theoretical approach. While Einstein was a fundamental theorist who approached theory-building on top-down bases, Bohr was a bottom-up theorist. I argue that the crucial difference between Bohr and Einstein was the *point of departure* from which we ought to build our theoretical understanding of physical phenomena and the *process* by which such theoretical representation is built.

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[2] See Bell 1964 and 1966.

[3] EPR-B refers to Bohm version of the EPR experiment.

Einstein believed in a unified theory. This kept him thinking that there will be a simple and beautiful theory that must entail all phenomena of nature. He believed that the mathematical structure of the unified theory is the real representative of the universe. Einstein wanted the theory to give complete answers for all the problems facing it without use of any other method. His examples originated in the theoretical structure of quantum mechanics. Bohr on the contrary starts from putting the hypothetical situations into plausible, if not possible (at the time), experimental set-ups and then taking quantum mechanics to be a tool to build models of these set-ups.

In the debate Einstein tried always to prove the inconsistency of the quantum theory by starting from its premises to arrive at a hypothetical experimental situation where the inconsistency is visible. Such an attitude is the direct opposite of Bohr's. Bohr always, as I will argue, started from the experimental and phenomenal to build the theoretical representations and descriptions of physical reality which can be expressed in a model: a phenomenological model. The mathematical schemes do not give us the descriptive power but the models do.

Bohr's concern was mainly about the type of story associated with the phenomena in quantum mechanics. His concern wasn't the formalism of quantum mechanics, but of the physical meanings that would be ascribed to such formalism. So, the important point for him is to find the correct way in which the formalism can model any suggested experimental set-up. I agree with Henry Folse[4] that Bohr is a kind of a realist, and I will show through his debate with Einstein that his worries were from a realist standpoint. But his realism is not compatible with that of other realists at his time. This disagreement between his standpoint and that of other realists has given the impression that he is an instrumentalist.

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[4] See his 1985, 1986, 1989, 1990, 1993, Henry Krips also claims that Bohr has a kind of local realism (Krips, 1993)

I think that Bohr, like the vast majority of physicists, knew well what he should accept as a representative part of a theory from its mere instrumental part. I will advocate that if a realist position can be attributed to Bohr then that position should be, if any thing, a kind of phenomenological realism. Let me first turn to give a more careful look at his arguments and concerns.

6:2 Bohr's Philosophical Grounds

In order to understand Bohr's position in the Bohr-Einstein debate it is important to understand his philosophical position. Bohr holds an idea that any complete theoretical description ought to have a pictorial description that would relate it to the physical system. This idea is cashed out in quantum mechanics by the quantum postulate (section 6:2:1) and the complementary principle (section 6:2:2). The complete theoretical description is related to natural phenomena. Bohr accepts that the new physics forces a new understanding of natural phenomena (section 6:2:3). These elements, along with his idea of objectivity, would shape his attitude toward realism and knowledge (section 6:2:4).

I claim that Bohr accepted a dialectical concept of knowledge that stresses the importance of human intervention as a part of objective reality. He accepts that what he calls 'theoretical descriptions' are real representations of nature. Such 'theoretical descriptions' are not related, according to Bohr, to one theory but use all accepted theories in physics as tools to construct them. These 'theoretical descriptions' are in my terminology phenomenological models. Bohr accepted that there is another important element in these theoretical constructions: the pictorial representation of a model. These philosophical positions are what shapes Bohr's idea of realism.

6:2:1 The Quantum Postulate

Bohr took the quantum postulate to state a major difference between a classical system and a quantum system: quantum systems have an inherited property of being discrete. What is the quantum postulate? According to Bohr the quantum postulate

> attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to the classical theories and symbolised by Planck's quantum of action (Bohr, 1928 89).

Bohr adopted such a position as early as 1913, though he did not formulate its importance for quantum mechanics until Heisenberg constructed the matrix formalism. A puzzling dilemma for Bohr was to reconcile the wave mechanics of Schrödinger with matrix mechanics. Bohr accepts the quantum postulate as a real representation of the phenomena occurring in the quantum world. This postulate seems to be in contradiction with the wave mechanics which Bohr took as a good model for the wave aspects of a quantum system. In the next section I will discuss Bohr's answer to this dilemma.

As I mentioned in chapter three, there are two kinds of models constructed with the aid of their theories. The difference depends on the process of building them and on their point of departure. Bohr was a theoretical physicist who thought that 'theoretical descriptions' are not built out of simple mathematical formalism -- like the principle of least action--, but up from experimental and phenomenological grounds[5]. Heisenberg acknowledged this fact when he said that Bohr's

> insight into the structure of the theory was not a result of mathematical analysis of the basic assumptions, but rather of an intense occupation with the actual phenomena, such that it was

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[5] I will argue in secession 6:2:3 that Bohr's concept of phenomena is different from that of a phenomenalist like Mach.

> possible for him to sense the relationships intuitively rather than derive them formally.
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> Thus I understood: knowledge of nature was primarily obtained in this way, and only as the next step can one succeed in fixing one's knowledge in mathematical form and subjecting it to complete rational analysis. (Heisenberg 1967, 94-95)

This is one of the major ways Bohr's standpoint disagrees with that of Einstein, who thought that theories should give a *'complete' description of the physical system out of their mathematical formalism*. The difference between the two standpoints is crucial to the way we ought to understand the debate. While Bohr's concern was the quantum phenomena and the ways they ought to be represented, Einstein's concern was the consistency between the new theory and other existing theories in physics. For now, let us see Bohr's idea of 'theoretical description' at work.

The first important theoretical work for Bohr was the model of the atom, or, as it is known now, the old quantum theory (or, sometimes, Bohr's model). In 1911 Bohr was a newly graduated physicist and was searching for a research project that he could work on. He went to Cambridge to work at the Cavendish with Thomson, and there he met Rutherford[6].

At that time Rutherford was working on his model of the atom in Manchester. Rutherford, as we know, had suggested, in line with a series of experiments, that the atoms must consist of a massive positively charged nucleus with negatively charged particles surrounding it. The problem with Rutherford's model was its inconsistency with the mechanical representations known at that time. The classical picture ends up by saying that, if such a description is given to a system, the electrons ought, when a loss of energy by radiation occurs, to collapse in a spiral-like path toward the nucleus. This means that the outcome spectrum should be a continuous spectrum of

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[6] For more details see Pais 1993.

radiated light. Experimental evidence shows instead that the radiation spectrum is a 'line spectrum'.

In his PhD thesis Bohr did not accept classical mechanics as a suitable framework for all situations. He claimed that classical mechanics cannot provide a solution for the chemical atom. In his view, a break from the classical picture is inescapable. Electrons are not free, reacting with different particles, but are bound. This attitude makes him a suitable candidate to tackle the challenge of experimental facts provided by Rutherford's experiments, and he was able to conceive a solution.

The newly discovered phenomenological facts indicate the failure of theories then current. Bohr used these facts as a starting point to construct a new model. Now, given the well-corroborated fact that classical mechanics could not solve the puzzle, Bohr searched for other tools and models that could be combined in constructing a descriptive model of the phenomena occurring in atoms. The first of these models was Planck's quantum of energy. The experimental result shows that the spectrum is a line spectrum with a discontinuity in the distribution of energy. So, it is justified to claim that there is an analogy between such a distribution and Planck's quanta of energy. Rutherford had claimed that the electrons should be in orbits around the heavy nucleus. So Bohr suggested using a second model: the planetary system. The electrons orbit round the nucleus on closed orbits with different energy levels. The third tool is taken from reformulating Einstein's 1905 paper on the photoelectric effect: when an atom radiates energy it means that the electron jumps from one energy level to another energy level emitting a photon with an energy equal to Planck's constant times the frequency.

In technical terms Bohr's model states the following:

1) Electrons move in circular orbits of radius r. They are restricted to these orbits due to the requirement that the angular momentum be an integer multiple of Planck's constant:

$$mvr = \frac{nh}{2\pi} \qquad (6.1)$$

This use of Planck's notion of quanta allows Bohr to define his concept of the quantum postulate for the first time. The atomic system is discrete. The values of its variables, like the angular momentum, are not continuous, but jump from one value to another by a factor of an integer *n*.

2) The electrons in their orbits do not radiate even if they are in a rapid movement; they are in a stationary state with a definite value of energy *E*.
3) Electrons can transfer from one stationary state to another either by absorbing or radiating energy with a frequency:

$$\nu = \frac{E-E'}{h} \qquad (6.2)$$

4) As a consequences of 1-3 the energy of each level (each possible orbit) can be given for an atom with a nuclear charge Ze and electron charge -*e* and mass *m*:

$$E = -\frac{2\pi^2 e^4 Z^2 m}{h^2 n^2} = -\frac{const.}{n^2} eV \qquad (6.3)$$

This point is another instance of Bohr's quantum postulate: the energy, in this case, is not continuous but jumps by an integer factor (n = 1, 2, 3...). In such a presentation of the quantum postulate we can see its importance in Bohr's programme. Bohr did not think that the numerical agreement between the mathematical result and the experimental results is a sufficient factor. He felt that it is important to represent a clear physical picture of the quantum system. This picture ought to be presented in an unambiguous language. The quantum postulate provides the physical picture for the mathematical equations. This is conveyed in the model by the pictorial model of the atom: a heavy nucleus with electrons moving in orbits.

The essential basis for Bohr's model was his suggestion that the phenomena we encounter at the atomic level are 'completely foreign' to that of classical level (although classical language and models are used in constructing the quantum models) in which a description of discontinuity and individuality is brought in to account for the puzzling results of experiments. Bohr continued to hold this kind of dual description of quantum systems: on the one hand, he accepts that these systems are alien to classical level; but, on the other hand, he still accepts the importance of classical language in constructing models to represent the quantum system. Bohr believed, as Folse claims (Folse 1985 and 1986), that the quantum postulate is a real (true) description of the situations in the quantum world. Bohr accepts that a true 'theoretical description' consists of:

1) A description of the experimental (environmental) set-up of the 'natural phenomena'. As we saw in his construction of the model of the atom he gave a detailed description of the environmental set-up of the atom and the reasons he thinks that such an environment cannot be represented using classical mechanics.
2) Mathematical formalism. These are tools chosen from more than one of the current accepted theories. In Bohr's model these where: planetary system, Einstein's photoelectric effect and Planck's quantum of energy.
3) Pictorial description that would represent the phenomenon in everyday language. An example of such a pictorial representation is the quantum postulate in Bohr's model.
4) A story that relates the mathematical formalism with the physical reality. This is conveyed in his discussion of the movement of the electron orbits around the nuclei, the excitation of the electrons and their movement from one orbit to another and how could the suggested mathematical formalism explain the experimental findings like the line spectrum and why electrons hold in their orbits and do not collapse into the nuclei.

Such a 'theoretical description' is a low-level theoretical representation that satisfies the definition of a phenomenological model laid out in chapter three.

This way of doing physics -- trying to capture an intuitive description of physical phenomena as a first step, then searching for a mathematical scheme which might fit the description, not the other way round -- was the way Bohr did physics from the early stages of his work, and continued to hold throughout his life. Heisenberg indicates that by saying:

> Bohr was not a mathematical minded man, but he thought about the connection in physics. He was, I would say, Faraday, but not Maxwell.
>
> ... there was a different sort of way of doing physics, ... one doesn't bother too much about the mathematical scheme. That is a later trouble. One first tries to see how things are connected- what they really mean. I would say that is really quite contrary against that kind of thing which Dirac does because Dirac starts from extremely nice mathematical schemes and never starts from the connections. This kind of physics which Faraday[,] Bohr and Ehrenfest tried to do really starts from the connections. (Heisenberg 1963, 30, quoted in Folse 1986, p99)

As I mentioned in chapter three, each model in any theoretical representation contains a story that helps in linking it with the real physical system. Bohr thought that the second important tool, which would shape the story associated with models of the quantum world, is the complementarity principle.

Bohr accepts that each system has its model relative to its environmental set-up. This model is a representative one. However, when Bohr starts to speak about quantum mechanics as a theory, he changes his tone. He puts his instrumental hat on. I will come back to this point in sub-section 6:2:4, but for now let me tackle the world of complementarity.

6:2:2 The Complementarity Principle

As I already mentioned that Bohr's main interest is to present a physical pictorial meaning for the mathematical schemes of quantum mechanics that are accountable to the phenomena. At the early stages there were two mathematical schemes: Heisenberg's matrix mechanics and Schrödinger's wave mechanics. Bohr was very worried about Schrödinger's wave mechanics. He was worried because the ontological bases of Schrödinger's wave mechanics were waves, while the ontological bases of Heisenberg matrix mechanics were particles.

Bohr also disliked Heisenberg's attitude toward wave mechanics (Beller 1992, 171-175). Heisenberg's only concern was to prove the compatibility between the two mathematical schemes, or, even better, to prove that his matrix mechanics is a better mathematical scheme than that of Schrödinger. Heisenberg tried, therefore, to present the uncertainty principle in a way that underlined an ontology which accepted photons and electrons as point particles. Also, as discussed in Jammer's book, *The Conceptual Development of Quantum Mechanics* (1966, 323-361), Heisenberg wanted to apply Einstein's method in quantum mechanics. Heisenberg thought that he can reverse

> the question and - instead of asking how nature can be described by a mathematical scheme – postulated that nature always works so that the mathematical formalism can be applied to it. (Jammer 1966, 325)

This is exactly the type of theoretical attitude that Bohr discarded. For him the mathematical scheme is arrived at from the question 'How can nature be described by a mathematical scheme?'

Bohr had a different programme in mind than that of reconciling the two mathematical schemes. "Bohr would not like to say that nature imitates a mathematical scheme, that nature does only things which fit into a mathematical scheme"[7] (Heisenberg 1963, 15). Because of that he wanted to think about the real quantum phenomena at the atomic level.

In the Como lecture, Bohr, counter to Heisenberg, tried to present the uncertainty principle not as the consequence of a mathematical scheme, but as arising from well-established experience[8]. The dispute with Heisenberg was related to the underlying ontology in the quantum world. The world of natural phenomena is rich and it is not possible to capture it within one mathematical scheme. Even if we can capture the behaviour of the quantum systems in terms of matrix mechanics, it is still a necessity to understand why it is possible to capture it by wave mechanics. Bohr considered both schemes as mathematical tools to deal with a rich nature. Heisenberg, on the contrary, thought that if it is possible to prove mathematically that both schemes might be equivalent then there exists no problem[9]. It is a matter of preference whether we want to talk in terms of wave mechanics or of matrix mechanics.

Bohr was not worried about the equivalency between wave and matrix mechanics but about the physical description of the phenomena in the quantum world. The experimental evidence, at the time, clearly indicated, in Bohr's opinion, that particles behave as 'waves' and as 'particles'. Moreover, the experimental set-up always has a measuring instrument which cannot be interpreted as a quantum mechanical device. From this accumulation of layers of complexity, the complexity of Bohr's argument of complementarity emerges, as we will see.

Bohr starts by indicating that the observation of a classical phenomenon can occur without disturbing it:

Indeed, our usual description of physical phenomena is based

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[7] This is not to say that a mathematical scheme cannot represent nature, with the rest of the theoretical representation.

[8] See for detailed account of the uncertainty principle and from different points of view: Folse 1985, 1986, 1993; Krips 1987, 1993. Pais 1993.

[9] At that time, Schrödinger's proof of compatibility between the two schemes was not yet known.

> entirely on the idea that the phenomena concerned may be observed without disturbing them appreciably. (Bohr, 1928, 88)

In contrast:

> the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected. Accordingly, an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation. (Bohr 1928, 89)

It is important to see the careful way Bohr puts his sentences: "any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected." This interaction will not affect the existence of an independent reality, but this independent reality will not be independent "in the ordinary physical sense". Later he rephrased the idea by specifying that the interaction is a real and physical interaction of the measuring instruments with the quantum system, and this interaction will serve as the objective conditions that "define the conditions under which the phenomena appear". He says:

> The crucial point, which was to become a main theme of the discussions..., implies the impossibility of a *sharp separation between the behaviour of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear*. (Bohr 1949, 210)

Hence, Bohr has a different sense of objectivity. An 'objectivity' which, on its face value, appears, if looked at from a classical perspective, as irrational. But it is far from being irrational. The only difference is that it takes a dynamic sense of objectivity, as we will see below.

This leads us to the next layer of complexity. Bohr accepts that each particular set-up provides the ways in which we ought to apply the quantum postulate:

> The circumstances, however, that in interpreting observations use has always to be made of theoretical notions entails that for *every particular case* it is a question of convenience at which point the concept of observation involving the quantum postulate with its "irrationality" is brought in. (Bohr 1928, 89)

The classical notion of observing a physical system does not (necessarily) involve disturbance, and the classical description of the physical system can be presented in an unambiguous way. However due to the interaction with the measurement instrument, an "unambiguous definition of the state of the system" in the classical sense "is naturally no longer possible". In order to restore clarity we ought to change the way we think about the description of the physical system. Bohr saw the change in terms of the possibilities of combining dialectically both theoretical entities with empirical outcomes:

> Indeed, in the description of the atomic phenomena, the quantum postulate presents us with the task of developing a "complementarity" theory the consistency of which can be judged only by weighing the possibilities of definition and observation (Bohr 1928, 90).

That is, there is a complementarity between the theoretical "definition" and empirical "observation". This "complementarity mode of description" is not a subjective judgement in which the observer can, by merely wishing or thinking, decide whether to observe this or that aspect (i.e. whether we want, in the case of the wave and particle duality, to observe the wave aspect of the system or the particle aspect of it); but, of objective conditions in which there would be one possible observation (of two non-commuting observers) in a particular set-up. As Bohr puts it:

> The complementary mode of description does indeed not involve any arbitrary renunciation of customary demands of explanation but, on the contrary, aims at an appropriate *dialectic* expression

> for the actual conditions of analysis and synthesis in atomic physics. (Bohr 1948, 317)

The complexity is built further as another factor is brought in: the experimental evidence. At the experimental level, for example, it did not seem possible, at that time[10], that a single experimental set-up can both be a set-up to observe the particle aspect of a quantum system and a set-up to observe the wave aspect of that system. In his discussion of this point Bohr starts with the nature of light:

> The two views of the nature of light are rather to be considered as different attempts at an interpretation of experimental evidence in which limitation of the classical concepts is expressed in complementary ways. (Bohr 1928, 91)

Of course the same can be said about the elementary particles. The "recent experience" and "the very expression of experimental evidence" prove that "here again we are not dealing with contradictory but with complementary pictures of the phenomena".

Let me put this argument in an experimental perspective: in the two slit experiment there are two possible experimental set-ups (at that time): the first is related to fixing the two slit frame and losing the momentum information while gaining position information. The second is by leaving the two slit frame loose in a way that can give us information about the momentum while losing information about the position of the particle. Bohr accepts that the first experimental set-up gives the particle picture while the second experimental set-up gives the wave picture. Both pictures are important in order to describe the quantum phenomena; therefore it is important to have a complementarity mode of description that incorporate these two kinds of experimental evidence, without needing to change the

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[10] Even now, it is not clear that we can have such a set-up. The current experiments related to the quantum optics, that claims the ability to have such combination are, to say the least, controversial. See for instant my article 'Three types of complementarity' (forthcoming).

existing mathematical tools. This point is crucial in Bohr's discussion with Einstein, as we will see bellow.

Here we have a quantum system that cannot be described in the same way as the classical system. Each experimental set-up allows the observation of one of two non-commuting pictures. But we have a classical language that is essential to present an unambiguous description of the quantum system. To this point, Heisenberg (1972, 137) tells us a story about a discussion between Bohr, Bloch, Carl Friendrich and himself. They were on a skiing holiday. One night, while they were washing the dishes after supper, they were discussing the importance of language in scientific discourse. The discussion arrived at a peak, at that point Bohr said:

> Our washing up is just like our language. We have dirty water and dirty dishcloth, and yet we manage to get the plates and glasses clean. In language, too, we have to work with unclear concepts and a form of logic whose scope is restricted in an unknown way, and yet we use it to bring some clarity into our understanding of nature

In this simple paragraph Bohr summarised his idea of an unambiguous language. So Bohr accepts that our daily language and the need for unambiguous communication forces us to use classical concepts to express concepts alien to classical physics using complementary pictorial techniques:

> For this purpose, it is decisive to recognize that, *however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms*. The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and of the result of the observations must be expressed in unambiguous language with suitable application of terminology of classical

> physics (Bohr 1949, 209).

In the classical physics language we have two pictures: waves and particles. Both of these pictures are important to the quantum system. Here comes Bohr's bright idea: complementarity. This complementarity can be expressed at all these levels: complementarity between two experimental set-ups, complementarity modes of description and complementarity pictures combining for the quantum system the two classical pictures: waves and particles. In sum, a phenomenological model. The only missing element is the mathematical expression. This as Bohr argues can be found in the uncertainty principle.

Bohr wanted to relate complementarity with Heisenberg's bright idea of uncertainty. He did not accept Heisenberg's way of representing uncertainty. Nevertheless, he was very enthusiastic about it, to the extent that he sent a copy of Heisenberg's paper to Einstein with a letter, where he said: 'This article, probably marks a very momentous contribution to the discussion of the general problems of quantum theory.' Here Bohr chooses his words 'the general problems of quantum theory' exactly because he thought of the uncertainty principle, as it was formulated by Heisenberg, as a theoretical contribution to the language of the quantum theory which would help it to overcome some of the disadvantages it had in contrast with the clarity of the classical theory. Bohr added

> through [Heisenberg's] new formulation we are given the possibility to harmonize the demand for conservation of energy with the wave theory of light, while in accord with the nature of description, the different sides of the problem never come into appearance simultaneously (Bohr to Einstein April 1927).

Here, it is clear that Bohr wanted to have the uncertainty principle as a theoretical tool connected to his idea of complementarity with [i.e. the mathematical element in his theoretical description (model)]. But the important point for Bohr is the way such an element ought to be brought into the picture. He accepts that the experimental evidence, and not any theoretical justification, are what lead to belief in the use of the uncertainty principle. Speaking about Heisenberg, Bohr said:

> In particular, he has stressed the peculiar reciprocal uncertainty which affects all measurements of atomic quantities. Before we enter upon his results, it will be advantageous to show how the complementary nature of the description appearing in this uncertainty is unavoidable already in an analysis of the most elementary concepts employed in interpreting experience. (Bohr 1928, 92)

The uncertainty principle is the outcome of the complementarity picture (i.e. the picture that combines in it two non-commuting instants: waves and particles), not the mathematical schemes. So, in Bohr's reconstruction of the uncertainty principle he starts from the many experimental situations that would demonstrate the ultimate uncertainty of finding a value if a choice had already been acted on to find its complimentary value to a high degree of precision. In such a presentation he insists on the view that the uncertainty relation is an outcome of the pictorial elements of the quantum system, such as his sentence: 'the essence of this consideration is the inevitability of the quantum postulate in the estimation of the possibilities of measurement.' (Bohr 1928, 98) This would be demonstrated by the different ways the accuracy of measurement of position or momentum might be affected by the measuring equipment. Finally in the context of the relation between momentum measurement and position measurement Bohr says:

> Just this situation brings out most strikingly the complementary character of the description of atomic phenomena which appears as an inevitable consequence of the contrast between the quantum postulate and the distinction between object and agency of measurement, inherent in our very idea of observation. (Bohr 1928, 103)

However, Bohr continued to accept the need for classical terms:

> [complementarity] is suited to embrace the characteristic features of individuality of quantum phenomena, and at the same time to clarify the peculiar aspects of observational problems in this field of experience. For this purpose, it is decisive to recognise that, however far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. (Bohr 1949, 209)

Bohr accepts that in the case of different experimental conditions, 'however far the phenomena transcend the scope of classical physical explanation', we need to use classical terms in describing the phenomena. He accepts that the classical concepts are accurate tools. In this he is exhibiting another feature of his kind of realism. It is possible to use any previously accepted tools in physics in order to present an accurate representation of a physical phenomenon. Bohr employed the classical concepts in order to present unambiguous complementary pictures which exhausts the possible information about the object (i.e. the information produced from the different experimental set-ups which implies that the quantum system behaves as particles in particular cases and as a wave in others). He says:

> [Consequent] evidence obtained under different experimental conditions cannot be comprehended within a single picture, but must be regarded as complementary in the sense that only the totality of the phenomena exhausts the possible information about the objects. (Bohr 1949, 210)

For Bohr, complementarity is the only way we can exhaust the information about the quantum phenomena.

However, even if complementarity was presented as an unambiguous solution to the phenomena in the quantum world, it generated deep confusion both in the physics and in the philosophical communities.[11] Even Heisenberg who believed that he and Bohr were in agreement that the uncertainty principle is a special case of the more general complementarity principle, said in 1959 that the complementarity principle

> has encouraged the physicists to use an ambiguous language, to use the classical concepts in a somewhat vague manner in conformity with the principle of uncertainty...When this vague and unsystematic use of the language leads into difficulties, the physicist has to withdraw into the mathematical scheme and its unambiguous correlation with the experimental facts (Heisenberg 1962, p).

It should be stated at this point that while the uncertainty relation[12] is a mathematical relation which puts an epistemic limitation to what can be measured in principle in quantum mechanics out of two non-commuting observables, the complementarity principle acknowledges the importance of using both "observables" in describing the quantum mechanical systems.

As any quantum mechanics text book will describe, the relation between any two non-commuting observables is given by a general commutative relation {if $[A,B] \neq 0$ then A and B are said to be non-commuting} which expresses the general idea of not being able to ascribe an exact measured value to two observables represented by non-commuting operators at the same time. The wave-particle duality, or let us say the relation between the position operator and momentum operator, will be just one of many and it will have no special place as a quantum postulate. In quantum mechanics if you pick any two observables, the possibility of them

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[11] Many philosophers have discussed this point see for instant Folse 1985, Howard 1993, Fine and Beller 1993, Krips 1987.

[12] I do not want here to inter into a foundational debate about the uncertainty principle and its role in quantum mechanics, but let me point out that there are many ways in which it is possible to go around the limitations imposed by the uncertainty principle to get readings of two non-commuting observables. See the work of Paul Busch and also the experiments conducted by Kiawt et. al., Greenberger et. al. and Home et al in relation to the principle.

being non-commutative is high. As Wigner mentioned in an interview in 1963, after the Como lecture von Neumann commented, saying: "Well, there are many things which do not commute and you can easily find three operators which do not commute." (In Jammer 1966, 354) Bohr knew this fact and was not addressing it in his lecture. His aim was not to address the mathematical schemes of quantum mechanics but rather to understand how to provide 'theoretical descriptions' of quantum systems.

The problem that motivated Bohr to adopt the complementarity principle as a solution to the wave-particle duality might not mean anything from an anti-realist or an instrumentalist point of view. An anti-realist or an instrumentalist would not care about the underlying ontology of the quantum world. He or she would be concerned about the extent to which the mathematical scheme would be successful in finding empirical outcomes. He or she would be concerned merely about the empirical adequacy of the mathematical (theoretical) scheme. On the contrary these issues are important for a realist because it might differentiate between what might be accepted as a representation of nature and what might be merely driven by theoretical motivations and do not represent nature.

Hence, if Bohr was an anti-realist he could have adopted a position similar to that of Heisenberg and need not have troubled himself with the issue of presenting a clear description of the quantum phenomena. But Bohr's extreme interest in this issue revealed his priorities. Let us now turn our attention to the next points in Bohr's philosophical position: natural phenomena and physical reality.

6:2:3 Natural Phenomena

Bohr has a distinct concept of phenomena. He was one of the first physicists/philosophers to clearly indicate that the concept of phenomena in physics differs in a subtle way from that used in the philosophical tradition.

Bohr clearly dissociates himself from the Machian ideas of phenomenalism (see Faye 1991). Furthermore I think that Bohr also was aware of the importance of dissociating himself from the German tradition of phenomenology. Bohr tried over and over throughout his work to clarify what he means by phenomena. Toward the end of his article "Discussion with Einstein" (1949), where he tried to condense his arguments for an interpretation of quantum mechanics, he pointed to this explicitly:

> Meanwhile, the discussion of the epistemological problems in atomic physics attracted as much attention as ever.... In this connection I warned especially against phrases often found in the physical literature, such as 'disturbing of phenomena by observation' or 'creating physical attributes to atomic objects by measurements'. Such phrases, which may serve to remind of the apparent paradoxes in quantum theory, are at the same time apt to cause confusion, since words like 'phenomena' and 'observation', just as 'attributes' and 'measurements', are used in a way *hardly compatible with common language and practical definition.* (Bohr 1949, 237 my emphasis)

He went on to discuss the point which he thought is the more puzzling: phenomena. He said:

> As a more appropriate way of expression I advocated the application of the word phenomenon exclusively to refer to the observations obtained under specified circumstances, including an account of the whole experimental arrangement. (Bohr 1949, 237-238)

Bohr's concept of a phenomenon is related to the whole experimental arrangement. Take for example Bohr's favourite example, the electron behaving as a wave. In this case Bohr would say that the natural phenomenon "electron behaving as a wave" would occur "only if an experimental set-up so and so is in place". So the natural phenomenon here is: “the electron behaves as a wave in the case of a certain experimental set-up”. Then, the description of a phenomenon would need more than a mere description of an empirical result.[13] Hence, a mathematical scheme that would yield the empirical result would not be sufficient as a description of the phenomena. The experimental conditions (or the environmental set-up) are crucial in building the model:

> It is certainly more in accordance with the structure and interpretation of quantum mechanical symbolism, as well as with elementary epistemological principles, to reserve the word ‘phenomenon’ for the comprehension of the effects observed under given experimental conditions.
>
> These conditions, which include the account of the properties and manipulation of all measuring instruments essentially concerned, constitute in fact the only basis for the definition of the concepts by which the phenomenon is described. (Bohr, in Folse 1985, 157-158)

A phenomenon, according to Bohr, is not to be interpreted without the whole experimental set-up and the concepts related to it. In the case of Bohr’s example: “electron behaves as a wave”, the electron will exhibit the wave aspect only if the experiment is set in such a way that will allow the electron to exhibit the wave aspect. Now there might be a different set-up which prevents the possibility of the occurrence of the wave aspect. If this happens, then Bohr would accept that these two set-ups are “mutually exclusive experimental arrangements”.

Bohr accepts that the theoretical description is a real representative of the natural phenomena and the elements that are described, element of those phenomena. For him every element in the mathematical formalism of

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[13] An implicit premise here is the acceptance of instrumentalists that mathematical schemes yield successfully empirical results, and for that it would be a successful description.

the theoretical description should have a counter part in physical reality. In the case at hand, he would say that the theoretical description -- which represents the phenomenon of 'electron behaving as a wave' with the experimental set-up that produces such a phenomenon -- is a real representation of the natural phenomenon, and every element in that description would refer to its counterpart in reality: real electron, a wave like behaviour, and so on.

So, Bohr believes that there are electrons, as well as photons and other quantum particles, and if he believes that there are electrons then some attributes should be given to electrons. But because he believes that the quantum mechanical phenomena related to the electrons can give two mutually exclusive attributes to the electrons (particles and waves), and he accepts that all possible outcomes related to electrons must be a part of any complete description of the quantum mechanical system, he thought that complementarity might save the day. He says:

> phenomena defined by different concepts, corresponding to mutually exclusive experimental arrangements, can be unambiguously regarded as complementary aspects of the whole obtainable evidence concerning the object under investigation (Bohr 1938, 24-25).

Even if Bohr took intervention from human activity to be crucial in dictating the environmental set-up of a phenomenon, he wanted an objective criterion for what a phenomenon can be:

> The extension of physical experience in our days has, however, necessitated a radical revision of the foundation for the unambiguous use of our most elementary concepts, and has changed our attitude to the aim of physical science. Indeed, from our present standpoint physics is to be regarded not so much as a study of something a priori given, but rather as development of methods for ordering and surveying human experience. In this

> respect our task must be to account for such experience in a manner independent of individual subjective judgement and therefore objective in the sense that it can be unambiguously communicated in the common human language. (Bohr 1960, 9-10).

So even if the present standpoint regards physics as the development of methods for ordering and surveying human experience, it ought to be 'independent of individual subjective judgement and therefore objective'. But Bohr was very much aware that some of the methods in physics will probably not be able to provide knowledge that is objective in the sense of 'corresponding' to the world as it is. For all physics knowledge is fallible and likely to change:

> Only by our experience itself do we come to recognize those laws which grant us a comprehensive view of diversity of phenomena. As our knowledge becomes wider, we must always be prepared, therefore, *to expect alterations in the point of view best suited for ordering of our experience*.... The great extension of our experience in recent years has brought to light the insufficiency of our simple mechanical conceptions and, as a consequence, has shaken the foundation on which the customary interpretation was based, thus throwing new light on old philosophical problems. (Bohr1934 1-2)

In this sense even quantum theory would be one of the ordering schemes in which we should be prepared to expect alteration.

6:2:4 Knowledge and Realism

Bohr's philosophical grounds are those of a realist. Nevertheless, Bohr accepts that some of the theoretical concepts are not realisable. Those are elements of high-level theoretical representations. I agree with Folse that an anti-realist tendency in Bohr's writing can be attached to his instrumental attitude toward theories:

> Thus this 'instrumentalist' tendency in complementarity could support characterising Bohr as an anti-realist with respect to theories. But this form of anti-realist does not compromise Bohr's robust realism with respect to the reality of atomic systems (Folse 1986, 102)

But I add that Bohr's realism was not about atomic systems only but also about all the potentially referring terms of the 'theoretical description' (not the theory as a whole). Bohr was a realist about both atomic structure and about elementary particles. He accepted that the description of these quantum systems is a real representative of them with all its theoretical input. This means that, for Bohr, the models describe the natural phenomena.

Bohr also dismissed Machian type scepticism about what might be known about atoms:

> We know now, it is true, that the often expressed scepticism with regard to the reality of atoms was exaggerated; for, indeed the wonderful development of the art of experimentation has enabled us to study the effects of individual atoms.
>
> However, at the same time as every doubt regarding the reality of atoms has been removed and as we gained a detailed knowledge of the inner structure of atoms, we have been reminded in an instructive manner of the natural limitation of our forms of perception (Bohr 1934, 103).

To further understand Bohr's realism I ought to explain his concept of knowledge. Bohr asserts that it is important to understand knowledge in its environment:

> ...For objective description and harmonious comprehension it is necessary in almost every field of knowledge to pay attention to circumstances under which evidence is obtained (Bohr, in Beller

> 1992, 147).

Bohr has a dynamical concept of knowledge. He thinks that knowledge is obtained by a process of interaction between natural phenomena and experience. Knowledge is objective only to the extent that we humans can communicate it in an unambiguous way. Bohr says that:

> The lesson of atomic physics has been that we are not simply co-ordinating experience arranged in given general categories for human thinking, as one might have liked to say in expressions of physical philosophy, but we have learned that our task is to develop human concepts to find a way of speaking which is suited to bringing order into new experience and, so to say, being able to put questions to nature in a manner in which we can get some help with answer. (Bohr, from a transcript of Compton Lectures 1957, in Folse 1985, 235-236).

So, the task of knowledge is to develop concepts that bring order into new experience, and to state the right questions, that is, the questions for which nature can help in finding answers.

The dynamics of knowledge expresses itself clearly in the history of science. There, the dynamics between theoretical description and empirical observation "lead to the recognition of relations between formally unconnected groups of phenomena". When such a recognition occurs it "demands a renewed revision of the presupposition for the unambiguous application of even our elementary concepts" (Bohr 1938, 28). In another place Bohr presents yet a stronger claim:

> The main point to realise is that *all* knowledge presents itself within a conceptual framework adapted to account for previous experience and that any frame may prove too narrow to comprehend new experience. Scientific research in many domains of knowledge has indeed time and again proved the necessity of abandoning or remoulding points of view which,

> because of their fruitfulness and apparently unrestricted applicability, were regarded as indispensable for rational explanation. (Bohr 1958, 67-68)[14].

So Bohr never had confidence that we would ever arrive at one theoretical scheme that we would stick with, and each of our theoretical schemes needs to be revised with the developments in science. This revision contributes toward the "clarification of the principle underlying human knowledge" (Bohr 1937, 289-290). The new experience should be established on an objective basis. This needs a new 'means of communication' that can represent natural phenomena in an unambiguous way. This need for an unambiguous representation is a difficulty which constantly confronts every scientist.

Having presented Bohr's philosophical grounds let me now go into the Bohr-Einstein debate.

6:3 The Debate

In this section I will concentrate on the first round of the debate. I will not discuss the second round, because it raised a cluster of issues that are beyond the scope of this thesis.

As I said earlier in 1927 Bohr worried about the physical interpretation of the new formalism of quantum theory. Bohr and Heisenberg worked on a daily basis to resolve the conflict between Schrödinger's wave mechanics and matrix mechanics and to suggest a way out of the problems of the non-compatibility between these two schemes. Heisenberg was worried mainly about the mathematical structure of the problem and he ceased to recognise Bohr's worries. Bohr went on a skiing trip for six weeks. It was at

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[14] Let us compare such a position with Bachelard when he speaks of the new scientific spirit: 'above all we must recognize the fact that new experience says no to old experience' (Bachelard, PN p9).

that time that he formulated his principle of complementarity, while Heisenberg wrote at the same time his paper on the uncertainty principle.

In 1927 the experimental information gave theoretical physicists a very shallow picture about the quantum world. The main experiments which had an impact on the theoretical debate were the Stern-Gerlach effect in 1922 and the Compton experiments in 1924. This left the theoretical physicists open to a different type of game: that of suggesting and debating hypothetical situations in which quantum mechanics should be applied and trying to figure out if the answer given for these hypothetical situations can be satisfactory from a theoretical point of view. Einstein was not happy about either the uncertainty principle or the complementarity principle. He thought that in spite of the mathematical accuracy of quantum mechanics and its agreement with experiments (a handful), it cannot be accepted as a complete theory. For him, there had to be another theory that can give the same level of accuracy in its mathematical results but does not carry the related philosophical baggage[15].

As I said there were few experiments to make any supportive arguments for or against quantum mechanics. Einstein tried hard to suggest thought experiments to disprove the completeness of quantum mechanics. A theory is complete in Einstein's sense when it can account for every element in physical reality.

Bohr presented his recollection of the events of the first round of the debate in his contribution to Schilpp's volume in 1948[16], *Albert Einstein: philosopher-scientist.* The story of the debate goes as follows. When Bohr gave his famous lecture, "the Como lecture", Einstein was not there. But later that year they met in the fifth Solvay conference. Bohr claimed that Einstein's main concern was that the 'causal account in space and time' was abandoned in quantum mechanics.

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[15] For a discussion of Einstein's position see Fine 1986.

[16] Bohr 1949, pp 201- 241.

However, Einstein's concern was related to the coherency between the new theory and previously accepted physical theory. As Arthur Fine argues in his book *The Shaky Game*, Einstein's criticisms of quantum theory during its early years are expressed in five points. These are:

> 1) the equations of the theory are not relativistically invariant; 2) it does not yield the classical behaviour of macroscopic objects to a good approximation; 3) it leads to correlations among spatially separated objects that appear to violate action-by-contact principles; 4) it is an essentially statistical theory that seems incapable even of describing the behaviour of individual systems; and 5) the scope of the commutation relations may not in fact be so broad as the theory supposes. (Fine 1986, 28)

It is clear from such a list that Einstein's concern was with the type of theory quantum mechanics is, and whether it would be compatible with other fundamental theories. Here Einstein took the mathematical formalism as the major element in the theory.

This standpoint was the drive behind his construction of a series of thought experiments. He started, according to Bohr's reconstruction, with a very simple experimental set-up:

According to quantum theory, in the case of a single slit between a source and a photographic plate, if a particle is shot at the slit, the theory cannot provide an accurate prediction of the exact point at which the particle would hit the photographic plate. The best it can provide is a probabilistic percentage for the particle to hit any given region. In such case, there will be an agreement between the theory and the experiment, given that the experiment is repeated a sufficient number of times. Einstein pointed out that if in a given single experiment the particle is recorded at a point (A) on the plate, that directly leads to the impossibility of observing any effect of that particle at any other point (B) which lies at a distance from (A). This would create a contradiction: The theory predicts that there is a possibility that the particle will hit (B), while if the particle was found to be at (A), then it is impossible that any trace of the particle can be found at (B).

Einstein raises here two points: one is about the statistical nature of the experimental set-up and whether this statistical nature is associated with the system itself or with the description of the system. Einstein wants to maintain that the statistical nature ought to be similar to that occurring in classical situations. He thinks that quantum mechanics with its statistical nature leaves plenty of questions unanswered, especially the question of defining an exact energy and momentum of the particle at all times. He thinks that there ought to be a "fuller description of the phenomena" which can "bring into consideration the detailed balance of energy and momentum in individual processes"(Bohr 1949, 213). That is to say that the particle can have a definite position with a definite energy, while at the same time has a precise momentum. This is related to the second point: the wave-particle duality and the theory constrains the description that can ascribe both the particle aspect and the wave aspect to the system. Einstein asserts that the theory cannot ascribe both aspect to the system at the same time cannot give us a full description of all the elements in physical reality.

Bohr's reply to such arguments begins, as he usually does, from stating the experimental set-up and analysing whether it is consistent, given the quantum postulate, to accept the argument. In this simple case, Bohr would state that the experimental set-ups that might provide information about the position and the momentum of the particle are different in fact. He explained that there exist two possible set-ups.

The first is similar to that suggested by Einstein. This set-up, according to Bohr, provides the basis for the phenomenon 'particle behaving as a wave'. In this phenomenon when the particle interacts with the slit it will undergo a change of momentum (Δp) which, according to the uncertainty principle, will lead to the impossibility to find the energy of the particle. This means that the experimental set-up will exhibit latitude in the location of the particle. The particle is behaving as a wave, which means that there is a possibility that it might hit the photographic plate at any point within a given region. But at the moment of measurement of the particle (where it hits the photographic plate) there will be another interaction between the particle and the measuring instrument and at that point the particle is behaving as a particle.

Now the model of the phenomenon 'particle behaving as a wave' uses tools from quantum mechanics to give us a general prediction to where the particle might hit the photographic plate. But also it contains a description of the experimental set-up and the story that describe why the particle behaves like a wave after interacting with the slit and how the particle alters its momentum when interacting with the slit. The story also tells us how we can detect that the particle is really behaving as a wave. This detection is not done on a single experiment basis but on an ensemble. Now Einstein is saying that in a single experiment the theory can not give us a description of both the wave and the particle aspects of the particle. Bohr answers this by saying that what counts is not the theory but the model which describes one of the two aspects at a time. And because it is a model of the phenomenon 'particle behaving as a wave' it needs not account for any other phenomenon like 'particle behaving as a particle'. Also the statistical nature of the model is not associated with the quantum theory but is associated, according to Bohr, with the ability to detect experimentally the wave behaviour of the particle.

The second set-up suggests a shutter in front of the slit: in this case the interaction between the shutter and the particle would allow additional latitude in the kinetic energy of the particle. This set-up has an uncertainty in the energy ΔE. Then in accordance with the uncertainty relation, there is a latitude in the exact time when the particle interacted with the shutter (The outcome of such an interaction would be ($\Delta E \Delta T \approx h$)).

Einstein's question is related to what extent we can control our knowledge of the momentum and energy so that we would obtain a specification of the state of the particle after passing through the slit. Bohr, in reply, claims that

> as soon as we want to know the momentum and energy of these parts [the shutter and the diaphragm] of the measuring arrangement with an accuracy sufficient to control the momentum and energy exchange with the particle under investigation, we shall, in accordance with the indeterminacy relations, lose the possibility of their accurate location in space and time. (Bohr, 1949 215)

In the case presented by Einstein, it is the assumption that both the diaphragm (with the slit) and the plate have a well-defined position that would not allow, in accordance with quantum mechanics, an exact prediction of the point where the particle might hit the plate. However, if in a similar case we have a sufficient latitude in knowing the position of the diaphragm (with the slit), then it is possible (in principle) to control the interaction between the slit and the particle. This would lead to the possibility of predicting the path of the particle from the slit to the plate.

Although this experimental arrangement is very simple and was familiar to physicists working in the field at that time, Bohr's first reaction toward Einstein's example was to say that he could not understand what Einstein meant. The notes taken by Kramers and kept in Bohr archives show that Bohr's reply to Einstein's simple objections starts by saying: 'I feel myself in a very difficult position because I don't understand what precisely is the point which Einstein wants to [make]. No doubt it is my fault.'[17]

What Bohr did not understand was not the experiment but the process in which the experiment was presented. Einstein complained about the theory while Bohr's own concern was the description. Because of that

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[17] In Pais 1991, p 318

Bohr insisted on representing the whole picture every time he wanted to reply to any of Einstein's critiques. He accepts that each particular experiment had its own description. So, in every case he needs to explain the detailed circumstances related to that case and how it is possible to construct the related quantum mechanical description.

To the case at hand, after explaining in detail the experimental and phenomenological facts (i.e. the way the experiment is set-up and the way the particle would react to the different set-ups: e.g. the loose diaphragm versus the fixed one), he asserts that in the quantum mechanical description

> we have to deal [..] with a two-body system consisting of the diaphragm as well as the particle, and it is just with an explicit application of conservation laws to such a system that we are concerned in the Compton effect where, for instant, the observation of the electron by means of a cloud chamber allows us to predict in what direction the scattered photon will eventually be observed. (Bohr 1949, 216)

It is clear here that this quantum mechanical description is pretty much related to the experiments at hand. Here Bohr does not talk about the quantum formalism; rather his concern is how to capture the intuition behind the experiment and what would be in fact possible to be performed experimentally. Moreover, Bohr insisted that these two experimental set-ups, the one with fixed diaphragm and the one with loose diaphragm, are mutually exclusive. For him this point 'clearly brings out the complementary character of the phenomena.' (Bohr 1949, 215)

Einstein took the debate a step farther and suggests another simple argument using the two slit experiment: It should be possible to suggest an experimental set-up in which it would be possible to measure through which of the two holes the particle entered. Here we see the theory-driven attitude of Einstein at its best. He asserted that the framework of contemporary physics does not accept that the act of observation affects the observed system in a way which we cannot control. The ultimate challenge is to plot the set-up that might give us the exact knowledge without affecting the observed object in an uncontrolled way. He suggests the following experiment.

In the last suggested set-up, another diaphragm with two slits is installed between the first diaphragm and the photographic plate. An electron source (or photon source) emits electrons to the first diaphragm (with one slit). Then the output electrons beam targets the second diaphragm (with two slits), lying at a distance d from the first diaphragm. This distance is at most twice the electrons beam wavelength. If the first diaphragm is fixed, quantum mechanics predicts that the outcome on the screen will exhibit an interference pattern.

Einstein proposed supporting the first diaphragm with a spring which can be affected by the slightest movement of the diaphragm. The momentum exchange between the particle and the first diaphragm will, presumably, define the position of the particle at that point, and then decide through which of the two slits it will pass toward the screen without distorting the interference pattern. Bohr relies on this last point, along with the uncertainty principle in the case of position and momentum, to prove that if this apparatus is secured then the first diaphragm will be affected by the uncertainty principle. The change in momentum of the diaphragm will eventually change the position by an unknown factor; the more precise the momentum measurement will be the less we can speak about the position of the slit. That affects the interference on the photographic plate with a factor equal to the uncertainty in the position of the first diaphragm. As Bohr says:

> In fact, if ω is the small angle between the conjectured paths of a particle passing through the upper or the lower slit, the difference of momentum transfer in these two cases will, according to $[E = h\nu \text{ and } P = h\sigma]$, be equal to $h\sigma\omega$ and any control of the momentum of the diaphragm with an accuracy

> sufficient to measure this difference will, due to the indeterminacy relation, involve a minimum latitude of the position of the diaphragm, comparable with $\frac{1}{\sigma\omega}$ [and] the number of fringes per unit length will be just equal to $\sigma\omega$ and, since an uncertainty in the position of the first diaphragm of the amount of $\frac{1}{\sigma\omega}$ will cause an equal uncertainty in the positions of the fringes, it follows that no interference effect can appear.(Bohr, 1949, 217)[18]

Einstein's other experiments suggested in that period evolve within the same argumentation still even if they were more complex in form. One of these other experiments (was suggested in 1930 in the sixth Solvay congress) is the clock in a box with a radioactive source. Even though Einstein tried to use a complex line of argument employing the special theory of relativity, the underling concept was to oppose complementarity. Bohr's reply uses also the uncertainty principle to prove the impossibility of finding two non-commuting variables simultaneously (in this case time and energy). The last attempt in this first round was made in a very short paper (two pages) published in *Physical Review* in 1931 under the title 'Knowledge of Past and Future in Quantum mechanics'[19] with Tolman and Podolsky employing concepts from relativity theory.

To conclude: I showed in this chapter that Bohr was a realist of a special kind. Then I showed that the main difference between Bohr and Einstein is their treatment to the theoretical forms. While Einstein insisted on a top-down approach, Bohr adopted a bottom-up approach. I think that the spirit of the Bohr-Einstein debate is a debate between a structural realist exemplified by Einstein and a phenomenological realist exemplified by Bohr.

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[18] We know now that the interference had nothing to do with the uncertainty principle (in the mathematical sense), but still at the experimental level the disappearance of the interference fringes in the two slit experiments was thought to occur whenever the set-up violated the situation in which the uncertainty principle should be applied

[19] Einstein, Tolman and Podolsky 1931, pp 780-81.

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