The London School of Economics and Political Science

Modelling High Temperature Superconductivity: A Philosophical Inquiry in Theory, Experiment and Dissent.

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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Abstract

This thesis tells the story of the Balkanization of the theory community in High Temperature Superconductivity (HTS) and of the many roles experimental evidence has been playing in the battles there.

In the twenty-five years that followed the discovery of HTS, the Condensed Matter Physics (CMP) community has experienced extreme difficulty in trying to reach a consensus on a 'final' theory. I will explore some of the reasons for such dissent, starting from testimonies that I collected through personal interviews with HTS physicists.

I will focus on the way experiments actively contribute to the formulation of theories. I claim that there is a tension between the different methods and aims of two scientific traditions as they implement the contribution from experiments. This tension will be illustrated through the discussion of several episodes from the history of Superconductivity and CMP research. In particular the paradigmatic quarrels between two of the major players in the history of superconductivity, physicists PW Anderson and B Matthias, will be presented to explore the meeting of theoretical and experimental driving forces and their impact on the evaluation of theories and research programmes.

I will also argue that the ambiguity in the theories of evidence employed by the warring camps in HTS allows each of them to claim empirical adequacy for itself and deny it to the opponents, and I shall raise questions about whether the standards of evidence employed there are consistently applied and grounded.

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CHAPTER ONE - INTRODUCTION

This thesis tells the story of the Balkanization of the theory community in High Temperature Superconductivity (HTS) and of the many roles experimental evidence plays in the battles there.

Superconductivity is a peculiar phenomenon observed in certain materials when they are cooled to below a temperature characteristic of that material, known as the superconducting transition temperature (Tc). Superconductors are characterized by the complete absence of electrical resistance and the ability to expel external magnetic fields.

Conventional or Low Temperature Superconductors (LTS) were first observed a century ago and later explained through the Bardeen-Cooper-Schrieffer (BCS) theory. BCS theory was formulated in the 1950s and won its proponents the Nobel Prize in 1972, becoming not only the leading theory in Condensed Matter Physics but also one of the most compelling successes of theoretical physics.

BCS derived a formula for the temperature at which a superconducting material makes a transition to the superconducting state (Tc) and from that an approximate upper limit to the transition temperature of 30 K. In 1986 new types of materials broke through that limit and showed unusually high transition temperatures (up to a recently patent pending superconductor which exhibits an extraordinary Tc of 200 K!). Ever since, the

CMP community has been struggling with the problem of how superconductivity arises in certain materials at higher temperatures than that of conventional superconductors.

Most scientists still agree that the electrons pair in the new materials, as they do in BCS theory, but disagree on just how that mechanism is mediated. Despite much intensive research in the twenty-three years that followed the discovery of high temperature superconductors and many promising leads, an answer to this question has so far eluded scientists.

"The field of HTS", says theorist Chandra Varma in my interview, "is *full* of dissent. There is an enormous amount of confusion and diversity of views, partly because this is a field with an enormous number of experiments. Every known technique in science has been brought to bear on these materials and lots of different anomalies have been discovered". Tens of theories have been offered so far and the community is rife with dissent and disagreement. To this date, even the most popular and promising candidate theories are seen as unsatisfactory. Each, however, is hotly defended by its own advocates and attacked by the advocates of opponent theories. Despite the fact that the different camps supporting different theoretical approaches all agree on the importance of accounting for the relevant data, this has done little to mitigate the fierce disputes. I shall argue that this is no surprise since it turns out that they have quite different views with regards to how data are relevant and what they are relevant for.

Specifically, I identify two different traditions, which I call *empiricist* and *principled,* and explore the epistemic value of the first.

In the Second chapter I briefly review the philosophical literature on experiment in science and show how the two traditions, as I define them, map onto pre-existing concepts used by current philosophy of science.

In the following chapters, I will tell the story of superconductivity, and expose within it both the dialogue between theory and experiment, as well as the dynamic between the two traditions introduced in the previous chapter, as they appear in the most relevant historical episodes, from the discovery of superconductivity a century ago to the recent debate in HTS. I draw much of my history from the illuminating French book by Jean Matricon and Georges Waysand "The Cold Wars – A History of Superconductivity". I also use several secondary and primary sources and have focused particularly on biographical works, essays and reports, which range from personal correspondences to conferences' reports, reviews and necrologies. I have personally interviewed many physicists in superconductivity research in the past three years, from Nobel Laureates to young researchers 'at the margins', and have added a few remarks extracted from these interviews to known pieces of history. In many cases their recounting has been obviously in line with that of historical sources and in those situations I have preferred to draw on and refer to those sources instead. I have kept the role of my interviews to one of collecting testimonies on the subjective experiences of the controversies in the field and on personal theories of evidence and views on methodology. This provides the opportunity for greater insight into the scientists' practices, which often defy historical and philosophical accounts, unveiling illuminating assumptions about the nature of theories and the roles of experiment assumed more or less implicitly by practitioners. A sample interview can be found in the Appendix.

In order to give a coherent and accessible historical account of the case, and at the same time discuss the philosophical lessons that I claim are contained in this history, I will loosely maintain a chronological order in the exposition, discussing the philosophical lessons as they make their appearance in the separate historical episodes. I then tie them together more fully in discussion sections. In view of the specific purpose of this thesis, my personal, ad hoc, recipe for the integration of history and philosophy of science will therefore be a compromise between the completeness of a historical account and the focus of its philosophical analysis. Such a compromise can be implemented in various ways but I have settled for one that will allow me to achieve the following goals:

1) To give a concise overview of the complex history of superconductivity.

2) To highlight the role of experiment and evidence not only for confirmation but also for exploratory and "revisionary" purposes and for formulating models for superconductivity.

3) To trace the evolution of the dialogue between the *empiricist* and *principled* traditions.

In the third chapter I present the first fifty years of the history of superconductivity. I examine the empiricist manifesto that led to the discovery of superconductivity, and show how this unexpected phenomenon resisted all theoretical efforts of the best minds of the century, emerging from the quantum revolution as a stubborn puzzle. I discuss Landau's theory of phase transitions and the analogy between ferromagnetism and superconductivity as examples of phenomenological approaches. In particular, the episode of the 'frozen flux' experiments will show the active dialogue between theory and experiment and the potency of the interplay between the traditions to foster this progressive dialogue.

In the fourth chapter, I will discuss the Landau-Ginzburg (GL) and BCS theories of superconductivity. I will focus on GL in analogy with London's theory as a triumph of conceptual development, which was not based on first principles. While in recent philosophical debates BCS has usually been set against London's and Landau's theories, I will focus on John Bardeen's views, and show in there a cooperative dialogue between the two traditions. I highlight BCS' points of contact with, as well as the points of departure from, the phenomenological theories with which it is usually contrasted, and focus on the relationship between theory and experiment in superconductivity according to his views. I contrast his view to the one expressed by Richard Feynman.

In the fifth chapter, I discuss the first challenges to BCS theory. I present the quarrels between Anderson and Mathias as a paradigmatic example of the clash between the *empiricist* and *principled* traditions. A different view not only of methodology, but also of the nature of scientific theories, emerges from the two physicists. I will discuss the difference in terms of criteria for consensus on theories, and also give a controversial example of Anderson's view applied to HTS.

In the sixth chapter, I illustrate the ways in which HTS has made a difference in the field of superconductivity, I introduce the dissent on HTS theories and discuss the role of experiments in the redefinition of the problem. Considering the capacity of scientists, in the words of Collins and Pinch, to subvert the force of experimental results, and drawing from the discussion of the previous chapters, I argue that we find in the interplay of the two traditions a crucial factor for controversy, and that this factor gives us insights on the controversies taking place in HTS research.

In the seventh and final chapter I return to the different roles of experimental evidence that I described in Chapter Two, and show how they are linked together in the relationship with theory. Then I show the contribution of powerful experimental techniques (such as ARPES), and discuss their importance for theory formulation and testing. Finally I show how the instrumental use of evidence for supporting competing theories fosters controversy, by considering examples of some recent publications in HTS in which opposite theories are supported using the same new empirical evidence.

In looking at these examples I pick apart and reconstruct the strands of a complex debate in which each of two warring camps claim to account for the same evidence, meanwhile claiming that this evidence is incompatible with the opposing theory. Using the language of Bogen and Woodward introduced in Chapter Two, I argue that in fact the two camps do not account for the same evidence but rather for different phenomena that each constructs from the data in its own contentious way from its own contended point of view. This strategy, I argue, devalues the importance of empirical evidence in testing and confirming theories and it fosters dissent in the HTS community. I close with the pessimistic hypothesis that use of this strategy may be becoming more widespread in contemporary physics, leaving things other than empirical evidence – like mathematics – to do the heavy lifting in theory confirmation, as Peter Galison suggests.

Until this disparity concerning the adjudication of evidence re theories is clarified, combining such vocal criticism of adversaries' hypothesis with differing standards for evidence is going to inevitably foster destructive dissent. "There are a lot of different materials, and room enough for everyone!" says Douglas Scalapino, and I join his call for a more tolerant pluralist approach, to the benefit of the field.

CHAPTER TWO

"Tradition is a guide and not a jailer"
William Somerset Maugham

In this chapter I present a central thesis. I claim that the hundred-year long quest for the understanding of superconductivity shows the interplay of two scientific traditions, which I call *empiricist* and *principled,* and that philosophy of science has overlooked the epistemic value of the first. In particular, the evolution of the debate within the history of superconductivity, up to the recent dissent on HTS, shows a crisis in that interplay.

This will be argued for, in the next chapter, through a detailed historical analysis. But in order to introduce and characterise in this chapter the empiricist tradition and the principled tradition for problem solving in physics, I will first give a short review of the philosophical literature on experiment in science that is relevant for my thesis, since most of the concepts and insights that constitute the two traditions are already found there. This will allow me to specify how the two traditions, as I define them, map into those concepts used in contemporary philosophy of science. The relationship between theory and experiment as it is understood in the "standard view" will be challenged by this analysis.

Having outlined such concepts in this chapter, my first goal in the next chapters will be to show the presence of the two traditions in the actual practices of the scientists involved in superconductivity research from its beginning.

My second goal will be to show how the dialogue between the two traditions has developed through both collaborative and competitive phases.

In particular, my third goal will be to exemplify the two traditions coming to clash through the quarrels of two of the most important physicists in the field, Anderson and Matthias.

2.1 Inspecting Experimental Practice.

Acknowledging the failure of 20th century philosophers of science in trying to derive universal and a priori precepts to model scientific conduct, some philosophers have in more recent times called attention to the fact that the role of observational and experimental practice has been vastly overlooked. In focusing primarily on theories of logic and rationality, the vast complexity of experiment in its several roles has been undervalued and left unexplored.

The unfavourable treatment reserved for experimental practice has a root in Logical Positivism. For the logical positivist, scientific theories are deductively closed collections of propositions. Observation reports employ terms which signify observables, and scientific explanation (and predictions, and confirmation) is built on inferential relations among sentence-like structures. Many philosophers have criticised the influence of Logical Positivism on modern philosophy of science[1].

Since then, seminal works have appeared which started filling the gaps left by the positivist account, exploring the gritty details of experimental practice. Philosophers such as Hacking (1983, 1992), Morrison (1998), Bogen (2002), Franklin (1989), Steinle (1997, 2003), Cartwright (1989, 1999), Rouse (2009) and others have articulated and responded to this need. Important work has appeared that deals more widely with the relationship between theory and experiment.

In classic discussions, this kind of relationship was pictured as rather static, a synchronic investigation of the finished product of scientific activities. Later, philosophers started to pay attention to the diachronic study of scientific change, following along the evolution of theories before and after they could be considered finished products. I will outline in what follows some of the main issues highlighted by this more recent body of works, and then show how the epistemic value of what I call 'the empiricist tradition' can be defended through it.

2.2 The Challenge to the Standard View on the Theory-Experiment Relation.

The issue of how empirical results help us explore, constrain, and evaluate, possibilities for the solution of a scientific problem is central.

[1] A review on this issue is found in Bogen (2002).

A first problem with the standard view is that received confirmation theories have repeatedly failed to fit with seminal episodes from the history of science. In particular, it has been argued that the uniform application of even the most popular approaches and principles, like Bayes' theorem, or the rules of a predicate calculus, are, up to now, inadequate to fully account for how empirical results argue for or against claims in virtually all cases of good practice - while remaining useful tools to model cases in either ideal or specially selected situations.[2]

A second issue concerns the simple acceptability of data, invoking the theory-ladenness of observation. Hanson (1958) pointed out that we tend to notice this or that *thing* only when we have expectations that highlight it as interesting or that give sense to it, and that such expectations are often of a theoretical sort. Popper puts it in a very radical way, which allows me to introduce the issue directly relevant to my thesis, as the interplay of theory and experiment in generating knowledge and explanation:

> "The theoretician puts certain definite questions to the experimenter, and the latter by his experiments tries to elicit a decisive answer to these questions and to no others. All other questions he tries hard to exclude.... It is a mistake to suppose that the experimenter [...aims] to lighten the task of the theoretician, or [...] to furnish the theoretician with a basis for inductive generalizations. On the contrary, the theoretician must long before have done his work, or at least the most

[2] According to Bayesians, empirical evidence, e, confirms a claim, h, only if the probability of h conditional on e and background knowledge, k, is higher than the probability of h conditional on k alone (and, according to some Bayesians, higher than some threshold probability above 0.5) (Earman & Salmon, 1992, pp. 89–100). "But many effects calculated from data which investigators accept as good evidence fail to meet this test, as do most of the data investigators rely upon. Often they fail by default because there are no non-arbitrary, objective ways to determine the prior probabilities required for the calculation of the relevant conditional probabilities" (Bogen, 2002)

important part of his work: he must have formulated his questions as sharply as possible. Thus it is he who shows the experimenter the way. But even the experimenter is not in the main engaged in making exact observations: his work is largely of a theoretical kind" (Popper 1959, p.107).

The picture that Popper gives of the relationship between theory and experiment is quite simplistic. As I will argue later, the *empiricist* tradition that I will present in this chapter pictures a great deal of scientific knowledge as built relatively independently from theory, and thus clearly defies the description given by Popper and the standard tradition it exemplifies.

Several philosophers, such as Hacking (1983, 1992), Bogen (2002), Cartwright (1989, 1999), Morrison (1998) and Steinle (1997, 2003), have questioned the undisputable dominance of theory over experimental work. I will present briefly those of their considerations which are important for my thesis.

2.3 Theory Independence of Observations and Conceptualization.

Discussing a large number of scientific episodes in his book 'Representing and Intervening' (1983), Hacking highlights what he calls 'noteworthy observations' such as Bartholin's experiments on Iceland Spar, Grimaldi and Hooke's experiments on diffraction, and Newton's experiments on dispersion of light. These examples are notable experimental results that have had revolutionary theoretical consequences and, most importantly, preceded theory, reversing the Popperian and principled ordering. He writes: "Now of course Bartholin, Grimaldi, Hooke, and Newton were not mindless empiricists without an 'idea' in their heads. They saw what they saw because they were curious, inquisitive, reflective people. They were attempting to form theories. But in all these cases it is clear that the observations preceded any formulation of theory" (p. 156).

Let me anticipate here my argument that the cases discussed by Hacking are exemplary of what I call the *empiricist* tradition. Can we say something more about these investigators' aims and methods, other than that they were not mindless empiricists, but actually curious, inquisitive and reflective? I believe we can, and I define the empiricist tradition as an attempt at characterising the aims and methods of the scientist such that scientists work through experimental enquiry towards a conceptual articulation of the world, yet without having a specific theoretical framework in their mind.

This characterisation bears several similarities with the classification of phenomenological and bottom-up approaches, and is indeed a generalisation partly rooted in those notions. I will shortly introduce phenomenological models, and then suggest that thinking in terms of an empiricist tradition is both more apt to describe crucial features of the HTS case and, in general, allows us to account more flexibly for the dialogue of theory and experiment in scientific progress. The generality of my definition naturally militates against precise characterization. Yet I maintain that this lack of precision is true to my intention to avoid an inappropriately 'clinical' methodology, and better reflects the complex actual practices of science, which present a mixture of skills, judgement, and preferences, resistant to clear systematization.

2.4 The Many Roles of Experiment.

The first step in our articulation of the two traditions is an expansion on our understanding of experiments in the many roles that I claim they play in physical research.

Most philosophy of science has focused mainly on its role for confirmation. Nevertheless, experiments have a life of their own and can play different roles (Franklin, 2009). Here are the roles I shall show them to be playing in the HTS case:

1) Experiments **define:** As we will see, superconductivity is initially a phenomenon observed experimentally - an unexpected behaviour of matter that has now troubled physics for a century. Since experimentalists characterise the features of this phenomenon, it is experiment which *defines* what is there to be explained.

2) Experiments **discover:** Aside from the initial discovery of the phenomenon of superconductivity one hundred years ago, experiments have been used to discover not only new materials and applications, but also new features of the phenomenon. On the one hand this could be taken as an almost theory-independent area of research, one that spurred the development of a new field of research called 'Materials Science'. On the other hand this feeds into theory, contributing to the role that I present below as my fourth point.

3) Experiments **test:** In the interest of confirming theories, experiment is the production of data (before and after interpretation) for the purpose of testing theories and their claims. (As many HTS theories are not mature enough to formulate clear predictions, this path is paradoxically the least

well trodden of the four, despite its importance in the standard view on the role of experiments).

4) Experiments **help articulate theory**. I will discuss this last role in more detail now, as it represents a focal point, which helps to legitimate the empiricist tradition.

This final role - of helping to articulate theory - naturally incorporates parts of the three other roles. In particular, one could argue that this role is reducible to the third role, that of testing theories. For example, suppose we confront a claim of a theory, such as a prediction of x, with experimental results, such as data that read out observable values of x. The test is negative. We can see this test as speaking in favour, if not of abandonment then of revision of the theory, hence helping the theory to grow (Paying due consideration to the problems expressed by the Duhem-Quine thesis). But this only suggests that the confirming role of experiment expressed by (3) *can* be sometimes reduced to the *revisionary* role expressed in (4), not vice versa.

In fact, in many cases, most notably exemplified by phenomenological models (As I will discuss shortly) we do not have the good fortune to start with *any* available theory, and in this sense the role of testing (3) is much more narrow, rigid and static than that of helping articulation (4). For instance, testing is mainly considered to describe the act of predicting to the best of our theoretical abilities what is in a black box, and then opening the black box to see, to the best of our observational and technical ability, what is actually there. This idealisation fails to describe the majority of cases in which experiments meet theory. In particular it is mostly inappropriate for cases such as HTS, that lack well-rounded theories capable of clear confrontation with evidence, and it is even more obviously unhelpful when we try to apply it to the initial stages of HTS and LTS research, where theory is to some extent a *goal*, not a given background in dialogue with experiments. The role of experiments in the formulation of models, theories and explanations will be discussed in more detail throughout the thesis.

The second ingredient needed for the definition of the two traditions is provided by the notions of *phenomena*, *phenomenological model,* and *exploratory experiment*, and in particular by the latter's epistemic significance.

Let me now introduce these notions with some appropriate background.

2.5 Phenomena and Models.

Acknowledging the contribution of experimental practice in its fourth role, as more than a passive test for theories, how can we understand the relation between theories and the world? Recent philosophical literature has emphasised the need for **models,** to articulate the content of scientific theories.

First of all, by 'model' we may mean a plethora of things: physical objects or descriptions of them; set-theoretic structures; or equations - and mixtures of these. Some models represent; they are models of things in the world, or idealized versions of things in the world. The billiard-ball model of a gas is an example. Other models are of theories, models that constitute what a theory represents, such as models of Euclidean geometry, which include any different structure that can be derived from Euclidean axioms.

There is no single definition of 'model' that applies uniformly to all the cases where models are produced and used, but this is not surprising and should not worry us. Philosophers that align themselves with the semantic view of theories take models as "the central unit of scientific theorizing" (Frigg & Hartmann, 2009), whatever may be their particular notion. To spell out the ways in which theories articulate the world, models have been suggested as mediators. Yet, as noticed by Rouse (2009), the discussion of models as mediators has generally focused more on the relations between theories and models than on the relations between models and the world.

Some progress towards bridging this gap is found in the literature on *phenomena*, as well as the literature on *representation.* The latter is concerned with the mechanics of how models represent, namely what exactly is a model is, and how it is used to represent target situations in the world. I, however, am concerned with a different set of issues: Supposing, for the sake of argument, that we understand how models represent targets, what differentiates the content of empirical and theoretical models, and how do the two kinds of content relate to each other and to the world? For the purposes of my thesis I will therefore focus on the contribution of the literature on phenomena.

How, then, can data be evidence for theory when theories do not strictly explain any particular set of data? In their seminal paper (1988), Bogen and Woodward suggest that *phenomena* are explained by theories, while data are not. Phenomena appear as a new *epistemic* and *ontological* mediator between data and theories, in that they are ineliminable and reliable features of the world, while at the same time being inferentially justified (Glymour, 2000). *Particular observations*, by contrast, are in their view contingent, bearing no direct relation to theories. A more complex reading of the link between phenomena and data suggests that we actually never even deal with *raw* data initially, but create *models of data*, each one inferentially justifying the subsequent one - a chain of models that reaches up towards the theoretical models it justifies. This way, models of data constitute evidence for phenomena, which in turn constitute evidence for models. We gather data, assess their reliability, and infer phenomena from observed patterns in the data. We then see whether the phenomena are, or can be, explained by theories.

McAllister (1997) has argued that, while this distinction is important, phenomena - like data - end up being features of the world that depend on the scientist's judgement in relatively loose ways. His argument is that since any data set is consistent with an infinite number of patterns (for a non-zero level of noise in the setup or in the signal constituting the data), the choice as to which of these patterns ought to constitute the phenomena is ultimately subjective.

Similarly, for Hacking (1983), creating a phenomenon is an achievement. Our conceptual understanding is rooted in our ability to analyse nature's complexity, and we do this not only by distinguishing different theoretical laws, but also by "presenting, in the laboratory, pure, isolated phenomena" (p. 226).

While we may disagree on the extent of subjective judgement actually exercised by scientists in the creation of phenomena, it is easier to agree that evidence alone is insufficient to constrain the patterns into which the data is supposed to fall, and hence to create particular phenomena.

2.6 Phenomenological Models.

McMullin (1968) defined *phenomenological models* as models that are independent of theories[3]. Along these lines, Cartwright (1999) questioned the 'covering law' account, which suggests that theories need only to be provided with the appropriate auxiliary assumptions for particular contexts in order to imply the data/phenomena. Fundamental theories, she said, are linked to the appearances by means of phenomenological laws, which are descriptive but non-explanatory. She took the separation between concrete and abstract laws to define the distinction between models and theories; the debate generated by this work has focused on the issue of supremacy between 'bottom-up' and 'top-down' approaches.

Einstein (1954) described the top-down approach (or *constructive*, as he also called it) as attempting to build "a picture of the more complex phenomena out of the materials of a relatively simple formal scheme from which they start out". In contrast, for the bottom-up approach, "the elements which form [the] basis and starting point are not hypothetically constructed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy".

So, in the process of understanding a phenomenon, for example, we can either start from the basic constituents and elementary processes that build 'up' a phenomenon, from the 'bottom'; or we can start from general principles, at the 'top', and deduce

[3] Nevertheless it has been pointed out that many models typically described under the 'phenomenological' label incorporate aspects of theories, like principles and laws, even though they cannot be strictly derivable from theory (Frigg, R., & Hartmann, S., 2009)

downwards the properties of individual processes from the requirement that these processes should follow from, or at least be in accordance with, what is stipulated by the general principles.

Cartwright asserted in 1983 that ''to explain a phenomenon is to construct a model which fits the phenomenon into a theory''(p. 17). According to her, the complexities of experience seem not to be properly describable with the concepts of any exact science, despite the hopes that they can be. Rather, they are at best only describable with concepts from mixed theories and domains, including a good number of descriptions from ordinary life and technical disciplines. When it comes to describing real world situations as accurately as possible, she argues, it is phenomenological models that do the job. With luck we can then eventually bring the phenomenological models into a mediated relationship with theoretical laws, but no direct derivation of phenomena from laws seems possible. Theories, she says, are not 'vending machines' into which one can insert a coin - a problem - and expect a model to pop out (Cartwright 1999, Ch. 8).

There are many cases that exemplify the lack (or the impossibility) of an immediate deductive relation between the elements of theory and the description of the phenomenon. One is the London model of superconductivity, which arrives at its principal, extremely successful, equation by phenomenological considerations, without any theoretical justification (Cartwright, Shomar, & Suárez, 1995). We will encounter London's model in the historical excursion into superconductivity, in the next chapter.

Hacking (1983) also gives an example from condensed matter physics: The theory of the properties of metal and alloys (1936), in which Mott and Jones give a list of experimental laws that were established long before there was any theory to explain them. Reversing the top-down hierarchy, any decent theory of metallic conduction of heat and electricity was required to cover those laws.

There is room for debate over whether phenomenological models are *almost completely* theory-independent. Margaret Morrison (1998), who argues that theoretical models also describe and represent reality, thinks that even what we typically call 'phenomenological models' are in some sense theory-laden. My analysis does not require that I take a stand on this debate. What is relevant for me is to clarify that while acknowledging the difficulty or even impossibility of an uncontroversial and clear-cut categorisation of models as either phenomenological or theory-driven, we can still roughly define, and make use of, two different sets of methods and aims that seem to *prima facie* roughly differentiate between the two approaches.

As I mentioned earlier, the two characteristic methods that roughly divide the two approaches are:

a) Building 'up' the theoretical representation by starting from the characterisation of the phenomenon, i.e. starting from empirically discovered elements that theory has to satisfy.

b) Reaching 'down' to the concrete and complex phenomena by starting from abstract elements, principles and simple formal schemes.

Taking these methods as tools, it is clear that they are not incompatible; one could in principle even go back and forth between them.

Now I consider aims. The different aims associated with the approaches have been outlined in different ways. A common view seems to be to distinguish between, on the one hand, aiming at a unified picture, coherent with all accepted theories and principles of physics, and, on the other hand, aiming at the most accurate description of reality in all its complexity.

For the purposes of my thesis, I will take the following aims as relevant:

A) To give the most accurate and complete description of the phenomena.

B) To explain the phenomena in a way that is consistent with accepted/acceptable principles and theories.

It is apparent that ideally scientists would like to achieve both aims. Some philosophers have argued that this is impossible. According to Cartwright (1999), for example, it is often the case that abstraction, and consistency with principles, conflict with accuracy in describing the many features of a phenomenon, and she argues that in the strongest sense this is actually always the case. The HTS story is to some extent compatible with Cartwright's claim, but without needing to commit myself to any such claims, it is important to recognise that in general a real theory in a non-ideal case will only be able to achieve these aims *partially*, leaving room for future improvements, even for the most successful theories.

This being the case, we will find that when we need to evaluate theories or models (or practices) we can try to assess how well they fit the two different desiderata; we would then evaluate their merits differently according to the different weights that we may assign to the implementation of each of the two aims.

I claim, and will illustrate in this thesis with the case of HTS, that we can distinguish two different traditions, in such a way that does not necessitate the wholesale *presence* of different aims and methods. Both types of method and aim may very well *coexist* in a particular physicist's approach. Instead we will find that the traditions influence the *preferences* that they exercise in their research programmes. Each physicist can be seen assigning her own degree of importance to each tradition.

2.7 Exploratory Experiment.

Before I finalise my characterization of these two traditions, I need to clarify one last concept, that of an exploratory experiment.

Among the types of systematic experimentation that are not guided by theory, Friederich Steinle (1997, 2006) introduces one called 'exploratory'. In his view, it typically occurs in those periods in which scientists are entering new research fields. In my view, and maybe compatibly with Steinle's, exploratory experimentation practices are found even in later, more advanced, stages of theory, particularly when, as in the case of superconductivity, the phenomena are very complex.

Despite its independence from specific theories, exploratory experimentation is characterized by a collection of different experimental strategies. Steinle identifies some of these methodological strategies, which he finds exemplified in various historical cases (Steinle, 1997).

Some of his most important ones are:

* Varying a large number of different experimental parameters,

* Determining which of the different experimental conditions are indispensable, and which are only modifying,

* Looking for stable empirical rules,

* Finding appropriate representations by means of which those rules can be formulated,

* Forming experimental arrangements which involve only the indispensable conditions, thus presenting the rule with particular clarity. (p. 70)

The aim of these strategies is *to find general empirical rules, and appropriate representations by means of which they can be formulated.* He gives several examples of such experimental practice, which we now present and discuss.

One example involves Charles Dufay and his research in electricity in the 1730s. The field at that time was in a situation rather similar to that of superconductivity at the beginning of the Twentieth century: A multitude of different phenomena were puzzling scientists all over Europe:

• Some materials could be always electrified by rubbing, others sometimes electrified, others not at all.

• Sometimes bodies could be made electric by contact with others, sometimes not.

• Sometimes electricity acted as attraction, sometimes as repulsion.

• Sometimes sudden changes regarding attractive and repulsive effects occurred. (Steinle, 2006)

Experiments on these phenomena were delicate and hardly repeatable, and the results unclear. Dufay's experimental attack was sophisticated. He worked with a large number of different materials, both in combination and in simple single samples. He explored varying all sorts of parameters, like shape, temperature, colour, moisture, air pressure, and the experimental setting (two bodies in touch; in close neighborhood; at a large distance; connected by a third; and so on). "His work led to remarkable results and bold claims", remarks Steinle, "such as all materials except metals could be electrified by rubbing, and all bodies except a flame could receive electricity by communication" (p. 184).

Newer and newer experiments kept providing local empirical rules and regularities, such as that when an unelectrified body was attracted by one that was electrified, and touched it, it would suddenly repel it after the contact, but these regularities were not sufficient to account for the whole complex behavior of materials. As Steinle (2003) explains, "after additional experiments, Dufay finally made what he called a 'bold hypothesis': If one did not speak of electricity in general, but of *two* electricities, with similarly electrified bodies repelling each other, and dissimilarly electrified ones attracting each other, then his experimental results suddenly started to make sense" (p. 412). Thus originated the conception of two classes of electric materials. Glass and wax were the most prominent representatives of these two classes, and gave the name to what are called "vitreous" and "resinous" electricities. His concepts allowed him to subsume hundreds of experiments under general regularities, and this success established the two electricities in the scientific thinking of the time.

Another example is Ampere's "astatic magnetic needle" experiment (Steinle, 1997). “A magnetic needle is supported such that it can rotate only in a plane perpendicular to the plane of magnetic dip. Thus terrestrial magnetism cannot affect its motion, and the effect of an electric wire, supported by two variable posts, can be studied by itself” (p. 66). Ampere pursued a long series of experiments in which he varied the relative position of the needle and a nearby connecting wire; he did not employ the guidance of any specific theories of electricity and magnetism, neither did he use them for interpretation. Eventually he formulated the general rule that the needle always swings perpendicular to the direction of the wire. “Ampere attributed to it a particular status: He called it a ‘fait general’ and he surmised that all interactions between wire and needle may be ‘reduced’ to it and a second ‘fait general’” (p. 67). - which we need not pursue here

“What is developed in all those cases” Steinle claims “is a new and fundamental conceptual framework for a certain field” (p. 72). This establishes a new language, and sheds new light broadly on the field. These developments are of immense epistemic significance.

> “Concepts and classifications-the very language used to deal with a certain field-determine on a fundamental level the development of the thinking and acting within that field. Any more specific theory necessarily makes use of those concepts and classifications. In the process of forming and stabilizing the conceptual framework, decisions are made to which a whole subsequent

> development of more specific theories may be lastingly-and sometimes unconsciously-committed" (p.72)

For this reason Steinle argues that non-theory-driven experimentation deserves much greater attention in philosophy of science. This is actually in clear contrast with the notion of theory as a necessary guideline for the discovery of regularities. In addition to the bell tolled by Popper, quoted at the beginning of this chapter, Kuhn (1961) expresses this principled view when he says that "to discover quantitative regularity one must normally know what regularity one is seeking and one's instruments must be designed accordingly; even then nature may not yield consistent or generalisable results without a struggle". But he then adds the qualification that "this does not mean that no one has ever discovered a quantitative regularity merely by measuring" (p. 189). His examples are Boyle's Law relating gas pressure with gas volume, Hooke's Law relating spring distortion with applied force, and Joule's relationship between heat generated, electrical resistance, and electric current which were all the direct results of measurement. There are other examples, he says, "but, partly just because they are so exceptional and partly because they never occur until the scientist measuring knows everything but the particular form of the quantitative result he will obtain, these exceptions show just how improbable quantitative discovery by quantitative measurement is" (p. 174).

I believe that the many cases presented by Steinle, and the case of HTS, show not only that these successes of experimental practice are not exceptional, but also that they cannot be accommodated by the tradition of theoretical dominance, as they happened in situations where much of the theoretical understanding was genuinely lacking and could not fulfil any guiding role.

With these elements in place, I can now indicate more fully what I call the empiricist tradition and principled tradition.

2.8 The Empiricist and Principled Traditions.

I claim that the enterprise of building scientific knowledge is driven by two sets of tendencies and aspirations – which I call 'traditions'. The first makes the requirement that a theory should be logically coordinated, and therefore aims for "a theory whose parts are all logically united" and coherent.

As Duhem remarks in his book "The Aim and Structure of Physical Theory" (1914), requiring that a theory be logically coordinated cannot be justified or proven as necessary or reasonable. He maintains that the requirement is legitimate "because it results from an innate feeling of ours which we cannot justify by purely logical considerations" (such as the principle of contradiction or the law of economy of thought) "but which we cannot stifle completely either". In his view, "Reason" naturally aspires to the logical unity of physical theory, even if we have nothing more than common sense to back-up this aspiration. On the other hand, "Imagination", faced with a conceptual framework and what we could call the yet-unrelated parts of theory, desires to embody these diverse parts in concrete representations (p. 102-103). Different scientists have different mixtures of these two tendencies, as if differently dominated by reason or imagination.

In my view, these remarks can be transformed into the two previously stated preferences, and define the two traditions.

For the Empiricist tradition a larger <u>*preference*</u> *is given to practices, experimentation, conceptualisations, and theorisations that give the most accurate and complete description of the phenomenon we want to tackle.* Representing the world (doing justice to its manifest complexities) is our primary goal in our efforts towards understanding it.

For the theory driven tradition, by contrast, a larger <u>*preference*</u> *is given to practices, experimentation, conceptualisations, and theorisations that are able to explain the phenomenon in a way that is logically coordinated and consistent with accepted/acceptable principles and theories.* Logically coordinating the abstract components of theories makes the world intelligible to us.

This bears similarities to the bottom-up/top-down distinction, but does not map precisely onto it. When taken to an extreme, the empiricist preferences may lead a scientist to ignore or reject the principles of our best and most harmonious theories with little remorse, when this appears necessary to accommodate facts, much to the horror of the theoretically-minded. Conversely, principled preferences may lead a scientist to shamelessly devise a series of ad hoc modifications and tangled-up repairs of the "worm-eaten columns of a building tottering in every part" (Duhem 1914, p.217), which would seem unacceptably childish from the point of view of the empiricist-minded. So from this perspective the extreme ends of the preference spectrum map onto, at one end, the empirical Baconian construction of the phenomenon from "amassing facts and discerning regularities" and, at the other end, the principled theorisation that relegates experimental evidence into little more than its confirmatory role.

The aims and methods of exploratory experimentation seem to fit more closely the empiricist extreme of the spectrum, and the top-down approach fits loosely the principled end. Phenomenological theories are therefore no longer in contraposition with principled theories, but in a somewhat mediating relationship between the sets of preferences.

In fact, while on the one hand phenomenological approaches fully acknowledge the basic autonomy of observed regularities, and build up parcels of theory starting from there, on the other hand they accommodate the logically ordering desire, by providing conceptualisation that can even be, in the best cases, subsequently mapped directly onto a first-principle account of the same phenomenon.

My stress on the understanding of these two traditions as a set of *preferences* allows me to make sense of a larger and more realistic set of situations than either can achieve alone. As I mentioned earlier, the desiderata expressed in the two traditions are not incompatible, and are actually considered by most physicists to be both essential requirements for successful physical understanding of the world. Yet we have seen in the discussion of phenomenological models versus theory-driven models, the argument that the nature of abstraction, as a basis for the principled formulation of highly consistent theories, tends to exclude the most accurate representation of reality. Without embarking on a universal defence of this claim, I will simply show this to be the case for HTS. It is irrelevant for my analysis whether we take this as a peculiar and untraceable contingency of the HTS case, or as a generally inevitable consequence of the definition of abstraction and representation (or a combination of the two).

As scientists try to answer a scientific question or solve a problem, such preferences (can) remain silent if no controversy or disagreement arises, even if different methods and aims are used in practice. The case of superconductivity is exemplary in terms of the extent of controversy; I claim that it shows the meeting and the clash of such preferences in practice.

I will next discuss the history of superconductivity, firstly to show how these preferences *met*, and subsequently, by analysing some more specific views held by some of the most prominent figures in the debate, to show how they *clashed*.

CHAPTER THREE

There are more things in heaven and earth
than are dreamt of in our hypotheses,
and our observations should be open to them.
(Cronbach, 1975)

In this chapter I present the first fifty years of the history[4] of superconductivity and extract from it some important philosophical lessons (that will be then carried over to further discussion in later chapters).

I examine the empiricist manifesto that led to the discovery of superconductivity, and show how this unexpected phenomenon resisted all theoretical efforts of the best minds of the century, emerging from the quantum revolution as a stubborn puzzle.

I will discuss examples of advances based on observation and not justified (in the standard sense) by theory, such as Landau's theory of phase transitions and the analogy between ferromagnetism and superconductivity. In particular, the episode of the frozen flux experiments will show the active dialogue between theory and experiment and the potency of the interplay between the traditions to foster this progressive dialogue.

I claim that we can appreciate the epistemic value of the *empiricist* tradition by studying several scientific episodes in the period leading from the discovery of superconductivity to the conceptual developments of Fritz London.

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[4] As I have explained in the Introduction, my main historical reference is Matricon & Waysand (2003). To improve readability I will occasionally put, in this chapter and in the following historical sections, only the page numbers when referring to this source across long sections (after the first full citation). Other sources will be fully cited according to the accepted standards.

3.1 *Door Meten tot Weten* – From Measurement to Knowledge

Low Temperature physics is a relatively young field in the history of physics. From its early days at the end of the nineteenth century until the end of the Second World War, the main centre for low temperature research was undisputedly Kamerling Onnes' laboratory in Leiden. Holland had been extremely active in physical theory since the seventeenth century, hosting scientific figures like Huygens, Spinoza and Descartes, and fostering pioneering work in the applied sciences. The Leiden laboratory, though, started a small revolution in the actual practices of physicists involved in what we today call 'condensed matter physics', and arguably for the whole of science. In fact, as I will show shortly, Kamerling Onnes quickly realised that the discipline needed a different foundation, and required theorists, mechanics, experimentalists, and glass blowers to actively work together (Matricon & Waysand, 2003). He not only created the model for many subsequent laboratories for low temperature research, but he also initiated developments that characterize modern scientific activity.

My story starts with what was initially an intellectual exercise, the liquefaction of gases. It had firstly captured the interest of Faraday in the early nineteenth century, and turned out to be one of the greatest driving forces for technology.

The engine of Onnes' scientific enterprise was indeed the liquefaction of Helium. Helium's history is a fascinating case in the history of science, and not only because it led to the discovery of superconductivity. I will sketch it here briefly.

Through compression, Faraday liquefied almost all the gases and, failing to liquefy the rest of them (even after compressing up to three thousand times atmospheric pressure), he coined the term 'permanent gases' for the sturdy ones that resisted his impressive efforts. Among them was oxygen. While its liquid state had yet to be obtained, liquid oxygen had already been suggested for several industrial applications, showing the remarkable entrepreneurial spirit of the time. This is a noteworthy historical example of pragmatic interests preceding theoretical speculation in high-level physical research. The predicted applications, such as steel refining through purified oxygen, were anxiously waiting for the necessary raw material, and a race to obtain the first drops of oxygen had started[5]. It was a war against permanent gases; science would defeat their alleged permanence at any cost.

The first results, and then the work of eminent scientists such as Joule and Kelvin, brought new qualitative understanding, i.e. that it was not just pressure but also lower temperature that was needed for gases' liquefaction. This started the 'Age of Low Temperatures' (p. 3). The techniques developed by researchers such as Louis Cailletet and Raoul Pictet marked the start of cryogenics (A term coined in 1878).

An important success was obtained by Dewar with the engineering of a double-walled container with a vacuum in between the walls, now named a 'Dewar flask' (or simply a 'Dewar'). The modern Dewar's design is pretty much identical to its ancestor, and represents one of the most successful items of design in history.

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[5] Steel-making with pure oxygen did indeed produce better steel, but was perfected only after the Second World War (p. 2).

The link between cryogenic successes and these simple and sophisticated containers made it quite clear that a neat separation of tasks and specialisations would hinder the quest for liquefaction. Onnes quickly realised, for example, that scientific labs needed the art of glassblowing, to design and produce the technology needed for lower temperatures, so that a lab could be independent, flexible and systematic in its exploration of the properties of matter. He advocated this and much more in a scientific programme that he pushed very strongly at Leiden.

His scientific programme is the first appearance, in this account of the history of superconductivity, of what I call the empiricist tradition, which stands at the base of the very discovery of the unexpected phenomenon, as I am now going to discuss.

In 1882, in honour of his appointment in Leiden, Kamerlingh Onnes gave his first public lecture, outlining a manifesto for research:

> *"Physics owes its fruitfulness in creating the elements of our material well-being, and its enormous influence on our metaphysics, to the pure spirit of experimental philosophy. Its primary role in the thoughts and actions of contemporary society can be maintained only if,* ***through observation and experimentation, physics continues to wrest from the unknown ever-new territory****.*
> *Nevertheless, a large number of institutions are needed for physics to play this role and they need resources. Both are now woefully insufficient when we consider how important it is to society that physics prosper. As a result, the person who accepts the task of forming future physicists and of managing such institutions must be particularly aggressive in putting forth his ideas about what is really needed to carry out experimental research these days.*
> ***Perhaps, like a poet, his work and all his activities are motivated solely by a thirst for truth****; to penetrate the nature of matter might be his principal goal in*

life. Nevertheless, the courage to accept a position that makes it possible to realize these goals must come from the conviction that ***his activities will be useful only if he follows certain well-defined principles.***

What I believe is that quantitative research, establishing relationships between measured phenomena, must be the primary activity in experimental physics. FROM MEASUREMENT TO KNOWLEDGE (Door meten tot weten) is a motto that I want to see engraved on the door of every physics laboratory" (p. 17)

Onnes strongly imposed this agenda in Leiden. Contrary to the practice of his most notable colleagues (for example Dewar) who had established a monopoly on their apparatus, machinery, and even technicians, Onnes allowed visits to Leiden to anybody interested. In fact he managed to 'steal' some of the best glassblowers in Germany, and then started a school for scientific instrument makers and glassblowers at the side of his laboratory. This school used to send his graduates to work at physics laboratories spread all over Europe and it is still graduating students. What is more, as a graduate student in physics in Leiden, the typical pupil would be assigned fifty hours of compulsory work and training in glassblowing and metalworking[6].

Onnes' focus on 'well-ordered' experimental practice and his eagerness for autonomy and efficiency were evident to the rest of the scientific community, which did

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[6] Emilio Segrè (1980), who was a particle physicist and a student of Fermi, pointed out that Onnes' lab represented the forerunner of the institutions of *Big Science.* He noticed that usually scientists or scholars associate the passage of physics to the large scales with the introduction of particle accelerators. While the seeds of big science are certainly visible there, several features that characterize the large scale model had already emerged in Leiden. "The association of science with engineering, the collective character of the work, the international status of the laboratory, the specialization of laboratories centred on one technique, the division of the personnel into permanent staff and visitors. A laboratory with all these characteristics had been formed by Heike Kamerlingh Onnes at the end of the nineteenth century for the study of low-temperature phenomena" (p.223)

not spare him their criticisms. The Dutch physicist Hendrik Brugt Gerhard Casimir, a student and assistant of Ehrenfest and Pauli, who was at Leiden and later would become famous for the Casimir effect, criticized Onnes' epistemological stance in his memoirs, pointing out that we can start to measure only when we know what to measure (Casimir, 1983). Casimir objected to "Door meten tot weten" writing that qualitative observation has to precede quantitative measurement. "By making experimental arrangements for quantitative measurement", he warned his colleagues, "we may even eliminate the possibility of new phenomena appearing"[7]. His remarks anticipate the best of Popperians, and are an example of the predominance of the principled tradition in fundamental scientific research.

Let me remark here, though, that Casimir's objection, and in general the contraposition of the two traditions as incompatible scientific paradigms, is sterile if we read the traditions simply as a set of preferences, as I suggested in the previous chapter. Even though Onnes' manifesto may sound dogmatic, it did not imply that theoretical speculation prior to experiment was of no use, nor that we shouldn't let theory tell us what is there to be measured (when theory manages to express that guidance). Nonetheless, Onnes emphasises the need to recognise the role of "establishing relationships between measured phenomena through quantitative measurement" as a fundamental one inside experimental physics, without dealing directly with concerns of coherence among different sets of relationships and larger explanatory goals. His manifesto exemplifies the importance he had assigned to meticulous and open-minded

[7] He supports this with the example of the discovery of X-rays, interpreted in his own way: According to him, the physicist Lenard "had an experimental set-up which was better for certain quantitative measurement than Rontgen's, so he did not discover X-rays" (p. 161) I will not discuss this further here.

exploratory research, and the great epistemic value he appointed to it; it is, in my view, an expression of preference that aligns him to the empiricist tradition. His work affirmed the strength of the empiricist tradition as a valuable asset in scientific progress, and his manifesto aimed at developing this strength to its full potential.

Regardless of criticisms, Onnes retained focus on his recipe of exploratory experimental practice as constitutive of his large-scale scientific programme, which he accepted as a long term one. In fact, the time needed to get this 'machine' to work (time needed for training, for recruitment, and for the progressive refinement of devices and techniques) kept Leiden for many years out of the race for quick results and publications. Onnes accepted this 'latency' time without reservations, justified by his long-term focus (p. 21). This acceptance turned out to be wise, since it led to the discovery of superconductivity. Liquefying Helium, achieved in 1908, was only the first step.

Nineteenth Century pioneering work on gas liquefaction contributed to scientific progress in several ways. The one most recognized in the theoretical literature, aside from superconductivity, is the development of concepts such as phase transitions and critical phenomena. These concepts are taken for granted today, but they were developed in the process of understanding the liquefaction of gases.

3.2 From Measurement to Superconductivity.

Even today, a typical course on thermodynamics teaches us that as we get closer and closer to absolute zero we approach a state of atomic immobility. As we cool Nature down, from cold to colder, we get the picture of its atoms slowing down. Helium appears in our historical reconstruction as the first serious challenge to this incorrect view. Through a race to ever-lower temperatures, and a stress on techniques devised to make sure that not a trace of air was left in the system, Leiden's laboratory achieved Helium's liquid state. Yet it seemed impossible to solidify Helium. And immobility was a concept that would naturally fit solids but was problematic for liquids.

In fact, the observation of a metal with no detectable electrical resistance (Onnes' consequent discovery of superconductivity) was a bizarre form of 'eternal movement' as temperatures dropped, and so was something that nobody had expected. It is no wonder that the simple observation of this phenomenon gained Onnes a Nobel Prize.

With the liquefaction of Helium, Onnes had all the ingredients he needed for his scientific programme. Helium was just the starting point. He decided that "the entire laboratory would embark on a systematic programme of measurements of the properties of matter at low temperatures" (p. 24). As in the manifesto of his first lecture at Leiden, he assumed physics' job was *to wrest from the unknown ever-new territory, through observation and experimentation*, without having a particular framework or theoretical goal in mind. Once again, even though his description may seem dogmatically empiricist in nature, Onnes did not hide that he had his own theoretical hypotheses derived from previously observed experimental patterns, for example the decrease of resistance (R) with decreasing temperature (T). His intuitions and hypotheses, though, were not the consequence of a theory he maintained as valid. As is typical of the empiricist tradition, he would focus on regularities and observed patterns to search for general empirical rules, and then try to formulate adequate representations of those regularities. Theoretical hypotheses were used as tools, and he was (at least initially) open to alternative incompatible ones as long as they could provide a better understanding of the observed patterns, without worrying too much about coherence.

Dewar had shown already that the resistivity of gold and platinum wires decreases with temperature, but further measurements were showing that the decrease in temperature reached a plateau. Having only observed such minimum value, he assumed that further observations at lower temperatures (for him unattainable) would show that resistivity would increase again. This assumption would fit the traditional thermodynamical picture, since high resistance would cause matter to slow down considerably as it approached zero.

The compelling techniques and the precision that characterized Leiden's laboratory standards, enabled Onnes to explore those cold territories like never before, studying various metals' resistivity. Resistivity (the resistance of a standard sample of the material) could be measured with a classic, simple, arrangement of resistors called the Wheatstone bridge.

To achieve the precision so dear to Onnes, the metal of choice for Leiden in 1911 became mercury. Dewar's gold was not too bad a choice: indeed it was an excellent conductor at room temperature, and its low melting point made it relatively easy to purify. The easily available mercury, though, is the only metal that is not solid at room temperature, as we all know from first-hand experience with thermometers. Given this property, one could easily dip a wire in liquid mercury, and obtain a pure and solidified mercury coat at lower temperatures, which would have much better contact with the wires of the Wheatstone bridge, to achieve maximum precision. Impurities were (rightly) suspected to play a role in conductivity, even though there was no model or principle backing up the idea of impurities as relevant (p. 24).

Even if problematic for the classic thermodynamical picture, an alternative prediction to Dewar's was possible, one based on the following quite reasonable intuition. If, as it seemed, R was indeed decreasing with T, one might expect to observe a smooth decrease of resistivity, eventually reaching zero only at zero degrees, which meant one would never really observe or measure zero resistivity, since one would never reach absolute zero. But instead the results of the experiments with mercury in 1911 showed that the resistivity dropped abruptly: within a range of only three hundredths of a degree, at a temperature of around 4.23K, resistance was suddenly undetectable.

Onnes later tried the experiment of starting a current in mercury below that temperature and observed a supercurrent, a current that showed no change over time. Since currents create magnetic fields, changes in currents cause changes in magnetic fields, and so one is able to measure a change in current with the highest precision available for measuring magnetic fields, a change expected if there is any operating resistivity that slows down the current even minimally. He observed no change. Paul Ehrenfest wrote his amazement to Lorentz: "It's very strange to watch the effects of these 'permanent' currents on a magnetized needle. I can almost feel in a tangible way how the ring of electrons is turning, turning, turning in the wire, slowly, almost without disturbance" (Klein 1970, p.214). Mercury was superconducting.

This surprising discovery started a new debate on electricity and the theory of matter. What followed was a characteristic example of extraordinary theoretical advancements motivated not by deduction or theoretical considerations, but purely by unexpected revolutionary results produced through good experimental practice.

How could one make sense of the bizarre phenomenon of superconductivity? The real difficulty, as physicists would soon find out, was that no one actually understood *simple* conductivity, even in ordinary metals (p.29). The current theories of matter and electricity were clearly inadequate. It is not often that theories are tested by explicitly trying to refute them. They are more commonly found to be faulty as they become inadequate to explain new facts experimentally observed and obtained via exploratory experimentation. This is one of the merits of revolutionary discoveries. They not only create new questions to be answered, and display new phenomena for theory to explain, but also often force the theoretical community to wake up to the notion that they had not actually understood sufficiently, or even understood at all, what they thought they had understood.

As this is a crucial point I will now sketch the evolution of our theories of metals, and briefly comment on it.

3.3 The Traditions on Traditional Metals.

A crucial set of observations, which was well known long before superconductivity, showed that two properties of metals, thermal and electrical conductivity, were closely and mysteriously related. Good heat conductors are good electrical conductors. We now know that the reason why a silver spoon in a teacup becomes quickly hot is the same reason why it conducts electricity very well (though it is too expensive to make electrical wires out of silver). But before the notion of free electrons – indeed before the discovery of the electron - this was a purely empirical observation, quantified in the Wiedemann-Franz law in 1853. This phenomenological law stated that the ratio of electrical conductivity to heat conductivity at a given temperature was the same for all metals. The law was very well confirmed experimentally. The challenge was on theory. The first person to find a model that could account for the law was Drude in 1900.

Drude's model involved remarkably simple intuitive assumptions, taking the electron as the carrier of the current (The electron had been discovered just three years earlier), and the atoms in the lattice as the cause of resistance. In this model, a metallic crystal is a lattice of atoms, which are like balls connected by springs that tie them to each other and restrict them to small movements. When one of them is disturbed, the disturbance is transmitted to the neighbouring atoms, which in turn start to vibrate. The thermal energy is the energy associated with the vibrations. The billons of ways in which we can disturb the lattice, creating a vibration, give us billions of different ways for that metal to be 'hot'. Itinerant electrons, while transporting electric charge, collide with atoms and hence transport thermal energy, which then propagates on through the solid 'as if through the springs' (p. 33).

The Drude model managed to explain the Wiedemann-Franz law - an immediate success. Even today, students taking an elementary course in physics learn this model of metals, and only more advanced students leave behind this simple idea, replacing it with the more theoretically accurate quantum-mechanical description.

The model, though, was to many physicists deeply unsatisfactory because it was crude and mathematically unsophisticated. In particular it was unsatisfactory to most theoretical physicists, who had a strong preference for the principled tradition. The theory involved calculations that ignored the fact that the free electrons constituted a gas.

The authoritative theory for gases at the time was Maxwell-Boltzmann's, so, driven by a natural, unquestioned, preference for logical coordination between principles and theories, Lorentz decided to redo the Drude calculations with more sophisticated mathematics [using gas theory]. His goal was to make the model consistent with accepted principles and theories, sure that that would amount to a much more complete and 'sensible' model (or, in his view, the 'true' theory) (p. 34).

Since, firstly, for good reasons there was no shortage of praise for the Maxwell-Boltzmann theory, which was the basis for the kinetic theory of gases, and is used in most circumstances today (for example, where quantum effects are negligible, and molecular chaos is absent), and, secondly, Einstein (1953) said that Lorentz was, at the turn of the century, rightly considered by theoretical physicists in every country as the leading mind among them, there was great confidence that his efforts in applying the Maxwell-

Boltzmann theory to Drude's model would produce a significant improvement. Instead, most of the predictions derived from his calculations[8] were a step backwards, largely in disagreement with experiment, even though his model was considerably more elegant (by standards of mathematical sophistication and logical coherence). The clash was evident and problematic. The community was stuck.

The next best attempt at the problem was the Debye model, in 1912 (This model, also, is still used today for practical purposes). It ended up being in clear contrast with Maxwell-Boltzmann's statistics.

Debye had started from an idea of Einstein's: To apply the quantum hypothesis, previously only applied to light and to the photoelectric effect (which won Einstein the Nobel prize and that will appear again in the last chapter as the basis of the ARPES technique for HTS), to the mechanical vibrations of atoms.

Through the application of Planck's formula, $E=h\nu$, we find in Debye's model the first introduction of the quantum of vibrational energy now known as the *phonon*, analogous to the quantum of electromagnetic energy, the photon. Debye assumed phonons of different frequencies, and ingeniously considered the solid *as a whole*, achieving a great fit with data at low temperatures.

Like Drude's, his model was able to achieve very good fit with experimental data and explain specific heat without including any contribution from the electrons, which was again in contrast with what was prescribed by Maxwell-Boltzmann's statistics, where the electron gas had to make a contribution roughly equal to the atom's contribution.

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[8] Performed between 1904 and 1905 (p. 34).

The contradictions remained troublesome for many years, until the five most prolific years of modern science, 1928 -1933, offered a solution to most of them (p. 36).

For instance, the formulation of Fermi-Dirac statistics offered a totally new way of looking at matter, and exposed Maxwell-Boltzmann's statistics as inadequate. To start with, Pauli formulated his exclusion principle, which immediately explained why all the experiments designed to count the abundant electrons that were expected to be drifting about (in abundance) had only found few: The only important electrons are the ones near the Fermi level, which can jump to an unfilled level of energy.

It is not relevant here to explain in detail the statistics of Fermi-Dirac. It is merely necessary to notice that the classical picture that imagined immobility at 0K fitted with Maxwell-Boltzmann theory, since if average kinetic energy is $3kT/2$ then at $T=0$ even electrons should be still. Fermi-Dirac statistics instead predicted that electrons in a metal at 0K run around at speeds that exceed 10000 km/s (p. 38), a very different micropicture that suddenly seemed to fit the revolutionary experimental discoveries.

This is a beautiful example of the constructive dialogue in action between the precepts expressed in the empiricist and principled traditions. What I want to stress is that the remarkable advances in our understanding of metals, outlined in the short history above, have come about through a step-by-step alternation of different criteria for theories. The phenomenological description, devised for the purposes of modelling a peculiar behaviour of matter, spurred the interest of some theorists who believed deeply in internal consistency, and explanation through fundamental principles, as the ultimate guidance for theory. As they applied such guidance they did not achieve the improved results they had expected. In the light of that failure it was not the experimental basis but the initially assumed principles and theories that were later questioned, even though such principles had been among the most successful available to theory. This brought about a revolution in the theoretical landscape (that went beyond the original scope of the questioning).

This analysis is not meant to elicit a surprised response from the philosopher, who may even find it comfortably familiar, but not all physicists accept its implications. As I will explain later, PW Anderson, in his early objections to the empiricist tradition, and in his dominant role in HTS research, gives an example of the principled tradition, in which experimental results and observed regularities are driven to be discarded when in clear contradiction with theoretical constraints that are supposed to be unquestionable. My analysis of this episode also violates the spirit of the criteria formulated in Anderson's "central dogmas". But I leave further discussion of this to the next chapters where I will introduce his view fully.

Now that a new statistics was in town, Arnold Sommerfeld applied Fermi-Dirac Statistics to the original Drude model, achieving immediate success. Specific heat was understood; agreement with data was almost perfect. Both parties were happy. Or almost... because the next hot thing for theorists was the newborn quantum theory.

Most of the new generation of physicists were enthusiastic about the emerging quantum picture and eager to apply it to contemporary problems (p. 40). The principled tradition, like the emperor, now had new clothes: A successful theory of matter that was not completely quantum had to be either incomplete or wrong.

This criterion was very hard on the theory that Sommerfeld had been formulating. His theory was troublesome for his young students because it was semiclassical. He is renowned for what physicists call 'semiclassical models', which still populate the landscape of physical theory. He treated the electrons like classical particles (Billiard balls, as in Drude's model); the quantum component was only in the statistical distribution of energy (The new Fermi-Dirac statistics as opposed to the classical Maxwell-Boltzmann one).

To many this was unacceptable. Felix Bloch, one of Pauli's students, decided to follow De Broglie's idea of treating electrons as quantum mechanical objects (waves of probability), the idea that had led to the Schrödinger equation. The mathematical expressions for the electrons as waves, which we find today in quantum mechanics textbooks under the name of 'Bloch functions', were developed in this pioneering work (Bloch, 1976), and they remain a tool in all quantum mechanical accounts of electrons. This up-to-date model was able to justify *a posteriori* Sommerfeld's model, while introducing several new theoretical tools. Thanks to these new tools, transport phenomena in metals, such as electrical and thermal conductivity and other crucial mechanical, magnetic and optical properties, were successfully explained.

One phenomenon, though, resisted all theoretical attacks: Superconductivity.

3.4 The Empiricist Tradition again: Superconductivity and Ferromagnetism.

While more and more features of metals were being explained, and the creation of a larger coherent picture of solid state physics (nowadays called 'condensed matter physics') was advancing remarkably, superconductivity managed to resist the most determined efforts of the best physicists around. Old and new generations met in failure; from Bohr to Einstein, from Bethe to Brillouin, from Heisenberg to Lorentz, all had tried and failed. Bloch expressed the exasperating nature of the tricky phenomenon by formulating a theorem; it seemed to him evident at this point that [Bloch theorem] "*Every theory of Superconductivity is a false theory*" (p. 44).

The phenomenon was so theoretically elusive that it was not even mentioned in the encyclopaedic compilation "The Theory and Properties of Metals and Alloys" (Mott & Jones, 1936) that offered instead the case study discussed by Hacking in "Representing and Intervening" (see 2.6).

If before the quantum hypothesis the impasse in superconductivity was troublesome, quantum successes in every problem but superconductivity made the impasse truly exasperating. All sorts of hypotheses were suggested, from the electronic ice of Bohr, to spiral electron trajectories by Einstein (p. 40-41). Ideas such as these were classical in nature, and most physicists dismissed them on principle. They were more or less guesses, and had various problems, although they have been recycled in subsequent theories, and were worked out in fuller detail many years after their quick dismissal (For example, Einstein's improbable idea of electrons moving somewhat artificially in spirals, as if in a bound state, is very similar to bound states suggested as the explanation of some HTS)[9]

As in detective stories where Sherlock Holmes faces a mystery and, stuck without a lead, meticulously accumulates clues, hoping for patterns and causal links to emerge from them after careful (and fortuitous) observation, the heroes of superconductivity started to absorb, from observations, some quantitative and qualitative features of the strange phenomenon.

The empiricist tradition was in vogue again, as principled guidance had proved unfruitful. Processes of abstraction, on which modern science thrives, incur the inevitable risk of becoming too detached from the world and the phenomena, as many scientists and philosophers have observed (Bogen 2002, Cartwright 1999). After all, it does not seem surprising that when abstraction leaves us at loss we 'touch base' and sit for a while, without good principled leads, getting reacquainted with the problem at the phenomenological level, from which the abstraction may have led us astray. This is one of the powerful ways in which theory and experiment mingle and foster innovation through an exploratory attitude.

For instance, returning to the historical account of superconductivity, it was at this moment of impasse of research in the late Twenties that two very different scientists, Lev Landau and Bloch, became interested in an apparently superficial empirical observation, the resemblance between ferromagnetism and superconductivity.

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[9] We may read this as a spontaneous manifestation of what Hasok Chang advocates in his notion of complementary science (Chang, 2004). Ideas get abandoned, yet can be resurrected later on and contribute anew, particularly during controversies, as if science gets less 'lazy' and has motivation, time, and resources, to explore more fully its options.

The observation of persistent currents in magnetism had been uncontroversial since Ampere's time, but there was nothing in the theory of metals, in that of magnetism, or anywhere else, that suggested a link between ferromagnetism and superconductivity. However, from a phenomenological perspective, the fact that the old experiments of Onnes with currents circulating unperturbed in superconductors 'looked similar' to the well-accepted permanent surface currents in magnets, was to Landau and Bloch more than sufficient reason to start a new path of research following the analogy between ferromagnets and superconductors (p. 45). I will now take a short detour to highlight its important consequences.

While most scientists were stuck trying to explain why permanent currents were theoretically possible, Landau gaily ignored the problem, assumed their existence, and believed that they would prove useful in gaining further understanding of the problem. Despite the fact that some parts of his resulting big idea were inaccurate, his intuition, derived from observations, led him to one of the biggest triumphs in the history of Physics: The theory of phase transitions.

It is not necessary to understand the details of this theory to appreciate the philosophical claims I defend, so I describe it here only briefly. It possibly represents the best fully developed example of a phenomenological model in physics.

Landau's interpretation of the behaviour of superconductors (and magnets) was built on a pre-existing simple notion: When a system goes through a transition from one phase to a second phase, one of the two phases is always more ordered than the other. Phase transitions come in two varieties: First order phase transitions are characterized by a migration from the first to the second phase that is immediate; second order phase transitions instead happen progressively. Borrowing Waysand & Matricon's analogy: The ordered set of pins in a bowling game that gets slowly destroyed after each throw is that of a first order phase transition, while the house of cards that, when perturbed, collapses all at once, is like a second order one. The main difference between the two orders is that while for first order transitions, such as melting ice, energy is needed, with latent heat as part of the process, for second order transitions the system in the two phases have essentially the same energy, and no latent heat is involved. To achieve a quantitative description of second order phase transitions Landau's goal became that of finding an "order parameter", a way to measure order that had to fit the given phenomenological input so that the difference in energy between the states would give zero for second-order and one for first-order transitions.

When textbooks talk of phenomenological methods in physics, Landau's theory of phase transitions is inevitably presented as the definitive masterpiece. Guided only by his intuition, he proposed a mathematical expression for the energy and added two parameters that depended on the specific transition at hand. Fixing the energy phenomenologically this way, all the other quantities of interest, from entropy to specific heat, could be calculated. It is true that his first attempt at applying this to superconductivity, using a new order parameter that would depend on permanent currents, failed (p. 45). But starting from a slightly different assumption, his second attempt, that we will encounter later, became the first complete theory of superconductivity, and arguably the most successful. It is still used in laboratories today for most modelling problems.

This theory provided a new perspective on the analogy between ferromagnetism and superconductivity, as was thus summarized in the famous paper published by Fritz London in 1937:

> "Bloch and Landau formulated a program whose realization has generally been considered as the task of a future theory of superconductivity. It seemed necessary to imagine a mechanism that, without any external field, would make it possible for a metal in its most stable state to support a *current*. The thermodynamic stability of the superconducting state and in particular the stability of the persistent currents themselves seem necessarily to lead to this idea. In this connection, one often thinks of the example of ferromagnetism, where the most stable states consist of permanent magnetization without the involvement of any external field" (London 1937, p. 9).

By developing his theory of phase transitions Landau had established his plan of attack on the superconductivity problem. But his conviction that superconductivity was a new kind of phase transition was one of two warring schools of thought. The other one campaigned for superconductivity as a kind of non-equilibrium phenomenon. What was clear to both parties, however, was that a resolution of the clash depended on the results of accurate thermal measurements, since the presence of thermal changes and of latent heat would be evidence for a phase transition. Even if the theories were not spelled out in detail, experiment was now asked to play its role in guiding physicists towards the evaluation and choice of hypotheses. But, as if Nature was trying to make the case more philosophically interesting, it turned out that the choice of experiments in the early Thirties happened to be very unfortunate.

This episode I consider paradigmatic, despite it being relatively unknown by philosophers of science, so I will now explore its lessons about the interplay of theory and experiment in the following subsection.

3.5 Misleading Experiments and Sturdy Theories.

The first thermal measurements, performed on tin, gave ambivalent results: While on the one hand the latent heat appeared in the experiments to be always zero (*contra* phase transition), on the other hand the specific heat showed a discontinuity at the critical temperature Tc. The latter was an important result as it showed that at least some thermal effects were present. In fact, it was the first time that something other than resistivity 'jumped at Tc': specific heat seemed to change in a characteristic way in superconducting materials below Tc (p. 51).

It may seem surprising to us now, but that electrons could be responsible for superconductivity was at that time just a guess. Since specific heat's electronic origin was clearly accepted, this new data therefore helped to encourage understanding superconductivity in terms of electrons.

Concerning the theoretical dilemma between the two schools, though, the experiments on tin were ambivalent and insufficient. Further experiments did not help.

Experiments on the specific heat capacity on lead instead showed no "jumping" effect. With current knowledge, this negative result is not surprising. Lead, compared to tin, has a very low Debye temperature, which means that at low temperature the density of excited phonons is large, which implies that the phonons, being dense, carry more energy than the phonons in tin. For this reason, when measuring the specific heat of lead, the contribution of the electrons to the specific heat is masked, while tin shows a large discontinuity at the critical temperature due to the large contribution of electron to the specific heat. This was not known at the time, so the choice of lead for experiments was simply unfortunate. It led scientists astray for quite a long time (p. 52).

Another more important misleading experiment pointing in the same wrong direction was carried out at the same time, as London recalled:

> "Some important experiments, particularly the famous experiment of Kamerlingh Onnes and Tuyn on the persistence of supercurrents in a sphere, seemed at that time to confirm a misconception. The state of a superconductor did not seem to be uniquely defined by the usual variables [like temperature or the strength of the magnetic field] but rather to be characterized by some kind of memory of how it became superconducting. Following this idea, the present state of a superconductor would depend on the path it followed during its history" (London, 1937).

If the state of the superconductor was indeed dependent on the path that had created the superconducting state, if it had some sort of memory of its history, then the superconducting transition would have to be irreversible. Phase transitions, instead, are reversible, and you can take away heat from water at different rates, but the transition to ice occurs nevertheless.

Onnes and Tuyn's trapped or frozen flux experiments were very hard to make but since they had been carried at the Leiden laboratory, which was notorious for rigour, they were not questioned or independently repeated. But Onnes had broken his empiricist credo, by having previously declared himself a strong believer in the frozen flux hypothesis. His explicit motivation behind the experiments was to confirm that hypothesis (p. 55). The purely exploratory attitude had been compromised in the presence of strong theoretical leads.

The fact that the frozen flux had been suggested by Maxwell's theory (again) was a strong contributing reason behind the unquestioning welcome these experiments received.

A believer in Popper's falsificationism would have reacted with rage to this episode. While a disconfirming experiment would have been repeated and inspected with a fine-toothed comb, one confirming instance was immediately accepted and, with the help of further questionable experiments, conclusively settled the dispute. Onnes and Tuyn's experiment, though, was biased towards the very hypothesis it was testing. And the other experiments were just unfortunate (or inconclusive and ambivalent, as with experiments on materials other than lead). Anyone who had taken the trouble to do precise measurements would have observed that there is always a small but detectable difference between the field inside the sphere and the initial field. One just had to look carefully for it, without settling too readily for the favoured result.

The history of this episode shows us that we can reasonably take the view that a theory that meets apparently fatal experimental evidence still has some chance of being right and winning the fight in the long run. The unfortunate choices, and biased interpretations, of experiments in the frozen flux episode are a case in point.

This view is basic to the position advocated by Phil Anderson, which I will discuss later on but introduce briefly now. In the quarrels with Bernd Mathias over theories of superconductivity, Anderson claims that the single correct methodology consists of starting from first principles, developing your theory and not worrying about contrary experiments when your theoretical lead is strong; experiments can be proven wrong, and your beautiful theory pass the test of time regardless of them.

In this frozen flux episode we find some justification for wanting to defend our theory even in the face of unfavourable evidence. But we should be careful in assessing this move. An important lesson to be taken from this episode is that while one may have reasons to avoid giving up too easily on a promising theoretical lead, this should not discourage in any way our best efforts towards a satisfactory reconciliation between the experiments and our theoretical hypothesis.

In fact, after years of frozen flux credo, motivated by some scepticism concerning the validity of Onnes and Tuyn's experiments on the lead sphere, physicists like De Haas, Voogd, and Jonker, repeated those experiments, and tried different kinds of wires (p. 57). New experiments demonstrated that something less ambiguous was happening when the magnetic field was brought into play. When applying a magnetic field on superconducting samples during the cooling down process, the experiments showed that the order of these operations did not matter, suggesting that superconductors did not remember their 'history', a result in opposition to the idea of frozen flux. Soon some theorists, in particular Gorter and Casimir, two of Ehrenfest's students at Leiden, started to think that perhaps they had given up on the phase transition notion a bit too easily, in favour of the frozen flux. After Casimir returned in 1933 from a year with Pauli in Zurich, with a new scepticism and the intention to reconsider what Bloch and Landau never doubted, the two started work together. Josina Jonker, who was Casimir's wife, had also thought of redoing the experiment on the lead sphere, the one that had been taken as conclusive evidence for the flux, and yet had never been properly explored or repeated. Soon after that, accurate measurements of the magnetic field produced outside and inside tin and lead samples, carried out at Walther Meissner's laboratory in Berlin, unveiled what is now known as the Meissner effect: Not only was the magnetic field not found inside the sample, frozen; it was actually zero, as if it had been magically kicked out. This time the experiment was repeated by several physicists such as Y.N. Riabinin, L.V. Shubnikov, K.Mendelssohn, and J.D. Babbit, in different laboratories (p. 61).

Meissner's result, and its revolutionary consequences, will be discussed in the next subsection. For the sake of the present discussion and its bearing on the more general thesis that I will present in the next chapter, I want to point out that experiments and their interpretation had so far been the crucial turning points for the shifts in theoretical beliefs, even when the experiments seemed originally misleading. The frozen flux experiments were not unimportant, and had not been ignored by those in the opposite camp. On the contrary, these opponents, and the agnostics, focused their efforts on exploring more fully the features of the experiments, and their potential interpretations, well aware that the observed behaviour had to be fully accounted for.

As we will see in the final chapters, Anderson talks of "irrelevant complexities", referring to some of the experimentally observed features of high temperature superconductors that he considers just a distraction from the problem of explaining superconductivity - irrelevant regularities or features that the theory can afford to ignore or pass over in silence. The difference that I want to stress between his view and the example presented by this case is that, as I think I can claim on the basis of the above discussion, the misleading experiments on frozen flux were not simply annoying mistakes along the way to a strong beautiful theory, which could or should have been ignored. They were not irrelevant complexities, but rather presented a serious challenge for theory, a buoy that theories were forced to circle in order to gain strength and consensus.

This is one of the lessons from the rich history of superconductivity. Sometimes our theories need to resist a while longer when under the attacks of contrary evidence; at other times our experiments provide us with completely unforeseen evidence for theoretical hypotheses, evidence that does not germinate from first principles, and we cannot but let that evidence in, and let it change the face of our theories. But in both cases we progress in our understanding by allowing an active dialogue along both directions of the arrows linking theory and experiment. I claim that the potency of this progressive dialogue gets damaged by following strict precepts that establish the dominance of the principled tradition over the empiricist one, and vice versa. In this thesis I focus primarily on the former kind of mistake.

I will return to this central claim in the next chapter.

To summarise: The series of experiments discussed above kept the myth of frozen flux alive; they were serious blows to the phase transition camp; they prevented most experimentalists and theorists from adopting the idea they later agreed was right. Nevertheless, even given their temporarily misleading impulse, they were at the heart of the next big shift in our understanding of superconductivity.

3.6 The Meissner Effect and London's Conceptual Revolution.

In the mid Thirties, as I explained above, the experiments were reinterpreted. What is more, they led to a discovery that changed forever the way superconductivity was understood: The Meissner effect.

In fact, the idea of a superconductor as simply a perfect conductor started to crumble when Meissner' experiments (Meissner & Ochsenfeld, 1933) showed that one could introduce a new and very unexpected feature as characteristic of superconductivity: B=0; the magnetic field inside the superconductor is always zero. The fact that inside a superconductor E=0 was expected and uncontroversial, since there is no other way to have a voltage difference inside the superconductor, which by definition has R=0[10]. If B is always zero at every point in the superconductor, *independently of the path followed* (whether B is applied before or after the sample is cooled through the superconducting transition), then the transition from the normal to the superconducting state is *reversible* (London, 1937). Yet Maxwell's equations predicted frozen flux and not the expulsion of the flux.

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[10] If V=RI, and R=0 then V=0.

It was Fritz London who then had the nerve to suggest that maybe something was missing in Maxwell's equations. He and his brother Heinz decided to proceed in a revolutionary way, adding a fifth equation to the pillars of Maxwell.

The London brothers (1935) started by making E=0 and B=0 an *initial assumption*. They imagined electrons as if they were freely moving under the influence of a uniform external electric field. These electrons, according to Lorentz's law, would encounter a uniform force, and thus they would accelerate uniformly. This simple observation, starting from taking E=0, is contained in the first London equation:

(1) $\frac{\partial j_s}{\partial t} = \frac{n_s e^2}{m} E$ (First London equation, for E)

Applying Faraday's law to (1), one obtains a differential equation for B. *This equation permits both constant and exponentially decaying solutions but London recognized that constant non-zero solutions were non-physical, because they would disagree with the Meissner effect.* The resulting simplification led to the second London equation, which was postulated to complement Maxwell's:

(2) $\nabla \times j_s = -\frac{n_s e^2}{mc} \mathrm{B}$ (Second London equation, for B)

The equation for B states that the curl of the current, j_s, is proportional to the magnetic field, B. The terms e and m are the charge and mass of the electron, but n_s was a new phenomenological constant loosely associated with the number density of superconducting carriers.

The proportionality factor turned out to have the dimensions of a length, and has since been called 'the London penetration depth', designated λ_L. This is easily verifiable by applying Ampere's law

(3) $$\nabla \times B = \frac{4\pi j}{c} \quad (\rightarrow \; j = \frac{c}{4\pi} \nabla \times B) \qquad \text{(Ampere's law)}$$

to (2), which gives a new differential equation for B, as in (4):

(4) $$\nabla^2 B = \frac{1}{\lambda^2} B$$

where London's penetration length is $\lambda \equiv \sqrt{\dfrac{mc^2}{4\pi n_s e^2}}$

This suggested a more sophisticated point. The Meissner effect did not mean that the permeability of superconductors was zero; it is just that the magnetic field cannot penetrate the surface layer beyond the London penetration depth. This startling prediction has been confirmed by many experiments, but the first ones only appeared in 1940.

London's ideas were initially very unpopular. Gorter and Casimir came up with an alternative model that had immediate appeal to the theoretical community. Theirs was the first thermodynamic approach to superconductivity, called the *"two fluid" model* (1934). This was simpler than London's, and interpreted the idea of a phase transition in the most conservative way, by postulating a second fluid that appeared at the transition temperature. Thus all thermodynamic quantities were linear combinations of the contributions from the normal and the superconducting electrons belonging each to a fluid. While it accepted the conclusion B=0, the model said nothing about how the circulation of two fluids was supposed to make it happen. Nevertheless the two fluid model remained for a long time the favoured approach (p. 65).

The attention of the community of theorists was in fact almost completely caught by models that proposed simple mechanisms but had no predictive power. Gorter and Casimir's was the first, and shortly afterwards Mendelssohn added another one, which I will not discuss here in detail but which was equally inadequate in terms of predictions, and even in simply accounting for the Meissner effect. London's equation, by contrast, had no theoretical intuitive justification, yet its *solutions* were easily describable and were appealingly intuitive.

The negative reaction that London received - when he was not merely ignored - was a treatment with which he was familiar. Although now considered unequivocally the true father of superconductivity, he spent almost all of his life as an outsider. He started his studies with both physics and philosophy, and at the age of 21 had published a paper in logic on "the formal conditions of purely theoretical perception" in Husserl's Journal, the *Jahrbuch fuer Philosophie und Phaenomenologishe Forschung.* From his early days his scientific work displayed an unusual conceptual basis that made him radically different from the majority of his colleagues. (Matricon and Waysand also highlight a peculiar virtue of London (p. 66), as disappointing as it may sound for the overall picture of the practices of scientists: Even from a purely formal point of view, no other physicist I know of can pride himself on ending every single paper with a declaration of what he wanted to do next, which he then did)

His first important work was the first quantum mechanical approach to the chemical bond, which marked the birth of Quantum Chemistry in 1925. This already contained the essential traits of his revolutionary work, and the idea of macroscopic coherence. Most physicists seem to think that it is not until after the Second World War that the idea of macroscopic quantum order appeared in the scientific literature. At a time when quantum mechanics was applied only to microscopic phenomena, London's ambition of applying it to molecules to explain macroscopic effects was a true novelty, and one that sounded almost too exotic for the average theorist (p. 71-72). In addition, the impressive number of then unexplored potential applications of quantum mechanics to solve all sorts of microscopic problems, where it had been so far so successful, made it look as if London's idea was an unnecessary and infertile deviation from a golden path. Furthermore, his reputation as a theoretical *chemist* kept his voice at the very back of the choir. He struggled to find a stable position; fleeing from the Nazis, he even had to submit another thesis in order to convince the establishment of his fitness for a professorship in France (p. 72).

At that time, and arguably still now, those theoretical physicists that were particularly keen on the principled tradition would usually consider chemists as lesser scientists, or at least as a kind of scientist that had little to contribute to the real understanding of Nature. This feeling was the most common one directed towards what I call the empiricist tradition, being merely more evident in the relationship between physicists and chemists. I will discuss this further as we meet the central character of Mathias in the next chapter, but I want to summarise here what makes London aligned, in my view, with the empiricist tradition.

In accordance with the central epistemic goals of exploratory experimentation, discussed in the previous chapter, London's approach was that of *finding appropriate representations by means of which general empirical rules or equations can be formulated.* In fact, he started from recognizing the Meissner effect, observed in experiments, as crucial. Then he recognized Maxwell's equation as incomplete if we are to account for this effect. Contrary to the precept of starting from [accepted] first principles, he boldly suggested changing one of the pillars of theoretical physics by adding an equation for the simple purpose of accommodating this new evidence.

Having derived his phenomenological equations, he proposed that these could be consequences of the coherence of a quantum state, borrowing the intuition he had explored in his molecular research. In this way, he introduced a completely new concept in the theoretical landscape, that of a macroscopic quantum coherent state.

As I said, his equations had no theoretical justification; but with the conceptual development of a macroscopic state, they satisfied the desire to fill the gap between a theoretical representation of Nature and the complexities of the superconducting materials exposed by the new evidence. In this sense London expressed quite clearly a preference for conceptualisations and theorisations that, even if they had to go beyond the structure of prior established principles, gave the most accurate and complete description of the phenomenon – in this case, the magnetic behaviour of superconductors. This is characteristic of the empiricist tradition.

There was only one physicist whose independent line of thought on superconductivity was similar to London's, even though the two had never met. That was Lev Landau. Another physicist, John Bardeen, could be counted among London's few sympathetic readers. He had received the London brothers' article from his Harvard teacher, John C. Slater, and discussed it with him with great fascination. Landau and Bardeen went on to become the two pillars of Superconductivity theory.

In the following chapter I will briefly recount the story of their seminal theories. In particular I will discuss the strong influence of (London's) methodology on Bardeen, even though the BCS theory is usually considered, because of its microscopic formalism, in conceptual contrast with the macroscopic phenomenology of London and Landau.

CHAPTER FOUR

"Yes, I said, I believe in evidence.
I believe in observation, measurement,
and reasoning, confirmed by independent observers.
I'll believe anything, no matter how wild and ridiculous,
if there is evidence for it"
(Asimov, 1997)

In this chapter, I will discuss the Ginzburg-Landau and BCS theories of superconductivity. I will focus on GL in analogy with London's theory as a triumph of conceptual development, which was not based on first principles. While in past philosophical debates (French & Ladyman, 1997) (Morrison, 1998) (Cartwright, Shomar, & Suárez, 1995) BCS has usually been set against London's and Landau's theories, I will focus on the historical formulation of BCS and on John Bardeen's views, and show in there a cooperative dialogue between the two traditions. Using several primary and secondary sources on Bardeen's work and thought, I highlight BCS' points of contact, as well as the points of departure, with the phenomenological theories it is usually set in contrast with, and focus on the relationship between theory and experiment in superconductivity according to his views. I claim that he conceived of the desiderata for theories as significantly correlated and most importantly believed that the road to the principled theory of superconductivity was via a phenomenological and descriptive approach. I argue for this claim as significant and non-trivial by contraposition to a different view expressed by Richard Feynman.

4.1 From London to USSR and USA.

London's bold proposal to take the true characteristics of superconductors to be perfect diamagnetism marks the start of the modern era in superconductivity theory. From then on, the histories of superconductivity and magnetism have been intertwined. Moreover, his concept of macroscopic quantum phenomena would not only change the face of this field, but also go on to revolutionise many areas of science. Sadly London was granted proper recognition for it only at the very end of his life.

London had proposed the existence of a macroscopic condensate that accounts for the supercurrent. This idea was startling, given that the charge carriers in a metal are fermions, ruled by a statistics that does not prefer condensate formation (in contrast to Bose Einstein's statistics, with its condensate of bosons). Ginzburg and Landau (GL), starting from Landau's theory of phase transitions, codified this idea in 1950 by introducing an **order parameter** field for the condensate of electrons, ψ, which would describe how deep into the superconducting phase the system is. The phenomenological equation they then formulated described the **free energy** of a thermal system as a function of that order parameter, associated with an effective wave function.

(4.1) $$F = F_n + \alpha|\psi|^2 + \frac{\beta}{2}|\psi|^4 + \frac{1}{2m}\left|(-i\hbar\nabla - 2e\vec{A})\psi\right|^2 + \frac{|\vec{B}|^2}{2\mu_0}$$ (Free Energy for GL)

where F_n is the free energy in the normal phase, α and β are phenomenological parameters, m is the effective mass, **A** is the electromagnetic vector potential and **B** is the magnetic induction.

The striking point of Landau and Ginzburg's choice of the order parameter is that the wave function was to be determined at each point in space by minimizing the free energy of the system.

The conditions for minimising the free energy were then relationships between the order parameter and the magnetic field, known since then as 'the Ginzburg-Landau equations'.

(4.2) $\alpha\psi + \beta|\psi|^2\psi + \frac{1}{2m}(-i\hbar\nabla - 2e\vec{A})^2\psi = 0$ (1st GL equation)

(4.3) $\vec{j} = \frac{2e}{m}\mathrm{Re}\{\psi^*(-i\hbar\nabla - 2e\vec{A})\psi\}$ (2nd GL equation)

Let me comment briefly on the nature of Landau's ingenious order parameter. The original motivation was to find a way to quantify the relative amount of ordered and disordered phase for superconductivity, that is to say, the relative order parameter among superconducting and normal phase, so that one could apply the theory of phase transitions to superconductivity. Superconductivity was meant to provide an example in support of his theory of second-order phase transitions.

Notice that, in the equations above, it is not just the value of the order parameter (at a given point) that matters, but also its gradient. The gradient of a quantity, as a mathematical tool, expresses the spatial variation of a quantity and so (4.2) expresses not only the local values of ψ but also the way the order parameter varies around a given point. Notice also that ψ is, for GL, a complex number. The use of complex numbers is extremely useful when we need to describe quantities that vary periodically, in both space and time, such as tides (p. 132). For the quantity of interest, the real and imaginary parts of the complex number then denote amplitude and phase. These choices of Landau's were completely intuitive, but well founded.

Depending on the applied magnetic field, one can derive the order parameter ψ from (4.2). Inserting it into (4.3) we then also obtain the superconducting current. These equations were not only able to account for known properties of superconductors, but were also, and most importantly, able to predict new ones. They have also found fruitful application in the most diverse fields of physics, including cosmology. One of the early successes of GL was the prediction by Abrikosov in 1957 of type II superconductivity. He also predicted that above a lower critical field (Hc_1) a magnetic flux penetrates the conductor in the form of quantized vortices. This feature was confirmed much later in high temperature superconductors as well[11].

Like London's model, GL predicted the existence of the characteristic length that determines the depth to which an external magnetic field can penetrate the superconductor, [this time] in terms of the new order parameter ψ. In addition, it predicted the existence of another characteristic length, the coherence length ξ, which describes the size of thermodynamic fluctuations in the superconducting phase (ψ_0 is the equilibrium value of the order parameter in the absence of an electromagnetic field)

(4.4) $\lambda = \sqrt{\dfrac{m}{4\mu_0 e^2 \psi_0^2}}$ (Penetration length for GL)

(4.5) $\xi = \sqrt{\dfrac{\hbar^2}{2m|\alpha|}}$ (Coherence length for GL)

[11] These vortices, consequences of GL, have also been recently observed in cold atom condensates.

The ratio $\kappa = \lambda/\xi$ is known as the *Ginzburg–Landau parameter*. We know now that type I superconductors are those with $0 < \kappa < 1/\sqrt{2}$, and type II superconductors (HTS among them) are those with $\kappa > 1/\sqrt{2}$. Conceptually this means that the two types of phase transition (first and second order) would define two different types of superconductors.

While it did not offer a microscopic mechanism, GL used general thermodynamic arguments to examine the macroscopic properties of a superconductor. GL contained and linked together all of the most relevant aspects of the materials: the idea of superconductivity as a phase transition; the seminal idea of a macroscopic order; and the tight relation to the phenomenon of magnetism. As I mentioned above, it contributed several important results, for example the prediction of type II superconductivity and of the characteristic lengths. It has been even generalised into a whole family of particle physics theories. GL was a triumph of conceptual development - and one not based on first principles.

However, in the landscape of the cold war, the remarkable achievements of Landau and Ginzburg were simply unknown to the western physical community (p. 129). There was an effective boycott of all publications coming from the Soviet Union. Apparently, the many volumes of Soviet publications in experimental and theoretical physics that were transported by ship to America would be literally dumped overboard once the ship docked in the Hudson River (Anderson, 1966). Less drastic than the Soviet boycott, London's ostracism was slowly fading, and his ideas starting to disseminate and create a new wave in the field, mostly thanks to John Bardeen. Nevertheless, both the

London and the GL models were still unsatisfactory for the *principled* oriented theorists, who awaited a microscopic mechanism of superconductivity married to quantum field theory. BCS theory would quench that thirst.

4.2 Bardeen and Traditions.

BCS theory has been widely discussed, in both its formal and technical content, in the philosophical and physics literature. Set against London's theory, and Landau's, BCS has been shown to be different in important ways. For example, contrary to the phenomenological approach, BCS provides us with both a Hamiltonian, and a description of the superconducting materials which arguably justifies it. It describes a microscopic mechanism for the emergence of the superconducting quantum state. Nevertheless, while I will present these, and other, features of BCS theory as the product that achieved the highest recognition in the theoretical development of superconductivity, I wish to offer a less common perspective, showing the way the theory was formulated, following Bardeen's methodological ideas. This will allow me to discuss Bardeen's stance on the dialogue between theory and experiment, and hence on the two traditions.

John Bardeen was originally an electrical engineer. He had been, like Landau, a child prodigy in mathematics and developed very early a keen interest in physics. He nevertheless landed in graduate school, seduced by the news that Einstein would be going to Princeton, only after years of working in an oil company. Einstein, unfortunately, started his position at the Institute for Advanced Studies, which is adjacent but not part of

Princeton. Furthermore he did not intend to take any graduate students. Bardeen's supervision, however, did not end up in poor hands. Eugene Wigner mentored him, and led him to the publication of an important calculation from first principles of electron-phonon scattering in a metal, a calculation which turned out to be very useful to him later (p. 146). By the time he had returned to his initial interest in attacking the problem of Superconductivity, twenty years later, he had already been awarded a Nobel Prize for the invention of the transistor, giving him a place in the list of "100 Most Influential Americans of the Century" (Barnes, 1991).

Bardeen made his methodology quite explicit to all his collaborators, as they have recollected later. David Pines, Bardeen's office neighbor at Bell laboratories for thirty-two years, and still a major player in superconductivity research, reports, in a biographical article on his mentor, Bardeen's agenda for solving scientific problems[12]:

- *"Focus first on the experimental results, by careful reading of the literature and personal contact with members of leading experimental groups.*
- *Develop a phenomenological description that ties the key experimental facts together.*
- *Avoid bringing along prior theoretical baggage and do not insist that a phenomenological description map onto a particular theoretical model. Explore alternative physical pictures and mathematical descriptions without becoming wedded to a specific theoretical approach.*

[12] In what follows, I am assuming the accuracy of Pines' report.

- *Use (thermodynamic and) macroscopic arguments before proceeding to microscopic calculations.*
- *Focus on physical understanding, not mathematical elegance. Use the simplest possible mathematical descriptions.*
- *Keep up with new developments and techniques in theory, for one of these could prove useful for the problem at hand.*
- *Don't give up! Stay with the problem until it's solved.*" (Pines, 1992)

Before telling the story of Bardeen's implementation of these maxims, let me offer three remarks on my own reading of them.

Firstly, by 'phenomenological description' I believe Bardeen means something close to what Cartwright calls *representative* models. These are "models that are intended to be reasonably accurate accounts of target phenomena and their sources" (1999). Specifically, he had in mind models like that of London, with *ad hoc* assumptions based on experimentally observed regularities. He wanted to develop them to account for more of the features and regularities that were emerging from further experiments.

Secondly, to "avoid bringing along prior theoretical baggage" should not be read as a literal claim. Taken literally, we would nowadays consider it naïve. It is impossible to start from scratch, without assuming *any* theoretical *datum*. He explains what he means better when he specifies that what is important is not to insist that a phenomenological description maps onto a particular theoretical model. The focus then is not on avoiding theoretical baggage entirely, but on avoiding becoming wedded to any specific theoretical approach. His key guideline is to engage in open exploration of the alternative physical pictures and mathematical descriptions, for the sake of better representing the phenomena.

Lastly, his remark on mathematical elegance should clearly not be read as a statement of preference for solutions that are not elegant. Read in the context of his methodological ideas, where he puts it in contrast with 'physical description', mathematical elegance refers to the activity of building sophisticated mathematical structures for the sake of bringing logical coordination to a description - without necessarily adding content to it. It is in fact a common explicit assumption of physicists that the process of unifying some otherwise independent aspects of a theory, or of independent theories, requires mathematical sophistication. The mathematics must be able, for example, to transform, or map, separate parameters (or concepts or laws) onto special instances or expressions of a single or more general, more fundamental, parameter. Physicists usually refer to the many cases of successful unification in science as examples of this. So with his remark on mathematical elegance, Bardeen is stressing the importance of maintaining focus on "physical understanding", recommending an instrumental use of theories and theoretical tools for the purpose of the description of the phenomena ("Keep up with new developments and techniques in theory, for one of these could prove useful for the problem at hand").

I will return to the above points after the following short history of BCS.

4.3 BCS.

For his first attempt, Bardeen had devised a weak-coupling approach to electron-phonon interaction, guided by his old work on phonons. Although phonons were at that point quite strongly believed to play an essential role in superconductivity, he soon realised his account was flawed.

The original appeal of phonons was their potential to explain the isotope effect[13] in superconductors. Before Maxwell and Serin discovered the isotope effect experimentally, Herbert Fröhlich (1950) had also set out a model of phonons predicting it. But neither Fröhlich nor Bardeen could calculate the relevant quantities that they were interested in (the superconducting wave function, the energy of the superconducting state, and the effective mass of the electrons) even if their phonon model was successful in accounting for the isotope effect. With hindsight, we can see now that both approaches focused unwisely on individual electron energies rather than collective energy - the energy that arises from the interaction of many electrons. Asking the question originally posed by Landau, they were looking to explain superconductivity by finding an interaction that made the total energy of the superconducting state lower than that of the normal state. Wanting to maintain the role of phonons, since it had proved successful in accounting for the isotope effect, their problem then became that the energy from the electron-phonon interaction had to dominate that arising from the ordinary Coulomb repulsion of electrons. As Hoddeson (2001) recalls in an insightful biography of Bardeen, Bardeen confessed his frustration to Rudolf Peierls, complaining that all the methods he had tried could not crack this problem. However, with his keen interest in emerging experimental results, he had singled out the observation that "the wave functions for the

[13] An isotope effect appears when, substituting an element by one of its isotopes (e.g. $^{16}O \leftrightarrow ^{18}O$), one observes a shift in the value of an observable.

electrons are not altered very much by a magnetic field" (Bardeen, 1951). That was exactly what London had assumed, with his 'rigid' macroscopic wave function. Thus London provided a concept that would pave the way to the success of BCS.

Schafroth, in 1951, provided a further piece of the puzzle, showing that as long as the electron-phonon interaction was treated as a small perturbation on the system, it was impossible to explain the Meissner effect. He was the first to realise that the magnetic desiderata were instead not a problem for bosons. Emphasising the importance of noticing how the equally bizarre phenomenon of superfluidity had recently been solved by Bose-Einstein condensation[14], he asked what in a superconductor could possibly behave like a boson (Matricon & Waysand 2003, p. 142)

Bardeen now proceeded in an orderly way to consider the following four points, following his maxims.

First, he set himself the goal of developing a phenomenological description of superconductivity, able to explain the essential emerging experimental facts. This was published in 1956 in a review article. It was an expansion of London's work that discussed, for example, the newly emerging observation of what we now call the 'isotope effect' (Bardeen, 1956).

Second, while Fröhlich had found that the effective interaction between electrons was attractive at low energies, due to phonon exchange, Bardeen was interested in seeing what influence the much stronger repulsive Coulomb interaction between electrons would

[14] "It thus seems reasonable [by analogy with superfluidity] to suppose that superconductivity in metals is due to the appearance of charged bosons in the metal", claimed Schafroth in 1955.

have. He reckoned that techniques developed for strongly coupled electron-phonon systems might be useful, and started to work on this (Landau's theory of polarons) with Pines.

Thirdly, he wanted to account more fully for the effect of the electron-phonon interactions. David Bohm had developed, with Pines, a field-theoretical approach to electron-electron interactions in metals, based on the random-phase approximation. Bardeen and Pines started to work on extending this approach to the electron-phonon interaction; (among other things) they found that for pairs of electrons with an energy difference of one phonon energy, the effective *screened* electron interaction could be attractive. For larger energy differences, the interaction would become repulsive. The mechanism was becoming clearer.

Lastly, experimental considerations pushed him to see whether electron interactions might give rise to an *energy gap*[15] in the excitation spectrum. Measurements on the electronic contribution to specific heat, on surface impedance, and on optical reflectivity, which he was following attentively, led him to be sure that a real energy gap was an essential characteristic of superconductors. Also, again from experiments, he was convinced that the gap had to be several orders of magnitude smaller than the energy carried by each electron. So he decided to examine the behaviour of a few electrons excited above the ground state, using matrix methods.

These four points constituted the exploratory part of his work. Pines points out that "in remarkable accord with John's [Bardeen] vision, each of [his] four thrusts turned

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[15] The band gap is the energy difference (eV) between the top of the valence band and the bottom of the conduction band; it is the amount of energy required to free an outer shell electron from its orbit about the nucleus to a free state.

out to play a role in the development of the microscopic theory of superconductivity by Bardeen, Schrieffer and Leon Cooper in 1957". He also adds that "in using the BCS wavefunction to calculate various properties of the superconducting state, Bardeen, Cooper and Schrieffer were guided at every stage by the phenomenological description John had enunciated two years earlier" (Pines, 1992).

Bardeen had accumulated almost all that he needed to formulate a theory of superconductivity. Cooper and Schrieffer provided the missing bits.

Bardeen, after the departure of Pines as his postdoctoral collaborator, was looking for a bright young theorist who could fully master the field theory techniques, and in 1955 the young Leo Cooper was recommended to him. At the same time, Bardeen encouraged his new PhD student, Robert Schrieffer, to join him on the quest for an account of superconductivity. The starting point was Bardeen's idea, inspired by London and in agreement with Landau's theory, that superconductivity had to come from the long-range order of a quantity.

In 1956, Cooper found out that even arbitrarily weak attraction could lead to pair formation in the presence of a Fermi sea. These pairs would behave as bosons, therefore explaining the emergence of the condensate that had been suggested by analogy with superfluidity and hinted at by experiments. In fact, positive ions are attracted to negative electrons and this, he suggested, polarizes the ions towards the electron. As the first electron escapes, a second electron sees this positive cloud and is attracted to its location, leading to the formation[16] of what have been called *Cooper pairs*.

Cooper pairs were exactly the kind of thing that London would have liked. They extended in all directions, like a sphere: their large spatial dimensions conferred to them a clear macroscopic character; there was no sense in talking of them as conventional particles, or simply as two electrons together, moving in classical trajectories. They were a new quantum state unlike any other known.

Schrieffer managed to describe the wave function for all the pairs, with the help of many-body theory, after he had returned from one of his first workshops, which happened to be on just these so-called many-body problems. They now had all the ingredients needed. They published their results (1957).

The set of ideas used by Bardeen and his colleagues in the BCS paper are not completely theoretically justified. For the Hamiltonian, they make assumptions about what states are significantly interacting, assuming for example that for pairs of electrons with equal and opposite momenta the scattering interactions would be much more significant. While these assumptions are *motivated* by general theoretical considerations and experimental observations, they are not justified but ultimately left to be tested by the success of the theory they constitute – by judging the theory's capacity to account for the many features of superconductivity. The BCS Hamiltonian has been argued to be, in this

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[16] "The pairs form despite the presence of the Coulomb repulsion of the individual electrons as they are at the same place, but at different times. Once the electrons become energetic enough relative to the ion vibrations (phonons), the attraction goes away, and the Coulomb repulsion wins out. This 'retardation' effect is what is responsible for limiting the superconducting transition temperature of electron-phonon systems to values significantly smaller than room temperature. The attraction that forms the pairs is a consequence of the positive ions and the fact that the ions and electrons have different time scales" (Bardeen, Cooper, & Schrieffer, 1957).

sense, both theoretically principled and phenomenological (*ad hoc*) (Cartwright, 1999). This leaves us in a somewhat ambivalent position.

According to Pines, Bardeen "believed in a bottom-up, experimentally based approach to doing physics, as distinguished from a top-down" approach; "to put it another way", he explains, "deciding on an appropriate model Hamiltonian was John's penultimate step in solving a problem, not his first". This, however, is also indecisive and does not mean that we can automatically enrol Bardeen in the same phenomenological camp as Landau and London, for the following reasons.

Landau-Ginzburg's and London's approach shared with Bardeen's a commitment towards the experimental grounding of the initial description, and, more importantly, a solid base in phenomenological descriptions as the starting point. From that common starting point, though, the aims of the two camps separated. While achieving a similar macroscopic description of superconductivity, BCS provided a microscopic mechanism and a Hamiltonian for it. Most importantly, BCS achieved the final Hamiltonian for the superconductor's quantum state through a process of abstraction, neglecting some effects, such as anisotropy, and inserting several simplifying assumptions to avoid disturbances that may undermine the model.

As I argued in the second chapter, the concepts usually employed in philosophical discussions both of the relationship between theory and experiment and of the role of models in superconductivity - discussions in which the theories of London and BCS are set against each other - are unsatisfactory for use in my integrated historical/philosophical analysis. This is one of the reasons why I suggested that adopting an interpretation of the practices of science in terms of the two traditions could bring a better understanding of the issues at stake in the debate on superconductivity theory. In fact, Bardeen's approach to the problem of superconductivity is emblematic of a *cooperative phase of interaction* between the two traditions, as I am now going to explain.

Bardeen was naturally aiming at achieving both of the goals that I previously introduced, namely to give the most accurate and complete description of the phenomena and to explain the phenomena in a way that is consistent with accepted principles and theories[17]. Nevertheless, the result of my description of Bardeen's ideas for the formulation of theories is to show that *he clearly conceived of the two goals as significantly related and most importantly believed that the road to the second is via the first.* While this observation may seem trivial at first sight, let me show why I think it is not.

An objection to the claim that the road to a logically coherent explanation of the phenomena is best pursued via the accurate (phenomenological) description of those phenomena may be that this claim trivially reduces to the discovery/justification distinction. One could say that it is not at all surprising that even the most logically based theoretical account needs to be discovered via all sorts of considerations, including phenomenological ones. However, my point is that, granting the fact that logically coordinated theories originate from a discovery phase that naturally includes (in some loose way) experimental observations, Bardeen's belief in the need for description to precede and guide first principle derivation is not generally shared. For example, I mentioned earlier that, for Bardeen, deciding on an appropriate 'model Hamiltonian' was the penultimate step in solving a problem, not the first (Pines, 1992). This in

[17] A. and B. in chapter 2.

contraposition, as we will now see, with Richard Feynman's line of attack on the superconductivity problem – which was expressed (with exceptionally bad timing) just as Bardeen, Cooper, and Schrieffer were completing the famous BCS paper.

So I want now to show that Bardeen's is not just a trivial methodological paradigm by showing that the opposite paradigm exists in physics, exemplified by Feynman.

4.4 On Feynman - Don't Ask What the Theory Can Do for You. Ask What You Can Do for the Theory.

Just a few months before the publication of the BCS paper, an important theoretical conference took place in Seattle. Richard Feynman, who, while not yet a Nobel Laureate, was already widely considered one of the greatest theorists alive, gave the main lecture on the status of theory in superconductivity, published soon after in Reviews of Modern Physics (1957). He had tried his diagrammatic techniques on the electron-phonon system but had not found the expected change from the normal metal behaviour. Such was his reputation that many physicists have confessed to being worried at the time that if he had tried extensively to solve the problems mathematically and had still failed, there was more likely to be something wrong with the formulation of the problem than with his math[18].

Feynman suggested a radical approach, the opposite of London's. The highest ambition that a theorist could have was to deduce an explanation from first principles,

[18] This reminds us of the episode of Lorentz trying to apply the most advanced theoretical tools to extend the empirically successful Drude model (see 3.3).

which, for superconductivity, Feynman thought meant Schrödinger's equation. Having thus set out his starting point, he suggested an unusual approach. His own words (Feynman, 1957) clearly express the solution he devised for the problem that the theoretical community faced:

> "***It does not make any difference what we explain, as long as we explain some property correctly from first principles.*** *If we start honestly from first principles and make a deduction that such and such a property exists—some property that is different for superconductors than for normal conductors, of course—then undoubtedly we have our hand on the tail of the tiger because we have got the mechanism of at least one of the properties"* (p. 209)

The goal was getting a single success in one bit of the problem, as opposed to the problem as whole. In this, he clearly assumes that succeeding in the first-principle derivation of a single, isolated, aspect of the problem would most likely provide clues for explaining the other properties, grabbing the tiger by the tail. Anything would do; it was not very important which property was explained:

> "***If we have it correct we have the clue to the other properties****, so it isn't very important which property we explain. Therefore, in making this attempt, the first thing to do is to* ***choose the easiest property to handle with the kind of mathematics that is involved in the Schrödinger equation****. I decided it would be easiest to explain the specific heat rather than the electrical properties. [..] But we do not have to explain the entire specific heat curve; we only have to explain any feature of it, like the existence of a transition, or that the specific heat near absolute zero is less than proportional to T. I chose the latter because being near absolute zero is a much simpler situation than being at any finite temperature.*

Thus the property we should study is this: Why does a superconductor have a specific heat less than T? "(p. 210).

In the end, Feynman reported Casimir's conclusion that there was only one way to tackle the problem. It had to be to simply *guess* the quality of the answer. "The only reason that we cannot do this problem of superconductivity", he concluded, "is that we haven't got enough imagination" (p. 212). As he said these words, John Blatt dramatically stood up and announced: "We *have* the idea, and we *have* solved the problem" causing a sensation in the audience (Anderson, 1966). He meant the pairing of electrons, which was indeed the BCS idea, but his Australian group, it turned out, was not yet close to using the idea to achieve a formal solution of the problem. That was comforting for Bardeen, Cooper, and Schrieffer, who were able to publish their final result a few months later.

Feynman's talk came at the end of a long phase of acute frustration in physical theory for researchers in the field of superconductivity. While this was soon to be relieved by the BCS solution, Feynman's attitude was neither accidental nor temporary. It was a clear expression of the long-standing theoretical tradition based on first principles, an approach that had found in the complexities of superconducting materials 'the toughest crowd for their show', since the very early days of cryogenic research with liquid helium. Indeed, this approach was not damaged as a result of the success of Bardeen - which had in any case achieved (at least partially) a principled derivation. We will find that it is common to many fundamental theorists.

After the conference at which Feynman had given his speech, BCS published their paper and quickly achieved success. This does not mean that their results were immediately clear. As Matricon & Waysand remark, "Bardeen had followed all [of his] precepts in his decisive work on superconductivity, the lack of mathematical elegance included" (p. 159). Their paper was a real craft of ambition. Filled with mathematics, and containing radical ideas interconnected in complex ways, combined with experimentally derived intuitions, it was from a technical point of view hard to swallow, and difficult to digest. Anderson observed that it was very lucky for his colleagues that the BCS article was so poorly written, since this opened the way for many publications to flourish in the following years, giving order, simplification, and a more robust derivation, to the BCS ideas, to the satisfaction of the more radically principled scientists.

What is now in fact meant when one mentions BCS theory is often its most important re-statement, due to Bogoliubov and his collaborators (Bogoliubov, Tolmachev, & Shirkov, 1958). The main virtue of Bogoliubov's version is the translation of BCS theory into the more sophisticated language of self-consistent fields (Hartree-Fock theory). This put BCS' disordered results into the desired order, guided by first principles. The method produced two equations that looked like Schrödinger equations, but with an extra term called '*pair potential*'. The bigger the condensation of pairs at some position x, the larger the value for the potential in x. This led to an important and encouraging result that was fed back to the first BCS skeptics: The pair potential could be shown to be the solution to Ginzburg-Landau equations. Theoretical success was now complete, and the physics community rejoiced. One voice, though, stood outside the choir, that of Bernd Mathias. His dissent will be the subject of the next chapter.

4.5 Methodology Meets Scientific Success – Feynman and Bardeen.

I am now going to make few comments on Feynman's talk, relating it to Bardeen's view and to the traditions as I conceive of them.

It is interesting to note that Feynman ends his talk with a remark by Casimir, and this provides an amusing parallel. Remember that Casimir was the critic of Onnes' empiricist agenda for science. Casimir had objected to Onnes that qualitative observation has to precede quantitative measurement; we cannot go on to properly measure anything if theory does not first tell us what to measure. Feynman's remarks can be seen as analogous to Casimir's. He suggests that we cannot know what observations are relevant, and that it is theory that shall guide the choice as we evaluate the fitness of that observation for a principled derivation.

One of the most striking features of Feynman's approach is its pragmatist spirit. Among the criticisms that theorists usually direct at phenomenological theories and at the empiricist tradition is the charge of being too 'utilitarian' and pragmatic. Chemists and experimental physicists in particular, attempting to solve quantitatively bits of a problem for concrete returns in terms of applications, are usually negatively labeled as 'pragmatic'. Putting to one side a discussion of the misguided normative implication, I think that Feynman's approach can be seen as similarly pragmatic, though in the following different sense. The choice of the feature to be explained by theory is not justified in any way, in his view, except by the simple fact that theory 'worked', explaining that feature. Given the apparent stalemate - the baffling behavior that Nature seemed to display whenever physicists looked deeper into the phenomena - theory had to compromise. Not wanting to give up the aim of a 'royal' derivation from first principles, yet also not accepting a full description of the phenomena without first-principle justification, Feynman had resolved to work out some small percentage of the phenomena, a part that seemed most likely to be soluble by known techniques and accepted principles. Hard times call for hard measures, and the best heroic attitude was for Feynman to be heroically pragmatic.

Most importantly, remember that, in contrast to Feynman's "anything goes" attitude, London had maintained that explaining diamagnetism was not just *a* path, but an *essential* path - the true face of superconductivity. In arguing for the importance of starting from a phenomenological description of the phenomena, Bardeen had accepted London's view of the superconductor as a whole. The Meissner effect was crucial. This view was not theoretically justified, and was motivated by qualitative arguments based on both the assessment of the general experimental features, and analogy with intuitively similar experimental situations.

It is in this sense, then, that I claim that while London was 'instrumentalist' in the use of theory, Feynman was 'instrumentalist' in the use of experiment.

To some extent, I see Bardeen's approach as embodying a part of both views, and it is in this sense that I identify within his research the cooperative phase of interaction between the traditions. The line of cooperation between the traditions, though, has a clear orientation. This is a crucial point, and I will now explain it further.

On one side, the epistemic role of the empiricist tradition in its exploratory nature, not guided by theory, which is evident in London's approach, becomes a significant part of Bardeen's approach and constitutes one difference from Feynman's. Bardeen's stress on the importance of starting from an accurate phenomenological description, not from first principles, led him to build the theory using concepts that were able to account for Meissner's effect. Looking for a microscopic mechanism, he was guided by considerations arising from that phenomenological description. Bardeen had to find something that explained the formation of a phase with higher order, which led him to the restriction to a macroscopic entity. Since the coupling between electrons (fermions) was unable to account for Meissner's effect, his discussion shifted to bosons, by analogy with superfluidity.

This, though, was not the full story. Having restricted the space of possibilities, he could start looking for a specific microscopic mechanism; in doing so, he returned to phonons, which he had studied extensively since his graduate years. Maintaining that this choice, like any other, had to be firmly grounded in experimental considerations, he extensively studied the emerging experimental literature, and noticed that the isotope effect could be accounted for via a specific electron phonon coupling. This displays similarities with Feynman's approach. In fact, while it was certainly a striking experimental feature, the selection of the isotope effect as relevant was somewhat arbitrary. The hidden strength of the choice was the fact that phonons seemed to be able to account for it. So in this sense Bardeen had singled out a feature mostly because of its fitness to be derived from first principles, in line with Feynman's approach. Nevertheless, Bardeen did not believe that phonons *had* to be the answer. They would be so only insofar they provided a mechanism that was helpful to the full description of the phenomena. The knowledge that phonons led to the isotope effect actually meant little until he could see the whole macroscopic picture coming together and accounting for the features that constituted the phenomenological model he kept with him at all times.

This is a remarkable aspect of his approach to superconductivity. In the fifty years since he proposed the theory, physicists have been fighting bitter battles defending phonons as the mechanism for superconductivity, yet the very scientist who had devised the solution based on phonons was relatively agnostic about them. In later discussion, we will see that the role of phonons in superconductivity is controversial. Shortly after the BCS paper, as evidence undermining phonons was starting to appear, Bardeen had already stated that probably another mechanism was at play. He never stopped studying enormous amounts of new experimental results, even after he had retired. By the time HTS appeared, Bardeen said that a spin-fluctuation mechanism seemed to stand the best chance, in his opinion. "It was characteristic of John", Pines writes, "that when he was considering possible mechanisms for high-temperature superconductivity, he didn't focus on the exciton mechanism he had developed. *He preferred to let the experiments decide*" (my emphasis) (Pines, 1992).

The difference, then, between scientists is in their degree of confidence that starting from, on the one hand, first principles or, on the other hand, the phenomena and their description, will lead them to the desired solution. This is then not merely a difference of goals, but a difference in conceiving the connection between them. It is not just a matter of preferring a complete description over a principled explanation. It is a matter of the scientist's judgment as to beliefs and confidence over which one is the better starting point from which to achieve the other.

These different methodologies do not exhaust their importance in methodology: They reflect the set of preferences expressed by the two traditions, and underlie different conceptions of what it means to formulate a theory and to succeed in problem solving. The set of criteria for the evaluation of theories employed by scientists underlie the reaching of a consensus on theories, and thus contribute to define what 'scientific success' means. In claiming that these criteria are deeply dependent on the preferences exemplified in the two traditions, I suggest that this dependence is thus transmitted to the issue of consensus, which will be the focus of the HTS discussion.

The difference in the approaches of Feynman and Bardeen is substantive, yet nuanced. The discussion in the next chapter of the views of Anderson and Mathias will show a clearer contraposition and support further the reading outlined above.

CHAPTER FIVE

Today's scientists have substituted mathematics for experiments,
and they wander off through equation after equation,
and eventually build a structure which has no relation to reality.
(Tesla, 1934)

In this chapter, I discuss the first challenges to BCS theory. I present the quarrels between Anderson and Mathias as a paradigmatic example of the clash between the *empiricist* and *principled* traditions. A different view not only of methodology, but also of the nature of scientific theories, emerges from the two physicists. I will discuss the difference in terms of criteria for consensus on theories, and also question the accepted story of BCS, according to which its first serious challenge was the unexpected revolutionary discovery of HTS. Briefly recounting the story of that discovery, I give a controversial example of Anderson's view applied to HTS. In the next chapter this will generate a discussion over dissent in HTS.

In the late fifties, the mutual impenetrability of the Western and the Soviet worlds was starting to end. The wonderful achievements of Ginzburg and Landau had slowly filtered through to Europe, and attracted more and more interest. While BCS theory was undergoing its sophisticated, principled, derivation by means of quantum field theory, though, experiments were increasingly locating different classes of superconducting materials, such as the inhomogeneous ones. BCS theory was the predominant paradigm, especially once it had been shown to lead to GL equations. Some results on inhomogeneous materials were obtained by means of the GL model, which had the flexibility to help with the understanding of disparate phenomena. Yet BCS, despite its considerable explanatory power, had limited powers of prediction. While good at providing a general explanation of superconductivity, and explaining transport properties, it made little or no contact with the new materials of interest to the experimentalists. It could not predict the existence and value of a gap, and had nothing to say on whether any particular material might or might not be superconducting.

The clash of the two traditions in this period of my history is dramatic. A unique character in this period, and an unusual one in the general history of science, was Bernd Matthias. He was at Bell Labs (the hub, until recently, of much superconducting activity) and so was Phil Anderson, who will oppose him in the story that I outline in the next section.

5.1 Posing the Challenge – Bernd Matthias.

Matthias was a true iconoclast, following empiricist precepts. He did not believe that current theories gave a true representation of Nature. Some of his colleagues described his work as alchemy. His techniques were a considerable advance on those commonly used by the chemists, who dominated material research, and they were a true novelty in the experimental landscape that generated the majority of results concerning superconducting materials and their physical properties.

The development, preparation and analysis of new materials were primarily the domain of chemists and metallurgists, who employed traditional methods to synthesise and characterise compounds. Furthermore, their research focused on simple metallic elements, and their simplest physical properties. This was linked directly to the desires of theoretical physicists, who loved to explore all sorts of elaborate theories modelling the simplest cases; the data, then, were coming from samples that were as pure as possible. Matthias instead started by recognising that if we were to understand why, and predict when, this or that substance became superconducting, we had to explore and understand a lot more about many other materials – including compounds. We had to ask a lot more questions. Matthias' dedication to experimental research stemmed from a strong physics background, and from the study of the many different concepts that had emerged in solid-state physics, such as ferroelectricity and ferromagnetism. Armed with those notions, he focused on the complex compounds and alloys of *transition elements* and, instead of just looking at their chemical properties, he explored their crystal structure and the relationship between their electronic configuration and superconductivity, also studying the different processes of chemical bonding. He developed his own technique - defined in his obituary (Clogston, Geballe, & Hulm, 1981) as "substitutional chemistry" - by taking atoms that had different properties from the ones in the sample, and substituting them for atoms in the sample. So, for example, ions of different dielectric polarisability were inserted in compounds as substitutes for elements of similar size, valence and coordination number. In his paper titled "A Search for New Superconducting Compounds", Matthias explained that the expansion of the class of superconductors was meant as a *tool* to "throw new light on the chemical and structural conditions governing the occurrence of superconductivity" (Matthias & Hulm, 1952)

He had in his office a huge Mendeleyev board, similar to those that populate chemistry labs where one can scribble notes in the boxes representing different elements. The peculiarity of his own board was that every box indexing an element was actually a drawer, in which he stored samples of the element, ready to play with. He was renowned among his colleagues for his intimate knowledge of every periodic element. He conceived of this exploration as a precondition for a theory, so he embarked on a systematic study of all the elements. When he was done with the elements he started the Herculean exploration of alloys and two-elements compounds. His achievements were indeed impressive: In the Fifties, he synthesized hundreds of superconductors; by the end of his life he had discovered thousands of superconducting materials.

After years of systematic exploration, he encountered a striking effect. By considering the number of chemical bonds that an atom develops with other atoms to achieve a *stable* chemical state, called *valence*, he discovered a continuous variation in superconducting critical temperature of natural elements, which depended on their valence. By 1955, he had formulated tentative empirical rules for superconductors that accounted for this, and other, regularities emerging from experiments (Matthias, 1955) (Bromberg, 1994).

Then BCS happened. Bardeen's quest had started several years earlier, and had not addressed any of the regularities identified by Matthias' recent work, to his great disappointment. Indeed Matthias and his colleagues were generally accused by theorists of doing "dirt physics". This was meant both as a slur on his work as a chemist (not too uncommonly belittled by physicists as I have also pointed out in the discussion on London, see 3.6), and as a characteristic label for his focus on impure, 'dirty', alloys and compounds. Complex materials with physical imperfections were not the target of models and theories aiming at abstraction. By contrast, the hypotheses about materials assumed by BCS were extremely simple. Indeed, when the BCS paper appeared, these assumptions seemed a bit *too* simple (Matricon & Waysand 2003, p.187) to sceptical theorists and experimentalists alike; for example, the Fermi surface was a plain sphere, and the electron-phonon interaction was isotropic. It seemed improbable that complex materials could fit into those simple representations. Anyway, it was taken for granted that the most sensible way for experimentalists to contribute to theoretical development was through the simplest, purest, experiments.

Matthias made it his goal to disprove BCS theory, and, in particular, the phonon mechanism. Accepting the fact that his valence regularities (and empirical rules) had been snubbed by theoreticians, he tackled the accepted empirical basis for this mechanism. Five years later, he felt he was finally in a position to question the universality of the phonon mechanism as the cause of superconductivity (p.186). He claimed that "when the occurrence and behaviour of superconductors is systematically investigated, they fall naturally into three different groups" (Matthias, Geballe, & Compton, 1963). These were the (i) transition and (ii) non-transition elements, and the (iii) intermetallic compounds. "Conclusions drawn from the behaviour of one group", he warned, "should not be generalised to include the other two without further consideration" (p. 1). The only elements in which the isotope effect, which you will recall was a key piece of evidence for the BCS theory, had been confirmed were in fact non-transition elements (Mercury, Zinc, Cadmium, Tin, Thallium and Lead). No isotope effect had been found for transition elements like Ruthenium and Osmium, and an isotope effect ten times weaker than expected was observed in some intermetallic compounds. With these painstakingly cumulative observations, Matthias showed that BCS theory was, at the very least, inadequate to account for *all* superconducting materials.

If a theory works only insofar as it works - if it is able to account only for some materials and not for others, without any proper reason - then what good is it? It can be at best a model, said Matthias in his most generous concession to BCS, but surely not the end of the story.

This was the main (though not the only) challenge that Matthias posed to the condensed matter physics community. Theorists reacted with annoyance; to almost all of them Matthias' demands seemed almost childish, in the face of the great success of BCS, which had changed the status of superconductivity "from one of the most obscure to one of the simplest and best understood of all the phenomena relating to the properties of matter" (Anderson in Anderson & Matthias 1964, p. 373). David Shoenberg, at a 1959 superconductivity conference at Cambridge, was led to make his classic remark "Let us see to what extent experiment can explain the theoretical facts" (Slichter, 2007). It is exactly in response to this overstated confidence that Mathias replied with his efforts in showing that victory had been sung prematurely.

In 1964, the journal *Science* presented a 'double feature' article called "Superconductivity", which consisted of two parts ("I. A Theoretical Approach" and "II.

The Facts") in which the different roles of theory and experiment were expressed through the voices of Anderson and Matthias.

5.2 Don't Ask What the Theory Can Do for You. Ask What You Can Do for the Theory – Revisited.

Philip Warren Anderson is one of the most famous condensed matter physicists alive. A Harvard graduate from Illinois, he worked at Bell labs for almost forty years where he developed, for example, the concept of localisation and the so-called Anderson Hamiltonian (describing electrons in a transition metal). He was awarded the Nobel Prize in physics together with Sir Nevill Francis Mott and John van Vleck, for their study of the electronic structure of magnetic and disordered systems, leading to the development of electronic switching and memory devices in computers. He is one of the most influential (and controversial) figures in the HTS landscape and a recurring reference in my interviewees' comments.

This discussion between Anderson and Matthias shows with particular clarity the issues - the preferences - which I claim not only predate these scientists, since I have located them acting in the history of superconductivity in its early stages, but stand behind the two broad traditions introduced in chapter two.

Anderson, right at the beginning of his half of the article, states his view on scientific method in a wonderfully simple exposition. He finds it necessary to discuss "the elementary facts about the nature of scientific theories and of scientific proof" since he "suspect[s] that many physicists do not think carefully about how the process [of scientific method] really goes in a mature science such as the physics of matter" (Anderson & Matthias 1964, p. 373). The challenges posed by Matthias's experimental results, he writes, were not troublesome, because the main way in which experiments provide a test for theories is indirectly, via their support for higher-level accepted principles and theories; what matters in science is the test provided by the whole set of experiments. This set manifests itself through the principles we have, on its basis, come to accept:

> "The experiments against which a theory must be tested are not merely those under direct consideration but the ones carried out over the past 50 to 100 years which have given us all-but-absolute, unshakable confidence in a certain structure of fundamental laws: quantum mechanics, general relativity, statistical mechanics, the consequences of symmetry, the regular nature of crystals, the band theory and so on. I could give you tens of examples of **the following general rule: a theory which contradicts some of these accepted principles and agrees with experiment is usually wrong; one which is consistent with them but disagrees with experiment is often not wrong**, for we often find that experimental results change, and then the results fit the theory.
> [...]
> **Thus the first - many would say the main - task of a solid-state theorist confronted with a phenomenon** (like superconductivity, ferromagnetism, or -an example I shall continue to use - semiconductivity) **is to find a way in which the special behaviour of matter under consideration can be accounted for in a way that is merely *consistent* with all the things we already know about solids and may already know about the phenomenon.** Actually, this is usually, for practical purposes, the end of the story: **there is usually only one sensible way to account for the facts about the phenomenon consistently with known principles of physics"** (emphasis in the original, bold added) (p. 373).

Close quantitative agreement with experiment represents for Anderson only the third and last stage of the advancement of theory, the second being qualitative agreement, and the first being consistency with first principles, advertised above.

He acknowledges that BCS' (coherence factor) predictions seemed violated by an effect called the 'Knight shift'. But he believes it will result, in the end, in an "apparent more than real" contradiction between theory and experiment. The reason for this, he says, is to be found in a "failure of communication" between theorists, who were discussing an idealized pure system, and experimentalists, who had (unwisely) made experiments on some of the most impure specimens of metals ever prepared. When examining "pure, bulk specimens, experimental results have not yet contradicted theory" (p. 376).

For Anderson, the reasons why physicists were initially much less ready to accept BCS than they had been to accept new theories in other, similar cases, were probably psychological, and clashed with the "overwhelmingly convincing" experimental support for it (p. 376). The main component of this overwhelming support was that Gorkov had formulated a version of it that would lead to the GL equations, a result which, he admits, "was not at that time widely appreciated in the West, mainly because we did not appreciate a 1957 paper of Abrikosov [future Nobel Laureate in superconductivity] which derived from these equations the entire theoretical apparatus necessary for understanding hard[19] superconductivity". The consequence of this failure of communication, he continues, was that these phenomena "were first clearly brought out by the

[19] Hard superconductors are type II superconductors which are *ideal* diamagnets.

experimentalists"; in reality, though, the "truth was hidden from the theorists" and it was just accidental that theorists couldn't claim credit for the discovery. "The theory was all right, it was just that *we* were stupid" (p. 376).

For what concerned the third and final stage for BCS (Precise quantitative agreement with data), Anderson defends BCS by reminding us that this stage had not been reached yet in several other comparable cases (e.g. Theory of melting points; ferromagnetic or anti-ferromagnetic Curie points); nevertheless the quantitative understanding of superconductivity was on its way, and looking promising.

I point out that, in the examples he provides, Anderson does not distinguish prediction and retrodiction as epistemically different in a relevant way. Defending the adequacy of BCS to predict the isotope effect, for example, he remarks that "on this issue theory and experiment have fought to a standoff: theory predicted intermediate values first, but experiment found near-zero values before the theory was accurately enough developed to provide a basis for understanding them" (p. 377). In Anderson's view, the ability of the BCS programme to adapt, through apparently ad hoc modifications, to fit values from experiments different from the ones originally calculated, is equally a triumph; its initial apparent failure in predicting correct values merely disguised its triumph - this being due to an insufficiently sophisticated development of the theory. This epistemic indifference between prediction and retrodiction is one of the characteristics shared by most of the theoretical discussions of HTS up to the present day.

In his conclusions, Anderson offers a quick, opinionated, answers to Matthias' open questions, such as whether other mechanisms operate. He accepts the possibility that they may occur in the future (for different superconductors), but that they very probably have not yet been found. For Anderson this question is anyway not relevant. Referring back to the statements of Feynman discussed in section 4.4 (which Anderson describes as seminal for his ideas), his point is that the theoretical tools available (and the ones that we had reason to expect to be available in the near future) do not provide the computational power to derive answers to questions such as the prediction of the transition temperature. As Feynman had suggested, theory must select the problems it had the ability to solve. The calculation of Tc required a presently unattainable degree of sophistication, and so could be discarded as unimportant for the evaluation of theories[20].

So, all things considered, BCS was for Anderson another example of his general rule, unsurprisingly. Consistency with principles is the main desiderata for theories, inevitably leading to the right solution.

By contrast, Matthias maintains that the most important things to be explained are, first, which substances are capable of exhibiting superconductivity, and second, their values of Tc. As I have said, Matthias believed that BCS was at best a model, and found it "not surprising that the acceptance of the generality of the phenomenon [which is a problematic for BCS], which had been considered rather limited until recently, is the result of the empirical approach of finding a great number of superconductors" (p. 379). This conclusion about the generality of superconductivity resulted from the discovery of almost a thousand superconductors, which were an impressive set if compared with the thirty or forty known thirteen years before. His experiments were gradually revealing that

[20] The majority of theorists I have interviewed share this point with Anderson, considering (for similar or different reasons) the calculation of Tc as irrelevant.

almost *all* metals could become superconducting if they were sufficiently cooled. For BCS, instead, superconductivity was the result of a more or less delicate balance between electron-phonon interaction and the Coulomb repulsion. For this reason, he pointed out, superconductivity, far from being, as now appeares, the normal ground state of most metals, had been considered an accident. Therefore, he concluded, "the present 'theories' are unable to state the rules for the occurrence of superconductivity, which, in my opinion, are essential in any explanation of the phenomenon" (p. 379).

As I anticipated earlier, Matthias claimed that "the variations in transition temperature, together with this entirely different isotope effect [e.g. for transition elements], would suggest to an unprejudiced observer that there is a drastic difference in the mechanisms causing superconductivity in the two groups of metals". Since the existing theories neither provided any criteria for the occurrence of superconductivity, nor permitted a calculation or an estimate of Tc, he said it was impossible for him to understand "recent attempts to enforce another consolidation of theory and experiment *at any price*" (emphasis added) (p. 380).

Answering the questions that Matthias had suggested as essential for understanding superconductivity was a task that clearly had to start from the empirical observation of regularities and patterns. Looking for patterns in the emergence of superconductivity in different materials under different laboratory settings was the obvious first step for Matthias, a task that required great care and the most robust and unbiased experimental practice. In this exploration, in the pure empiricist tradition, while it was acceptable to have theoretical hypotheses in mind, one had to make sure they did not bias the observations and their interpretation; in particular, when the phonon mechanism was inadequate to account for the observations, he found it unsound to continue to insist on it as the true mechanism.

Let me finally remark that Matthias suggested that the difference in the transition temperatures of some elements, such as Yttrium and Lanthanium, while not accountable in terms of electron-phonon interaction, could be explained on the basis of a magnetic interaction (p. 381). I will show in the next chapter how the arguments for and against the phonon and magnetic mechanisms are a signature that is still found, forty-six years later, in an impressively large number of publications discussing new features of HTS.

5.3 Traditions in Discussion

In the years after BCS, research into superconductivity progressively transformed into a branch of material research, dominated by chemists and experimentalists, a transformation widely criticised by many physicists (Matricon & Waysand 2003, p. 181). Nevertheless, it was no more than fair that the absence of predictive power in the theory would strongly encourage chemists to respond to the emerging technological interest in superconducting materials, to achieve working applications, and justify funding for general superconductivity research. Mathias opened the path that led to the tens of different families of materials known nowadays, but for my purposes here, it is important to stress that the scientific motivations of material research were not purely technological. The progressive study of the disparate materials was challenging theory, showing, for example, that to assume the phonon mechanism to explain the superconductor's behaviour seemed often artificial and sometimes highly problematic. What is more, some of the empirical observations were insightful enough to highlight regularities that the theory was missing in principle, like those of Matthias already mentioned, and the generality of the occurrence of superconductivity in metals. The latter, I suggest, contributed to a shift in the general conception of the phenomena from exceptional to normal, hence to a reformulation of the problem at stake. This can be seen in the fact that nowadays, according to most theorists, the anomalous thing that theory has to account for is no longer the superconducting state but the normal state; the normal state suddenly seems to be the unexpected behaviour, in the face of the generality of the occurrence of superconductivity which had been strongly argued for by Matthias.

Despite the ongoing debate between experimentalists such as Matthias and theorists such as Anderson, the [standard] story of HTS emerging from nowhere as a revolutionary discovery that shook the solid foundations of BCS theory (challenging the current understanding of superconductivity) is widely accepted. However, the historical events, and, in particular, the challenges to theory that I presented above, with the example of Matthias, show a more complex and interesting picture. The standard story of BCS is probably due to the fact that by the Seventies the theory had gathered a widespread consensus, and that theorists appeared to be able to accommodate discordant bits and pieces like those pushed by Matthias by inserting ad hoc assumptions and modifications to theory. The consensus on such modifications was not at all complete and in many cases the debate was highly controversial, but the apparent capacity of BCS to at least potentially accommodate such data conferred strength on it and created a shield for the phonon mechanism.

I stress, though, that it was not just that BCS could not predict all of the properties, sub-phenomena, and anomalies, that Matthias presented as regularly occurring; it was also that BCS theory seemed increasingly inappropriate in accounting for all superconducting materials, and wrongly predicted some of their features. Matthias's experiments on the isotope effect in transition elements are a case in point. Most reviews and papers on superconductivity start by telling the same story of the revolutionary discovery of HTS, and usually mention the breakdown of the expected isotope effect observed for HTS as the most serious challenge to the phonon picture, which opened the road to all sorts of alternative accounts of high temperature superconductivity. This breakdown, though, was exactly the one already suggested by Matthias. Additionally, several other anomalies were known long before the discovery of HTS, such as the Knight shift, the values for electronic heat conductivity and a whole series of anomalous superconductors; Matthias' systematic hunt for materials had shown that they were troubling for BCS. Matthias must have regarded Anderson's faith that BCS' quantitative agreement with data was at hand as delusional, yet his evidence was not sufficient to overthrow BCS, given that it had a number of correct predictions (and retrodictions) confirmed; the award of the Nobel Prize to Bardeen, Cooper, and Schrieffer, many years later (an award that Bardeen gave in large part to fund the Fritz London Memorial Lectures at Duke University) shows that the broad consensus on BCS was intact. While I accept that the evidence was not decisive against BCS, the challenge presented to it by experiments was valid and troubling, and clearly anticipated the same problems encountered with the discovery of HTS, such as, once again, the lack of an isotope effect.

The example that I gave in chapter 3 of the tension between the frozen flux data and the idea of superconductivity as a phase transition offers an interesting parallel. There, I explained that physicists could be justified in wanting to defend their theory even in the face of unfavourable evidence, such as the frozen flux data. At the same time though, this should not discourage serious attempts to find a satisfactory reconciliation between the controversial experiments and our theoretical hypothesis – indeed, quite the opposite. It was because the frozen flux data was important for the opponents of the frozen flux hypothesis, that they put their best efforts into studying, repeating and evaluating it. I have argued that those experiments were not simply annoying mistakes to be ignored. They presented a serious challenge for theory. The road to consensus was through a reconciliation of data and hypotheses.

In the present story, by contrast, we see that Anderson did not accord that privilege to Matthias' experimental challenges. The principled tradition exemplified by Anderson came with a strong belief in the 'proper' way to achieve the solution; this belief, I argue, translates into a view on how to reach consensus on theories in the community. In my reading of Anderson's view, consensus on a solution is reached when the theory explains the phenomena in ways that are consistent with previous fundamental principles. The explanation of the phenomenon does not have to be complete or completely satisfactory. A better agreement with data can be reached *afterwards*, as we add developments and small corrections (which do not affect the fundamental principles of the theory), once the consensus on the "right" principled approach is reached. In this way, stating conclusively what the theory accounts for is deferred.

For Matthias, however, consensus on a solution should be reached when our description of the phenomena is consistent with all the established experimental facts, and, more importantly, when such description has the largest predictive power. A theory that is able successfully to predict novelty but is not highly consistent with well-known principles is to be preferred to a theory that is consistent but lacks predictive power. As with Anderson's account, this is naturally not the end of the story or the final stage. The job is then to expand the explanatory power of the theory, having established what it has to explain.

I want to point out that these two views do not necessarily express what philosophers of science conventionally call 'epistemic virtues', but rather different preferences among them. The importance of one virtue is, at most, denied "locally" in relation to the other. As I said, in Anderson's view a theory which suffers to some extent under the weight of experimental evidence is still saved by its consistency with first principles, which suggests not only how important internal logical coherence is for Anderson but also the extent to which empirical adequacy and predictive power are seen by him as secondary. More precisely, it shows beliefs about some epistemic virtues as *consequences* of others. These differences in beliefs and sets of preferences correspond in my account to the sets of preferences that I characterize in the two traditions. Let me explain this further.

Anderson, for example, may still maintain that empirical adequacy is a clear desideratum for theories, but he would suggest that this desideratum should only be the focus of the last phase of development of a theory. This is not a trivial matter of prioritising, but is justified on the basis that finding the (usually single) way to account for the phenomena consistent with first principles represents "usually the end of the story", after which quantitative agreement will inevitably follow. This point appears also in his central dogmas, which I discuss below (5.5).

For Matthias, by contrast, a model that lacks internal consistency and a first-principle derivation has a chance of leading to the true mechanism as long as it seems to be empirically adequate, and it is then from there that a more principled account can be built, if we need or want one (though for purely practical purposes we may not). I do not intend to embark on a normative discussion of the two views, but I want to point out that in an ideal situation both camps aim at a theory that ultimately equally satisfies the epistemic virtues both of empirical adequacy, and of logical consistency within itself and with other accepted principles. It is also a possibility that in ideal cases the two approaches would reach their final stages and find that at that stage they are equivalent or compatible. A final theory may even claim robustness on the basis of its predictive power, when this was a virtue that was not *initially* considered, by its principled advocates, to be of primary importance. Alternative accounts which performed better in the light of that epistemic virtue may have actually been discarded as unpromising, sometimes even as unreasonable. In general, then, a characterization simply in terms of different, static, epistemic virtues, would fail to account for the complexities of the Anderson-Matthias disagreement.

As I have said, the difference seems to lie in the methodological considerations concerning the best path to success for theories. I have also previously mentioned, though, that this is not only an issue of interest for methodology. In fact, adding the *diachronic* dimension and stressing the historical evolution of the theories and the historical evolution of their acceptance in relation with experiments, I claim we learn something that goes beyond a history of methodology. Not only do we gain new insights into both the nature of scientific theories and theoretical and experimental practices, through the eyes of physicists; we also gain a new perspective on the issue of dissent in science. I agree with Lakatos that when we ask a question about Nature, how we find an answer is part of the answer. As I will discuss further in the final chapter, by shifting the discussion to the different criteria for consensus, we can instead make sense of 'dissent' in HTS as the clash of evolving preferences and of different traditions. From the point of view of a rational *a posteriori* reconstruction of the contentious issues between theories, this issue of contention on methodology, and criteria for consensus, may be invisible. By looking at the dialogue between the two different traditions in problem solving in physics, the issue becomes visible, open to interpretation, and able to contribute to our models of scientific progress and to the discussion of dissent in science. In other words, to summarise, behind different criteria for consensus lie different preferences over the methodologies employed to achieve the desired solution, and, ultimately, behind that, different views on what the desired solution is supposed to look like. I postpone further discussion for the next chapter, and conclude my historical story by briefly recounting the discovery of HTS, a story which will be continued shortly.

5.4 Can the Theory Do What We Ask of It, Anyway? – the Limit for Tc and the Discovery of HTS.

In the pre-HTS phase of superconductivity, the final episode that I am going to mention is Anderson and Morel's calculation of Tc from BCS, an attempt to defend BCS from the stubborn attacks of experimentalists such as Matthias by bringing it to its third stage of quantitative agreement.

One thing clearly postulated in BCS was that there was an attractive interaction between the normally repulsive electrons, mediated by the phonons. Qualitative consideration of the electron-phonon interaction could lead to quantitative prediction of at least the *maximum* values allowed in the balance between electron's repulsion and the strength of the coupling with phonons. A formula was thus derived by Anderson and Morel (1962) that expressed the value of the critical temperature Tc in terms of the electron-phonon coupling, and obtained the upper limit to Tc by qualitative estimation of the possible strength of the coupling allowed by the systems. This calculation, while important, was only a meagre consolation for theorists, who remained ultimately unable to contribute much to the quest for new materials and the understanding of new phenomena. Still, the upper limit for Tc, estimated at 30K, seemed safe enough; after all, the highest Tc ever observed had been only 23K, and that was considered quite exceptional[21].

By 1969, one year after the death of Landau[22], Ginzburg was at Stanford to give a talk to an important conference on superconductivity. He was revealing his latest research

[21] The main progression in superconducting materials had been the discovery of superconductivity in Mercury (Tc = 4.2 K) in 1911, followed by Lead (Tc = 6 K) in 1912, Nb (Tc = 9.25 K) in 1930, NbN (Tc = 16.1 K) in 1941, Nb3Sn (Tc = 18.1 K) in 1954, and Nb3Ge (Tc = 23.2 K) in 1971.

[22] who had withdrawn his invaluable input to scientific research in 1962, after two months in a coma caused by a car accident from which he never recovered and which ultimately caused his death

on organic superconductors, research motivated by London's original idea of superconducting macromolecules. It was generally considered by his colleagues as nothing more than a divertissement. In his talk, Ginzburg predicted that an organic material might have a critical temperature around 40K. At this unexpected remark the whole audience broke into laughter (Matricon & Waysand 2003, p. 194), which gives us an indication of how shocking the subsequent discovery of high temperature superconductivity must have seemed to most theorists.

Twenty years later, and few years after the death of Matthias, the original estimated upper limit was broken. Alex Müller, a Swiss physicist who had gained his freedom to pursue research on oxides after retiring, and George Bednorz, a chemist colleague he had recruited, achieved superconductivity in a barium-lanthanum-copper oxide at a temperature of 35K.

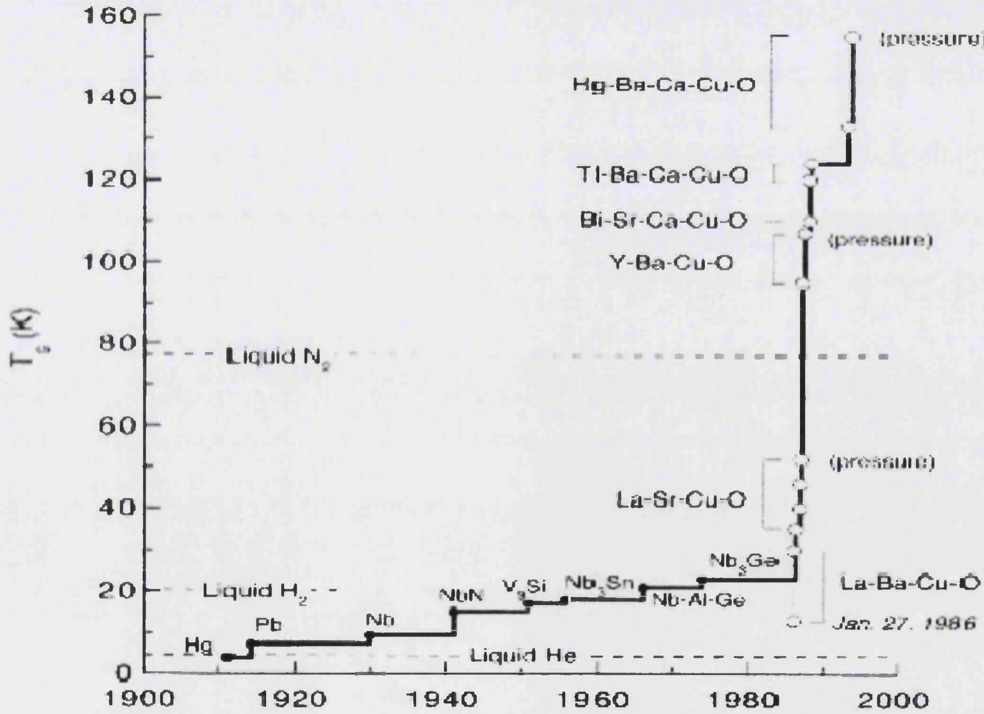

Figure 1: progressive discovery of ever-higher Tc and the HTS jump.

This was a very welcome success for Müller, who had aimed explicitly at breaking through the predicted barrier for Tc. Thomas P. Sheahen (1994), author of one of the few current textbooks on HTS, recalls asking Fuller: "Since you knew of the theoretical upper limit of 30 K, why did you keep on looking [for a higher-temperature superconductor]?" Fuller replied " I asked the theorists to explain to me how they got that limit, and when they were all done, I did not understand it, so I went back to work in the lab". Sheahen comments that this may be the perfect prescription for winning a Nobel prize. "By their discovery", he remarks, "Bednorz and Müller proved again that experiment always prevails over theory" (p. 119).

The dominance of theory over experiments expressed by Anderson in the previous section was nevertheless alive and well among theorists. Years after the discovery of HTS, Anderson (1994) fairly admitted that "Bernd [Matthias]'s claim to immortality will be that he dragged solid-state physics, much against its will, into taking seriously the many, many substances which did not fit into our neat, logical – but probably incorrect – categories" (p. 132). However, despite this concession, his philosophical views on scientific theories and the criteria for their acceptability did not suffer radical changes. An example is a series of Cargese lectures starting in 1989, entitled "The Central Dogmas" later published in (1994). They also formed the basis for his own theory of high temperature superconductivity, published as a book in 1995, under the immodest title "THE Theory of Superconductivity in the High-Tc Cuprates".

5.5 The Central Dogmas

In his Cargese lectures Anderson (1994) launches his attack against what he considers to be the wrong path to discovery in science, one that he labels "stamp collecting". Inspired by Francis Crick, he states his "central dogmas" as the reasonable way to go about solving the problem of superconductivity. Suggesting a parallel with molecular biology, where Crick had propounded a "central dogma" to constrain the structure of the mechanisms of reproduction and transcription of biological information, Anderson states that the field of high-Tc superconductivity is in an overcrowded and confused state similar to that of Biology before Crick and Watson's discovery. "There is enough irrelevant complexity that an unwitting theorist may never reach the neighborhood of the actual problem, even though he is working along a line which is widely represented in the literature" (p. 638). His dogmas are motivated by his belief that in "the maze of alternative paths, almost all can be eliminated by simple logic using simple and well-founded experimental or theoretical reasoning, of a sort which should be immediately persuasive" (p. 638). As I will discuss in the next chapter, the context for this was the impressive variety of different theories had proliferated since the discovery of HTS; after what reporters called the "Woodstock of Physics", the huge conference where the rumored result of Bednorz and Müller was presented, the field had exploded in a frenzy of experimental and theoretical activity.

Anderson's charges become pointed as he complains that the lack of success for theories of HTS is due to the fact that "few theorists nowadays are familiar with the process of rigorous deduction from theoretical concepts, combined with a broad range of experimental facts". Since many theories are able to "tie in to" a few valid experimental facts or theoretical concepts, he explains, they contain germs of truth, but not much more, since they don't take into account "the key requirements of overall consistency with the complete picture". Others are instead just plain "inconsistent with the basic realities of the subject" (p. 639).

> "The theory must touch its base in the quantum theory of these entities. *There remains very little flexibility*, and one would feel there was no hope of finding such a theory except for one's knowledge that physics actually does work and there has to be one – and only one – solution. This is then the methodological clue – that one must retain one's faith that the solution exists. Thus *when one has found a way through the maze of conflicting requirements, that is certain to be the way, no matter how many deep-seated prejudices it may violate and no matter how unlikely it may seem to those trained in the conventional wisdom.*" (underscore in the original, emphasis mine) (p. 639)

Many of the papers written in this field, says Anderson, are not just potentially easily falsifiable, but actually falsified from the outset, if we see them through the lens of this precondition of consistency. For this reason, his dogmas are needed to help get rid of the speculation that has ruled the field[23], and get back to a de-fragmented way of doing science based on overall consistency.

Of his six dogmas, three aim at logically eliminating other (unsound) possibilities, "restricting the Hilbert space", as he puts it. The first two are there to avoid a theory paying attention to anything but a single band of the one-electron spectrum; this good band is the antibonding symmetry band on the CuO2 planes, labeled "dx_2-dy_2" or d-

[23] "speculation in the mode of particle physics, where there is no solid a priori foundation" (Anderson 1994, p. 640)

wave; all the other bands can be eliminated [ignored] because they are separated by large energy gaps. The third dogma states that in this band there is only one relevant electron interaction able to open up yet another gap, and hence further 'restrict the Hilbert space'. The dominant interactions are *repulsive* and their energy scales are all *large.* His assumption here is that what matters are terms that open up gaps in the spectrum, assuming that all the other terms can be replaced by effective interactions among the other degrees of freedom, and therefore can be eliminated [ignored].

Once we have restricted possibilities this way, we can explore and give descriptions of the state of the "normal" non-superconducting metal. So the fourth dogma describes the normal state as that of a "Luttinger liquid", an unconventional state which possesses a Fermi surface but no conventional electron quasiparticles. The fifth dogma specifies that this state is strictly two-dimensional. The last dogma, finally, tells us something about the superconducting state. The question asked is: Which of the residual interactions that have survived the selection can be strong enough to give us the unconventionally high temperature of HTS? The answer seems to be: Interlayer hopping. That is, the mechanism for HTS is based on something that Anderson defines as 'interlayer tunneling deconfinement', and this accounts in turn for Tc. In Anderson's view there seem to be no other plausible competing theories.

I am not going to discuss the technical content of these six dogmas. I am taking them as an example of what Anderson considers the right way to approach a problem, and to show the assumptions about scientific theories and methodology contained in it. Taken at face value, the structure they sustain does not go very far: Anderson's theory of interlayer tunneling has been long abandoned, and Anderson confessed to me during our interview that he still bitterly regrets that it took him ten years too many to become convinced it was wrong. Yet, this outcome notwithstanding, some of his dogmas are still accepted. Some scientists publish theoretical papers following the precepts of these dogmas (apart from the abandoned last one), and a few others publish papers giving evidence intended to challenge one or the other (Steele, 2001) (Tahir-Kheli & Goddard, 2009).

While I grant that suggesting and adopting criteria to filter through the flood of theories in HTS (and also to guide the ordered formulation of theories) is a useful tool for theorists involved in complex problems, Anderson's proposal paradoxically leaves, on the one hand, a lot of space for subjective preferences, for example over experimental features to be accounted for, while on the other hand restricting the possibility of growth for theories, by denying that disagreement with experience is always problematic. As I have pointed out in my historical analysis of superconductivity, considering experimental disagreement as problematic for theory has naturally driven scientists towards further understanding, revision, and innovation, in theory and practices. Employing dogmas of Anderson's kind also incurs the risk of shielding the theory from much needed revisions and conceptual innovation, especially if the concepts and principles involved are taken as dogmatic starting points[24].

These issues will be discussed in the following chapter, where I finally consider the nature of the 'dissent' in the field, and the controversial role of evidence in the evaluation of theories.

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[24] As Wittgenstein (1969) observed, "Where there is no doubt there is no knowledge either".

CHAPTER SIX

"Give your evidence, said the King; *and don't be nervous,*
or I'll have you executed on the spot. [..]
There's more evidence to come yet, please your Majesty,
said the White Rabbit, jumping up in a great hurry;
this paper has just been picked up."
(Carroll, 1865)

In this chapter, I illustrate the ways in which HTS has made a difference in the field of superconductivity, I introduce the dissent on HTS theories and discuss the role of experiments in the redefinition of the problem. Considering the capacity of scientists, in the words of Collins and Pinch, to "subvert the force of experimental results", and drawing from the discussion of the previous chapters, I argue that we find in the interplay of the two traditions a crucial factor generating controversy, and that this factor gives us insights into the controversies taking place in HTS research.

6.1 Why Is Hotter Different?

When Bednorz and Müller published their paper called "High Temperature Superconductivity Possible in the System BaLaCuO" (1986), the community at first did not react. After all, it was not the first time that someone had claimed to have found a notable superconductor with unusually high Tc[25]. The excitement, though, grew exponentially as other researchers confirmed the result of Bednorz and Müller[26].

As a result of the explosion of interest in new high T_c superconductors, the APS decided to sponsor a special session on recent developments in the field, open to all researchers, as a last-minute addition to the 1987 APS March Meeting in New York. This special session, meant originally merely to accommodate the increasing interest on HTS, turned out to be a historic event. Thousands of scientists waited for hours for the opening of the doors to a room that could only contain twelve hundred people; closed-circuit TV monitors were fitted at the last minute into the hallways to accommodate more scientists, some of whom even got injured trying to enter the main room.

The show started with Müller discussing the breakthrough he had published with Bednorz in the previous year. Then others, such as Tanaka and Chu, added new materials, and reported the first striking features of HTS. The presentation of new superconducting oxides such as $La_{1.85}Ba_{0.15}CuO_4$, $La_{1.85}Sr_{0.15}CuO_4$, and $YBa_2Cu_3O_7$, with their transition temperatures of 30, 40, and finally 90K, electrified the audience. The discussion continued into the early hours, as Brian Maple, the chair of the Woodstock of Physics, recalled in my interview. Michael Schlüter of Bell Labs, referred to the legendary Woodstock Music and Art Fair (1969) during a press briefing, so the event became known as the 'Woodstock of Physics'.

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25 e.g. CuCl. Usually further inspection would show either that results of the sort were not reproducible or that the material could not be identified with a stable bulk superconducting phase. (Doran & Woolley, 1979).

26 Koichi Kitazawa obtained the chemical formula for the superconducting material for others to check, confirming the superconducting phase in $Ba_2La_2CuO_4$.

Figure 2: courtesy of Brian Maple.

It is a bit easier to understand the excitement that transformed an extra session of a routine conference into a rock concert if we consider that it had taken more than sixty years to obtain superconductors with the previous highest values of Tc, and that the record value of 23K (achieved by Matthias) had already seemed extraordinary compared to the 4K of the first superconductor discovered by Onnes. The transition temperature of the new superconductors was not simply unusually high; it broke the limit imposed by theory. This limit had meant that superconductivity required sub-liquid nitrogen temperatures, which was unfortunate given that liquid nitrogen was very cheap and widely available. If the upper limit for Tc was no longer definitive, then hope for previously unimaginably high Tc (perhaps as high as room temperature!) was no longer utopian; the gates for futuristic applications suddenly opened.

Many applications have been suggested since 1987, showing the potential for HTS to be revolutionary. HTS materials promise resistance-free low cost electric transmission, and electronic devices with greatly lowered heat dissipation. Most of the possible applications so far remain unachieved, but not all. The most recent applications range from life-saving contributions to biomagnetism (HTS' non-invasive MRI - Schlenga, De Souza, Wong-foy, Clarke, & Pines, 1999) to almost ideal power generators; they contribute to fields as disparate as warfare, with the deployment of "E-bombs"[27], and climate change, with technologies for the reduction of greenhouse gas emission (Gibson, 2001). Further possible applications constantly appear. A demonstration project using high temperature superconductor wires in the US national energy grid has started in 2009 (Sanderson, 2006).Another example is the research undertaken in recent years on the development of high power-density superconducting motors for aircraft propulsion and on fuel-cell-based power systems for aircraft. The requirements in terms of weight and volume for these components cannot be achieved with conventional technologies, so HTS

[27] A reprehensible military device that makes use of strong, superconductor-derived, magnetic fields to create a short, high-intensity, electro-magnetic pulse to disable an enemy's electronic equipment - first used in March 2003 when US Forces attacked an Iraqi broadcast facility (*CBS News, 25/03/2003).*

could be the enabling technology for all-electric aircraft development, valuable for environmental protection (Masson, Brown, Soban, & Luongo, 2007).

The new materials generated enthusiasm not only for their possible applications. Brian Maple recalls that one of the exciting features of the new high-Tc superconductors oxides was how easy they were to prepare. Polycrystalline material could easily be obtained by simply mixing powders of metal oxides (e.g., Y2O3, La2O3, CuO) and carbonates (e.g., BaCO3) and then firing them (in air!) at around 800 °C. This had the striking consequence that suddenly most laboratories could make a high Tc superconductor, without needing expensive facilities.

The combination of the failure of the theoretical calculation of the limit for Tc, the promise of technological applications, and the accessibility of HTS production for small laboratories and poor countries, resulted in an unprecedented explosion of research in the field. It attracted unexpected amounts of funding, and generated a flood of publications, not all valuable. Some writers, praying for success, claimed high Tc for materials without even specifying their exact composition (Cava, 1997). Koichi Kitazawa referred to these "sightings" as 'USO's' (Unidentified Superconducting Objects)[28].

To accommodate this exponential interest, a large number of new journals were founded, with much higher subscription rates to guarantee speedier, and hence more marketable, publication. As Matricon and Waysand recall, poor countries were able, for the first time in the history of physics, to play the game on almost equal terms with the rich. To sustain the cost of the experimental equipment and labour, "some of these groups adopted the unusual strategy of volunteering to publish the articles flooding publishers

[28] Named after UFO's (Unidentified Flying Objects). Note that a clever secondary meaning for 'USO' comes from Japanese, where USO means "lies".

and referees. This was the case, for example, with the National Physics Laboratory of New Delhi. Beyond carrying out research on new materials, it helped spread the innumerable publications in which the authors vaunted their merits" (p.213). With the best will in the world, no scientist is now able to read more than few percent of the field's literature, contributing to the formation of epistemic islands. I will discuss this further when I introduce what I call the 'toothbrush' phase of theories in the next chapter.

6.2 Why is Hotter a Problem for Theory?

What did all this mean for theory? In contrast to the resistance encountered by Matthias when he attacked BCS, the impression that the conventional BCS mechanism could well be inadequate for HTS spread quickly in the community. The high critical temperature was not the only evidence that the mechanism for superconductivity in these materials was non-conventional. Experimentalists were already reporting in 1987 that the oxides did not show the expected isotope effect. Their magnetic properties were also different from what was observed in conventional LTS[29]. Furthermore, specific heat showed a linear temperature dependence in the new materials, while in a conventional superconductor specific heat had showed dependence that was exponential.

Competition, then, spread not only amongst the labs but also amongst theorists. In 1987 Anderson developed one of the first models for HTS, called the resonating valence bond model, or **RVB** (1987). This time his model had nothing to do with the electron-

[29] We know now that this is a general difference between type-I and type-II superconductors. Note also that not all type-II superconductors are HTS; some LTS are also type-II.

phonon mechanism that he had defended in BCS, but was entirely magnetic in origin. Consider, for example, the new La_2CuO_4, an antiferromagnetic material in which all the copper atoms are divalent and carry magnetic moments. Suppose we dope it with trivalent strontium, an alkaline earth, and observe the emergence of the superconducting phase. According to RVB, the substitution creates a charge imbalance, and converts divalent copper ions into trivalent copper ions. Then, extra electrons can move between adjacent copper atoms and, because the material loses its antiferromagnetism, these electrons have their spins oriented antiparallel to each other. This suggests the generation of pairs of electrons with spin up and spin down, which could play the role of Cooper pairs, and give rise to superconductivity.

HTS was considered from the start as a new manifestation of the superconducting phenomenon, one that deserved a separate theoretical account. The door to alternative accounts having been opened, the parallel between ferromagnetism and superconductivity, which had been for years the focus of, amongst some others, Matthias' research group, was once again in vogue, and provided new insight into the mechanism at play in HTS. In 1958 Matthias had studied isomorphous compounds, and claimed that the close relationship between superconductivity and ferromagnetism was apparent in them. Indeed, in the famous dialogue paper with Anderson, discussed in the previous chapter, he remarks that many of the materials he had encountered in his experimental practice suggested to him not only that that there was this relationship, but also that magnetic interactions could be responsible for superconductivity, and that even the reverse might be true (Superconductivity could be somehow responsible for ferromagnetism). It is important to remark that the empirical rules for superconductors that he had formulated did not actually turn out to be generally valid, but his stress on the overlooked parallel between ferromagnetism and superconductivity seemed to be vindicated in the early years of HTS, at a time when the accepted novelty, and evident anomalous behaviour, of the materials allowed room for the proposal of unconventional mechanisms.

After Anderson's conjecture, many questions began to arise. How does a doped RVB state behave? And why does doping stabilise an RVB state when the un-doped state prefers an ordered magnetic state? As I said in the previous chapter, Anderson has subsequently abandoned the theory he developed from the original RVB model, but a few years ago he proposed another version of it, based on the same RVB approach – a version in which he and many others appear to have considerable confidence, despite it still not being fully developed, and even though it is still not able to answer the above questions.

After Anderson's, many other models started to emerge. The next section is a short summary of the contributions of theorists, meant to give a glimpse of the over-crowded landscape in the theoretical understanding of HTS.

6.3 Theories of HTS.

One approach to the problem is to distinguish between LTS and HTS through angular momentum. In low-temperature superconductors the electrons pair together so that their total orbital angular momentum is zero - an 's-wave state'. In high-temperature superconductors, instead, the pairs are found to be in a d-wave state, a superposition of states in which the angular momentum is non-zero. Years ago this **d-wave symmetry** has been somewhat clarified by experiments (Rice, 1999). Discussion remains intense concerning how one can turn the Coulomb repulsion between electrons into a form of attraction that binds them in Cooper pairs – which are still widely accepted as the carriers of charge.

The phonon-based pairing mechanism in BCS requires the Coulomb repulsion to be described in terms of so-called Landau-Fermi liquid theory. In this theory the properties of single electrons are changed or 'renormalized' by interactions with other electrons to form 'quasiparticles'. The properties of the material can then be understood in terms of weak residual interactions between quasiparticles. Maurice Rice, another important theorist from Bell Labs, now based in Zurich, explains that theorists worked for many years with the Landau-Fermi theory - which has been very successful in most metals – because it allowed them to account for the formation of Cooper pairs with non-zero angular momentum using Coulomb repulsion alone. The emergence of HTS challenges this notion, as it seems that the family of high Tc superconductors known as *cuprates* never forms a Landau-Fermi liquid. Chandra Varma and co-workers developed the **marginal Fermi liquid theory** to explain many of the anomalous behaviours in cuprates[5 A]

Varma's radical theory (1989) suggests that high temperature superconductivity (and related phenomena) occurs in certain materials because quantum-mechanical fluctuations, which usually decrease with temperature, in these materials increase. In 1996 Varma formulated a theory by which he was able to support this radical hypothesis by assuming that superconductivity is associated with the formation of a new state of matter in which electric current loops form spontaneously, going from copper to oxygen atoms, and then back to copper. His theory concluded that the quantum-mechanical fluctuations are the fluctuations of these current loops (Kotliar & Varma, 1996). Physicists consider these fluctuations in the current loops to be fluctuations of time.

For many others, the main interest is instead in what causes the breakdown of the otherwise highly successful Landau-Fermi liquid theory, and in the nature of the resulting electron liquid. Rice suggests that a breakdown of this sort can occur near a symmetry-breaking transition, such as the transition from paramagnetism to antiferromagnetism, and that this transition has been shown to lead to superconductivity in the heavy fermion metals at high pressure. Others are sceptical. They argue that the Landau-Fermi model breaks down above the superconducting transition temperature, and over a wide range of doping levels. They also point out that the onset of superconductivity is often the only symmetry-breaking transition that actually occurs (Superconductivity breaks an abstract mathematical symmetry called 'gauge symmetry') (Rice, 1999).

Further experiments have shown that underdoped cuprates possess a ***pseudogap*** (Yamada & Yanase, 2002). A pseudogap is a new energy gap in the non-superconducting state where anomalous physical properties appear below a temperature T that is much higher than Tc, which defines the usual energy gap needed to break the electron pairs. The origin of the pseudogap has become a challenging and controversial issue of the last fifteen years, which many think is crucial for the identification of the HTS mechanism. One interpretation of it suggests that Cooper pairs form at a temperature that is much higher than Tc but that because of large **phase fluctuations of the pairing field** (Emery & Kivelson, 1995), superconductivity only appears at Tc. Others have postulated the formation of **electronic stripes**, **antiferromagnetic ordering**, and other exotic order parameters to account for the anomalous behaviour, parameters that are in competition with superconductivity (McElroy, 2006). Some, for example, conjecture that the 'large pseudogap' in cuprates is the consequence of a particular kind of charge density wave with d-symmetry and therefore supports the **Charge Density Wave (CDW) theory** (Li, Wu, & Lee, 2006).

Other competing theories include those based on **fluctuating stripes**; in certain cuprates at low temperature the doped holes are observed to localise along parallel lines, called stripes, in the copper-oxide planes. A number of models for correlated electron systems predicted the presence of phase separation at small levels of doping, with the system bifurcating into hole-rich (metallic) and hole-poor (magnetic insulating) regions. In some models, these regions form a lamellar pattern, i.e. one-dimensional 'stripes'. An attractive feature of the stripes model is its one-dimensional physics. While it is still unclear whether Fermi liquids are inherently unstable in two dimensions, they are definitely unstable in one dimension. These one-dimensional models naturally contain non-Fermi liquid normal states exhibiting spin–charge separation; the pseudogap is nothing more than the spin gap associated with the magnetic domains. Pairs of holes from the stripes can obtain pairing correlations by virtually hopping into the magnetic domains[30]. Below some temperature, the stripes phase has been shown to lead to long range (three-dimensional) superconducting order. That is, the system crosses over from a one-dimensional non-Fermi liquid normal state to a three-dimensional coherent superconducting state (Norman & Pepin, 2003).

Other theorists propose to unite the superconducting and antiferromagnetic phases in a larger symmetry group - so-called **SO(5) theories**. The relationship between

[30] The fluctuating Néel order in the magnetic domains favours antiparallel spins on neighbouring Cu sites. See also note 31.

ferromagnetism and superconductivity led to the idea that the three components of the spin ordering, and the two components of the superconducting order parameter, can be unified into an effective Hamiltonian with an approximate symmetry of the five-dimensional rotation group, SO(5). In the SO(5) picture, the '5' stands for the three degrees of freedom of the Néel order[31] (Nx,Ny,Nz), and the two degrees of freedom of the superconducting order (real and imaginary parts). In an imaginary world where these two order parameters are degenerate, the underlying Hamiltonian would have an SO(5) symmetry (Norman & Pepin, 2003).

Another set of theories are based on the polaron mechanism, and seek to exploit the strong coupling between electrons and phonons in oxide materials. The term 'polaron' was coined by the late Lev Landau to describe the lattice distortion (polarisation) caused by the charge on the electron, which follows the electron as it moves through the solid. One side effect of this is that the electron's effective mass is increased. Bipolarons are bound pairs of polarons, mutually attracted by the lattice distortion (Rodgers, 1998). The **Bipolarons theory** initially gained credibility because it was the working hypothesis adopted by Bednorz and Müller when looking for their first HTS (and it was also a working hypothesis for Bardeen, as I mentioned in 4.3)

An important model that is used by a great number of theoreticians as a basis for more detailed theories is the **Hubbard model.** This is an approximate model used to describe the transition between conducting and insulating systems. The Hubbard

[31] Antiferromagnetism at low temperatures vanishes above a certain temperature, called the Néel temperature (named after Louis Eugène Félix Néel, who had first identified this type of magnetic ordering). Above the Néel temperature, the material becomes paramagnetic.

Hamiltonian is extremely simple, containing only two terms[32]. While it is a very useful tool, it only describes ideal situations, excluding, for example, the case where long-range interactions between the particles cannot be ignored. Some physicists have challenged the use of the Hubbard model for HTS. A few years ago, for example, Nobel Laureate Bob Laughlin (2006) proposed a new notion for high Tc superconducting cuprates, that of **Gossamer Superconductivity.** In a gossamer superconductor, the superfluid density is tenuous, in contrast to the conventional superconductor. He proposed an explicit many-body wavefunction for that state, which is a partially (Gutzwiller) projected BCS state. The partial projection operator enables one to construct its inverse operator. Using these operators, Laughlin has proposed a Hamiltonian, for which the partially-projected BCS state is an exact ground state - an appealing result, since exact solutions play an important role in many physical problems. By expanding that Hamiltonian, Laughlin showed that the superconducting ground state requires a large attractive interaction in addition to a large on-site Coulomb repulsion. This raises doubts as to whether the Hubbard model used in many theories captures the basic physics in cuprates (Gan, Zhang, & Su, 2005).

Colin Humphreys, head of Materials Science at Cambridge University, proposed years ago that existing theories fail because they do not take into account the distribution of the holes. He argued that each copper-oxide plane consists of square "nanodomains", separated by channels that are one unit-cell wide - rather like a grid of streets surrounding blocks of houses. Holes at the edges of adjacent blocks are magnetically paired, he says, and superconductivity occurs because these hole-pairs march collectively along the channels, like trams on pairs of tramlines running between the blocks of houses. There is

[32] A kinetic term allowing for tunneling ('hopping') of particles between sites of the lattice, and a potential term consisting of an on-site interaction.

one hole on each tramline, according to the model, and the pairs of holes move down the channels, hopping from oxygen to oxygen via adjacent copper sites. There is more than one group working on these **theories of Hole Superconductivity** (Physics World, 1997).

The long list of theories continues and contains, obviously, the weak-coupling mean-field BCS theory, based on electron-phonon interaction, which is still strongly defended by some groups. In the Nineties it was considered by the majority as clearly unsuitable for accounting for the high superconducting transition, especially transitions associated with strong correlations. Nevertheless in recent years phonons have experienced a resurgence of support in many publications as a cause for HTS. I will present, in the next chapter, examples of some recent publications that show the debate between two competitors, the phonon and the magnetic modes.

6.4 Dissent is Hotter.

This short review of theories of HTS shows the intense level of debate and controversy that persists twenty-three years after the discovery of HTS. It also provides a perspective on the frustration that Anderson expressed in his 'central dogmas' paper, where he complained about the many theoretical paths which he saw as based on little more than speculation (This being the unsound method that he felt one needed to exclude *a priori*, following his dogmas). Many, though, take a different view, as I will explain shortly.

While the understanding of many aspects of materials, and the theoretical implications of various proposed theories, have advanced considerably, the problem remains unsolved. Every three years, material scientists, chemists, theoreticians and physicists of every background who work on theory and applications of LTS, HTS, exotic superconductors, strongly correlated systems, heavy fermions, superconducting semiconductors and so on, convene at the "International Conference on Materials and Mechanisms of Superconductivity and High Temperature Superconductors". At the last one[33], held in Dresden three years ago, the final talk summarising the 'state of the art' of theory after twenty years of HTS fell to Douglas Scalapino. He remarked, not without irony, that the number of serious theories seemed in the last year to have reached saturation; at least it was not growing.

The rather chaotic set of theories being openly debated is one of the peculiarities of the HTS case, characterising it as an interesting case of exacerbated dissent. Many factors contribute to the general feeling of frustration in the field, and all the physicists that I have interviewed (excluding, of course, the few that joined the HTS field later) recall that at the very beginning there was a generalised confidence that a solution could be reached in five to ten years. I propose here a tentative list of reasons for this expectation, which I have extracted from my interviews:

- The great success of BCS theory, and the concomitant idea of mimicking this approach to find a solution for the new unusual materials; even in case one needed a new mechanism, the theoretical *structure* of the theory had been laid out.

[33] Many of the interviews that I accumulated during my PhD research took place during this international conference.

- The entry of several important scholars, and several Nobel Laureates, into the newborn field, following the conviction that solving the problem would certainly lead to a Nobel Prize.
- The great success and prominence of research laboratories such as Bell Labs, which gave its scientists unusual freedom to pursue research in superconductivity, following their interests[34]
- General enthusiasm due to the acceleration of research results in the Eighties. In those years the remarkable increase in computing power, and the increase in variety and precision of data, had led to a very optimistic attitude about the ability to produce the desired scientific results, given concerted effort.

By fuelling such expectations, and stressing revolutionary future applications, physicists in HTS initially attracted much attention and funding. As the passing years brought no substantive result, funding was drastically cut, forcing scientists in the Nineties to struggle to pursue research (a struggle that naturally hit more strongly the 'minor' labs, and the less established individuals).

The experiments on HTS materials have continued to build an unprecedented library of data. Several hundred thousand publications have appeared, reporting new features and effects, observed in disparate materials - advances mostly obtained without guidance from theories. I will present in the next section some philosophically interesting ways in which these experiments have shaped the theoretical definition of the problem.

[34] Anderson recalls in the APS interview that Bell Labs had agreed to buy the expensive machinery for low temperature research with liquid helium when Matthias, who had been offered a professorship at UCSD, had required the equipment as a condition for his remaining at Bell Labs for six months of each year.

6.5 Setting the Standards with Experiments.

The impressive library of data on these materials has helped deepen the understanding of HTS by showing that the problem cannot be understood just as a more or less complex manifestation of a phase transition between a normal and a superconducting state. An enormous number of experiments started to show the presence of several intermediate phases; more sophisticated experiments showed that some of these phases compete against each other, showing correlations of different sorts. In fact, every presentation in the field now usually starts by showing what is called the *phase diagram*, that is a type of chart that shows conditions at which (thermodynamically-distinct) phases occur. Ideally, this chart is meant to be general and valid for all HTS, but in practice there are different phase diagrams for different families of superconductors. In some cases, materials that seem to have different phase diagrams can claim the right to a new label in the HTS taxonomy.

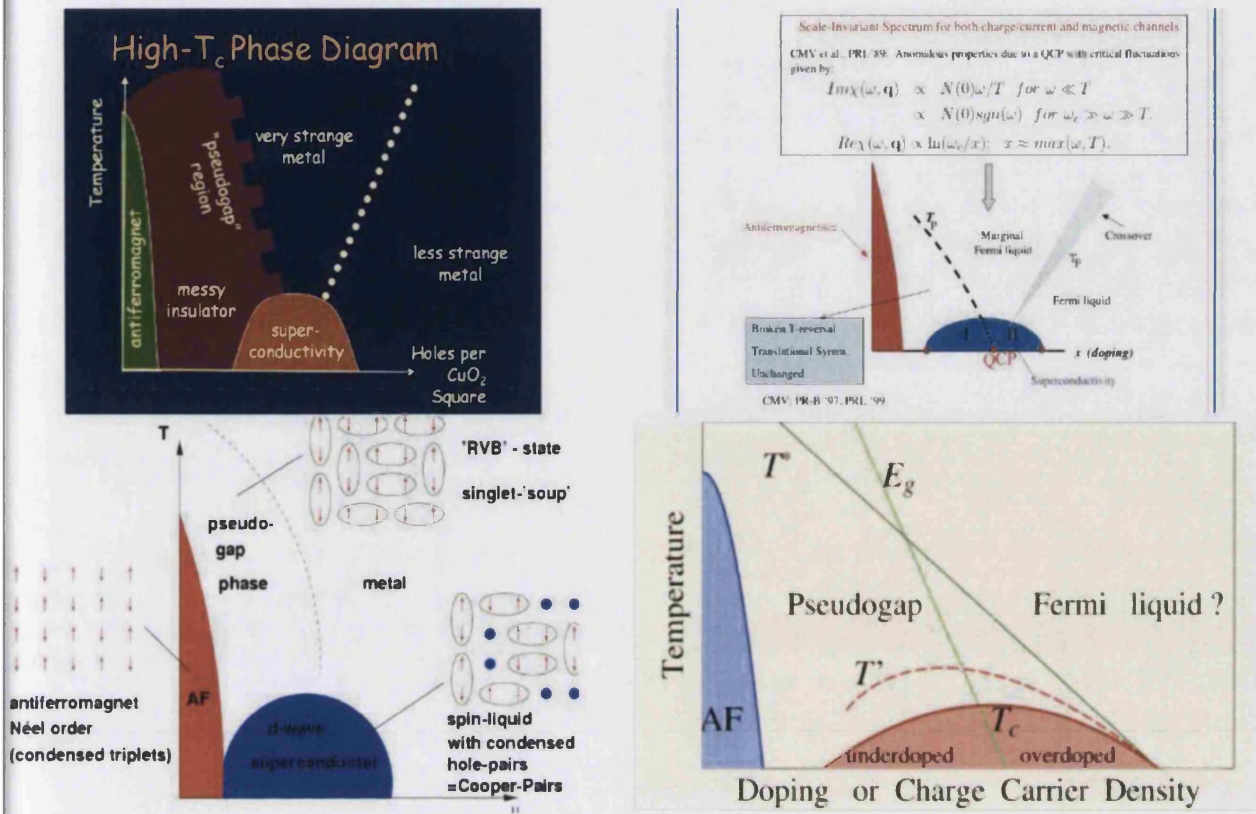

Figure 3: examples of phase diagrams for HTS (Temperature Vs Doping). provided by (clockwise, from top left): The Shoenberg Laboratory for Quantum Matter, Cambridge University; C.Varma UCSB talk, 2005; Curty & Beck (2003); Werner Hanke, Martin Joestingmeier, Theoretical Physics I, University of Wuerzburg.

The vast majority of efforts, both in experiments and in theory, are directed at a special family of HTS, the *cuprates*, and their phase diagram. These materials have been selected as the materials of choice for theoretical study since they all seem to be variations on a single theme - lightly doped copper-oxide planes. This is a relevant commonality, considering that more than fifty superconducting cuprates are now known. Many more materials show different behaviors, different phase diagrams, and bizarre properties. Most are not as 'ordered' and simple as cuprates, though there are notable exceptions. For example, a remarkably simple superconductor is Magnesium Diboride. It consists of hexagonal planes of Boron atoms separated by planes of Magnesium atoms, with the Magnesium centred above and below the Boron hexagons. It defies all the accounts given for cuprates, yet remains for now a marginal problem - to the disappointment of some.

The understanding of cuprates has advanced slowly and, as I said, the phase diagram has become a central piece of the puzzle. "The challenge", explains Rice, "is not simply to find a reasonable formula that predicts the uniquely high values for the superconducting transition temperature in the cuprates [as Matthias stressed]. Rather, superconductivity is but one aspect of the unique and complex phase diagram exhibited by this class of materials. Depending on the temperature and the level of doping, the cuprates can be insulators, metals or superconductors. The non-superconducting or 'normal' phase also exhibits unusual properties" (Rice, 1999).

The experiments, introducing complexity to the phase diagram, have thus advanced the conceptual understanding of HTS, in a similar fashion to the conceptual understanding achieved by experiments in the episodes of superconductivity that I have discussed in the third and fourth chapter. This complexity, in turn, defines anew the problem of HTS. This is clearly shown by the fact that many investigators have felt the need to clarify repeatedly *what is it* that the theory must account for. Rice gives a nice example - addressing issues encountered in the Anderson-Matthias quarrels such as the power of theory not only to explain and describe, but also to predict and help the development of new materials. He says: "We can ask what the final theory should predict. First, it should describe the full complex phase diagram. Second, it should reveal the special conditions in the cuprates that lead to this very special behaviour. From this should follow some suggestions for other materials that would show similar behaviour. While it may not be possible to predict T_c accurately - because, for instance, of a lack of precise input parameters - the final theory should give the correct order of magnitude and explain the trends that are observed in the cuprates"

At this point, the tension between the two traditions can be seen in the new light of HTS. I will discuss this now in more detail.

6.6 Traditions and HTS

As I have discussed so far, the aims and desiderata expressed in the two traditions are common requirements for any overall successful physical understanding of the world. Yet, in practice, there is often a tension between representing the world with a reasonably complete and accurate description, and the principled formulation of highly consistent theories. This is naturally the case in the HTS field, which, explains Varma in my interview, is stuck between the unprecedented complexity of the phenomena observed experimentally in the new HTS, and the sea of theoretical accounts which attempt to derive the phenomena from first principles, but can only tackle a small portion of the evidence. For twenty-five years, first-principled theories have advanced, but their inability to address the phenomena at large has led to considerable debate.

Given the complexity of the experimental landscape of HTS, compromising the descriptive and predictive power of theories for the sake of logical coordination can be a justifiable step in the progression to a more satisfactory and general theory, but I stress that the *selection* of crucial experimental features of the materials, and the assessment of the *relevance* of these features, is largely open to debate and discussion. Aiming at a theoretical representation of a given phenomenon, scientists normally have to identify and characterise the target phenomenon; in this task, as I have discussed previously, exploratory experiments play an important epistemic role, linking, in many ways, observations to conceptualisations (e.g. Recognition of patterns, formulation of empirical laws and regularities, and creation of novel concepts to fit and explain new taxonomies), and shaping the language of future theories of the phenomenon. This epistemic role, though, is not to be located only in the early stages of the problem. I claim we find it even in more advanced stages of scientific problems.

In the case of HTS, the problem is borne out of a pre-existent theoretical framework, developed after decades of cumulative work by brilliant scientists, which had reached the stage of consensus and international recognition in the community. The problem of superconductivity was considered solved and, as I suggested earlier, the emergence of HTS was welcomed initially with the belief that, even though the specific theory of BCS had been considered insufficient to *fully* understand the new materials, the pre-existent successful framework would have at least greatly aided theoretical efforts in that direction. Several theories emerged, but the experiments largely retained a 'life of their own', fuelled by the growing interest in applications and HTS technology. Experimentalists take a pragmatic approach most of the times. Bob Cava, an eminent experimentalist, said in our interview that one should, "I say, believe all religions, one may be right! So if I hear an interesting idea from a theoretician about how something works, I try to put that into my chemistry searches, somehow", otherwise, one keeps working independently and without much help from theory. Many of these experiments can be seen as resembling the exploratory experiments that Matthias was keen towards, those which aimed at identifying regularities and empirical rules among different materials and properties. But they are different from the exploratory experiments that Steinle discusses, which occur in contexts in which theories are genuinely lacking and cannot fulfil any guiding role. Rather, HTS experiments are pursued every day with the clear knowledge that the results are going to meet a whole zoo of pre-conceived theoretical interpretations. Furthermore, pre-existing concepts such as Cooper pairs, Bose-Einstein condensation, d-wave and s-wave states, and so on, are taken as background knowledge for those experiments. A metaphor for experimenting in HTS can remain 'exploration'; however, it is no longer that of the adventurer exploring a mysterious jungle, but rather that of a puzzle enthusiast tackling a riddle he has extensively studied before. The search for regularities and patterns, typical of the exploratory experiments in standard cases, is maintained even in the presence of pre-existent theoretical backgrounds (backgrounds that experimentalists use as a toolbox rather than as a manual, as expressed by Cava). This exploratory attitude *re*defines the problem, and *re*assesses our understanding of the phenomena.

The last two decades have brought elucidation of what is to be modelled and explained, but this process has been variously controversial for different theorists (As we saw in the case of Matthias' provocations). The selection of experimental results and of certain measurable parameters brings about a (retrospective) definition of the target phenomenon. The debate on theories then becomes a complex mixture of controversies involving related factors. *The relevance of different experimental results - the definition* *of the target problem – is not only disputed in itself, but it is also guided differently by different preferences, in the spirit of the two traditions.* Allow me to discuss this further.

We know already the position of Anderson, with his credo of 'first principles first', and Feynman's view that we should select parts of the problem that can be dealt with by the most promising hypotheses. Another interesting voice is that of Chandra Varma, a distinguished professor and a colleague of Anderson, and, like many other characters in this story, at Bell Lab during the golden years of superconductivity research.

"The field of HTS", says Varma, "is *full* of dissent. There is an enormous amount of confusion and diversity of views, partly because this is a field with an enormous number of experiments. Every known technique in science has been brought to bear on these materials and lots of different anomalies have been discovered". In his view, part of the confusion is due to the tendency of physicists to "concentrate on some particular peculiarity. We call them 'epi-effects', a little [effect] here, a little [effect] there. Because of the tremendous infusion of money in this field in terms of new instrumentations, instruments that measure things very accurately have been brought to bear, so that they can measure very small effects". This is a tricky point, says Varma, which directs theoretical attention wrongly, towards experiments that do not match the appropriate energetic description of the phenomenon of superconductivity. He means that "the HTc phenomena is not from the point of view of energy a *subtle* effect, it is not a *small* effect; all the energies involved here are very large. It's a *new* effect, yes, but it is a LARGE effect". This has caused, says Varma, a harmful confusion. "One way to avoid confusion would be to concentrate on broad general properties which are the same across every cuprate, things that are happening in a larger energy scale in every cuprate in a similar fashion." Instead, Varma continues, the field has been highly influenced by theorists like Anderson who "speak only of things they understand and know about", that is, using principles and models they have mastered, and trying to fit experimental results into them. Physicists like Anderson, says Varma, are "playing the game of picking this [peculiarity] up and picking that up and saying 'See? Isn't that strange?' and whatever is strange must be responsible for superconductivity [..] But there is no causal connection between the things he [Anderson] says and the broad general phenomenon that we're talking about. One has to talk about things systematically and have some causality, relations, or some common theme with which one can select a minimal set of principles which can organise the *whole* prominent experimental behaviour."

I certainly do not want to adjudicate between the internal quarrels of individual physicists who are undoubtedly amongst the greatest contributors of the last decades in CMP[35]. But it is important to notice that, much as in earlier phases of the history of superconductivity, there is live debate both concerning the definition of the problem, and the methods apt to solve it.

Collins and Pinch discuss in "The Golem: What you should know about science" (1998) the capacity of theorists to dispute experimental results:

> "In deeply disputed science the potential that is always there to question and reinterpret the meaning of an experiment is actualised. When the stakes are high, the trust disappears, the embedding culture loses its power to maintain consensus, and scientists no longer accept what they have been told. This takes away the

[35] For the sake of my argument, I am also taking for granted the intrinsic value of the major candidate theories in terms of their own scope and methods. In fact I stress that I have no intention of supporting any theory, nor do I intend to give philosophically-based arguments to evaluate their potential.

potency of experiment. Note that this does not mean that experiment should cease to be a feature of the scientific method. If we stopped doing experiments we would no longer be doing science. But we should be clear that the determined critic can always subvert the force of any experimental result" (pp.175-6).

The power of physicists to "subvert the force of any experimental result" is the major source of dissent in the field so the HTS case seems to confirm the claim of Collins and Pinch. I also agree with them that this power is always potentially present in science. I argue, though, that in the case of HTS the subversion of experimental results is not simply, and naively, a consequence of the high stakes, or the lack of trust caused by the intense level of dispute. These are very probably factors contributing to the crisis of consensus, but *I claim that one central relevant factor is found in the clash between different views on theory formulation, and consequently different criteria for consensus.* As I have argued in the previous chapter, behind different criteria for consensus lie different preferences over the methodologies employed to achieve the desired solution, and, ultimately, behind that, different views on what the desired solution is supposed to look like. The vast majority of theorists in HTS assume without hesitation that what is missing, and needed, is an explanation of the phenomena from first principles, so this is not in dispute. The bone of contention is the different 'senses of duty' towards the accurate description of the phenomena. Since experiments have created a very complex picture of the phenomena that contains even more features and anomalous properties than in the phenomenological description of LTS, different scientists can create all sorts of different description of the high Tc superconductors' phenomena by selecting different combinations of experimentally observed features which they deem relevant. From these descriptions, a theorist can then build hypotheses and explanations to derive the high Tc superconductors' behaviour from first principles. Then, when the theory is sufficiently developed, a theorist may be offered different pieces of evidence which may or may not fit into her description, and which may or may not be accountable for by her theory. *At this point, the importance given to the aim of providing the most accurate description, relative to importance given to the aim of the logical coordination of the theory, becomes crucial.*

As I have discussed in the previous chapter, Anderson maintains that there is enough irrelevant complexity that an unwitting theorist may never reach the neighborhood of the actual problem. Once the irrelevant complexity has been excluded, and a 'proper' description that contains only relevant features is set, the maze of alternative paths, says Anderson, can be reduced by simple logic ("When one has found a way through the maze of conflicting requirements, that is certain to be the way, no matter how many deep-seated prejudices it may violate and no matter how unlikely it may seem to those trained in the conventional wisdom", see 5.5). But what are the criteria for relevance here?

Anderson's claim that relevance is obvious, and implicitly determined through the simple use of logic, is surely an oversimplification, and probably only rhetorical. It is, of course, impossible to specify criteria for relevance, and the subjective skills and intuition of different scientists guide different decisions. But this complexity does not exclude the possibility of philosophical insight. This is exactly what I claim is provided by thinking in terms of the two traditions. Behind the methodological difference between Anderson and Matthias lies an assumption that was clearly expressed by Feynman when he claimed that achieving a first-principles derivation of *any* property meant *grabbing the tiger by the tail* - obtaining at least a true *aspect* of the underlying mechanism for superconductivity. While for Matthias an explanation that failed to include the whole set of observed regularities was inadequate, for Anderson the inclusion of regularities not initially fitting the principled theory would be achieved by further development of the theory; this final step was *warranted* by the plausibility of the theory in terms of first principles. This latter attitude allows a theorist to ignore evidence not initially included in the theory, since the logical coordination of the theory most likely suggests that the inconsistencies are not worrying, and eventually will prove "apparent more than real".

In this light, the high level of dissent in the HTS debates can be interpreted in a way which goes beyond controversies on the interpretation of data. Dissent becomes a bitter war among theorists armed with different preferences on how to define the problem, how to solve it, and how to reach consensus on the solution. I will give the testimony of some of the main players in HTS to further illuminate this.

In conferences and workshops, the scene of scientists contending between different mechanisms for HTS by presenting different sets of data, which specifically work in their favour, is a common one. During a heated debate on the use of ARPES data to settle the issue of what are the important excitations that couple to the electrons, Doug Scalapino, a pioneer in HTS, expressed his frustration at the controversial use of isolated sets of data to push one hypothesis or the other: "To get wide spread agreement in the physics community one will need to show that a given approach fits *all* of what is seen as the relevant [cuprate] data" (Scalapino, 2007). This allows me to clarify an important point. There are two ways in which a theorist can fail to comply with Scalapino's plea. On one hand a theorist may disagree on what is the *relevant* cuprate data, and therefore seek confirmation by confronting other theories on incompatible battlegrounds, i.e. by comparing the empirical adequacy of two theories on different empirical subsets. On the other hand she may agree on what is the relevant data, but fail to account for the whole set, addressing only a subset of it. A large part of the disagreement takes place in the second scenario, and this is the scenario that Scalapino attacks, promoting overall consistency with the whole set of data. Moreover, a theorist may not only address just a subset of data, but may also attempt to establish it as the *only* relevant set and deny that new or different data is relevant. This point is of concern for some physicists. I will give the example of Varma, who explicitly articulated his ideas on this in my interview.

Varma starts by recounting an example from his experience, that of a laboratory in France which, a few years ago, produced some striking results in neutron scattering experiments showing unexpected behaviour in cuprates[36] (Fauqué, et al., 2006). He recalls that their experiment encountered enormous difficulty in being published, because the referees could not believe in the validity of an experiment with such a counterintuitive result (a hidden magnetic order characterizing the pseudogap – suggestive of a new order parameter). He considered this highly disappointing, since his theory was able to predict their result, which he considered a crucial one. After two years the result was finally published. When he talked with Anderson about this result, Anderson sent him an email saying, according to Varma, "I saw this experiment. This experimental data really

[36]The group, led by Philippe Bourges of the Commissariat l'Energie Atomique, observed current loops in experiments involving the diffraction of polarized neutrons.

worries me. But I have talked to a friend of mine who is an expert in neutron scattering and he told me that polarised neutron scattering is a very difficult experiment, and so for the present time I am going to ignore it". This attitude is the same we encountered in response to Matthias's experiments with inhomogeneous materials, which, for Anderson, probably showed spurious results and could be ignored. These are clear examples of the capacity to subvert experimental results explained above by Pinch and Collins.

There is no clear way to assess the plausibility of this kind of judgements, and I only intend to discuss the instrumental role that this capacity has to create dissent among researchers in the field. Varma judges this attitude quite openly:

> "I compare this to burning books in the middle ages. As you know, they used to burn dangerous knowledge. The phase diagram that most people draw in the experiments, if it is correct, is bad for Anderson's ideas. So in fact he gives a talk with a complete different diagram. I call that *book burning*, dangerous information that you want to go away. I hope that when the experiments are very clear the book burning will stop. But within my scientific career, since 1969, I have not seen anything as dramatic as this happening in HTS. [..] One says let's take what we understand and let's somehow try to fit this [new piece of evidence] into it. And it is understandable. But there is the book burning, and also [something I call] the *ayatollah effect*. There are 'prophets', and these prophets have issued declarations about how it is. They even declared them, in some cases, *fatwas* [Anderson's central dogmas]"

The fatwas that Varma mentions, intended as dogmatic stances over different subsets of data as relevant, are not arbitrary. In his view they are postulated because they *fix* the observations needed for the assumption of a model, a hypothesis, or an approximation. They are instrumental to the hypothesis we want to defend. This is in contrast to the picture in which hypotheses, approximations and models are selected according to their ability to fit the "whole prominent behaviour".

It is surely not a novelty that scientists, like anybody, may have a tendency to defend their own pet theories even in the face of contrary evidence - when that evidence is sufficient to convince otherwise a scientist who is not personally attached to the same theory. This is conduct, though, most likely to be of interest to psychologists or sociologists[37]. For the sake of my argument I only need to point out that the attitude lamented by Varma, and encountered in Anderson's views, underlies a belief in the structure of theories that takes logical coordination as a precondition for empirical adequacy, and deems the complete description of the phenomena a by-product of the successful derivation of theory from first principles. *The power of a principled theory to withstand the challenge of contrary experimental evidence resides in a smaller preference given to the aim of providing the most accurate description, a preference often justified on the grounds of the (subjective) belief that logical coordination ultimately leads to the desired empirical adequacy.* As I have explained in chapter two, I do not conceive of the two traditions as separating the different scientists into two camps. Instead I have argued that we should conceive of the traditions as involved in sets of preferences. I am arguing that *this notion contributes to the understanding of the debates in HTS.*

[37] This conduct may even serve science, by allowing theories to be defended against all odds, and allow theories fair hearing for as long as the community's deliberation or 'common sense' allows it.

I have argued, with reference to such cases as Matthias and London, that the empiricist tradition, associated with the broad preference for practices aimed at a reasonably full description of the phenomena, is associated with an instrumental use of theories. In the cases of Feynman and Anderson I have argued that a new type of instrumentalism is proposed, in which experiments are selected or used instrumentally for the sake of achieving a principled theory. These two types of instrumentalism are in addition to the predictable controversies concerning technical issues in competing theories; they form the very basis for the dissent between physicists in HTS.

In the next and final chapter I will show how the instrumental use of evidence for supporting competing theories creates this dissent, by considering examples of some recent publications in HTS in which opposite theories are supported using the same new empirical evidence.

CHAPTER SEVEN

"The most valuable tool of the theoretical physicist
is his wastebasket."
(Albert Einstein)

In this chapter I return to the different roles of experimental evidence that I described in Chapter Two, and show how they are linked together in the relationship with theory. Then I show the contribution of powerful techniques such as ARPES, and discuss their importance for theory formulation and testing. Finally I show how the instrumental use of evidence for supporting competing theories fosters controversy, by considering examples of some recent publications in HTS in which opposite theories are supported using the same new empirical evidence.

In looking at these examples I pick apart and reconstruct the strands of a complex debate in which each of two warring camps claim to account for the same evidence, meanwhile claiming that this evidence is incompatible with the opposing theory. Using the language of Bogen and Woodward introduced in Chapter Two, I argue that in fact the two camps do not account for the same evidence but rather for different phenomena that each constructs from the data in its own contentious way from its own contended point of view. This strategy, I argue, devalues the importance of empirical evidence in testing and confirming theories and fosters dissent in the HTS community. I close with the pessimistic hypothesis that use of this strategy may be becoming more widespread in contemporary physics, leaving things other than empirical evidence – like mathematics – to do the heavy lifting in theory confirmation, as Peter Galison suggests.

7.1 Experiments and Theories of Evidence in HTS

I proposed, in Chapter Two, a number of different roles that experimental evidence plays: to define, to discover, to test, and to help theory formulation. In this section I want to tie together the strands of experimental practice explored in previous chapters to illustrate these roles further.

My story has shown that superconductivity is an experimentally observed phenomenon, an unexpected behaviour of matter that has troubled theorists for a century. The role of experiments for scientific discovery is obvious, and clearly evident in superconductivity. Indeed, it is more evident than in most other current fields of physics, as the eminent particle physicist Steven Weinberg explains (Weinberg, 2007). In his view, elementary-particle physicists are not generally very excited by the phenomena they study. He remarks: "The particles themselves are practically featureless, every electron looking tediously just like every other electron." But condensed matter physicists "are often motivated to deal with phenomena because the phenomena themselves are intrinsically so interesting. Who would not be fascinated by weird things, such as superconductivity, superfluidity, or the quantum Hall effect?". Most elementary-particle physicists, continues Weinberg, are in their field "neither because of the intrinsic interestingness of the phenomena that we study, nor because of the practical importance of what we learn, but because we are pursuing a reductionist vision". HTS research, instead, "aims at making discoveries that are useful" (Weinberg, 2007).

As I have shown, the community of experimental physicists have used their own phenomenological/empirical laws and 'chemistry' throughout the history of superconductivity to *discover* new materials, with higher transition temperatures and new appealing features. This has not only been an independent path of research, but also a big asset to the HTS program in general. This is firstly because it has generated a huge library of data that contributes to the theoretical research as well, and secondly because it is the main reason why HTS attracts big funding, including even from the private sector – money which can then be partly invested in theoretical work.

In the theoretical attempt to characterise the features of superconductivity, experiments *define* what is there to be explained. In particular, as I have argued in Chapter Six, experimental practice in the HTS problem involves a delicate balance between pre-existent theoretical background and "empiricist" exploration which has *re*defined the problem, and *re*assessed our understanding of superconductivity at large (e.g. By highlighting that the normal state is not that 'normal' after all as it displays, in high Tc materials, several anomalous properties which do not fall under current theoretical models). This experimental role is crucial, being directly tied to the role of experiments in *helping formulate theories of HTS.*

In the previous chapter I introduced the type of instrumentalism in which experiments are selected or used instrumentally with the aim of achieving a principled theory, and identified it as a source of controversy in the field. Contrary to the idea that quarrels between physical theories are based primarily on technical matters soluble through calculations and charts, or in laboratories, I argued that dissent in HTS is also fuelled by contentions about methodology, consensus, and the theory-experiment relationship.

Many of my interviewees (e.g. Varma in the previous chapter) expressed concern over these sources of dissent. The concluding paragraph of a review by Gary Steele (MIT/Delft physicist) titled "The Physics of High Temperature Superconductors" (2001) contains a clear statement of concern directed at the instrumental use of evidence. In the future, argues Steele, to clarify the issues at stake in the theoretical discussions reviewed in the article "we must do more experiments [..] and make sure we do them properly". The author stresses that the misuse of data for the sake of defending specific theories (i.e. what I call 'the instrumental use of evidence') has been a serious obstacle to the advancement of our theoretical understanding of HTS, diminishing the contribution of this evidence: "It is *crucial* that experimental results should be presented is such a way that they stand on their own. Too many experimental papers have been rendered useless because the authors went to great efforts to squeeze the data into the context of some theory, leaving interpretation outside of this context nearly impossible". His plea is for independent and careful experimental practice. "The cuprates have a very rich phase diagram that is abundant with interesting and unique many-body states of the electron liquid. There is much phase space to explore: charting it all carefully can only serve to deepen our understanding of the spectacular physics of the cuprates." (emphasis in the original) (p.30-31)

I will now, for illustrative purposes, give a sketch of a possible theory/experiment relationship in HTS. Suppose that there has been some experimental investigation of the behaviour of a superconductor. A large body of data, "D", has emerged, and needs interpretation. The measurement of the relationships between the various observable parameters (such as energy, temperature, resistivity, doping, saturation, velocity ratio, dispersion) taken altogether constitute D. Different theoretical groups with different explanatory programmes extract from the experiments subsets of D, maybe the same, maybe different, and then interpret them, arguably trying to maximise the size of their subset; they are trying to account for the largest possible amount of data - ideally *all* of D.

Let us take two examples from among the currently most debated and popular interpretations: Phonons, and magnetic modes. A truly enormous amount of literature is generated on phonon and magnetic modes, and a large part of the community takes both of them as plausible candidates, because they seem to be exceptionally good at explaining some (usually different) subsets of D. At the same time, both have apparently serious problems accounting for *other* subsets of D. So, simplifying somewhat, they both find justification for pursuing their research programme in their successful explanation of a subset of D, and use that justification to push for further research aimed ideally at covering the rest of D. This is in accordance with what is expressed by the HTS community members in my interviews, which Scalapino summarises with his remark, "we need to fit *all* of the relevant cuprate data". It is *prima facie* evident that a requirement for the one finally successful theory is for it to fit the largest possible body of experimental evidence. Yet I want to show that the instrumental use of evidence in the HTS practices presents a more complex picture than this.

Laudan stresses that "the views of former scientists about how theories should be evaluated must enter into judgements about how rational those scientists were in testing their theories in the ways they did [...] Models of science which did not include a scientist's theory of evidence in a rational account of his actions and belief are necessarily defective". Laudan also points out that the views of the scientific community on how to test theories, and on what counts as evidence, have changed dramatically through history. Different proponents (as in the HTS case) may appeal to different epistemic virtues when they argue for their theories or against those of others, but in practically everyone's book, accounting for the evidence is a central - if not *the* central - epistemic virtue. But what is 'accounting for the evidence'? The answer to this question constitutes, in a given context, what I call a 'theory of evidence' for that context. In highlighting the interplay of the two traditions in the history of superconductivity, I have shown how the interplay of theory and experiment is indeed a living and evolving organism, and in this chapter I will show further indicators of variations in the theories of evidence employed by physicists in the community.

Peter Galison claims that, in modern physics research, satisfying mathematical constraints may be sufficient for a theory to be judged acceptable and discusses examples such as Monte Carlo simulations to "uncover the practices underlying this talk of 'experiment' done on keyboards rather than lab benches": Math is the new physics lab, he argues (Galison, 1996). Perhaps he is right. If this shift has taken place, though, the HTS case is surely a questionable test case. On the one hand, heavy reliance on mathematics and first principles as criteria for judging theories is a recognizable trend in HTS research, as I have argued. On the other hand, the proponents themselves seem to endorse the more conventional view that "sufficient" and "decisive" empirical evidence is required in order to justify the choice of one theory over another. As we examine the practices in the field, we find that the tension between the different desiderata for theories – between the two different traditions I have been describing – has reached crisis point.

In the HTS case, I will claim that underestimating the importance of cooperation between the two traditions, and in particular underestimating the importance of achieving an independent and reasonable description and representation of the phenomena, elicits incongruous uses of evidence, and ends up fostering dissent.

7.2 Underdetermination and the Contribution of ARPES.

In the literature, one finds several strategies used in this battle of theories; here are the most common ones, taken from a large sample of the HTS publications. Proponents of one theory (T_1) can:

- Show how well T_1 explains a particular subset of data, d_1.
- Show how their subset d_1 cannot possibly be accounted for by an opposing theory T_2.
- Ignore the rest of D, which doesn't fit their theory.
- Ignore the subset d_2 that T_2 claims to cover (implicitly rejecting it as good data without specifying why).

Less commonly, but maybe more admirably, they can:

- Acknowledge the difficulty with the rest of D, hoping or promising to solve the discrepancy by future work, or with future discoveries.
- Suggest promising research paths and hypotheses.

They may also engage with the *data* d_2 of their adversary T_2, and may:

- reject T_2 as false since d_2 is erroneous in some way, ideally suggesting an explanation for the mistake on the adversary's part.
- reject T_2 when the subsets d_1 and d_2 considered by the two different candidates overlap.

The case in which the subsets d_1 and d_2 overlap is the most interesting and controversial. Since we expect the strongest theory to be the one which covers a large and increasing data set, two advanced (candidate) theories that claim to do so sufficiently well (and which become more popular as they enlarge their data subset) will inevitably overlap in the data sets they claim to cover.

Let us imagine that T_1 and T_2 both consider some piece of evidence, such as the experimental measure of dependence between parameters x and y (How x varies with y), and imagine that they each explain such dependence through their own theory, even though T_1 and T_2 are mutually exclusive. How is this possible?

Helen Longino, among others, points out that the same evidence could be evidence for different incompatible theories because of the involvement of different auxiliaries. So, it is entirely possible that T_1 & $aux_1 \rightarrow e$ and also T_2 & $aux_2 \rightarrow e$. This model is however quite simplistic; we will need to raise further considerations before we can apply it to the real case of HTS.

For instance, it may be the case that the evidence under consideration is something that T_1 and T_2 are not fully able to derive from their own assumptions, perhaps because either the theory is not spelled out enough to do so, or the evidence is too complex. If this is the case we can note, at best, that evidence e is *compatible* with T_1 or

T_2, leaving the auxiliaries paradoxically doing all the work. Some part of the HTS literature is in fact content to do just this: To show the mere compatibility of some evidence with some theory. This is obviously quite weak, so many writers seek further support for their theories from 'between-the-lines' principled arguments. In essence they resort to saying that T_1 is good primarily because we know it to be good for other reasons, adding, as an additional, secondary, merit, that it is now *also* compatible with the new evidence e. This is automatically controversial, since the 'good reasons' and 'principled arguments' are precisely what others, adopting different hypotheses and assumptions, reject.

The fact that phonon models are capable of accounting for the isotope effect in LTS, for example, is hardly a strong argument in favour of its unverified capacity to account for the phenomenologically different isotope effect in HTS. These arguments remain ultimately *ad hoc* until they receive further confirmation through evidence. So while one could use them to argue that the phonon picture *remains appealing* notwithstanding the apparent experimentally contradictions, we still need further evidence to eventually *confirm* it, showing that the contradictions vanish under a different interpretation, or that a modification in the theoretical treatment 'saves the phenomena'.

As in the story of the frozen flux experiments, we can be justified in wanting to defend a theory even in the face of unfavourable evidence, as I have discussed already at some length, yet we clearly need to recognise, and attempt to meet, the challenge that the experiments are offering, if we are to achieve robustness and consensus.

Nevertheless, when T_1 and T_2 are able to *accommodate* the same subset of data in their own theory (their d's "overlap") - in this weak sense of being *compatible* to it - their proponents may be tempted to claim that they have expanded their coverage of D, hence gaining confirmation. While this claim is clearly not legitimate in the face of the Underdetermination thesis (Many models can be compatible with the same data), it is easy to understand it, and eventually to accept it, as a rhetorical tool that a physicist uses to make her case stronger in the competition with other accounts, most of which she deems implausible.

In the HTS case so far all of the candidate theories fail to formulate predictions in great number and with great clarity. (For example, in some cases they are not spelled out sufficiently to derive from the dynamics clear observable consequences; in other cases there *are* such consequences, but they are not yet "bold", to use Popper's characterisation.) Indeed the moves that I have described occur in most of the HTS literature. This contributes to dissent, in that it allows for a continuing fight between opponents, a fight which is only apparently evidence-grounded, and is based on arguments which are ultimately weak.

In rare situations where T_1 and T_2 instead have predictive power, the instrumental use of evidence, which I have presented earlier, brings its additional contribution to dissent. I now discuss this situation, referring to further detailed cases from the HTS literature in which T_1 and T_2 not only engage with the same subset of data d (Their 'd's overlap), but they also have something substantive to say about the overlapping data, ideally confronting such data with what their theories can predict (or retrodict!). To enable the reader to appreciate my discussion of thee cases, I need to first explain a striking technique that is the basis of a considerable number of experimental publications: The ARPES technique.

As condensed matter physicist Dmytro Inosov (2008) explains, "over time, the focus [has] gradually shifted from finding a theoretical description of an experimentally observed phenomenon to distinguishing between multiple models that offer comparably reasonable descriptions" (p.9). To understand the mysterious mechanism that causes superconductivity in high Tc superconductors, physicists need to characterize and understand the many-body interactions that influence the behaviour of the charge carriers in these materials. Standard Fermi Liquid theory calls these charge carriers "quasiparticles" and describes their behaviour by what is known as the spectral function A(k,E) - the imaginary part of the single-particle Green's function[38] which is known to describe the propagation of an electron in a many-body system, where k is the momentum (in the 2D Brillouin zone) and E the energy (measured with respect to the chemical potential). The interactions that influence the behaviour of these quasiparticles are quantified in the spectral function through the complex self-energy term Σ. The real part of Σ shows how these collective interactions influence the dispersion relations/band structure of the material (i.e. the E vs. k curves), while the imaginary part of Σ indicates the scattering rate or lifetime of the quasiparticles. For an idealized metal with a non-interacting free-electron gas, the self-energy term is zero. This means that there is no deviation from the non-interacting electronic band structure/dispersion ($\mathrm{Re}\Sigma=0$) and it has zero scattering rate/infinite lifetime ($\mathrm{Im}\Sigma=0$). *So if one can actually measure this*

[38] $A(\mathbf{k},E)=-(1/\pi)\mathrm{Im}G(\mathbf{k},E)$.

A(k,E), one can gain a lot of insight into the interactions that influence the behaviour of the quasiparticles that are responsible for superconductivity in high-Tc superconductors (Campuzano, Norman, & Randeria, 2004).

The very powerful technique called Angle Resolved PhotoEmission Spectroscopy (ARPES) is based on the photoelectric effect, and sets out to do exactly this.

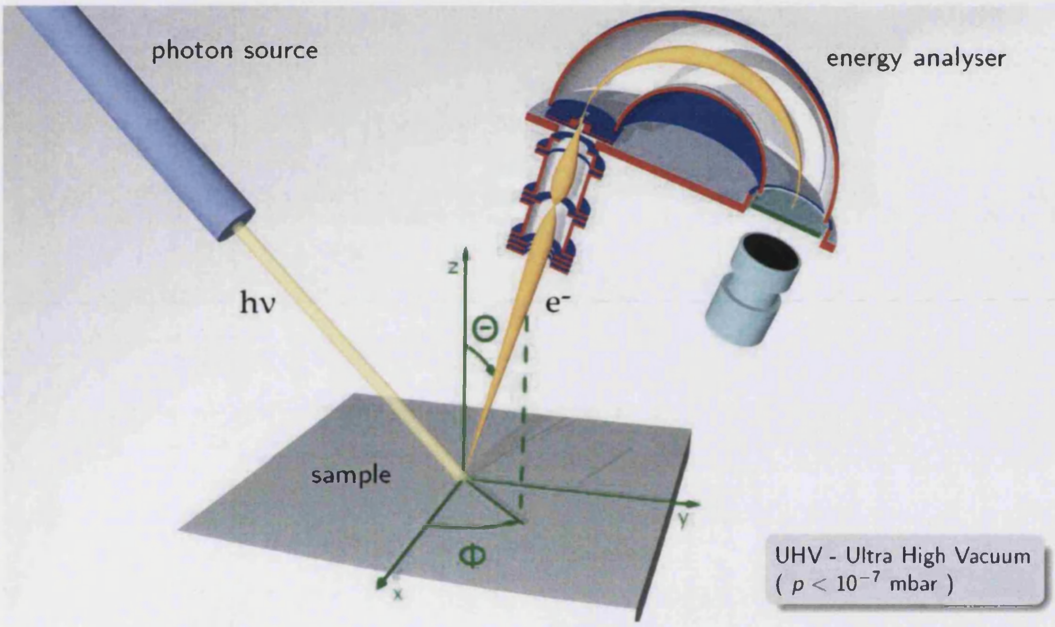

Figure 4: photoelectric effect as employed for ARPES experiments on surfaces.

A considerable shift in the experimental exploration of cuprates took place ten years ago, when the ARPES technique was enormously improved; it now allows not only the collection of the energy distribution curve (EDC) at a particular momentum, but also the momentum distribution curve (MDC) at a particular energy *simultaneously*. Thanks to this, a collection of 2D raw data of energy distribution and momentum distribution of the photoelectrons within an energy and momentum window was suddenly available to scientists, who now could obtain the full dispersion curve. ARPES' increase of power has been impressive: Today ARPES experiments with 2 meV energy resolution and 0.2 degrees angular resolution are possible. Because of its bi-dimensional photoelectric effect setup, ARPES has become especially attractive in application to quasi-2D materials, which include the cuprates, since in this case one can directly and completely determine the band structure

This is a clear example of the assistance that experiments and phenomenological input provides for the formulation of theories. In fact, the papers that I will now discuss start from some surprising results obtained with this technique, which seem to point clearly towards some bosonic 'glue', which, if identified, could indicate the mechanism for HTS. Cuprates such as the BSCCO family are easily cleaved *in situ* along their 2D planes (CuO planes), exposing a pristine surface perfect for photoemission studies (Campuzano, Norman, & Randeria, 2004). When this technique was used on the BSCCO family in 2001, the dispersion curve clearly showed a distinctive "kink". This kink is the deviation of dispersion curve from the non-interacting dispersion, which is taken to signify a coupling to some bosonic mode. It was immediately clear, then, that this bosonic mode might be the 'glue' that forms Cooper pairs, and that the nature of this kink was crucial. The race to account for this kink was on.

7.3 The Kink and the Toothbrush Theories of HTS.

The papers by Kaminsky et al. (2001), and Johnson et al. (2001) argue that the *magnetic* channel is responsible for the electronic properties in the BSCCO cuprates, and is the responsible mechanism for superconductivity. On the other hand, the paper by Lanzara et al. (2001) argues for the *phonon* mechanism in the same materials. What is interesting is that all three papers claim support for their mechanisms from the ARPES generated kink. These three papers were the first to appear after the observation of the kink. I will summarise their arguments, and then discuss them.

Previous to these three papers, a similar kink had been observed in Beryllium and had been associated with coupling with surface phonon modes[39] (Hengsberger, Purdie, Segovia, Garnier, & Baer, 1999). The authors of the first paper (Johnson et al), while acknowledging the previous result (that the kink could reflect coupling to phonons), report that neutron studies indicate that these phonons occur at the same energy, independent of doping or temperature and that this is in complete contrast to the temperature dependence of the kink energy observed in their study: "If phonons were the source of coupling, the scattering rate would saturate at frequencies greater than the highest phonon frequency (~100 meV) with a marked temperature dependence in that range. This is clearly in contrast with optical conductivity and photoemission experiments [Refs given by authors] [where the lack of saturation in the scattering rate points to the absence of a well-defined cutoff]". The dismissal of the phonon picture as a contender is argued for on the basis of previous experimental results and theoretical calculations, quoted as references.

Similarly, Kaminski et al. acknowledge in their paper that "similar kinks in the dispersion have been seen by ARPES in normal metals due to the electron-phonon

[39] In that paper, as in the ones discussed here and many others, the authors go to great lengths to attack an opponent theory. They report that no gap is observed down to 12 K. From this they deduce that "any interpretation of the data based on charge density wave formation or surface superconductivity, can therefore be discarded".

interaction", but continue: "Phonons cannot be the cause here, since our kink disappears above Tc. Rather, our effect is suggestive of coupling to an electronic collective excitation which only appears below Tc".

As a negative argument we can simplify it this way: Given presented data A (here the kink) *below* Tc, while phonons are able in principle to account for A, they are to be rejected as the 'glue' for HTS because they would predict (according to evidence B and calculations C, mentioned as references) the occurrence of the same A *above* Tc, and this is not observed. The magnetic channel, as a coupling mechanism to an electronic collective excitation, is then suggested as a candidate, since this coupling appears only *below Tc*, where A is observed, and therefore has at least initial plausibility as an explanation. But whether it *is* an explanation is in fact left open, in need of further evaluation and study; hence what we are mostly offered is an anti-argument, in the sense that it argues for a position mostly by rejecting its strongest adversary.

As a positive argument, it remains quite weak. Their contribution to the literature is their data showing that dispersion and line shape anomalies have a continuous evolution (throughout the zone of interest where the kink appears) but always show the same characteristic energy scale. According to Kaminski, "this leads us to suggest that the same electron-mode interaction determines the superconducting line shape and dispersion at all points in the zone, including the nodal direction. In essence, there is a suppression of the low energy scattering rate below the finite energy of the mode. [...]We suggest that this suppression of the scattering rate below Tc at all points in the Brillouin zone is due to the presence of a gap and a finite energy collective mode, which we identify with the magnetic resonance observed by neutron scattering." So they infer, from the "same energy scale" observed, the suppression of the low energy scattering, and *suggest* that the suppression is indicative of a finite energy collective mode, which in turn *suggests* their magnetic glue as a mechanism. This amounts to showing little more than mere compatibility between the data and their theory, and is hence, as I have said, rather weak.

On the other side of the battlefield, Lanzara et al. maintain that a coupling of an electron with a phonon would result in an abrupt change of the velocity and scattering rate near the phonon energy, as acknowledged by the other two papers. Using ARPES to probe electron dynamics - velocity and scattering rate - for three different families of cuprates, they observe in all of them "an abrupt change of electron velocity at 50-80 meV, which we cannot explain by any known process other than to invoke coupling with the phonons associated with the movement of the oxygen atoms". Comparing the ARPES data with neutron-scattering data, they find that a particular phonon mode coincides precisely with the kink. This, they claim, is a strong argument for the phonon picture - indeed they conclude "that electron-phonon coupling strongly influences the electron dynamics in the high-temperature superconductors, and must therefore be included in *any* microscopic theory of superconductivity" (my emphasis).

But what about Kaminski and Johnson's objection?

Interestingly, what Kaminski and Johnson take to be the *weak* point of the phonon interpretation (the phonons' temperature independence) is in Lanzara's paper exactly the *strong* point. Lanzara et al. report that "both the angle dependence and the temperature dependence are consistent with the phonon interpretation, *as phonons are more isotropic* *and will not disappear above Tc*". The phonons' isotropy is what makes them behave[40] the same way above and below Tc. This isotropy is exactly what Kaminski's and Johnson's papers showed to be the problem, as they observed a *dependence* on temperature, which excludes phonons precisely because their contribution does not disappear above Tc.

What is more, Lanzara et al. use against the adversaries the same strategy found in Kaminski and Johnson. "The data also rule out the proposed explanation in terms of coupling with the magnetic mode at 41 meV, because of its temperature dependence and its ubiquity. [..] These considerations, *based on direct experimental evidence*, leave phonons as the only possible candidates", they conclude, "since the phenomenon is observed above Tc, where the magnetic mode does not exist" (my emphasis).

Let me explain this rather confusing situation further, hoping to generate some clarity. First a set of data appears from ARPES experiments which shows this kink. All the groups, having seen the robustness of ARPES data, start working on this kink, writing papers exploring the capacity of their models to account for it, since it obviously *qualifies as relevant data the final theory must account for* (with its additional potential to highlight the exact nature of the bosonic glue). In particular these papers, the first to account for the kink, repeat the experiments, using their own experimental setups on the same material to study the nature of the kink further. In doing so, they claim to be testing their theories T_1 and T_2 against a robust piece of evidence they clearly agree on - the ARPES generated kink data. In fact, however, though they intend to explain and account for the kink, independently observed and considered important, they actually *confront*

[40] I won't go into the details of phonons theory and of isotropy, as they are irrelevant here for this argument.

their theory with something else, namely data derived from autonomous reproductions of the phenomenon (the kink). They all claim that this is direct experimental evidence, but it clearly cannot be so. Naturally, it is not problematic in principle to reproduce experimental results and confront one's theory with these reproductions, but since these two groups' reconstructions give opposite, *incompatible,* results, this is actually clearly problematic. They appear to explain and account for a kink that displays in one camp temperature dependent, and in the other temperature independent, behaviour. They seek confirmation by accounting for a common piece of evidence which the community has agreed is relevant, but they do so by confronting their theory with data sets which are incompatible and irreconcilable, and obviously not agreed. Thus, as in the previous example of apparently overlapping subsets of D, they are not really obtaining confirmation through the same d, but actually creating different phenomena out of the same d (creating once again the problem of criteria for relevance).

I have borrowed the term 'phenomena', as used above, from the classic data/phenomena distinction advanced by Bogen and Woodward (1988), which I have presented in Chapter Two. On Bogen and Woodward's account, models of data constitute evidence for phenomena, which in turn constitute evidence for theories. We gather data, assess their reliability (such as the robustness of ARPES data), and infer phenomena from observed patterns in them. We then see if the phenomena are explained by theories. I have already also presented McAllister's argument that phenomena (like theories) end up being features of the world that depend upon the scientist's judgement in relatively loose ways, since any data set is able to exhibit an infinite number of patterns. For the sake of my argument here, I simply start with the notion that the evidential relation between data and theories is not direct, and that a series of inferences (via either a series of data models, or straightforward creation of phenomena) is drawn to justify the use of data for accepting or rejecting theories. In Bogen and Woodward's terms we would say that the HTS community continuously creates phenomena out of data, and then requires that the final theory has to account for all these phenomena. For example when patterns emerge from a consistent experiment that uses the ARPES technique, the patterns gain epistemological and ontological status and need to be explained by the theory, as is the case with the kink. In fact, due to ARPES' ingenious technique and its recently achieved high accuracy, ARPES-generated data are able to garner a considerable consensus in the community - a rare event. Naturally, the consensus on ARPES data is fallible, but this is presently irrelevant, as I am investigating the practices of groups of scientists dealing with data that they have agreed on at one time, regardless of whether the consensus will fail in the future.

By HTS standards, as I explained, a theory would gain confirmation if it explains the agreed 'phenomenon' (the kink) that we have created out of stable data. I argue that in the present cases, while the authors claim to gain confirmation this way, they are, on closer inspection, each explaining a surrogate phenomenon, two different patterns from which an almost identical kink appears in almost identical settings - a simulacrum of the original phenomenon. If it was the same phenomenon, its characteristic features would be identical, and the candidate theories would then try to account for these features. Instead in these papers not only do the phenomena that emerge from the data in the accounts of the two adversaries have contradictory features (It is either dependent or independent of temperature), but also it is exactly by virtue of these different features that each claims to gain confirmation.

It is certainly useful to enquire further and try to establish whether or not the nature of the kink is indeed dependent or independent on temperature, or even whether this feature is entirely irrelevant to the explanation of the phenomenon. But I stress that such enquiry is not part of the baggage of the "kink phenomenon" we start with, and no consensus has yet been reached on further features of the kink. If the warring theories actually were able to account for the kink, and if one camp had additionally observed a *further* pattern of data (a phenomenon), related it to temperature, and shown that their theory predicted that pattern while their adversary's did not, I would not have my present concerns. The objection is that the theories do not actually account for the kink *yet,* and that in their argumentation it is these *extra* features of the kink that do the entire job. These features are obviously not agreed on, since the two groups give two opposite stories about them. I argue that claiming to gain confirmation by explaining a robust phenomenon while in reality explaining two different, incompatible, phenomena is illegitimate. As in the previous example, while we may concede that a theory can increase its appeal when theorists show how some bits and pieces of the story fit well with it, or fit badly with the adversary, no real confirmation is thus achieved.

Steele's (2001) complaint that "too many experimental papers have been rendered useless because the authors went to great efforts to squeeze the data into the context of some theory, leaving interpretation outside of this context nearly impossible", is exemplified by these cases, which I present here as 'creation of phenomena'. As a result of this debate the community has even less clear knowledge about the features of the kink, and its trust in reported results, even when they appear in the most prestigious journals, is further damaged, fuelling destructive dissent.

The disparity between the theories of evidence employed by the HTS proponents allows the warring camps to each claim empirical adequacy for itself, and deny it to opponents. The kink debate continues, as a ping pong match between phonons and magnetic modes. In 2007, two papers appeared in the same month reporting a higher energy kink (around 340meV) in addition to the previous low energy kink (Xie, Jepsen, Andersen, Chen, & Shen, 2007) (Valla et al., 2007). This result has been argued as being in favour of *magnetic* modes (Valla et al., 2007). The same year, Zhang's group observed some kink's features that they argued might not be accountable for by *either* modes (Zhang, et al., 2008). In 2008 a paper studied the kink in Bi-cuprate compounds, finding evidence for the *phonon* origin of the kink (Graf, et al., 2008). Shortly after, others introduced new theoretical parameters taken from inelastic neutron scattering experiments, and produced a model that they claim can explain all the ARPES results identifying *magnetic* modes as the dominant mechanism (Dahm, et al., 2009). On June 2009, an article was published that calculated the phonon contribution to the kink concluding that phonons only contribute ~10% to the kink, while non-phonon sources contribute the other 90% (Schachinger, Carbotte, & Timusk, 2009). Lastly, in August 2009, a paper showed that the kink has a momentum dependence on the number of CuO planes per unit cell, which is claimed to be inconsistent with the magnetic coupling scenario, thus pointing to the electron-phonon coupling as the origin of the kink (Lee, et al., 2009). The debate does not seem to approach resolution with the increase in experiments and publications - and, after all, this is just one example of the clash of two 'schools' (phonon and magnetic modes) among many.

As I have discussed, the different phenomena are constructed in ways that fit the very theories that are supposed to be tested by them, and fit them in ways that are not sufficiently specified and theory-independent. *This instrumental use of experiments weakens the concept of evidence.* It is omnipresent in HTS practice. The same trend is recognizable elsewhere in physics; the present remarks could be expanded into a more general investigation of the shifting attitudes in modern physics towards evidence, as exemplified by an extreme case such as String Theory, which is hotly debated in both the physical and the philosophical community.

This shifting attitude hinders the constructive interplay of theory and experiment, and the cooperation of the two traditions, both of which, I have argued, have played a crucial role in the entire history of superconductivity. Ambiguity in theories of evidence, and the instrumental use of experimental results, damages trust, and encourages physicists to take evidence less seriously – to demote it to a secondary role. This diminishes attention on the task of achieving theories that give a reasonably full description of the phenomena, in favour of using criteria for theories based on first principles – a shift denounced by many HTS players. Balkanization of the theoretical HTS community is encouraged, and theories become like *toothbrushes*: We each have our own, and we do not share.

7.4 Epilogue.

I conclude with an entertaining anecdote (Rodgers, 1998), with a nice lesson.

A few years ago, some experimental results appeared that seemed to rule out one of the candidate theories - the Bipolarons theory, developed by Chakraverty from Landau's idea. Since the very same theory is still strongly supported by many "fans", this episode does not represent much of an exception to the 'toothbrush trend' - the individualistic proliferation of theories. Chakraverty and Ranninger, though, themselves wrote, in a seminal paper published on Physical Review Letters, that they felt that these experiments were lethal for their Bipolaron theory, and that they were consequently giving it up. They concluded that "*the tragedy of beautiful theories, is that they are often destroyed by ugly facts. One perhaps can add that the comedy of not so beautiful theories is that they cannot even be destroyed; like figures in a cartoon they continue to enjoy the most charming existence until the celluloid runs out.*"

While many bipolaron fans rejected the authors' intention to drag the whole programme down with them, others welcomed the paper as a refreshing episode – a breath of fresh air (Rodgers, 1998). Anderson, a long-term critic of the bipolaron theory, expressed satisfaction: "It had worried me that two theorists as competent as Ranninger and Chakraverty kept on with some support for the bipolaron theory of high-T_c [when] the rest of the serious many-body community had long since rejected it." He continues: "I rather like physicists to express these kinds of sociological and methodological ideas, when the editors let them get away with it, and when it is done as eloquently as this". Campuzano added that he found the paper "almost charming" and that the original authors of the polaron theory had shown "a wonderful sense of humour" in pointing out that their own theory was wrong. "Would science not be a much more pleasant enterprise if more of us were willing to admit our *faux pas*?" he says. Campuzano has said it well. In the meantime, I add, perhaps some healthy pluralism would also do some good.

APPENDIX – Sample Interview

Here I list the physicists I have interviewed as part of my research and I the transcripts of a sample interview (Steven Kivelson).

INTERVIEWS:

Bob Laughlin, Interviewed at Stanford University on April 28, 2008

Chandra Varma interviewed first in Dresden on July 12, 2006 and a second time at Irvine University on April 19, 2008

Brian Maple, interviewed at University of California San Diego May 15 2009

Piers Coleman interviewed first at Rutgers University on September 22, 2008 and a second time in London at the IOP on October 17, 2008

Elihu Abrahams interviewed at Rutgers University on September 22, 2008

Phillip Warren Anderson interviewed at Dresden on July 11, 2006

Roberto Arrigoni interviewed at Dresden on July 14, 2006

Pavol Banacky interviewed at Dresden on July 10, 2006

H. F. Braun interviewed at Dresden on July 13, 2006

Robert Cava interviewed at Dresden on July 13, 2006

Evandro De Mello interviewed at Dresden on July 10, 2006

Joerg Fink interviewed at Dresden on July 15, 2006

John Dow interviewed at Dresden on July 12, 2006

Steven Kivelson interviewed at Dresden on July 15, 2006

Patrick lee interviewed at Dresden on July 12, 2006

Hsin Lin interviewed at Dresden on July 11, 2006

Thomas Maier interviewed at Dresden on July 13, 2006

Markievicz interviewed at Dresden on July 11, 2006

Harbert A. Mook interviewed at Dresden on July 12, 2006

Andrei Mourachkine interviewed at Dresden on July 14, 2006

Maurice Rice interviewed at Dresden on July 14, 2006

George Sawatzky interviewed at Dresden on July 13, 2006

Sample Interview:

Steven Kivelson is currently a professor of Physics at Stanford and at the Stanford Institute for Theoretical Physics. He received his Ph.D. from Harvard University in 1979. He was a postdoctoral fellow at the University of Pennsylvania and the Institute for Theoretical Physics at the University of California, Santa Barbara and then a professor of physics at the State University of New York, Stony Brook, and at the University of California, Los Angeles. He is a distinguished researcher on the theory of high-temperature superconductivity, the quantum Hall effect, and on conducting phases with novel patterns of broken spatial symmetries in highly correlated electronic fluids. He received an Alfred P. Sloan Foundation Fellowship, a John Simon Guggenheim Fellowship, and a Miller Fellowship. He is a fellow of the American Physical Society and the American Academy of Arts and Sciences.

Our interview was recorded in Dresden 14th of July 2006, at 5pm, at the bar in the main Plaza at the end of the 8^{th} International Conference on Materials and Mechanisms of Superconductivity and High Temperature Superconductors (9^{th}-15^{th} of July). I have edited for 'nonverbal noises'.

[Missing intro registration. I ask permission to record.
Kivelson starts by saying that if I am interested in philosophical issues about high temperature superconductivity I should look at Anderson's papers where he expresses his general ideas.]

Start of recording:

Me: …[What is] his [Anderson's] point of view?
Kivelson: He [Anderson] puts it forward, and discusses a little bit of WHY he thinks that, and.. it's very philosophically upsetting to me, because it's not the way I think of… (pause)
M: what is upsetting? In your point of view.
K: So basically they declared that, you know, they guessed the answer.. that it's a logical answer, it makes sense and.. implicitly that all these extremely insightful accomplished physicists believe it to be true, and therefore it's true. And it doesn't have the.. [we order a coffee]
So there…the point is not without ..merit. The point is that these strongly correlated systems, are very hard to understand in any sort of…
You know high temperature superconductivity is something that has been a fantasy of our fields for decades. And here it was; the understanding of conventional superconductors was one of the two or three really great triumphs of our fields, so now it was back to ???

days. So that.. it became clear that we really did need a new way of dealing with the strong correlation without that [BCS].
And.. it's not universally accepted, but there actually very rapidly emerged the broad consensus that that statement was true. But what you do with that statement wasn't clear, so.. it wasn't clear anymore what an answer would be, so for instance it's a very hard problem. In the past was sort of clear what an answer meant, you know, you did your band structure calculations, you started through Schrödinger's equation, you worked hard enough, you got an answer and if somebody worked harder and got an answer that was quantitatively closer to experiments probably they had done a better job.
Here… it was sort of obvious you weren't going to be able to solve the problem in microscopic detail, it was clear you weren't going be able to really get quantitative answers, there was going to be some phenomenological aspect, so… it wasn't clear, and there was broad disagreement, on even the idea of what an answer would look like. What a theory Means. What a successful theory means. I have strong feelings about that, but my strong feelings are different from lots of other people. So it's like, you know, very much like.. you know, I re-read Thomas Kuhn's book on….. (pause)
M: The structure of scientific revolutions
K: yeah, and you know it really is describing, you know.. (laughing) well, whether it's going to lead to a revolution, you can't tell that until afterwards; but its description of the breakdown of normal science before the revolution, REALLY sounds like a description of this field.
And where …. much of the bitterness of the fighting is because, you're not agreeing on how to formulate the problem and therefore , if this formulation is the right one, then, nothing about this formulation is useful, it's not that, you know.. little pieces of it can .. (pause)
M: OK..
K: uhm.. so that's made for an unpleasant sociology, but the flipside of it is it's lead to incredible innovations in.. in everything. It's been an amazingly productive field, it has driven people to develop experimental new techniques at a rate just unprecedented.. to.. develop new theoretical methods and theoretical perspectives .. you know, there's no

doubt that the field accomplished a tremendous amount. But it's not obvious it has accomplished what it "set out to" (laughs)

M: OK. So.. what about your view? You said that you don't agree on what is a theory , what are we leading to, where will we arrive at..
K: Yeah.. so, uhm.. … so, I mean, like everybody I sort of floundered around, and tried different things.. I come from a.. more statistical mechanics, quantum field theory background; so, my natural inclination, and it's an inclination that is shared by a lot of people who worked in the field, is to look for universal long wavelength low energy phenomena, things that one can derive .. with.. a certain amount of mathematical rigour, that are insensitive to details; OK? so that's where I come from. And there's been a lot of interesting work on that. On the other hand, that's not acceptable. The reason that it's not acceptable is that the field for instance is called high temperature superconductivity. You CARE about the magnitude of the superconducting transition; now that's a non universal property, it depends on microscopic details, it's something that you can never understand from the field theory point of view; so.. so on the one hand I had to abandon what is the natural framework in which I like to think about things, framework informed by the theory of critical phenomena , for instance; on the other hand.. I.. haven't really abandoned the idea that theoretical physics has an integrity of its own and that ..and the first and foremost job of a theoretical physicist is to solve the stated problem in a way that can be demonstrated with a certain degree of rigour, I mean.. rigour is a slippery word, what physicists call 'rigour' and what mathematicians call 'rigour' are two different things, but, you know, something that depended on the basis of a small parameter or of an exact solution. So there is a little bit of incompatibility between my commitment on understanding factual numbers in the problem which require coming to grips with the microscopic complexity of the problem.. and with my desire to look at things that one can … control theoretically. (pause) And so, what it seems to me is the only things left to do, for me, I don't insist that this is the way everyone should approach the problem, is to .. Abstract from the problem …(pause).. pieces of the physics that I consider central, simplify the problem to a place where I can solve it, in a sense, and then try to learn qualitative lessons from those solutions, such as … what aspects, as I change

certain aspects of the model problem, what aspects cause the transition temperature to go up, OK? And then I am prepared to discuss as a second step, not as a scientist but as a lawyer , the evidence that this piece of physics which I claim I know solidly in some model, whether or not this applies to real materials. So there's sort of a.. in my mind, there should be a two stage attack of the problem: one based in really solid theoretical results and then, the other based on more qualitative arguments, and .. actually maybe even at serial of arguments where I say "Look at how much of the phenomena I can understand in a natural way, based on this viewpoint" and somebody else will say "Well, but what about this, this is a problem and I can understand it better with this point of view". I think that's a productive way of ..analysing.. the connection between the theory and the real phenomena.. but I, really like the theory to be able to stand on its own, in its own … domain.

M: Right..

K. Does that make any sense? (laughing)

M: Yes. .. so the idea of what would be the solution, or the theory that we will get at (K: right) .. do you, do you believe in a SINGLE mechanism? Sometimes when you hear all these incredibly huge number of experiments, and so many different points of view and techniques that have been developed, there are so many ways to tackle the problem from different angles.. and everybody is "kind of right" (in their own point of view), it makes sense, they extract what they think is relevant, what is crucial to the problem, they judge and evaluate it, and they demonstrate from some experimental data that "yes, that is indeed important!". And there are so many important points! You think [the final solution] could be a synergy of those crucial point (and mechanisms) or that we are missing the real single mechanism ?

K: OK. So.. look that's a point of religious conviction. But, I guess I do think that there is an underlying essential piece of Physics, largely that's based on .. maybe aesthetics, and (smiling) wishful thinking (pause)

M: Induction? from the history of science..

K. well… there are problems that have been around for a long time , and that just haven't been solved, and (smiling) maybe the reason why they haven't been solved is 'cos they don't have one simple explanation. But you know, the things that make me somewhat

confident of that, is that certain of the central features of the phenomena, for instance the high transition temperature and something about how the transition temperature varies with doping and so on, seem to be very robust, in many different materials they all have [sic, 'all' refers only to cuprates] at the core these copper oxide planes but they have lots of other wrinkles and flourishes there, different crystallographic structures… different phonons , they're different in all sort of things.. and yet the basic phenomena looks the same. [end of second part of registration]s
[..] an essential feature; and as long as you have that you have high temperature Superconductivity.. and then on the other end the cuprates are very special materials so, it's not something that it's completely generic to solid, there clearly IS something that's very special about the cuprates that you need so I think .. that that's at least .. reason for optimism.
On the other hand, coming along with that, it is clear although there are many interesting phenomena in the cuprates, it's not clear.. I mean, it's clear that most of them aren't part of that essential problem. The trick is (laughing) figuring out which of those are.
M: Right. Why do you think, If you think so, that HTS is special
K: What do you mean it's special.. it just occurs in cuprates.
M. No, no. As a field, a field of research. Is it showing a peculiar aspect of scientific research?. and how this community works together.. the sociology of it.. is it very different?
K: I (pause) I don't think it is very different. It IS more extreme because there are so many people and you care so much about it but.. it's not.. well.. OK, I don't.. I only really know condensed matter physics, but within subfields of condensed matter physics I don't think it's .. it maybe.. it may be the nastiest, but it's not much nastier than the next nastiest (laughing) subfield. Uhm.. (pause) well, no, that's not completely true. There are areas that I think are uninteresting. Where the basic physics is understood and it has been working out lots of things and those tend to be much less contentious fields, and I think it's for obvious reasons.
M: Where you don't have many competitors?
K: And also, you know.. people battled in heavy fermions, they battled in .. metal insulator transitions.. they battled in.. there's still fundamental controversies about what's

going on and so people fight with each other. High Tc is bigger, people care more so they fight a little worse

M: Do you think there is room for even political dissent? By which I mean, several people (here) addressed that there is a higher layer of big figures, big names, several nobel prizes working on this, (K: yeah, yeah) and some people said that that slowed down the beginning.. or what Varma calls the "medieval book burning", do you think there is some kind of fear.. or I don't know..

K: OH it's much worse than that. So there was, in the beginning, there were a lot of young bright people that went into the field; And they were all killed. And actually one of the really exciting things about this meeting was that I didn't recognize most of the people. And that means a lot of new young people are going into it and that's an extremely good sign for the field. Well that was true for a long time; this meeting.. well, you know it's one meeting, it's not statistically relevant but..

M: Sure. that's one concern. When I hear my colleagues they are very frustrated.

K: Yeah. I mean, look, in the first few years the young people that came in were absolutely uniformly murdered. I don't think one of them survived. So, for years, for more years than I can count I was the youngest person at almost every meeting I went to. And I was already.. no, I was an associate professor, but I was tenured when the field begun

M: (smiling) So you had chances to survive

K: right. I mean, my job wasn't at stake. And actually I also, after a few years I got so upset about being beaten up so badly in the field that I worked for a while on Quantum Hall Effect again. I mean, I'm glad I did, it's an extraordinarily interesting field

M.. it turned out to be useful..

K: right, but .. you know.. it's not quite true Su Chen Zhang.. and Cho Gon Wang .. Su Chen was.,. I don't know if you talked to him, he was here, (M: No) he was a student of mine, and he was a post doc of Schrieffer and he went into the field and, got the crap beat out at him, as a junior person, he's very tough guy and.. he stuck it at and he's doing fine (laughing) but .. Oh, I am talking in theory [among theorists], I think it was a little better in experiments. there were just quarrels (???) of bright young people that were driven out of the field. And that was certainly bad.

I think it's less bad now just because .. I don't think.. See, there were few senior people who… commended so much respect.. of the field.. Rightly so for the accomplishments they made, but.. who, I think, didn't behave the optimal way, but.. as a consequence people have become less enamoured of authority. And so I don't think it's as bad now.
M so what is the thing that shifted, or made things shift?
K: Just that.. it wasn't solved so it was obvious to everybody that you needed new people bringing in new ideas.
M: so.. from an outsider point of view.. if young people didn't have a particularly active role at the beginning.. there are two ways that one could naively think of the problem. Either that there was an initial lack of dissent, that there was dissent but it was totally suppressed.
K: it was even different. There were.. you know.. principalities.. and they didn't talk to each other they enforced political homogeneity in their principalities . There was always dissent in the field. A Bitter dissent. But there was no individual able to purse a.. or better, young individuals.
M: OK, and then? What changed? If it changed..
K: yeah, well.. so I think that.. the fact that the problem wasn't solved.. you know at some point it becomes obvious you can't insist on "This is the only way" if you don't eventually produce the solution!
M: a lot of people are still complaining that they cannot publish radical new approaches.. that there's domineering, maybe in peer reviews
K: yeah.. it's true. You know the other thing that happened is that that doesn't really matter (smiling) I mean.. with electronic bulletin boards.. I mean, I don't read ANY peer reviewed journal. Of course, I fight referees like everybody else (laughing) but at the end of the day it doesn't make a bit of a difference (laughing).
M: One can find his own way to be heard.
K: yeah . you know you do have to be.. also you have to be self confident in this field because there's no doubt that for any strong idea you're going to take a lot of heat for it.. even not so young people! (laughs)
M: the problem maybe remains at a low level, when you need to be sometimes associated to some figures to have a position.. to have funding..

K: right. So.. I did learn some very upsetting things at this meeting. I think there is almost nobody.. in universities in the United States that's explicitly funded to do High Tc research. All the people I THOUGHT were funded to do High Tc research are actually funded to do other things and they're stealing money from that. So.. that's a pretty dire situation.

M: but is it because of some recent cuts of fundings or..

K: No. it's not a recent development. I think.. that.. I mean, I don't know at the very beginning; I was at least young enough then that I didn't think about broad societal issues. I was just aware of who was pummelling me. But .. (laughs).. it's not a new.. it hasn't appeared suddenly. This has been, maybe a growing problem but .. it's been may years ??. So there is a perception form outside high Tc 'cos they see so many people working on it.. that it's getting all these fundings, it's so easy for people in this field, there's a lot of possibility ; from the outside. And then of course within the inside, ?? been saying people like to fight with each other so there is sort of NO support for any individual research, almost no matter how good.

M: One of the things that Anderson, for example, told me is that .. he quoted Benjamin Franklin .. I don't remember it exactly but the idea is that "it is difficult to evaluate the understanding of people when the career of those people depends on not understanding". And he uses this to say that actually the problem has been solved (and that he is right) but what happens in the field is that people keep going with this more or less fake or useless dispute about High tc theory because they need to justify all the funding, all the machinery and the position of a lot of persons (even though they know he is right)..,. a very radical point of view.

K: Right. So that's actually changing the names of the players that very rightly helped you. I think it's completely wrong. I don't really believe that anybody in our field is that scheming. I think you can accuse people of being good at deceiving themselves but I think that Everybody in our field is really trying hard to be honest to advance knowledge in the best way they can .. I mean, I must say that, you know, when I get together with my friends over lunch I say exactly the same thing, you know, "Why is this person purposely not understanding" (smiling) but I don't actually think it's true. (we laugh)

M: Alright. So when you have two or more competing theories.. this maybe a general question, for science in general, but how does it in High Tc in particular.. On which grounds one should choose? Of course there's empirical evidence and adequacy to explain phenomena .. but it seems that the theories that we have so far are very hard to.. they find many difficulties in expressing clear predictions.. and the complexity of the materials involved.. (he interrupts)
K: Right. So that's where.. I don't think I articulated it very .. So one thing that I think is that the correct theory must prove itself mathematically. It must prove that, you know, if you think that the Hubbard model has the essential physics than, by gosh, you have to show that what you say it's true about the Hubbard model before you tell us that that is true about the cuprates. OK? That's a very radical point of view, that's a point of view that is widely not accepted, so I don't speak for the field but.. I think that would limit the number of theories by a lot (smiling).
M:Yeah, that's why I was asking this. Because.. do you think you need justification for this or is it as you said before a religious belief?
K: OK. I guess I feel comfortable.. talking for.. what.. I would find convincing. I'm not sufficiently sure that I'm right that my proposal is established as a criterion for the field. So, you know, most work these days is done on mean field approximation and variational ansatz. That's one very sharp .. For me, the criterion of mathematical demonstration that its correct predictions about a model is serious and that seriously limit the number of allowable theories. Then, in terms of what's going to establish, what's the right thing for the experiment.. I mean you are right that any theory that is half-way honest has a very hard time actually making very precise predictions. Especially since the things that we care about like the Tc are quantitative … You know at a phase transition you have .. critical exponents and, divergences of properties ; you know that's really a startling and .. either you got it right or you got it wrong.. I mean there is no.. do you get the cross over or the resistivity right.. or it goes like this.. you know.. there's not all that many features in the curve.. it's not that easy to tell whether.. even if you agree with experiments whether that means anything. So it's hard. I don't .. .. people talk of the idea of a smoking gun experiment and my guess is that it's not going to go that way. It is going to be that .., with time.. a lot of the things that we see about these materials will become easier and

easier to understand, looking at it form the point of view of the theory that is ultimately considered right .. it's not that it will contradict any other theory but you're going to have to, you know, explain it by adding this correction and .. modifying that .. so that there will be some sense of naturalness in …a combination of many things that will resolve the discussion.

K: And so you think that is compatible with your desired mathematical cogency..

K: I do. I think so. My.. insistence on control theory makes it even harder to make quantitative predictions. So it does make the comparison with experiments harder. But I still think that there are so many aspects of this problem that are being propped that.. that it's unconceivable to me that it won't eventually triangulate it on the right theory. But , (smiling) my future's happiness depends on this, this is what I do professionally so .. (we laugh)

M: Do you see a mistake that someone is doing, looking in the wrong way, in the wrong side of things?

K: so.. OK, so look. There are things people are looking at that I think have a very low probability of turning out to be right. I have a different view on this than Phil does. I agree with Phil that phonons are extremely unlikely to be an interesting part of the problem. Nonetheless I think it's very important that the people that were advocating phonons to push the idea as far as they can. ????. And second place if they do it honestly, and seriously.. and if I'm right, then they will.. the evidence for that will make them stronger. I think it's a mistake to try to suppress them. Which I think has been done at various stages.

M: at the beginning of the evolution of new theories, on a shift of paradigm, you can have at that stage a lot of theories that compete with each other, they all disagree, they dissent.. and then suddenly there's a shift .. of point of view.. and then a Big jump.. and you get to a new theory. Do you think that will happen or that we already have in all this magma of different points of view, something that is overshadowed by the competition and the dissent in the field, or we still miss a jump?

K: you know, theoretical physicists must be optimist. They have to think.. that you know, I can say things that threw a bit attention somehow. (laughing) There's a lot to be worked out. I think it's quite radical but.. I do think we have the basic understanding in the end.

M: what would be the evidence that if one would show it to you, you would be led to abandon your theory?
K: (pause) I think the main thing would be.. uhm.. a compendium of evidence that I'm (??) to apologise more and more. There are different aspects of the approach.. so.. for instance.. if one found a material with similar structure than cuprates, but no magnetism .. or similar phonons cuprates but no magnetism.. and it had high temperature superconductivity that would be pretty strong piece of evidence that the magnetism wasn't the essential piece of the problem. Uhm.. I'd be very surprised! But .. it would certainly ..
M: Bad news!
K: Yeah. I'm very concerned about theoretical consistency, so it seems to me that within five or ten years we will know the solution of the Hubbard model. First place, numerical methods are improving, and second place it may be able to be simulated by cold atoms systems .. so one way or the other I guess we will know whether the Hubbard model in its very and various application of the Hubbard model.. is right. [end of third part]
If, as I think, the uniform Hubbard model is not enough but then a suitable deformation of it .. you can make a suitable HTc superconductor .. I think that will establish it. If it's.,... none of these are high T superconductors again.. it will force me to be very.. seriously rethink my position.
M: As one physicist is struggling, coping with one problem, they are still struggling with their own understanding of the problem, they are really trying to make something more clear and they can't, .. Has it happened to you.. that you get to a point in which you have an "eureka" and you really feel you have the understanding of the thing .. you feel a very strong understanding and you really think you are grasping it… and then you're wrong?
K: Yeah. (Laughing) I've done it
M: And in that case… one may get strong and stops listening to others, 'cos he's so convinced…a lot of people could have many illusions or conceptions of understanding the true nature of things..

K. yeah, well that happened to me a lot. And, you know.. it's painful (laughs). I recently read a biography of Benjamin Franklin, that made me think a lot, and I'll tell you something that impressed me. He had slaves as a young man , not many you know.. two or something like that, and completely accepted the idea that blacks were inferior to whites. But, at some point he got.. in his mind that it was inefficient to not educate blacks, 'cos they would have been more useful if they were educated ; so he actually founded the first school for black children. And.. he.. visited the school at some stage, and saw that black students were learning as well as white students and he understood that this meant that his assumptions were wrong and became a very committed abolitionist at that stage. I think that's amazing! To be able to understand that the evidence proves to you that your prejudice was wrong on something .. and.. so, it's possible!

M: And you are confident in this capacity in science in general? To do this self-critique..

K: It's not a community of saints , we don't do as well as we should but in the end I think.. more or less.. people are.. when the evidence is presented to them.. most people in our community are willing and able to accept it. Often what they do.. actually… the more common response .. is to re-write history and misremember the fact that you.. advocated X and in your own personal history you always advocated Y.. (laughing)

M: From the interview with Fink [Chair of the Dresden conference], the only different interview, since we talked of more organisational stuff, he said that he changed the mechanism for the committee policy and the organisation from the previous conferences; and he said he found that in Rio de Janeiro there were polemics about under-representation of an opposing side, and that caused an almost boycott..

K: I didn't go to Rio.

M: But in this conference instead of having the program committee .. being so important for who we are going to invite who's going to have the main talks, how we put them together .. since he witnessed in the last conference a strong domination that was narrowing down the variety of opinions.. So in this conference he wrote emails to the international advisors or committee to ask their own suggestions, collected them and formed a rank.. but then it was his local committee that decided, only then, who to invite.. this is quite controversial, and mirrors the problem of these political commitments ..

K: I don't think they did for Rio either, I mean .. they may have met but most of the program committee didn't come. I don't know exactly how the decision was made but a friend of mine was in the program committee and sent lots of recommendations and .. so I complained bitterly about.. actually he was also on the program committee this time.. but then you complain bitterly about other stuff (laughs) .. I don't know, so.. you're presumably referring to the under-representation of phonons.. I actually more or less agree with the party line of people that were complaining about this and I also think that people advocating phonons here was a really good idea.

In the early days of High Tc I got some work done that I was really really proud of .. and.. I got the crap beat out of me.
M: What do you mean by that, practically what happens?
K: Well I had problems publishing things.. (laughs) .. I'm used to that.. that happens in every subfield.. but, well.. you know personal attacks form people I revered .. that was the most painful thing I guess. And when I started coming back into High Tc, I did it in collaboration with Dick Emery who was a senior physicist at Brookehaven, but.. the fact that he was senior wasn't the important point. The important point was that.. I had close collaborators that.. you know.. we could buck each other up and .. Dick died in 2000 and .. but I didn't start in collaborating really closely with another person till ??? so I am very consciously not a lone contributor .. I collaborate with people whose skills and talent complement mine and.. they're very good intellectual piece of work (??) but at the core there is an emotional reason for it , it is very much easier to absorb equanimity about this if you, you know there's somebody on your team (laughs)
M: Which connects to the thing you said that it is more difficult to be a single proponent or dissenter.
K: Yeah, it's an emotional difficulty. Certainly at my stage there is zero practical consequences of being attacked

[END of time. Extra question:]

M: So you believe we are slowly converging.. how do you see your future?
K: Scalapino said something interesting today. That the number of serious theories has not continued to grow. In fact I think honestly it has cranked (??) a little. There's still a lot of competing ideas but I think that there are many of them that are loosing steam 'cos they're not being supported. Again that's not .. it's not that there is one smoking gun that had killed them .. but..
Yeah so I don't know how you measure theories and others as derivations but for some time they have grown very rapidly and now I think they are somehow doing like this. (gestures a curve slightly tilted downwards)

[end of interview]

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