## CHAPTER 7

# *The role of models in the application of scientific theories: epistemological implications*

*Mauricio Suárez*

## 7.1 INTRODUCTION

The theme of this book is reflected in the slogan 'scientific models *mediate* between theory and the real world'. It is a theme with, at least, two aspects. One aspect is methodological. Model building is a pervasive feature of the methodology (or methodologies) employed by scientists to arrive at theoretical representations of real systems, and to manipulate reality. Many of the contributors to this book engage with the methodological issues, and they all agree that the activity of model building is central to scientific practice. The methodological implications of the *slogan* are clear: much of scientific practice, perhaps the totality of it, would be impossible without models.

Another aspect of the theme relates to issues such as the nature of explanation, the form of scientific confirmation and the debate over scientific realism. These are traditional philosophical issues, and in this paper I concentrate on one of them: models provide theories with genuine empirical content, by 'filling in' the abstract descriptions afforded by theory, hence making it possible to apply theories to natural phenomena. How do models perform this role? What are the consequences for the realism issue? The focus of this paper is on the implications of models for the epistemology of scientific knowledge.

## 7.2 MODELS AS MEDIATORS

There are many kinds of models in science. In this paper I focus on one of them: *mediating models*. First, in this section, I introduce the notion of a mediating model, and I briefly outline some of its main features. In the remaining sections I make the notion more precise by considering the key role that mediating models play in the application of scientific theories, and the implications of mediating models for the epistemology of science.

Mediating models have been recently discussed by a number of authors. Adam Morton[1] has referred to them as the providers of physical insight; Margaret Morrison[2] has studied and discussed some of their properties carefully; and among historians of science, Norton Wise[3] has unearthed some of the mediating models and instruments that operated in Enlightenment France.

### *7.2.1 Features of mediating models*

Mediating models always stand between theory and the physical world. Their main function is to enable us to apply scientific theory to natural phenomena. A mediating model often involves a novel conception of a particular physical phenomenon that facilitates the application of some established physical theory to such phenomenon. Morrison has identified three main features. First, mediating models are not derivable from theory. In a very specific sense the construction of these models is not *theory-driven*; I will emphasise this feature later on in this paper. Secondly, these models are not necessitated by the empirical data either (although they may be consistent with the data and they can be suggested by the phenomena). In contrast to a data-model which is determined by the data together with established statistical techniques, a mediating model '*is more than simply a phenomenological classification constructed as a convenient way of representing [data]*' (Morrison forthcoming b). In other words, mediating models typically involve substantial theoretical and conceptual assumptions. Finally mediating models have a very significant property: they can replace physical systems as the central objects of scientific inquiry. Morrison (forthcoming b) writes:

> Not only do models function in their own right by providing solutions to and explanations of particular problems and processes, but in some cases they even supplant the physical system they were designed to represent and become the primary object of inquiry. In other words, investigation proceeds on the basis of the model and its structural constraints rather than the model being developed piecemeal in response to empirical data or phenomena.

Research for this paper was partly carried out at the Centre for the Philosophy of Natural and Social Sciences, London School of Economics. I thank everyone involved in the Research Group in Models in Physics and Economics for their encouragement.

---

[1] Morton (1993); also in conversation, Bristol, May 1997.

[2] Morrison (1998) and (forthcoming a and b).

[3] Wise (1993).

This is an essential feature of mediating models; it distinguishes this type of model from other closely related types, such as for instance Heinz Post's *floating models*. As reported by Redhead (1980), floating models may also satisfy the first two features ascribed to mediating models. Redhead (1980, 158) describes a floating model as

> a model which is disconnected from a fundamental theory $T$ by a computation gap in the sense that we cannot justify mathematically the validity of the approximations being made but which also fails to match experiment with its own (model) predictions. So it is disconnected from the fundamental theory and the empirical facts. In Post's graphic terminology the model 'floats' at both ends. It has, in this sense, no theoretical or empirical support.

Post's parody of a floating model was an example he called the Farm Gate Contraction. Redhead reports this example as follows:

> A farmer investigates the relation between the length of the diagonal strut and the length of the rails and stiles of a farm gate. Although he is familiar with Euclid the derivation of Pythagoras's theorem is utterly beyond his deductive powers. So he invents a model theory, a linear one, in which the lengths are related by $l = x + y$ instead of $l = \sqrt{x^2 + y^2}$. Now [the model] has many properties analogous to [the theory] for $x = 0$ or $y = 0$ it gives correct values for $l$ and $l$ increases monotonically with $x$ or $y$ in the model as in the correct theory. But detailed measurement shows that [the model] is false. So the farmer now introduces a new effect, the Farm Gate Contraction, to explain the mismatch between the predictions of the model and the experimental results.

The Farm Gate Contraction is a correction to a floating model. The model, even when corrected in this way, is certainly not *required* by the data, as is shown by the fact that there are alternative models that fit the data just as well (the 'correct' theory is one of them); and it is not supported by any fundamental theory as it is only an inspired (although ultimately mistaken) initial guess. Floating models are not derivable from either theory or empirical data. In that sense a mediating model is a kind of floating model.

However a mediating model has a further essential feature, one that is not necessary for a floating model. While a floating model may convey no new knowledge at all, a mediating model mediates between high level theory and the world by conveying some *particular* or *local* knowledge specific to the effect or phenomenon that is being modelled. This is why the model itself becomes the active focus of scientific research. While a floating model is typically only a computational tool, a mediating model is a carrier of specific, or 'local' knowledge. Morrison (forthcoming b) writes:

> It is exactly in these kinds of cases, where the model takes on a life of its own, that its true role as a mediator becomes apparent. Because investigation centres on the model rather than nature itself its representative role is enhanced to the point where the model serves as a source of mediated knowledge rather than as simply a mediator between high level theory and the world.

Hence this third feature, the capacity a model may have to replace the phenomenon itself as the focus of scientific research, is an essential feature of mediating models. It distinguishes mediating models from the far larger class of floating models. In this chapter I develop a further feature of mediating models, which is essential for a full understanding of the role that these models play in the application of scientific theories. Mediating models will often fix the criteria that we use to refine our theoretical descriptions of a phenomenon. These criteria are required to apply theory successfully to the world. Before discussing this fourth feature of mediating models it may be worth emphasising the differences with some of the types of model that are commonly discussed in the literature.

### *7.2.2 Mediating models in the philosophy of science*

A very distinguished, although perhaps languishing, philosophical tradition equates models with interpretations of theory. This tradition assimilates the distinction between scientific theories and scientific models to the syntax/semantics distinction in linguistics. The theory is a purely syntactical entity, while the models provide us with the semantics of the scientific discourse. The relation between the models and the theory is one of satisfaction: the model must make the theory's axioms true.

It is difficult to see how models are to literally 'mediate between theory and the world' if the view of models as providing the semantics of theories is correct. If models are interpretations, or partial interpretations, of theories they are in a sense supererogatory on theory. A theory will define an elementary class of models; hence it will greatly restrict the class of permitted models. An inconsistent theory, for instance, restricts the class of permitted models to the empty set. However, it is a presupposition of the notion of models as mediators that there are three distinct objects (theories, models, and the world) and that they are ordered with the theory at the most abstract end, the world at the opposite end, and the model as the interface between the two. Moreover the model conveys specific physical knowledge. The view of models as interpretations of theories allows for a trichotomy between theory, model and world but it seems to order these objects the wrong way around, with models at the most abstract end, and theories at the interface (as model/theory/world rather than as theory/model/world). Moreover, it implies that models do not convey any significant novel physical information that is not already encoded in theories. Surely this is partly the reason why proponents of this view have so often attempted to construe the relation of confirmation as a purely syntactical connection between a theory, on the one hand, and evidence, on the other.

It is possible on the syntactic view to see the world itself as a possible model of a theory. The theory is a set of axioms in some formal system, and it implicitly defines an elementary class of models. We may then say that a theory is true if it has the world as one of its models, and false if the world is not among its models. In so far as the world itself is to be a model, the distinction between model and the world collapses, and we are left with a dichotomy theory/world. So on this view, models mediate between the theory and the world only in the sense that the set of permitted models of a theory can be said to include the world itself. The activity of model building reduces, on this account, to investigating ways the world would have to be if some specific scientific theory was true. This assumes, once more, that the totality of scientific knowledge about the world is encoded in theories.

There is also, of course, the semantic conception of theories advocated by Suppes, van Fraassen and others. Here the distinction between theory and model collapses as, according to the semantic view, theories *are* models – they are really nothing but collections of models. On this view there is a hierarchical structure of models, from low-level data-models to high-level theoretical models. So the contrast between theories and models disappears. Besides, on the semantic view of theories the domain of application of a scientific theory is assimilated to its domain of empirical adequacy.[4] But mediating models play a key role in the application of theories, precisely in cases in which the theory's domain of application does not coincide with its domain of empirical adequacy. Hence the semantic view lacks the resources to provide us with an understanding of how, in practice, models mediate between theory and the world.

---

[4] This is argued in detail in my PhD thesis (Suárez 1997).

## 7.3 THEORY-APPLICATION: THE ROLE OF MODELS

In this section I describe a specific proposal for theory-application that involves models as idealisations. This proposal, essentially due to Ernan McMullin, is intended to go further than the traditional accounts of scientific theorising, by placing the activity of model-building at the very core of scientific practice. I argue that, despite its intention, McMullin's proposal effectively dispenses with the need for models as mediators because it invariably construes models as approximations to theories. In section 7.4 I try to illuminate and explicate this practical role of models as mediators by using an example from the history of superconductivity. In section 7.5 I discuss the epistemological implications.

### *7.3.1 Forms of idealisation*

How does scientific theory get applied to the world? Ernan McMullin (1985) has proposed a realist account of theory-application. Theoretical descriptions, argues McMullin, are always idealised; they apply only under very special circumstances, often not realisable in practice. But the idealisation inherent in theory is not epistemologically problematic. Although theoretical descriptions are often not *absolutely* true or false, they are *approximately* true or false.

McMullin finds support for this view in Galileo's idealisation techniques. In *The New Sciences* Salviati, Galileo's alter ego, argues against the Aristotelian views of some of Galileo's contemporaries, personified mainly in the character of Simplicio. The discussion centres around the techniques of approximation required to apply theory to concrete problem situations and to validate the theoretical claims of Galilean mechanics. Two examples are repeatedly used: parabolic trajectories of projectiles, and motion of rolling objects on inclined planes. Consider the latter. Galileo's claim is of course that the motion of a perfectly symmetrical sphere under the earth's gravitational pull on a frictionless plane in a vacuum follows a very strict mechanical law. But any real plane will exhibit friction, any real object is bound to be only imperfectly spherical, and in any actual experiment there is bound to be dampening due to the presence of air. To establish his mechanical conclusions on the basis of actual experiments, Galileo has to claim that the imperfections can be accounted for, and that there is a well established and unique method of introducing corrections into theory to account for 'impediments', the imperfections of nature.

In order to show that there is indeed such a method, Galileo (and McMullin) need to appeal to the notion of approximation. There are, broadly speaking, two methods for approximating theory to the world. One is the approximation of the theory to the problem situation brought about by introducing corrections into the theoretical description – the theory is refined to bring it closer to the problem-situation. The other is the approximation of the problem-situation to the theory by means of simplifications of the problem-situation itself. In the latter case the theory is left untouched, while the problem-situation is altered; in the former case the converse is true: the problem-situation is left untouched, while the theoretical description is corrected.

Let me first consider the former kind of approximation whereby the theoretical description is refined to bring it closer to the problem-situation. This is a form of approximation towards the real case: the corrections introduced into the theoretical description are intended to account for the imperfections that occur in the problem-situation. The same method can be reversed (this is not yet the second method of approximation) by *subtracting*, rather than adding, the required corrections. We may call this an *idealisation*; for the result of such subtraction is of course a more, rather than less, idealised description of the problem-situation. The important feature of this idealisation is that the subtraction of corrections is performed on the theoretical construction, while the description of the problem-situation is left entirely unaffected. For this reason McMullin (1985, 256) calls the first form of approximation *construct* idealisation.

The second method of approximation brings the problem-situation closer to theory. We idealise the description of the problem-situation, while leaving the theoretical construction unaffected. McMullin calls this *causal idealisation* because the description of the causes present in the problem-situation is altered to bring the description into the domain of the theory. In the practice of physics this process can come in either of two forms. It can come first in the form of conceptual redescriptions of the problem-situation, performed only in thought, and not in reality. In such 'thought-experiments' interfering causes are idealised away and the result is a simplified description of the problem-situation. Secondly, there is also the possibility of physical 'shielding' of the experimental apparatus, which will involve changes in the actual experimental set-up.

Such changes are designed to minimise the influence of interfering causes, or to block such influences out altogether. It is perhaps instructive to quote Galileo in full:

> We are trying to investigate what would happen to moveables very diverse in weight, in a medium quite devoid of resistance, so that the whole difference of speed existing between these moveables would have to be referred to inequality of weight alone. Hence just one space entirely void of air – and of every other body, however thin and yielding – would be suitable for showing us sensibly that which we seek. Since we lack such a space, let us (instead) observe what happens in the thinnest and least resistant media, comparing this with what happens in others less thin and more resistant. If we find in fact that moveables of different weight differ less and less in speed as they are situated in more and more yielding media, and that finally, despite extreme difference of weight, their diversity of speed in the most tenuous medium of all (though not void) is found to be very small and almost unobservable, then it seems to me that we may believe, by a highly probable guess, that in the void all speeds would be entirely equal. (quoted in McMullin 1985, 267)

It is uncertain whether Galileo actually performed any of these experiments. If he did, he would certainly have needed to use a technique of 'shielding' to minimise the influence of interfering causes. If, on the other hand, he did not actually perform the experiments then in this passage he is describing a series of *thought-experiments* that gradually minimise the effects of interfering causes – in the mind, of course, not in reality. The dynamics of moveables in the void that he concludes will exhibit equal speeds is in either case a *causal* idealisation. Starting with a concrete problem-situation (i.e. the motion of an object in the earth's atmosphere) Galileo constructs a set of simpler problem-situations. If relations between quantities measurable in these gradually simpler thought experiments converge to a law we can then enunciate the law for the ideal (simplest) case. The resulting law is a *causal* idealisation, because the simplifications correspond to missing causes in the problem-situation.

McMullin summarises the main features of each form of idealisation concisely as follows:

> We have seen that idealization in this context takes on two main forms. In construct idealization, the models on which theoretical understanding is built are deliberately fashioned so as to leave aside part of the complexity of the concrete order. In causal idealization the physical world itself is consciously simplified; an artificial ('experimental') context is constructed within which questions about law-like correlations between physical variables can be unambiguously answered. Causal idealization, instead of being carried out

experimentally, can also be performed in thought, when we focus on the single causal line in abstraction from others and ask 'what would happen if'. (1985, 273)

In this chapter I focus only on *construct idealisation*, the kind of idealisation whereby simplifications are worked out on the theoretical description, rather than on the problem-situation. This is because I believe that every case of theory-application will involve, in practice, at least some degree of construct idealisation. *Construct* idealisation requires no thought-experiments, nor does it require tampering with the real experimental situation. Only one problem-situation, namely the real case, is entertained. It is the theoretical description that gets modified by introducing correction factors that represent 'impediments', the special circumstances that make up the particular problem-situation. In other words, in construct idealisation, the theoretical description is refined gradually to make it applicable to the problemsituation.

In actual practice we look for approximations to the theory that can be applied to a particular problem-situation. Redhead (1980) refers to these approximations as *impoverishment* models. The theoretical description may be very complicated: there may be no analytic solutions to the theoretical equations. How then can we derive the correct impoverishment model? How can we choose among all possible approximations the very one that accurately represents the behaviour of the system? The important point, that I shall now stress, is that the theory itself must contain the information required to select the correct approximation if the approximation in question is to count as a *de-idealisation* of theory.

### *7.3.2 Idealisation and scientific realism*

A theory can be applied by finding a simplifying approximation to it that is adequate for the description of a phenomenon. Not all approximations, however, guarantee that the theory is confirmed by its applications. It is essential to McMullin's realism that the corrections introduced into the theoretical description should not be *ad hoc*. The corrections have to be well motivated *from the point of view of theory*. If the theory is to receive confirmation boosts from its applications, the corrections need to be not only consistent with the theory, but also if not dictated by, at least *suggested by*, the theory. If in a particular application the necessary corrections turned out to be inconsistent with the theory, the theory could be said to be disconfirmed; if the corrections were consistent with the theory, but not suggested by it, the theory would neither receive a confirmatory boost nor a disconfirmatory one. McMullin explicitly acknowledges this important point: according to the (*construct*) idealisation picture of theory application, the manipulations exerted on the theoretical description must be 'theory-driven' because the theory itself is to be truth-apt (a 'candidate for truth' in Hacking's (1982) terminology) and is to gain confirmation through its applications. If the corrections were not suggested by the theory then the resulting description would be *ad hoc* and, from the point of view of a realist epistemology, it would be unable to provide any evidence for the truth of the theory. Thus McMullin writes:

> The implications of construct idealization, both formal and material, are thus truth-bearing in a very strong sense. Theoretical laws [. . .] give an approximate fit with empirical laws reporting on observation. It is precisely this lack of perfect fit that sets in motion the processes of self-correction and imaginative extension described above [i.e. *deidealisation*]. If the model is a good one, these processes are not *ad hoc*; they are suggested by the model itself. Where the processes *are* of an *ad hoc* sort, the implication is that the model is not a good one; the uncorrect laws derived from it could then be described as 'false' or defective, even if they do give an approximate fit with empirical laws. The reason is that the model from which they derive lacks the means for self-correction which is the best testimony of its truth. (1985, 264)

In this passage McMullin is not using the term 'model' to describe a mediating model, as I do in this paper. I have taken 'mediating models' to be distinct from established theory while McMullin is here taking 'model' to stand for a theoretical description, as in the semantic view of theories. McMullin makes it clear that the corrections introduced into a theory to generate predictions in a particular physical problem-situation have to be suggested by the theory itself; otherwise, the corrections would be *ad hoc* and the resulting description, no matter how well it fitted the particular case, would not yield any confirmatory boost for the theory. If the corrections were not suggested by the theory there would be no way to account for the effects that those corrections have upon the final predictions. As McMullin notes (1985, 256) it is essential that there be '*a way of dealing with the fact that construct idealizations "depart from the truth". If this departure is appreciably large, perhaps its effect [. . .] can be estimated and allowed for.*'

By requiring that the corrections into a theoretical model be well motivated from the point of view of theory we make sure that we are always able to estimate their contribution to the final description. In other words, application must be *theory-driven* in order to provide confirmation for the theory. I shall refer to this sort of theory-driven approximation of the theory to the problem-situation that results in a refinement of the theoretical description as *construct de-idealisation*, or *deidealisation* for short, as an approximation of this kind is nothing but the converse of construct idealisation. In forming construct idealisations we idealise away, by subtracting from the description, those features of the problem-situation that are either (a) irrelevant to the theoretical description, or (b) relevant to the theoretical description, but also known to have effects that are precisely accountable for. (In the latter case construct idealisation is often used for the purpose of improving the mathematical tractability of the problem.) In either (a) or (b) a strict criterion of theoretical relevance is presupposed. It is the theory that tells us the relevant features to be idealised away, and suggests how to account for their effects. The same criterion of theoretical relevance must be in place if the converse process of 'adding back' features is to count as a meaningful *deidealisation*. The requirement that the introduction of corrections into a theoretical model be well motivated from the point of view of theory ensures that this criterion is firmly in place.

The above discussion is perhaps sufficient to make clear why the idealisation account of theory application satisfies the realist's constraints. For applications which follow the idealisation account, the theory receives confirmation boosts from the applications. The corrections that serve to generate successful applications are necessarily consistent with theory, because they are suggested by theory. They are corrections suggested by some strict relevance criterion – a criterion that is wholly and unambiguously theoretically determined. So, an application of a theory that conforms to nature provides a good reason to believe that the theory itself is true.

Let me now briefly address the sense of 'approximate truth' that is involved in the idealisation account. McMullin is not arguing that scientific theories are approximately true or false. The theory, on McMullin's view, contains its own criteria of application; so, indeed, the theory contains all theoretical descriptions of problem-situations in its domain. Hence the theory is either true (if it contains one true description of every problem-situation), or false (if it fails to do so). It is because of this that a successful deidealisation of a scientific theory to a particular problem-situation should always be taken as an indication of the theory's truth: it shows that the theory contains one true description of the problem-situation.

The realist's claim is then rather that *theoretical descriptions* of a particular problem-situation may be approximately true or false. His intuition is roughly as follows: successive approximations of a theory to a problem-situation have a degree of confirmation inversely proportional to their 'distance' from the problem-situation as measured on the 'idealisation scale'; but – for a realist – degree of confirmation is degree of truth; so 'distance in the idealisation scale' measures degree of truth. Given two representations A and B of some concrete problem-situation if A is less idealised than B then, in a very precise sense, A is *truer* than B. To pursue a Galilean example: the representation of a sphere rolling down a frictionless plane is less idealised if described in the actual atmosphere (description *A*) than if described in a vacuum (description *B*). The description in the atmosphere has to involve a measure of the dampening due to air. The realist claims that this description is *truer* than the description of the sphere in a vacuum, in a totally unobjectionable sense of the notion of objective truth. For a scientific realist, such as McMullin, Galilean idealisation provides the *model* for the notion of approximate truth.

## 7.4 PROBLEMS WITH IDEALISATION

It is always open to the opponent of realism to attack the inference from the past success of a theory to its future success, and from its pervasiveness in practice to its truth. An instrumentalist may after all have no qualms with Galilean idealisation: it is a technique of application, it is often used, and sometimes with some conviction that it carries epistemic weight, but in fact it is only a tool, and it can give no genuine warrant for belief other than the psychological comfort offered by the familiarity of its use. But here I do not attempt a general philosophical rebuttal of the realist view. This would take us one step back, in the direction of the traditional disputes concerning arguments for scientific realism – disputes that have not been settled, possibly because they could never be settled.[5]

On independent grounds the realist view will not work. The realist wants to claim that the idealisation account captures the essential features of the procedure of theory-application. I argue that the idealisation account is seriously flawed and that it can not explain the role of models

---

[5] In the philosophy of science this *quietism*, or perhaps simply 'pessimism', towards the realism/antirealism debate has been most ably defended by Arthur Fine – see chapters 7 and 8 of Fine (1986a).

in scientific practice. The inadequacy of the idealisation account stems from the fact that, in practice, theory-application does not typically follow the pattern of *deidealisation*. But the realist does not rest content with this base-level claim; in addition he claims that the idealisation account also agrees with scientific practice at an *epistemological level*. Scientists' confidence in a scientific theory typically increases on account of its many successful applications. The realist seeks support for the idealisation account also on these epistemological practices of scientists. And, indeed, on the idealisation account a theory gains confirmation through its applications, in the manner described in the previous section.

To sum up, there are two distinct claims that the realist makes on behalf of the idealisation account: first, that this account agrees with the practice of theory-application and second, that it agrees with scientific epistemology. In this chapter I contest the truth of the former claim, and I argue that the latter claim, although true, does not provide ammunition for the realist account of theory-application.

### 7.4.1 Idealisation and mediating models

I like to illustrate the idealisation account of application with a simple example in mechanics due to Giere (1988, ch. 3). The example brings out very clearly what, in my view, is the major defect in this account. Consider the derivation of the equation of the damped linear oscillator from that of the simple harmonic oscillator. The equation of the simple harmonic oscillator is:

$$m\frac{d^2x}{dt^2} = -\left(\frac{mg}{l}\right)x, \tag{7.1}$$

while the equation that describes a damped harmonic oscillator is:

$$m\frac{d^2x}{dt^2} = -\left(\frac{mg}{l}\right)x + bv. \tag{7.2}$$

The process that takes one from the theoretical description of the frictionless harmonic oscillator to the damped harmonic oscillator is a successful deidealisation in the attempt to apply classical mechanics to a real-life pendulum. The extra term $bv$ represents the dampening due to air friction that any real oscillator must be subject to. The introduction of this correction term into the idealised description afforded by the equation of the simple harmonic oscillator is motivated by theoretical considerations: in classical mechanics friction is modelled by a linear

function of velocity.[6] By introducing well-motivated corrections into the theoretical description of the simple harmonic oscillator we obtain a less idealised description of a real-life pendulum in ordinary circumstances, namely the description of a damped harmonic oscillator.

Equation (7.2) tends to equation (7.1) in the limit $b \rightarrow 0$, as required for an approximation. Hence the two descriptions agree in the asymptotic limit. There are of course plenty of equations that, just like (7.2), tend to the original equation (7.1) in some mathematical limit. Equation (7.2) is special because it is derived from the equation of the simple harmonic oscillator by a process of deidealisation. The damped harmonic oscillator and the simple harmonic oscillator are objects defined implicitly in the theory by their satisfaction of the corresponding equations; hence it is the theory that determines the relations between them. The correction terms introduced into the equation of the simple harmonic oscillator are justified by the putative relations between the objects themselves. Equation (7.1) is satisfied by a linear oscillator with no friction; equation (7.2) is satisfied by a linear oscillator subject to friction. The theory contains all the necessary techniques to represent this difference formally.

Hence the idealisation account makes superfluous the use of models in theory application. Theories must be seen as entirely self-sufficient in the task of generating genuinely realistic representations of problem-situations.[7] Where the idealisation account is true, or generally true, it follows that models cannot *mediate* between theories and the world: in the application of scientific theories that satisfy the idealisation account, there is essentially no work for mediating models to do.

The idealisation account assumes there is a final representation of every system in the theory's domain of application. In practice we may never be able to write this representation, as it may be hideously complicated; but the representation must exist because it can be approximated to an arbitrary degree by successive deidealisations of the theory. However, even in the simple case of the harmonic oscillator the presumption that such a final theoretical representation exists seems profoundly perplexing. The equation of the damped harmonic oscillator is certainly not a final representation of this kind. It is not a theoretical representation of any concrete real system in the world. Admittedly the equation of the damped harmonic oscillator is a less idealised

---

[6] For a discussion of modelling friction see e.g. Goldstein (1980, 24).

[7] Specifically, and to anticipate the main issue in what is to follow, theories do not (*must not*) rely on independently-standing models in order to fix the corrections required for successful *deidealisations*.

representation than the equation of the simple harmonic oscillator for real-life penduli. But this does guarantee that the theory contains a (true) representation of a real-life pendulum. The theory may be incomplete; there may well be some aspects of the problem-situation left unaccounted for, even after all the relevant corrections suggested by the theory have been added in.

But now the promised sense in which models were to mediate between theory and the world is definitely lost: models mediate only between theory and further models. On the idealisation account the theory does all the work required for its own application by determining, in stages, sets of increasingly *less idealised* representations. These representations, however, may never truly represent anything real at all.

## 7.5 HOW MODELS MEDIATE: THE CASE OF SUPERCONDUCTIVITY

The problem becomes acute when it is noticed that in practice the criteria of theoretical relevance presupposed by the idealisation account are rarely operative in cases of successful theory-application. On the contrary, it is often the case that scientific representations of effects or phenomena are not arrived at as deidealisations of theory. My case study in superconductivity illustrates one way in which models typically mediate between theory and the world.[8] The first theoretical representation of the Meissner effect was not found by applying a criterion of theoretical relevance for the introduction of corrections into the electromagnetic equations of a superconductor. These correction terms were not given by, and could not have been given by, classical electromagnetic theory but were rather derived from a new *model* of superconductivity. The model was motivated directly by the phenomena, not by theory. The criterion required for the application of electromagnetic theory could only be laid out when the model was in place, and an adequate classical electromagnetic description of superconductivity (the London equations) could then finally be derived.

This is, I want to claim, an important sense in which models *mediate*: they establish the corrections that need to be introduced into a theory in order to generate many of its applications. My case study shows how the derivation of a theoretical representation of a physical effect can result from corrections that are suggested by a mediating model, which is independent from theory. The approximation used to generate an appropriate representation is not a deidealisation of theory, because the criterion of relevance that guides the introduction of corrections is not theoretically motivated.

I have chosen the Londons' account of superconductivity for a number of reasons: first, because it is such a well-known episode of successful theory-application; second, because of the high esteem and reputation of the two scientists involved; finally, because it is a case of application that is to a large extent explicitly not a deidealisation. But this case study is not exceptional or isolated; on the contrary, I believe that it is paradigmatic of the activity of theory-application in many branches of physics.

### 7.5.1 *The hallmarks of superconductivity*

The electromagnetic treatment that Fritz and Heinz London (1934) proposed for superconductors in 1934 is one of the most celebrated cases of theory-application in the history of twentieth-century physics. It was the first comprehensive electromagnetic theory of superconductivity and it remained the fundamental account of superconductivity for nearly twenty years until the advent of the BCS theory (which was heavily informed by the Londons' account, as were all subsequent theories of superconductivity). Superconductors are materials that exhibit extraordinary conducting behaviour under specific circumstances. The hallmarks of superconducting behaviour are the following two well established phenomenological findings: resistanceless conductivity and the Meissner effect.

In 1911 Kamerlingh Onnes (1913) found that when mercury is cooled below $4.2K^{\circ}$ its electrical resistance falls to near zero. In 1914 he discovered that the effect does not take place in the presence of an intense magnetic field. This is the first phenomenological trait of superconductivity: under a certain critical transition temperature, and in the absence of strong magnetic fields, a superconductor exhibits almost perfect resistanceless conductivity. Almost perfect resistanceless conductivity is confirmed by the presence of a stationary current through, say, the surface of a superconducting ring. The current flows at virtually the same constant rate and does not die off.

The second, equally important, trait of superconductivity was found in 1933 by Meissner and Ochsenfeld (1933). The *Meissner effect* is the sudden expulsion of magnetic flux from a superconductor when cooled below its transition temperature. The flux in a superconductor is always

---

[8] Aspects of this case study have been published in a joint paper with Nancy Cartwright and Towfic Shomar (1995). I want to thank them both for helpful conversations on this section.

vanishingly small, regardless of what the flux inside the material was immediately before the phase transition into the domain of superconductivity took place.[9]

### 7.5.2 *Applying electromagnetism*

Superconductivity was initially considered an electromagnetic phenomenon and providing an electromagnetic treatment became the main theoretical task. This was a formidable task in view of the Meissner effect. Maxwell's equations on their own are totally ineffective: for a medium of perfect conductivity (a '*super*conductor') Maxwell's equations are inconsistent with the Meissner effect. Perfect conductivity occurs when the scattering of electrons in a medium of low resistance is so small that the electric current persists even in the absence of a supporting external electric field. For a conductor in a vanishingly small electric field, for which $\mathbf{E} = 0$, Maxwell's second equation $\nabla \times \mathbf{E} = -\frac{1}{c}\frac{\partial \mathbf{B}}{\partial t}$ predicts that $\frac{\partial \mathbf{B}}{\partial t} = 0$ and hence that **B**, the magnetic field, must remain constant in time in the transition to the superconducting state. In other words, Maxwell's equations predict that the flux through a coil surrounding the metal must remain unaltered during the phase transition. The experiments of Meissner and Ochsenfeld showed that in fact there is a sudden change in the value of the external magnetic field, consistent with the total expulsion of the magnetic flux density from within the superconductor.[10]

Of course by 1933 there was much more to electromagnetic theory than just Maxwell's equations. In the construction of their theory of perfect conductivity Becker, Sauter and Heller (1933) had to appeal to further assumptions about the media, the shape of the conductor, the forces that propelled the electrons in the absence of electric fields and, crucially, the form of the law that linked the electric current to external

fields. Their 'acceleration' theory accounted for a persistent current in a superconductor, but it was shown by the Londons to be in contradiction with the Meissner effect.

In a normal conductor the current either induces an external electric field or is supported by one, and Ohm's law predicts that the current is directly proportional to the field, $\mathbf{j} = \alpha\mathbf{E}$. With the discovery of resistanceless conductivity, Ohm's law had to be abandoned for superconductivity because the current persists in the absence of an external field. Nevertheless all proposed treatments of superconductivity continued to assume that there existed some relation between the superconducting current and external electric fields – not a proportionality relation obviously, but *some* relation nevertheless. The Londons' fundamental contribution was to make unambiguously clear that superconducting currents are in no way supported by electric fields, but by magnetic fields.

What prompted the Londons' suggestion? Why did previous attempts to understand superconductivity continue to assume that the current was physically linked to electric fields? The answer cannot be found by inspecting the state of electromagnetic theory in 1933. No significant contribution or substantive addition to the theory was made during these years that could help to explain the Londons' breakthrough. The significant event was the proposal, by the Londons, of a new *model*.

Historically, the discovery of the Meissner effect signalled the turning point. This unexpected discovery brought about a change in the *conception* of superconductivity. A superconductor was initially conceived in analogy with ferromagnetism: just as a ferromagnet exhibits a magnetic dipole moment in the absence of any supporting magnetic fields, a superconductor exhibits a permanent current even if unsupported by electric fields. The superconducting current is constant in the absence of an electric field, and what this indicates is that the field is not proportional to the current, as in Ohm's law. As a replacement Becker, Sauter and Heller proposed the following 'acceleration equation', where the field is proportional to the time derivative of the current:

$$\Lambda \frac{d\mathbf{j}}{dt} = \mathbf{E} \qquad (7.3)$$

where $\Lambda = \frac{m}{ne^2}$ (a constant that depends upon the mass $m$, charge $e$ and number density of electrons $n$). In the absence of an external field ($\mathbf{E} = 0$) the 'acceleration equation' predicts a permanent current: $\frac{d\mathbf{j}}{dt} = 0$.

---

[9] A distinction is usually made between Type I and Type II superconductors. In Type I superconductions *all* magnetic flux is expelled in the phase transition. In Type II superconductors the expulsion is only partial. Type II superconductors only appeared much later, and the distinction played no role in the historical instance that I wish to discuss. In this paper by 'superconductors' I refer to type I superconductors only. These are thin films made out from metals like zinc, aluminium, mercury, lead.

[10] The inconsistency of the Meissner effect, perfect conductivity with $\mathbf{E} = 0$, and Maxwell's equations is often emphasised in textbook discussions (see, for instances, Bleaney and Bleaney (1976, ch. 13) and Hall (1974, ch. 11).

### *7.5.3 Enter the model*

The Londons understood that the Meissner effect pointed to an entirely different model. They modelled a superconductor as one huge diamagnet, and replaced Ohm's law with a new electromagnetic relation between the superconducting current and the *magnetic* field. The Londons went on to attempt a microscopic explanation of the coherence of the 'magnetic dipoles' in terms of a coherent macroscopic quantum superposition.[11]

By modelling a superconductor as a diamagnet the Londons were able to introduce an important correction into the 'acceleration equation' theory of Becker, Sauter and Heller. Diamagnetism is associated with the tendency of electrical charges to shield the interior of a body from an applied magnetic field.[12] Following a proposal by Gorter and Casimir (1934), the Londons began by assuming that a real superconductor is constituted by two different substances: the normal and the superconducting current. They then proposed that Ohm's law be restricted to the normal current in the material, and the description of the superconducting current be supplemented with an equation that determined the relation of the current to the background magnetic flux. The 'London equation' for the superconducting current takes the form:

$$\nabla \times \Lambda \mathbf{j} = -\frac{1}{c}\mathbf{H} \tag{7.4}$$

where $\mathbf{j}$ is the current, and $\mathbf{H}$ represents the magnetic flux inside the superconductor.

It is important to understand that this equation was not derived from electromagnetic theory, but was suggested by the new model of diamagnetism. Although analogy was certainly involved, this is not just simply a case of reasoning by analogy. The Meissner effect does not just mean that the equations that describe magnetic flux in a superconducting material must be formally analogous to the equations for flux in a diamagnetic material. It rather means that a superconductor *is* a kind of diamagnet. Equation (7.4) was derived from a correction to the solutions of the old 'acceleration equation' theory – a correction prompted by the conception of the superconductor as a diamagnet. According to this conception the fundamental property of a superconductor is not nearly perfect conductivity but, of course, the expulsion of the magnetic flux within the material during the transition phase. Superconductivity is no longer characterised as the limit of perfect conductivity, but as the limit of perfect diamagnetism. Hence the phenomenon of the expulsion of the magnetic flux cannot, and should not, be explained by the emergence of a superconducting current. Superconductivity is truly characterised by two independent and nonreducible phenomenological hallmarks: perfect conductivity and the Meissner effect.

In the theory of Becker, Sauter and Heller the absence of an electric field entails that the Meissner effect is impossible, as expected from our initial consideration of Maxwell's second equation in the case of perfect conductivity. Indeed the 'acceleration equation' entails the following equation for the magnetic flux inside the superconductor:

$$\Lambda c^2 \nabla^2 \frac{d\mathbf{H}}{dt} = \frac{d\mathbf{H}}{dt}. \tag{7.5}$$

Integrating with respect to time one finds the following nonhomogeneous equation:

$$\Lambda c^2 \nabla^2 (\mathbf{H} - \mathbf{H}_0) = \mathbf{H} - \mathbf{H}_0 \tag{7.6}$$

$\mathbf{H}_0$ denotes the magnetic field at the time t = 0 (i.e. at the time the transition phase occurs). Its value depends entirely on the value of the ambient field because a superconductor behaves exactly like a normal conductor before the phase transition, and the external field penetrates completely. The solutions to this equation are given by $\mathbf{H} = \mathbf{e}^{-\sqrt{\Lambda}cx} + \mathbf{H}_0$, where the exponentials $\mathbf{e}^{-\sqrt{\Lambda}cx}$ decrease very quickly with distance $x$ from the surface of the material. So the 'acceleration equation' predicts that the field inside a superconductor will remain invariant throughout the phase transition. No change in the external flux will be observed and a surrounding coil will experience null induction. As London and London (1934, 72) write of the theory of Becker, Sauter and Heller:

> [t]he general solution means, therefore, that practically the original field persists for ever in the supraconductor. The field $\vec{H_0}$ is to be regarded as 'frozen in' and represents a permanent memory of the field which existed when the metal was last cooled below the transition temperature [. . .] Until recently the existence of 'frozen in' magnetic fields in supraconductors was believed to be

---

[11] Superconductivity is of course ultimately a quantum phenomenon. The definitive quantum treatment was given in 1951 by Bardeen, Cooper and Schrieffer (1957) who explained the emergence of coherence by appealing to the formation of Cooper pairs at low temperatures. The history of the BCS theory is fascinating in its own right, but it is of no relevance to my present argument.

[12] See, for instance, Kittel (1953, ch. 14).

proved theoretically and experimentally. By Meissner's experiment, however, it has been shown that this point of view cannot be maintained.

On the other hand the Londons' diamagnetic model suggests that the field inside the material once the transition has occurred decreases very quickly with distance $x$ from the surface of the material. So the correct solutions must exclude the value $(\mathbf{H}_0)$ of the initial field, and must contain only the exponentials $\mathbf{e}^{-\sqrt{\Lambda}cx}$. These are solutions to the following homogeneous equation: $\Lambda c^2\nabla^2\mathbf{H}=\mathbf{H}$. From this equation, the fundamental equation of superconductivity (7.4) can be derived, since $\nabla\times\mathbf{H}=\frac{1}{c}\mathbf{j}$.

To sum up, the Londons suggested that the superconducting current is maintained by a magnetic field. The relation is of inverse proportionality, so that if the field is greater than a certain threshold value the superconducting current will virtually come to a halt, as predicted by Onnes. This equation was determined, in the manner described above, by a new model of superconductivity; the model was in its own turn suggested by the phenomena. This reconstruction explains why no satisfactory theory of superconductivity was derived before the discovery of the Meissner effect. A novel conception, embodied in the model of the superconductor as one huge diamagnet, was required for a successful electromagnetic treatment of superconductivity, and such conception was not available before the discovery of the Meissner effect.[13]

---

[13] It may be tempting to construe some commentators as suggesting that the Londons' only contribution was to restrict the set of initial conditions in the old 'acceleration equation'. Bardeen, for instance, in his impressive review article (1959) states that 'The Londons added (5.4) to the earlier "acceleration" theory of Becker, Sauter and Heller to account for the Meissner effect.' I do not find the reading of these passages in terms of a restriction of the initial conditions at all possible. And yet, this is precisely how French and Ladyman (1997) have read this episode in their response to Cartwright, Shomar and Suárez (1995), and to previous drafts of this work. But the replacement of the set of solutions that involve the initial field in the superconductor by the family of exponential solutions *is not a restriction* of the old theory to the case where the external field before the transition vanishes, i.e. to the case $\vec{H_0}=0$. It is true that the 'acceleration' theory and the Londons' theory fully agree in that particular case. Nevertheless, the whole point of the Londons' theory is to show that the flux inside the superconductor is vanishingly small *even if* the initial flux was not zero at the time when the transition took place. Whenever the magnetic field is not vanishingly small outside the material before the transition the theories will yield inconsistent predictions as regards the expulsion of the flux: the 'acceleration' theory predicts no expulsion, while the new theory predicts a brutal change, consistent with the Meissner effect. The Londons of course accept that in the case $\mathbf{B}_0=0$ the 'acceleration equation' theory gets it right. But they do not remain silent about those other cases that this theory does not get right. They provide a whole new theory that has the same predictions for the $\mathbf{B}_0=0$ case, but gives the correct predictions for the other cases. In general, writing down a new equation for the value of a physical quantity in a theory is not equivalent to restricting the initial conditions on the old equations.

## 7.6 APPLICATION IN PRACTICE: PROBLEMS FOR REALISM

In providing a macroscopic description of the Meissner effect in electromagnetic terms, the Londons effectively succeeded in providing a satisfactory application of electromagnetic theory to superconductivity. However, they did not *deidealise* electromagnetic theory. Instead they came up with a model that permitted them to impose a novel constraint upon the original theoretical construction. This case study is not exceptional; on the contrary, *many* scientific applications are derived in this way. In astrophysics, for example, there are several models of stellar structure. A certain conception of the internal constitution of a star, which determines the form of the convection forces in the stellar plasma, has to be assumed before the quantum theory of radiation can be applied. For each different conception there is a corresponding application of the theory, a family of models, that could not have been derived from the theory alone. Similar conclusions could be derived from other case studies, some of them contained in this volume, by authors studying the role of mediating models in different areas of science. The idealisation account is then not a universal account of scientific theory-application. It is far too restrictive. It imposes constraints so severe that they are not always – indeed are rarely – met in practice.

### *7.6.1 The epistemology of theory-application*

What are the epistemological implications of the rejection of the idealisation account? I shall focus the discussion closely upon the case study. The Londons built an application of electromagnetic theory to superconductivity; and yet, on McMullin's account, the theory was in no way confirmed by the phenomenon of superconductivity. Confirmation requires that the theory itself must suggest the introduction of corrections into the theoretical description. For, as McMullin points out,[14] a theoretical description is *ad hoc* with respect to a theory that does not suggest or motivate its derivation; and an *ad hoc* description, or hypothesis, cannot increase the degree of confirmation of a theory with respect to which it is *ad hoc*.[15]

---

[14] See the discussion in section 7.3.2, and in particular the passage quoted from McMullin (1985).

[15] Hempel too makes this claim (1966, 28–30), although he there ascribes a slightly different meaning to the term *ad hoc*. For Hempel, a hypothesis is *ad hoc*, with respect to some theory, if it has no surplus empirical content over the theory other than the particular phenomenon that it is specifically called to account for.

In constructing their account of superconductivity, the Londons introduced a correction into the previously available theoretical description. The correction was certainly not arbitrary, since it was justified by a new model of superconductivity. However, this model was not suggested by the theory – it was suggested by a newly discovered physical effect. On McMullin's confirmation theory, classical electromagnetism was not in this instance genuinely confirmed at all. Was it *neither* confirmed *nor* disconfirmed, or was it simply disconfirmed? The answer to this question depends on what we take electromagnetic theory to be *circa* 1933.

There are two possible pictures. It is possible to take 'electromagnetic theory' in an extended historical sense, as constituted by all applications to electromagnetic phenomena known to the Londons. The 'acceleration equation' theory is part of electromagnetic theory, when construed in this extended sense. But this theory was seen in light of the Meissner effect, to be highly unrealistic, as it made the false assumption that a superconductor would behave as a ferromagnet; when in fact a superconductor is a diamagnet. And, as we saw, the Londons gave an account that contradicted the acceleration equation theory predictions in a range of cases. Hence, if taken in this 'historical' sense, classical electromagnetism was indeed *disconfirmed* by the Meissner effect.

Alternatively, one may provide an abstract reconstruction of electromagnetic theory. The standard reconstructions normally assume that classical electromagnetism is constituted by the deductive closure of Maxwell's equations. Now, the 'acceleration equation' theory, although not inconsistent with Maxwell's equations, is not a logical consequence of these equations. It can be postulated alongside them, in just the way Ohm's law is often postulated alongside Maxwell's equations, but it cannot be derived from them. Nor is the Londons' account a logical consequence of Maxwell's equations; although it is also consistent with them, and can be postulated alongside them.[16] Thus, neither the 'acceleration equation' theory nor the Londons' account is part of electromagnetic theory, understood in this abstract manner. And it follows that, in this abstract reconstruction, the Londons' account provided neither a confirmatory nor a disconfirmatory boost for classical electromagnetism.

And yet, the Londons' treatment did increase scientists' confidence in electromagnetic theory. Superconductivity had proved difficult to model in classical electromagnetism for a long time, and many were beginning to despair that a consistent electromagnetic treatment woul ever be found. The diamagnetic conception played a key role in th Londons' explanation of the phenomenon of superconductivity, whic reveals the extent to which a mediating model carries genuine physic knowledge. The Londons' theory was generally accepted to accour rather accurately for the rate of magnetic flux expulsion from a supe conductor during the phase transition reported by Meissner an Ochsenfeld in their experimental investigations.[17] From this applicatio of electromagnetism we learn that superconductivity is an essentiall diamagnetic effect; that a superconductor is not a ferromagnet; anc moreover, as the Londons' account correctly predicts the rates of expulsion of magnetic flux observed by Meissner and Ochsenfeld, we gain theoretical understanding of the Meissner effec does not appear as a mysterious side-effect of superconductors; instea it takes centre stage, it becomes a fundamental hallmark of superconductivity.

The Londons' account of superconductivity provided an extra 'boost of confidence in classical electromagnetism which the old 'acceleration theory could not provide. But, as we have seen, on McMullin's idealisation account of application, the Meissner effect does not make electromagnetic theory more likely to be true. It seems that this extra boost o confidence in electromagnetism cannot be captured by the standarc realist theory of confirmation, so I shall refer to the kind of support that the Londons' treatment provided for electromagnetism as *degree of confidence* rather than degree of confirmation.

The fact that the Londons' equation accounts for the Meissner effect gives grounds to believe that classical electromagnetism is *instrumentally reliable*. But it does not constitute evidence for the truth of classical electromagnetic theory. Here *degree of confidence* and *degree of confirmation* seem to depart. Degree of confidence, unlike degree of confirmation, does not point to the likelihood of the theory to be true; it only points to the reliability of the theory as an instrument in application. The theory is a reliable instrument if it is capable, perhaps when conjoined with good enlightening mediating models, of generating successful applications. And from the fact that the theory is instrumentally successful, the truth of the theory does not follow.

Or does it? Would it not be a miracle if the theory was false, yet instrumentally successful? Does the instrumental success of scientific theories

---

[16] It is perfectly possible for a theory $T$ to be consistent with each of two mutually inconsistent assumptions $a$ and $b$, – as long as $T$ entails neither $a$ nor $b$, of course.

[17] Although there was some initial resistance to the Londons' theory on empirical grounds. In particular Von Laue disagreed; for the dispute between Fritz London and Von Laue, see Gavroglu (1995, 123–7).

not argue for scientific realism? Arguments of this kind in favour of realism are, of course, well known in the literature.[18] Typical antirealist responses to this argument are equally well known. For instance, Fine (1986b) responds that the 'no-miracles' argument is riddled with circularity: it assumes that precisely the very sort of inference from explanatory power to truth that realism sanctions and instrumentalism contests for scientific practice, is valid at the 'meta-level' and can be used as part of an argument for realism in general. As a response, scientific realists have turned to the pragmatic virtues of realism, and they have tried to show that no version of antirealism is in any better shape. In particular the debate has focused upon Bas van Fraassen's version of antirealism, known as *constructive empiricism*.[19]

The issues about realism that I am raising are tangential to the recent debate between scientific realists and constructive empiricists. Scientific realism and constructive empiricism share a common core, which is rejected by instrumental reliability. On either view a minimum requirement for the acceptance of a scientific theory is that the theory must be empirically adequate – i.e. that what the theory states is the case about the phenomena must indeed be the case. The constructive empiricist argues that the acceptance of a theory need only involve the belief that it is empirically adequate. Theories may have other virtues besides empirical adequacy – such as simplicity, explanatory power, aesthetic value, or even the virtue of being true . . . – but belief in a theory's empirical adequacy is the only doxastic attitude required for the acceptance of the theory. By contrast, the realist argues that the belief that the theory is true, or likely to be true, and not just empirically adequate, is also required for its acceptance. For the realist a good theory, in addition to being empirically adequate, should also be true, or likely to be true – not only true to the phenomena, but true *tout court*, true to the world.

Thus, the scientific realist and her opponent, the constructive empiricist, agree that only highly confirmed theories should be accepted; we should have confidence in theories that are highly confirmed, and only in those. This is because on either view, confirmation always goes *via* empirical adequacy. A theory is confirmed when its observable predictions are borne out. The theory is empirically adequate if *all* the predictions of the theory – past, present and future – are borne out.[20] The realist takes a high degree of confirmation a a strong indication that the theory is true, or very likely to be true because on her view empirical adequacy is a guide to truth. So, fo the realist a high degree of confirmation is required for acceptance For the constructive empiricist a high degree of confirmation is onl an indication that the theory is empirically adequate, nothing more But, as the constructive empiricist thinks that belief in the empirical adequacy of a theory is required for its acceptance, he will readily agree with the realist that a high degree of confirmation is a requirement for accepting a theory.

The London model does not raise the degree of confirmation of electromagnetic theory; it raises its *degree of confidence* – that is, it gives us a reason to believe that the theory is instrumentally reliable, i.e. that it will go on to provide successful applications. The instrumental reliability of a theory provides grounds neither to believe that the theory is true, nor that it is empirically adequate – it points neither towards scientific realism, nor towards constructive empiricism.

The contrast between *degree of confidence* and *degree of confirmation* is not captured by the current debate on scientific realism. Degree of confirmation measures either the degree of a theory's empirical adequacy, or of its truth. Degree of confidence, as I would like to define it, is not grounded on an confirmatory relationship of the truth-conferring type between a theory and phenomena. Increased *confidence* in classical electromagnetism need not be accompanied by an increase in one's estimated probability that it correctly describes the world, i.e. that it is true. The success of the London model does not provide warrant for that. Neither does it warrant an increase in one's estimated probability that the theory correctly describes the phenomenal world, i.e. that the theory is empirically adequate. Unlike degree of confirmation, *degree of confidence* is not a function of a theory's empirical adequacy. It is not confirmatory but pragmatic, a function of the success of a theory in generating applications to diverse phenomena whenever conjoined with the appropriate mediating models. I call this feature of theories 'instrumental reliability' in order to distinguish it sharply from empirical adequacy. The instrumental reliability of a theory does not require, nor does it necessarily follow from, its empirical adequacy. This is of course in

---

[18] The original 'no-miracle' arguments are due to Putnam (1975), and Boyd (1973 and 1984).

[19] For Van Fraassen's constructive empiricism see Van Fraassen (1976), reprinted with corrections in Van Fraassen (1980). A collection of papers by critics of constructive empiricism, together with responses by Van Fraassen is contained in Churchland and Hooker (1985).

[20] We may never be in a position to know if a theory is empirically adequate or not. The claim that a theory is empirically adequate carries *precisely the same commitment* to the correctness of a theory's future predictions, as does the claim that the theory is true. In this respect the constructive empiricist sticks his neck out *exactly as much as* the realist.

agreement with my case study: the fact that classical electromagnetic theory can be applied to superconductivity should not be taken as an indication that the theory is *true* to superconductivity phenomena.

### *7.6.2 Conclusions*

Let us grant that scientists do see a theory's applications as providing some degree of confidence in the theory. Does this not argue for the idealisation account, and hence for the realist epistemology that underpins it? Some scientific realists, such as McMullin, think so. The idealisation account, they think, is *required* by scientific epistemology. On the idealisation account the explanatory power of a theory is exhibited through its applications, and the theory is more likely to be true in view of the success of its applications. So, realism is required to make sense of the epistemology of theory-application.

However, the instrumentalist can give a similarly good account of the epistemology of application. Scientists' increased confidence in a theory that has generated many applications is a result of the theory's instrumental success in modelling the phenomena. This gives, at most, confidence that the theory will continue to generate successful applications in the future, i.e. that it is an *instrumentally reliable* theory. And this argues against realism: a successful application of a theory need not constitute evidence that the theory is true. The applications of a scientific theory do not necessarily yield the kind of evidential support for the truth of the theory that scientific realism requires them to.[21]

## 7.7 FINAL REMARKS

My case study points to the existence of a variety (a 'plurality') of ways in which scientific theories are applied. Some scientific applications are, perhaps, deidealisations of theory. But many successful applications are not; it is in those cases that mediating models play a key dual role. First, they help in the application of theory, by guiding the introduction of corrections into the theory required in order to accurately describe the phenomena. Second, they provide us with physical insight into the nature of the phenomena. Because of that, mediating models are not just *useful fictions*; on the contrary they are carriers of significant and very specific genuine knowledge of the phenomena. However, the role of mediating models in theory-application entails that a realist construal of scientific theory becomes highly problematic. The theory itself is used only as an instrument in application, and no attempt is made to confirm or disconfirm it at all.

---

[21] Why was it felt that a quantum treatment was none the less required? Scientists felt that a more robust explanation of the phenomena could be achieved in quantum mechanics. It was a desire for a more robust explanation that led the search for a quantum treatment, and gave rise to the BCS theory. Whether this subsequent episode constitutes ammunition for scientific realism is beyond the scope of this paper. Antirealists will presumably want to deny the inference to the truth of the BCS explanation, no matter how robust the explanation. Constructive empiricists, for example, could claim that what led the search for the quantum treatment was a desire for an empirically adequate theory to provide the explanation – for classical electromagnetism is not empirically adequate of superconductivity phenomena. We are surely back into the muddy waters of the traditional debate between realism and antirealism.

## REFERENCES

Bardeen, J. (1959). 'Theory of Superconductivity', *Encyclopedia of Physics*, 274–369.

Bardeen, J., L. Cooper and J. Schreiffer (1957). 'Theory of Superconductivity', *Phys. Rev.*, 108, 1175–1204.

Becker, R., F. Sauter and G. Heller (1933). 'Uber die Stromveteilung einer supraleitenden Kugel', *Z. Physik*, 85, 772.

Bleaney, B. and B. Bleaney (1976). *Electricity and Magnetism*, 3rd edn, Oxford, Oxford University Press.

Boyd, R. (1973). 'Realism, Underdetermination and a Casual Theory of Evidence', *Nous*, 7, 1–12.

(1984). 'The Current Status of Scientific Realism', in J. Leplin (ed.), *Scientific Realism*, Berkeley, CA, University of California Press.

Cartwright, N., T. Shomar and M. Suárez (1995). 'The Tool-Box of Science', pp. 137–49, in *Theories and Models in Scientific Processes*, Poznan Studies in the Philosophy of the Sciences and the Humanities, Rodopi, vol. 44.

Churchland, P. and C. Hooker (eds.) (1985). *Images of Science: Essays in Realism and Empiricism*, Chicago, University of Chicago Press.

Fine, A. (1986a). *The Shaky Game: Einstein, Realism and the Quantum Theory*, Chicago, University of Chicago Press.

(1986b). 'Unnatural Attitudes: Realist and Instrumentalist Attachments to Science', *Mind*, 95, 149–79.

Frassen, B. van (1976). 'To Save the Phenomena', *Journal of Philosophy*, 73, 623–32.

(1980). *The Scientific Image*, Oxford, Oxford University Press.

Gavroglu, K. (1995). *Fritz London: A Scientific Biography*, Cambridge, Cambridge University Press.

Giere, R. (1988). *Explaining Science: A Cognitive Approach*, Chicago, University Press of Chicago.

Goldstein, H. (1980). *Classical Mechanics*, Addison-Wesley, 2nd edn.

Gorter, C. and H. Casimir (1934). 'On Supraconductivity I', *Physica*, 1, 306–20.

Hacking, I. (1982). 'Language, Truth and Reason', in M. Hollis and S. Lukes (eds.), *Rationality and Relativism*, Cambridge, MA, MIT Press.

Hall, H. E. (1974). *Solid State Physics*, John Wiley and Sons.

Hempel, C. G. (1966). *Philosophy of Natural Science*, Englewood Cliffs, NJ, Prentice Hall Inc.

Kittel, C. (1953). *Introduction to Solid State Physics*, John Wiley and Sons.

Leplin, J. (ed.) (1984). *Scientific Realism*, Berkeley, CA, University of California Press.

London, F. and H. London (1934). 'The Electromagnetic Equations of the Supraconductor', *Proceedings of the Royal Society*, A149, 71–88.

McMullin, E. (1985). *Galilean Idealization, Studies in History and Philosophy of Science*, 16, 247–73.

Meissner, W. and S. Ochsenfeld (1933). 'Ein Neuer Effect bei Eintritt der Supraleitfahingkeit', *Die Naturwissenschaften*, 787–88.

Morrison, M. (1998). 'Modelling Nature Between Physics and the Physical World', *Philosophia Naturalis*, 38:1, 65–85.

(forthcoming a). 'Approximating the Real: the Role of Idealizations in Physical Theory', in M. Jones and N. Cartwright (eds.), *Idealizations in Physics*, Rodopi.

(forthcoming b). *Mediating Models*.

Morton, A. (1993). 'Mathematical Models: Questions of Trustworthiness', *British Journal for the Philosophy of Science*, 44, 659–74.

Onnes, K. (1913). 'Commun. Kamerlingh onnes Lab', Technical Report 34b, University of Leiden.

Putnam, H. (1975). *Mathematics, Matter and Method*, Cambridge, Cambridge University Press.

Redhead, M. (1980). 'Models in Physics', *British Journal for the Philosophy of Science*, 44, 145–63.

Suárez, M. (1997). 'Models of the World, Data-Models and the Practice of Science: The Semantics of Quantum Theory', PhD thesis, University of London.

Wise, N. (1993). 'Mediations: Enlightenment Balancing Acts, or the Technologies of Rationalism', pp. 207–58, in P. Horwich (ed.), *World Changes: Thomas Kuhn and the Nature of Science*, Cambridge, MA, MIT Press.