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Cultures of Modelling: Rudolf Peierls on 'Model-Making in Physics'

Abstract. The philosophical debate about scientific models has, over the past thirty years or so, reached a high degree of sophistication. Yet, in spite of efforts to seek common ground with scientific practice, there remains the suspicion that philosophical accounts are sometimes too 'free-floating', in that they do not adequately reflect scientists' views (and actual uses) of models. The present paper deals with one such scientific perspective, due to physicist Sir Rudolf Peierls (1907-1995). Writing thoroughly from the perspective of a theoretician with a deep appreciation for experimental physics, Peierls, in a series of papers, developed a taxonomy of scientific models, which – in spite of some inevitable arbitrariness – exhibits surprising points of convergence with contemporary philosophical accounts of how scientific models function. The present paper situates Peierls's approach within the philosophical and scientific developments of his time, engages (in an immersive way) with his proposed taxonomy, and argues that Peierls's views – and others like them – warrant the recent philosophical shift from a focus on model-based representation to non-representational (e.g., exploratory) uses and functions of models.

Keywords: scientific models, modeling, scientific representation, exploration.

1. Introduction

Scientific models and the activity of modelling in science have, in recent years, attracted considerable attention from philosophers of science. Sophisticated philosophical accounts have been proposed regarding how models represent their targets and allow us to infer knowledge about them, and a plethora of case studies from the various special sciences have been worked out, many of which engage with the cutting edge of contemporary science. What has sometimes been neglected, however, is the perspective of scientists themselves. To be sure, there are laudable exceptions, notably Daniela Bailer-Jones's analysis of scientists' thoughts on scientific models [Bailer-Jones, 2003]. Yet how the widespread adoption of scientific modelling across the sciences 'adds up', so as to shape the production of new knowledge, remains a philosophically neglected question. At the risk of disappointing my readers, I must be up-front and admit that the current paper will not fill this lacuna. Any attempt to do so would require a breadth of coverage and a level of detailed analysis that would be impossible within the constraints of a single paper. What I will attempt, instead, is to show, by way of example, how scientific models have become the preferred way for scientists to deal with, and reflect on, a range of worthy goals: representing reality, reducing complexity, getting a grasp on novel and elusive phenomena, deriving potential explanations, exploring constraints and theoretical structures, implementing approximations, and studying limiting cases. Specifically, I will be engaging with the work of Sir Rudolf Peierls (1907-1995), who was at the centre of many important twentieth-century developments in physics, without ever achieving the pop-science stardom of some of his contemporaries (notably Richard Feynman). Two papers, written in the 1980s after his retirement, explicitly discuss different model types and their functions; while these lack philosophical rigour, they provide an interesting glimpse into the epistemic culture of modelling in physics, as experienced by one of its prime exponents.

The rest of this paper is organized as follows. Section 2 discusses some of the history of scientific models, focussing on the shift from emphasizing mechanical models to a more inclusive notion of 'model' that accommodates, among others, analogical reasoning in physics. It also dicusses Mary Hesse's influential mid-twentieth century text 'Models and Analogies in Science' (1963), which marks the beginning of a rapid growth of philosophical interest in models and their role in science. Section 3 surveys some of the theoretical tensions that afflict any philosophical attempts to come to a global characterization of what models are and how they function in inquiry. Section 4 summarizes in some detail the seven-fold taxonomy of 'model types' proposed by Rudolf Peierls in a semi-popular article published in 1980 as 'Model-Making in Physics' (and reprised in his 1987 'Models, Hypotheses and Approximations'). Peierls's perspective is that of a practicing scientist who suspends his immediate research agenda and reflects on the broader direction of physics; he is not primarily concerned with the finer philosophical points concerning models and their functions. Taking his remarks at face value, then, requires immersing oneself in the – often 'hands-on' and outcome-oriented – epistemic culture of scientists using models for a variety of purposes. Such an immersive approach is rewarded, however, by insights into the practice of model-making and its guiding values, such as its recognition of pluralism – first and foremost, the realization 'that different models serve quite different purposes, and they vary in their nature accordingly' [Peierls, 1980, p. 3]. The fifth and final section relates the material presented thus far to recent attempts to shift philosophical attention from model-based representation to non-representational uses and functions of models. Some of these attempts have coalesced under the label of 'exploratory modelling' [Gelfert, 2016, pp. 71-100]; on this view, models – beyond representing actual targets – can probe modal and theoretical structure (e.g., by considering various counterfactual, e.g. higher- or lower-dimensional scenarios), generate potential explanations, or provide 'proofs-of-principle'. Furthermore it is argued that models often play a regulative role in scientific inquiry more generally, by giving direction to prospective research programmes and setting them on course for future successes.

2. Historical background: from analogies to models

When looking at the history of models in science, one may feel tempted to survey the history of science from the vantage point of our current understanding of the term 'scientific model' (which itself is far from uniform) and look for episodes that appear to fit with one's preferred definition of what constitutes a scientific model. Yet such an approach would hardly do justice to the varied history of the term 'model' in scientific discourse – a task which is also beyond the scope of this paper. It will nonetheless be instructive to look at the (surprisingly recent) emergence of 'model talk' in science, and in physics in particular.

It seems safe to say that systematic self-reflection on the uses and limitations of models in physics did not begin in earnest until some time in the nineteenth century. While methodological reflection and sophisticated analyses of the status of hypotheses, theories, and observations can be found throughout the history of science, including in its early stages, these did not coalesce into a systematic discussion of the role and significance of models in scientific inquiry. In philosophy of science, the recognition that central models are central to the pursuit of science did not set in until even more recently. Only from the middle of the twentieth century onwards did philosophers of science shift their focus from theories to models – which, until then, had often been regarded as playing a merely auxiliary role in applying fundamental theories to specific situations. One important transformation that contributed to the remarkable rise of scientific models in physics from the nineteenth century onwards, and to their proliferation in the twentieth century and beyond, was the shifting of emphasis from mechanical models (i.e., real or imagined mechanical 'stand-ins' for real target systems) to a far more inclusive notion of 'model' (as reflected in twentieth-century expressions such as the 'standard model' in particle physics).

Consider Pierre Duhem's endorsement of the use of *analogy* in physics. The idea of analogy derives its utility from the thought that relations in one domain resemble those in what may be an otherwise entirely separate domain, such that *A* is related to *B* (where *A* and *B* belong to one domain) like *C* is related to *D* (where these belong to the other domain). Whether such resemblance is merely formal or is underwritten by material similarity is of secondary importance for our purposes; at any rate, Duhem conceives of analogy primarily as a relation between sets of statements, more specifically between one theory and another:

> Analogies consist in bringing together two abstract systems; either one of them already known serves to help us guess the form of the other not yet known, or both being formulated, they clarify the other. There is nothing here that can astonish the most rigorous logician, but there is nothing either that recalls the procedures dear to ample but shallow minds. [Duhem, 1954, p. 97]

A good example is Christiaan Huygens's proposal, in 1678, of his wave theory of light. In developing his theory, Huygens was guided by an analogy with the theory of sound waves: the relations between the various properties and qualities of light are *like* those of sound waves, as described by acoustic theory. Understood in this way, analogy is, for Duhem, an entirely legitimate tool for studying one domain on the basis of our (more secure) knowledge of quite another domain. Sound waves, in our contemporary scientific vernacular, provided Huygens with a good *theoretical model* for how light propagates and behaves in various settings.

This contrasts with Duhem's forceful rejection of *mechanical models* as a way of expounding a new theory (rather than, say, merely illustrating it). Taking a textbook presentation of Maxwell's theory of electromagnetism as his target of choice, Duhem strikes a polemical tone in his dismissal of what he takes to be an undue reification of theoretical relationships into mechanical processes:

> Here is a book intended to expound the modern theories of electricity and to expound a new theory. In it there are nothing but strings which move round pulleys which roll around drums, which go through pearl beads, which carry weights; and tubes which pump water while others swell and contract; toothed wheels which are geared to one another and engage hooks. We thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory. [Duhem, 1954, p. 7]

It is important to be clear about precisely what Duhem is criticizing. Given his earlier endorsement of analogical reasoning, Duhem cannot be categorically opposed to the idea of relating one domain to another (qualitatively different) one. Instead, what the quoted passage mocks is a *style* of reasoning – one in which the desire to visualize physical processes in purely mechanical terms masks the theoretically more ambitious task of understanding them in their own right. His hostility is thus directed at mechanical models only—as is also clear from the contrast implicit in the title of the chapter ('Abstract Theories and Mechanical Models') from which the quoted passage is taken—and extends neither to 'theoretical models' nor, necessarily, to other contemporary uses of the term 'scientific model' in physics. Whereas mechanical models encourage the hasty identification of the entities being visualized ('pulleys', 'drums', 'pearl beads', 'toothed wheels', etc.) with the (unknown) actual physical processes, analogy makes it possible for us to make inferences about one domain on the basis of knowledge about another, while at the same time acknowledging their qualitative difference.

A first sketch of a philosophical account of how analogies can underwrite the use of models in science was presented by Mary Hesse in her influential 1963 essay *Models and Analogies in Science*, which is explicitly conceived of as a dialogue between a 'Duhemist' and his opponent, the 'Campbellian' (after the English physicist Norman Robert Campbell, 1880-1949). The dialogue begins with the Campbellian attributing to the Duhemist the following view: 'I imagine that along with most contemporary philosophers of science, you would wish to say that the use of models or analogues is not essential to scientific theorizing and that […] the theory as a whole does not require to be interpreted by means of any model.' The Duhemist, after conceding that 'models may be useful guides in suggesting theories', replies as follows: 'When we have found an acceptable theory, any model that may have led us to it can be thrown away.' The Campbellian, by contrast, insists: 'I, on the other hand, want to argue that models in some sense *are* essential to the logic of scientific theories.' [Hesse, 1963, pp. 8-9] What is at stake in this dispute, then, is both the ontological question of what, essentially, models are in the first place and, importantly, also the extent to which they are admissible in inquiry.

Hesse's own analogical account of scientific models begins by drawing a three-fold distinction between 'positive', 'negative', and 'neutral' analogies. Consider the billiard ball model of gases, which portrays gases as being composed of tiny elastic 'billiard balls' that fill up a given volume and sometimes bounce off each other, in a way that is meant to account for properties such as pressure and temperature. Some characteristics are shared between the (imagined) billiard balls and the target system consisting of gas atoms: for example, the momentum that can be ascribed to individual constituents and the phenomenon of collision between them. This set of shared characteristics constitutes the *positive* analogy, whereas properties we know to belong to billiard balls, but not to gas atoms—such as colour—constitute the *negative* analogy of the model. Yet the positive and negative analogy together do not exhaust the set of all properties, as there will typically be properties of the model for which it is as yet unclear whether they (also) apply to its target system. These constitute what Hesse calls the *neutral* analogy of the model.

It is the neutral analogy that injects an exploratory element into the process of inquiry, since it offers the prospect of gaining new insights into the target system by studying the model in its place—a prospect that might ordinarily have seemed slim: 'If gases are really like collections of billiard balls, except in regard to the known negative analogy, then from our knowledge of the mechanics of billiard balls we may be able to make new predictions about the expected behaviour of gases.' [Hesse 1963, p. 10]

The 1950s and 1960s—that is, exactly the period during which Hesse and other philosophers of science began to consider models in their own right—were a period of rapid growth in physics, much of which was driven by the development of ever more ambitious and sophisticated models in physics. In particle physics, the 'standard model' was beginning to be conceived—even if, arguably, it served more as a framework for theorizing than as a representation of any one target system in particular; quantum theory had gained sufficient maturity to also enter more applied subdisciplines—leading, amongst other developments, to the formation of solid-state physics as a separate subdiscipline and to the development of a myriad of quantum many-body models.[1] The other sciences, too, witnessed a growing reliance on models, driven, not least, by mathematical models in disciplines ranging from biology to economics. This helped prepare the ground for further scientific and conceptual explorations concerning models and their role in inquiry—a development that continues unabated to this day.

3. The ontology of models and the practice of modelling

The proliferation of models across the sciences makes it difficult to give a comprehensive and uncontroversial answer to the ontological question of what, in general, a scientific model is. Disagreement on general terms such as 'knowledge', 'theory', or 'model' is, of course, part and parcel of the philosophical enterprise, yet there is an unavoidable trade-off between achieving

[1] For the emergence of solid-state physics as a recognized stand-alone subdiscipline, see especially [Weart, 1992]; for a philosophical survey of the variety of quantum many-body models in condensed matter physics, see [Gelfert 2015].

conceptual clarity through stipulation and retaining adequate scope in relation to the issues that initially motivated our philosophical inquiry. In the present case, what generated philosophical interest in scientific models in the first place were the perceived growth of 'model talk' among scientists and the growing presence of models in scientific practice. In their attempts to make sense of what scientists call 'models', and of how they use them, philosophers of science created their own sprawling taxonomies, as is evident from this list of model-types, found in the *Stanford Encyclopedia of Philosophy*: 'Probing models, phenomenological models, computational models, developmental models, explanatory models, impoverished models, testing models, idealized models, theoretical models, scale models, heuristic models, caricature models, didactic models, fantasy models, toy models, imaginary models, mathematical models, substitute models, iconic models, formal models, analogue models and instrumental models' [Frigg & Hartmann, 2012]. In light of this dazzling diversity, it is perhaps no surprise that Nelson Goodman, as early as in his 1968 *Languages of Art*, voices the following lament: 'Few terms are used in popular and scientific discourse more promiscuously than "model".' [Goodman, 1968, p. 171] If this was true of science and popular discourse in the late 1960s, it is no less true today.

The great variety of models employed in scientific practice makes vivid just how central the use of models is to contemporary science and, perhaps increasingly, to the self-image of scientists how rely on them. As John von Neumann once put it: 'The sciences do not try to explain, they hardly even try to interpret, they mainly make models.' [von Neumann, 1961, p. 492] It might, however, also lead one to ask whether it is at all reasonable to look for a unified philosophical account of models. Given the vast range of things we call 'models', and the divergent uses to which they can be put, a one-size-fits-all answer to the question 'What is a model?' may simply seem out of reach. One reaction has been to try to assimilate models to theories, thereby treating them as entirely auxiliary and subordinate. On this view, models may be, as Richard Braithwaite put it, 'the most convenient way of thinking about the structure of the theory' [Braithwaite, 1968, p. 91], but they are just that: ways of thinking about an underlying theory. Even more sternly, Rudolf Carnap urged his readers 'to realize that the discovery of a model has no more than an aesthetic or didactic or at best a heuristic value, but is not at all essential for a successful application of the physical theory' [Carnap, 1969, p. 210].

Another standard reaction to the puzzling diversity of what constitutes a scientific model has been to argue that, as Gabriele Contessa puts it, 'if all scientific models have something in common, this is not their *nature* but their *function*' [Contessa, 2010, p. 194]. Amongst functional characterizations of models, a further distinction can be drawn between instantial and representational views. According to the former, models have their function in virtue of instantiating the axioms of a theory, where the latter is understood in terms of linguistic statements. By contrast, on the representational *view*, 'language connects not directly with the world, but rather with a model, whose characteristics may be precisely defined'; the model makes contact with the world only inasmuch as there is a 'similarity between a model and designated parts of the world' [Giere, 1999, p. 56]. Generally speaking, proponents of the instantial view regard models as primarily being in the business of 'providing a means for interpreting formal systems', whereas those who favour the representational view consider models to be 'tools for *representing the world*' [Giere, 1999, p. 44]. The representational view, in turn, can be construed by either highlighting the informational aspects of models or their pragmatic role in inquiry. The basic idea of the former is 'that a scientific representation is something that bears an objective relation to the thing it represents, on the basis of which it contains information regarding that aspect of the world' [Chakravartty, 2010, p. 198]; by contrast, the *pragmatic* view of model-based representation holds that models represent their targets in virtue of the cognitive uses to which human reasoners put them.

The turn to pragmatic (or 'practice-oriented') aspects of scientific models has been a fairly recent development. It acknowledges that models are the outcome of a process of model construction which is itself responsive to the context of inquiry. On this view, the question 'What is a model?' simply cannot be answered satisfactorily without a proper consideration of the activity of *modelling*, which, according to pragmatic theorists of models, is characterized by 'piecemeal borrowing' [Suárez & Cartwright, 2008, p. 63] from a range of representational resources. Thinking of models as standing in purely objective (e.g., informational) relations to one another, and to their target systems, would overlook the ineliminable role of beliefs, intentions, and cognitive interests of on the part of model users, as well as of the material constraints that come with the heterogeneous components that, typically, make up any real-world model of a phenomenon or target system. Shifting attention away from models and the abstract relations they stand in, towards modelling as a complex activity pursued by human agents, also involves – as Tarja Knuuttila puts it – a shift away from 'the model-target dyad as a basic unit of analysis' [Knuuttila, 2010, p. 142] towards a 'triadic' picture that acknowledges the equal importance of model, target, and user.

4. Rudolf Peierls and the culture of 'model-making'

Given the relative recency of the aforementioned turn towards scientific practice in philosophical accounts of models, it is perhaps prescient that Rudolf Peierls, in 1980, published a paper titled 'Model-Making in Physics', in which he speaks of a 'model-making habit' and attributes to physicists a tendency 'to use models of various kinds to aid their understanding of complicated physical situations' [Peierls, 1980, p. 3]. At the time the paper was published, Peierls was in his early seventies and had already spent six years in retirement; his breezy presentation of various models in physics is, therefore, less of a summary of state-of-the-art scientific modelling than a reflection on the proliferation of models in physics since the middle of the twentieth century.

Peierls's own career is closely linked to many of the main developments in twentieth-century physics. Born in 1907 in Berlin, Peierls studied at the universities of Berlin, Munich, and Leipzig, with stints in Switzerland and the Soviet Union, before escaping the deteriorating political situation in Germany by emigrating to Britain in 1933, where he eventually became a citizen in 1940. This allowed him to take up war work and led to his joining the Manhattan Project, though he remained critical of the pursuit of nuclear weapons (and later campaigned against their proliferation). His scientific work was unusually diverse, with Peierls being described, by the mathematician Herbert S. Green, as 'a highly competent, though not a notably creative mathematician; his principal interest was clearly in making calculations which would lead to a deep understanding of physical phenomena, and he was adept at finding approximations which gave trustworthy numerical results' [Green, 1999]. Unlike some of his more famous contemporaries, Peierls did not seek the public limelight, and his choice of research topics, too, reflects a decidedly 'middle-of-the-road' preference for soundness and applicability; this, I would argue, makes him a better representative than most for the bulk of research activity that constitutes post-war twentieth-century physics.

In 'Model-Making in Physics', Peierls adopts very much the perspective of a practicing scientists who suspends his immediate research agenda and instead reflects on the broader shape of his discipline. He does not explicitly set out to seek common ground between, say, physics and philosophy of science; neither does he promise a unified theory of how scientific models work. While he makes it his goal 'to examine the nature and purpose of [...] in some detail', he immediately acknowledges 'that different models serve quite different purposes, and they vary in their nature accordingly' [Peierls, 1980, p. 3]. This in itself is noteworthy since it shifts, almost effortlessly, the emphasis from the ontological question 'What is a model?' to the more pragmatic question of how models achieve their varied functions. Before summarizing Peierls's answer, however, it is worth emphasizing that he never intended his paper to be an up-to-date contribution to the philosophical debate. As a result, he makes no effort to engage with whatever philosophical debate of scientific models had developed by the time the paper was published – of which, by 1980, there had been a considerable amount. All the references are entirely to other papers in physics, and most of them refer to case studies Peierls is using for illustrative purposes, not to discussions of how models are being used in physics in general. While one might lament the lack of engagement with the extant philosophical literature, along with a number of conceptual infelicities on Peierls's part that a closer engagement with philosophy of science might have prevented, his text nonetheless deserves to be taken seriously. This is why, in what follows, I have decided to take Peierls's text at face value, treating it as an expression of a certain epistemic culture of model-based physics, and immersing myself in it, rather than taking him to task for, say, eliding various philosophical distinctions.

Peierls's taxonomy of models lacks hierarchy and systematicity, and he readily acknowledges that his choice 'of the categories, and the assignment of specific models to them, is of course very subjective'[2] (3), and that there is bound to be disagreement about individual assignments, yet not about 'the width of the spectrum' of cases considered. Peierls's choice of the first of the seven 'types' of models he distinguishes – '*Hypothesis ("Could be true")'* – already makes clear that he has no truck with extant philosophical distinctions, given that hypotheses are not usually lumped together with models. Yet, speaking as a scientific practitioner, Peierls notes that 'hypotheses are often called models', and any taxonomy of uses of models (as well as uses of the term 'model') had better comment on its relation to hypotheses. And, to be sure, models are often invoked by hypotheses that 'consist of a tentative explanation of a phenomenon'. Examples would be early models of the atom, such as the textbook models put forward by J.J. Thomson and Ernest Rutherford, which 'amount really to statements about the nature of the Universe which may or may not be correct' (4). Peierls's second type – '*Phenomenological model ("Behave as if…")*' – is more in line with established taxonomies in philosophy of science, though he is vague on the question of precisely what it takes for a model to count as 'behaving as if' it were the real target system. According to Peierls, in a phenomenological model 'a physical phenomenon [is] accounted for by a certain mechanism, but there is insufficient evidence to convince us that this is the correct explanation' (5). For Peierls, whether or not a model counts as phenomenological, appears to be less a matter of "saving the phenomena" (while remaining agnostic about what underlying processes might have brought about an observed phenomenon), than a matter of uncertainty about whether the proposed underlying mechanisms are, in fact, realized or not. Phenomenological models, then, are regarded as characteristic of research in its early stages. As a 'very characteristic example' (6), he discusses Pierre Weiss's model of ferromagnetism, which posits that elementary magnets contribute to the magnetization of a substance not only via their response to an external magnetic field, but also via 'a "molecular field" proportional to the number of magnets already aligned'. This simple model helped explain the existence of a transition temperature, the Curie temperature, 'at which the

[2] For the remainder of this section, numbers in round parentheses refer to the corresponding page number in [Peierls, 1980].

spontaneous magnetization goes continuously, but very steeply, to zero'. While later work revealed the model to be inadequate in a number of ways, it is 'still useful if we want a quick orientation on the likely behaviour of a ferromagnet in unfamiliar circumstances'. Phenomenological models, beyond merely reproducing observed phenomena and (sometimes) identifying potential causal mechanisms, also importantly play a way in *regulating and guiding inquiry* – a topic I shall return to in the final section.

The next two types of models – '*Type 3: Approximation (Something is very small, or very large)*' and '*Type 4: Simplification (Omit some features for clarity)'* – are again labelled in a somewhat misleading manner, in that they really refer to different (though potentially co-existing) methodological approaches. When Peierls refers to them as 'models', this is best understood elliptically as referring to models generated by the (predominant) use of one or the other. 'Approximation' – which, judging from the parenthetical characterization as 'Something is very small, or very large', includes the consideration of limiting cases – is required whenever no closed solutions exist to the model equations and is considered, by Peierls, to be an 'art' that is 'much more subtle than that of solving an equation exactly' (7). The example discussed by Peierls is that of linear response models, which describe how a target system responds to an infinitesimal disturbance. Once again he notes that, even where (by some measure) 'better' models exist, linear approximations have their legitimate uses: e.g., 'the stability of a system depends on the sign of its linear response coefficients for various possible disturbances' (8). At the same time, one must take great care to also respect the inevitable limits on when a system's response can be modelled as linear. For example, when calculating the electronic shell structure of the atom, higher-order calculations are important in order to lift degeneracy in the energy levels – contenting oneself with the linear approximation would miss out on key aspects of the physics of the atom. When to deploy the right sorts of approximations, Peierls argues, is a matter of 'judgement and experience'; for the purpose of model-making and using models successfully, we cannot abstract away from the context of inquiry.

'*Simplification*', or the omission, for clarity, of some (known) features of the target system ('*Type 4*' [9]), is likewise an important approach in the construction of models. Peierls's preferred example here is Peter Debye's model for the specific heat of solids. Debye essentially proposed a formula that interpolates between the (known) behaviour of solids at near-zero temperatures (when only a diminishing number of low-frequency vibrations of the solid are possible) and the maximum frequency of lattice vibrations (which, in reality, is determined by the lattice structure of the crystal, but which Debye chose 'so as to get the total number of modes right' [10], i.e. the maximum number of possible excitations for the total number of atoms in a crystal). This resulted in a model that is surprisingly useful even at intermediate temperatures. Yet the success of any given model also creates pitfalls, in particular for those who lose track of the model's limitations. One such case for the Debye model is the thermal behaviour of beryllium. For beryllium, the difference between the 'predicted' Debye curve and actual measurements – that is, the curve that results from subtracting one from the other – had a hump-like structure, which some researchers – incorrectly – interpreted 'as suggesting a transformation in this substance'. Yet this interpretation overlooked entirely that the Debye curve merely interpolates between two known constraints; its numerical values in-between make no claim to realism.

The fifth type, '*Instructive model (No quantitative justification, but gives insight*'), according to Peierls, achieves 'even greater simplification' at the cost of moving 'even further away from a realistic description', while 'still retaining enough similarity with the true situation to help understanding something about its nature' (13). This characterization, which Peierls acknowledges is 'less sharp than previous dividing lines', is admittedly vague: How much similarity with the true situation is 'enough', and how specific to the target system must our 'understanding something about its nature' really be? Yet, often what is needed at a certain point in inquiry is not numerical accuracy, or even qualitative similarity, but an estimation of, say, the expected order of magnitude of a process or phenomenon. This, Peierls argues, is just what such models are good at providing, for example in such cases as the mean free path model, transport phenomena, and conductivity: 'For general orientation [such a] model is so useful that it is being used all the time even by those who are familiar with is weaknesses and pitfalls', yet it stands to reason that there is, in turn, a very real risk of the model being used unthinkingly, by those who are less able to spot potential pitfalls.

Peierls's sixth type of model, '*Type 6: Analogy (Only some features in common)*', echoes Mary Hesse's account of scientific models as analogies (discussed in a previous section of this paper), though it is unlikely that Peierls would have closely engaged with Hesse's work. Peierls posits that, in many situations, we 'learn something about a physical system from the study of a simpler system which does not resemble it in all essentials, but has some of its typical features' (14). Interestingly, he quickly moves to the discussion of scientific examples, most of which – Debye's model for phonon scattering, the Ising model, the London model of superconductor – could have easily been fitted under one of the other categories. Had Peierls aimed for a more systematically ambitious taxonomy, he might have chosen analogy as an overarching framework that subsumes the various types of models – just as Hesse did – rather than as a separate category, or 'type', in its own right. Nonetheless, what is significant is Peierls's recognition that even models that are known to be fundamentally flawed – such as Ising's model of ferromagnetism, which ignores many aspects of quantum many-body systems that are known to be of the utmost importance in determining the collective behaviour (including phase transitions) in solids – can still function as a source of insight: 'Nevertheless much can be learnt from the model.' (15) Importantly, 'variants of the Ising model have served as a proving ground for methods and approximations in this field' (15). Once again, Peierls ranks pragmatic utility, and the way in which models – in spite of their flaws – can keep scientific research programmes progressive higher than their representational accuracy in absolute terms, as it were.

The final type of model, '*Type 7: Gedanken experiments (Mainly to disprove a possibility)*' covers thought experiments such as the Carnot cycle, Maxwell's demon, Heisenberg's gamma-ray microscope, and the Einstein-Podolsky-Rosen paradox. While again a non-standard usage of the term 'model', Peierls's decision to include thought experiments among the various types of models is nonetheless instructive. He notes that, by considering thought experiments, it is often possible to derive constraints on what is, and isn't, possible. Thus, in thermodynamics, considering the Carnot cycle 'can, for example, place limits on the efficiency obtainable from an engine working in a given temperature range' (16); the idea, then, is not that the

Carnot cycle could, or should, be brought about in the real world, but rather that any real-world process must respect certain constraints that are brought into sharp focus – albeit counterfactually – by the corresponding thought experiment. Sometimes, thought experiments do not establish (im)possibilities, but – like models used for instruction – serve illustrative purposes. This, Peierls argues, is the case with Heisenberg's γ-ray microscope, which imagines attempting to see an electron in a microscope: in order to achieve a sufficiently good resolution, one would need to use radiation of sufficiently short wavelengths – i.e. γ-rays – yet any encounter of an electron with such radiation would cause an uncontrollable change in its momentum, leading to an unavoidable trade-off between locating the electron and measuring its momentum – Heisenberg's famous uncertainty relation. Heisenberg's thought experiment 'was not used to prove anything, because the uncertainty relation could be deduced directly from the formalism of quantum mechanics' (16), but it helped physicists 'understand the nature of the new principle without recourse to the mathematical formalism' (17).

Peierls's seven model 'types' span different levels of inquiry, ranging from general methodological approaches – approximation and simplification – to specific contexts (e.g., instruction), uses (e.g., in order to 'save the phenomena'), and formats of representation (e.g., 'statements about the nature of the Universe which may or may not be correct'). One may deem his taxonomy haphazard and unsystematic – yet, arguably, no more so than the messy and disunified character of scientific practice. Indeed, it could be argued that the fact that Peierls foregoes any strongly normative stance – except for the recurring injunction to remain aware of the limitations of one's models – simply reflects the epistemic culture associated with 'model-making' in physics. Some models – because of their wide applicability, or because they feature prominently in shared curricula of physics education – constitute common ground, to which even proponents of competing research programmes can jointly retreat. Others are themselves hotly contested. And even where there is in-principle agreement on the utility of a particular approach, trade-offs between different desiderata – simplicity, numerical accuracy, generality, etc. – are the norm rather than the exception. As noted above in relation to the Debye model, some researchers place considerable (even excessive) faith in the realism of their models; others adopt a thorough-going instrumentalist attitude.[3] Yet, as in the case of the messy world of experimental physics, '[w]hen added together, these goings-on in a particular domain form what one might call an *epistemic culture*' [Knorr-Cetina, 1991, p. 107]. Usually, the term 'epistemic culture' has been associated with specific sub-disciplines in science. Karin Knorr-Cetina famously characterized the epistemic culture of particle physics in terms of its intensely collaborative research environment and its extreme reliance on computational methods, leading simultaneously to a 'relative loss of the empirical' and 'a loss of epistemic status of the individual' [Knorr-Cetina, 1991, p. 120], as compared with other subdisciplines in physics and other branches of science. One might worry that referring to model-making in physics as an 'epistemic culture' creates an illusion of unity and masks the great diversity of domains across which, as we have seen, models in physics are being deployed. At the same time, it has often been observed that models in physics 'travel' from one discipline to another: every physics student is familiar with the ubiquity of the harmonic oscillator equations in various seemingly unrelated branches of physics, and even models that were originally intended for highly specific target systems, such as the Ising model of ferromagnetism, have over time been applied to a wide range of phenomena, from spin glasses to systems of neurons. Without a shared body of tacit knowledge about when, and how, to deploy models successfully – without, that is, a shared culture of modelling – it would seem difficult to explain such mobility of models across disciplinary boundaries.

5. Representation, exploration, and regulation

Our discussion so far, like much of philosophical discourse on scientific models, has – almost unreflectively – used the idiom of *representation* in connection with how models function. It is indeed commonplace to find models characterized exclusively as 'tools for *representing the world*' [Giere, 1999, p. 44] and to measure the success of a given model by how well it matches physical reality. When a model is tailored to specific phenomena and is intended to

[3] Preferences for realism or instrumentalism among scientists are often fleeting; on this point, see [Gelfert, 2005].

represent specific target systems, such a view has a lot going for it. Who, in all seriousness, would doubt that scientists often help themselves to models as 'stand-ins' for real target systems which scientists know of, but which they cannot directly access or give a complete characterization of? The ability of scientific models to enable us to draw inferences about actual target systems is surely one of the great attractions of using models in scientific inquiry.

Yet it takes only a moment's reflection to realize that representing actual target system – or representation, *simpliciter* – is just one way in which models are being applied in science. One might think that this is because of the inevitable use of abstraction, idealization, and approximation in the construction of models, all of which render model descriptions strictly speaking false. Yet representations obviously do not need to be completely accurate or complete in order to represent their targets. Partial representation, and even misrepresentation, are entirely compatible with models being 'stand-ins' for actual target systems. After all, for a model to represent its target, we should not require that it be a perfect, or even a particularly good, representation.[4] What we should require, though, is that, for something to be a representation, it should have a target in the actual world – put crudely, it should be doing some *representing*. This is not to deny that there are some hard ontological questions that a full account of model-based representation ought to be able to address. How much can a putative representation get wrong about its target, while still being considered a representation of said target – rather than a failed, or vacuous, attempt at representing (as, arguably, in the case of phlogiston)? Furthermore, there have been sophisticated attempts to widen the range of admissible targets, e.g., by including fictions among them. Notwithstanding pronouncements to the effect that 'there is absolutely no difference in kind between fictional and real-object representation – other than the existence or otherwise of the target' [Suárez, 2004, p. 770], even such an inclusive approach does not adequately capture the more overtly non-representational uses of models.

In recent years, there has been a growing recognition that the activity of *scientific modeling* goes well beyond the task of deriving representations of real-world target systems. Recent papers in this vein speak of the activity

[4] For a critique of the ideal of perfection in scientific modelling, see [Teller, 2001].

of 'modeling without models' [Levy, 2015] or of 'models in search of targets' [Gelfert, 2018]. As Arnon Levy notes:

> When a model is proposed it might not be clear at first what target it is tied to, and there might be a period in which the right target is sought. But later, assuming the model is retained, this issue is usually clarified. [Levy, 2015, p. 796]

A similar sentiment was evident in Peierls's discussion of different model-types: Recall how Peierls discusses Weiss's model of ferromagnetism – which posited the existence of 'microscopic magnets' even in the absence of a theory of what might constitute these – as 'very characteristic' of early-stage modeling; similarly, in his discussion of the Ising model of ferromagnetism, he noted the tenuous nature of assigning target systems:

> The model is unrealistic for ferromagnetism, because if the atomic spin is greater than ½ it has more than two orientations, and if it is ½, quantum effects are not negligible. The model is slightly more realistic for alloys, except that in metallic alloys the interaction between atoms is in part mediated by the conduction electrons, and such an interaction is by no means limited to nearest neighbours. [Peierls, 1980, p. 15]

As it turns out, for a model to function as a tool of scientific inquiry, it need not – at least not initially – refer to any real-world target system in particular. Even in the absence of a uniquely identifiable target, 'much can be learnt from the model' [Peierls, 1980, p. 15].

It would be wrong, however, to think of this indeterminacy with respect to a model's target as solely due to initial confusion or ambiguity in the early stages of inquiry, which always must be – and eventually will be – rectified. Tailoring a model to a specific target may be a promising strategy when we have already developed a good grasp of what constitutes, and what causal factors might contribute to, the phenomena in question. Yet in the absence of comprehensive theoretical knowledge – that is, in the context of *exploratory research* – the varied tasks of stabilizing phenomena, delineating their causal substrate, separating signal from noise, etc. are hardly straightforward. Sometimes it may not even be desirable to prematurely focus on certain target systems at the expense of others. Much of scientific modelling serves the purpose of exploring theoretical relationships, establishing 'no-go' theorems (thereby exploring the modal structure of potential phenomena), providing proofs of principle (which need not, in fact, be instantiated) – in all of these cases, focussing on the goal of representing specific real-world target systems would run the risk of impeding, rather than furthering, our understanding of the science involved, as a closer look at exploratory modelling in science reveals.

Recent case studies of exploratory modelling in science have shown that, under conditions of exploratory research, models can function in a variety of ways: as starting points for future inquiry, as proofs of principle, as sources of potential explanations, and as a tool for reassessing the suitability of the target system. (See also [Gelfert, 2016, pp. 71-100].[5]) This list is meant to be neither exhaustive nor mutually exclusive; as already mentioned, sometimes a fruitful line of future inquiry can be identified by looking at a range of potential target systems and considering whether any of them (or any aspect of them) are captured by a given (e.g. mathematical) model. Interestingly, this is just what Peierls states with respect to the Ising model. Having noted that the Ising model's failure to successfully represent does not preclude learning from it, he continues as follows:

> For example the Onsager solution for the two-dimensional case [of the Ising model] demonstrates that the specific heat is not merely discontinuous at the critical point […], but tends to infinity as the critical point is approached either form below or from above. This helped in the development of the theory of phase transitions, which by now is a very sophisticated branch of statistical mechanics. Several variants of the Ising model have served as a proving ground for methods and approximations in the field. [Peierls, 1980, p. 15]

In other words, Peierls explicitly recognizes that models can serve as 'proofs of principle' and explore the structure of underlying theoretical frameworks, not *in virtue of*, but *instead of* representing actual target systems.

There is another way in which the utility of models in physics extends beyond their representational success. Models often 'give stability to scientific practice'

[5] The framework of 'exploratory modelling' has since also been fruitfully applied to case studies from bioengineering [Poznic, 2016], mathematical biology [Gelfert, 2018], astronomy [Wilson, 2017], and mesoscopic physics [Shech and Gelfert, this issue].

[Gelfert, 2015, p. 224] by serving a regulative purpose. As mentioned earlier, the same model templates – e.g. in the case of mathematical models, the same equations – may travel across disciplinary boundaries, adding to the cohesion of the scientific enterprise. Scientists themselves acknowledge that models are not only used to derive predictions about specific target systems, but, as Peierls puts it with regard to the Weiss model, are 'useful if we want a quick orientation on the likely behaviour of a ferromagnet in unfamiliar circumstances' [Peierls, 1980, p. 8]. Deploying models at crucial junctures, whether in order to gain a 'quick orientation' or contribute to the development of a serviceable theory, can be an effective way of giving direction to a given process of inquiry and set it on course for future successes. Making decisions about when to rely on models, and which model to use, is 'where judgement and experience comes in', specifically that of 'the experienced physicist' [Peierls, 1980, 8]. Who is to be credited with the requisite experience is determined, in part, by the recurring norms and directives associated with the prevailing epistemic culture. Criteria of model choice themselves evolve from the collective repetition of the acts of choosing (and, where appropriate, dismissing) models; over time, they become what may be called 'normative-directival complexes' [Moraczewski, 2014, p. 41], which feed into, and in turn are shaped by, scientists' collective practices of model-making.

There will always remain the unavoidable risk of mistaking one kind of model for another, and even the best model – if ever there could be such a thing – may, on occasion, lead us into error. But, as Peierls puts it in a later paper, 'one also has to guard against the opposite mistake, of being too timid in learning something from an approach whose basis is not formally established' [Peierls, 1987, p. 95]. Yet such is the fallible nature of all inquiry, and exploring the structure of the empirical world around us requires us to take a chance. Science is not for the timid.

References

D. Bailer-Jones, "Scientists' Thoughts on Scientific Models," *Perspectives on Science*, vol. 10, no. 3, pp. 275-301.

R.B. Braithwaite, *Scientific Explanation: A Study of the Function of Theory, Probability and Law in Science*, Cambridge: Cambridge University Press, 1968.

R. Carnap, „Foundations of logic and mathematics," in *Foundations of the Unity of Science* (vol. 1, eds. O. Neurath, R. Carnap, and C. Morris), Chicago: The University of Chicago Press, 1969, pp. 139-214.

A. Chakravartty, „Informational versus functional theories of scientific representation," *Synthese,* vol. 217, no. 2, pp. 197-213, 2010.

G. Contessa, „Editorial introduction to special issue," *Synthese,* vol. 2010, no. 2, pp. 193-195, 2010.

P. Duhem, *The Aim and Structure of Physical Theory*. (Transl. P.P. Wiener), Princeton: Princeton University Press, 1914/1954.

R. Frigg, S. Hartmann, „Models in science," *Stanford Encyclopedia of Philosophy*, 2012. [Online]. Available: plato.stanford.edu/entries/models-science/. [Accessed 10 February 2018].

A. Gelfert, "Mathematical rigor in physics: putting exact results in their place," *Philosophy of Science*, vol. 72 (2), 2005, pp. 723-738.

A. Gelfert, "Between rigor and reality: many-body models in condensed matter physics," in *Why More is Different: Philosophical Issues in Condensed Matter Physics and Complex Systems* (eds. B. Falkenburg and M. Morrison), Heidelberg: Springer, 2015, pp. 201-226.

A. Gelfert, *How to Do Science With Models: A Philosophical Primer*, Cham: Springer, 2016.

A. Gelfert, "Models in search of targets: exploratory modelling and the case of Turing patterns," in *Philosophy of Science: Between the Natural Sciences, the Social Sciences, and the Humanities* (European Studies in Philosophy of Science, vol. 9; eds. A. Christian, D. Hommen, N. Retzlaff, and G. Schurz), Heidelberg: Springer, 2018, pp. 245-269.

R. Giere, „Using models to represent reality," in *Model-based Reasoning in Scientific Discovery* (eds. L. Magnani, N. Nersessian, and P. Thagard), New York: Plenum Publishers, 1999, pp. 41-57.

N. Goodman, *Languages of Art*, Indianapolis: Bobbs-Merrill, 1968.

H.S. Green, "Review of *Selected scientific papers of Sir Rudolf Peierls*," *Mathematical Reviews*, MR1632685 (99e:01035), 1999.

M. Hesse, *Models and Analogies in Science*, London: Sheed and Ward, 1963.

K. Knorr-Cetina, "Epistemic cultures: forms of reason in science," *History of Political Economy*, vol. 23, pp. 105-122, 1991.

T. Knuuttila, "Some consequences of the pragmatist approach to representation," in *EPSA Epistemology and Methodology of Science* (eds. M. Suárez, M. Dorato, and M. *Rédei)*, Dordrecht: Springer, 2010, pp. 139-148.

A. Levy, "Modeling without models," *Philosophical Studies,* vol. 172, no. 3, pp. 781-798, 2015.

K. Moraczewski, *Cultural Theory and History: Theoretical Issues*, Poznań: WNS, 2014.

R. Peierls, "Model-making in physics," *Contemporary Physics*, vol. 21, no. 1, pp. 3-17, 1980.

R. Peierls, "Models, hypotheses and approximations," in *New Directions in Physics: The Los Alamos 40th Anniversary Volume* (eds. N. Metropolis, D.M. Kerr, and G.-C. Rota), San Diego: Academic Publishers, 1987, pp. 95-105.

M. Poznic, "Modeling organs with organs on chips: scientific representation and engineering design as modeling relations," *Philosophy & Technology*, vol. 29, no. 4, pp. 357–371, 2016.

E. Shech and A. Gelfert, "The exploratory role of idealizations and limiting cases in models", *Studia Metodologcizne* (this issue).

M. Suárez, "An inferential conception of scientific representation," *Philosophy of Science,* vol. 71, no. Proceedings, p. 767–779, 2004.

M. Suárez and N. Cartwright, "Theories: tools versus models," *Studies in History and Philosophy of Modern Physics,* vol. 39, no. 1, pp. 62-81, 2008.

P. Teller 2001, "Twilight of the perfect model model," *Erkenntnis*, vol. 55, pp. 393-415.

J. von Neumann, „Method in the physical sciences," in *Collected Works Vol. VI. Theory of Games, Astrophysics, Hydrodynamics and Meteorology*, ed. A.H. Taub, Oxford, Pergamon Press, 1961, pp. 491-498.

S.R. Weart, "The solid community," in *Out of the Crystal Maze: Chapters from the History of Solid-State Physics* (eds. L. Hoddeson and E. Braun, J. Teichmann, and S. Weart), New York: Oxford University Press, 1992, pp. 617-669.

K. Wilson, "The case of the missing satellites," *Synthese* (Online First, 2017, https://doi.org/10.1007/s11229-017-1509-6).

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