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Strategies of Model-Building in Condensed Matter Physics: Trade-Offs as a Demarcation Criterion Between Physics and Biology?

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Abstract

This paper contrasts and compares strategies of model-building in condensed matter physics and biology, with respect to their alleged unequal susceptibility to trade-offs between different theoretical desiderata. It challenges the view, often expressed in the philosophical literature on trade-offs in population biology, that the existence of systematic trade-offs is a feature that is specific to biological models, since unlike physics, biology studies evolved systems that exhibit considerable natural variability. By contrast, I argue that the development of ever more sophisticated experimental, theoretical, and computational methods in physics is beginning to erode this contrast, since condensed matter physics is now in a position to measure, describe, model, and manipulate sample-specific features of individual systems – for example at the mesoscopic level – in a way that accounts for their contingency and heterogeneity. Model-building in certain areas of physics thus turns out to be more akin to modeling in biology than has been supposed and, indeed, has traditionally been the case.

1. Introduction

The present paper contrasts and compares strategies of model-building in the biological and the physical sciences. In particular, it discusses what role trade-offs play in the process of model construction. This question has received perhaps the most attention in population biology, from both scientists and philosophers of biology, who have debated the question of whether there might be necessary trade-offs between the theoretical desiderata associated with their mathematical models.[1] In particular, it has been argued that population models in biology – unlike, for example, many of the mathematical models used in classical physics – do not allow for the simultaneous maximization of the desiderata of generality, precision, and realism. This has given rise to the suggestion

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[1] In economics, too, there has been significant discussion of trade-offs in model-building, not least in the controversy over the status of ‘unrealistic’ assumptions; see, for example, Bronfenbrenner (1966).

that the existence of trade-offs in model-building might function as a demarcation criterion between biology and other – perhaps 'more fundamental' – sciences.

In the present paper, I wish to challenge this suggestion. While it is true that trade-offs of the sort mentioned above have been most salient in biology, this, I argue, is at least partly a side effect of the selective emphasis that has traditionally been placed – especially in physics – on the investigation of classes of target systems (and phenomena) that display a high degree of homogeneity (in a specific sense to be identified in Section 4). Whereas in physics such homogeneous classes of systems and phenomena can often be found in nature, or can be readily singled out in the laboratory, in biology one is typically dealing with *evolved* systems that exhibit considerable natural variability. Recent developments in condensed matter physics, however, have led to ever more sophisticated experimental, theoretical, and computational methods, and I argue that, when viewed in their entirety, these developments suggest that model-building in physics may be becoming more akin to modeling in biology than has been supposed and, indeed, has traditionally been the case. While 'traditional' modeling strategies in physics will continue to have their place in attempts to explain the behavior of highly homogeneous systems, as physics – starting from condensed matter physics – takes it upon itself to study systems of ever greater complexity, more of it will be affected by the kinds of trade-offs that have traditionally been associated with models in biology.

The structure of the paper is as follows. In the next section (Section 2), I shall discuss the origins of the debate, within population biology, about trade-offs in model-building, specifically in relation to the desiderata of generality, precision, and realism. In Section 3, I shall turn to condensed matter physics by contrasting two strategies of model-building found in theoretical analyses of the phenomenon of superconductivity. The divergence of these two approaches, I argue, constitutes evidence for the existence of theoretical and methodological trade-offs in condensed matter physics. Section 4 discusses the – sometimes latent, sometimes explicit – suggestion that the existence of trade-offs can function as a disciplinary demarcation criterion of sorts. Section 5, finally, makes the case that recent developments in the physics of nano-scale phenomena, such as the analysis of 'fingerprint effects' in mesoscopic systems, are beginning to render obsolete any contrast that may have existed between biology and the physics of complex phenomena, in terms of their unequal susceptibility to trade-offs in modeling.

2. Trade-offs in model-building: Origins of the debate

The discussion of trade-offs in model-building is an interesting case where a debate that should be at the heart of general philosophy of science has emerged from a specific debate within the philosophy of biology. In his 1966 paper "The Strategy of Model-Building in Population Biology", Richard Levins argues that the models used in population biology are subject to inescapable constraints, insofar as certain theoretical desiderata – in particular, generality, precision, and realism – cannot simultaneously be maximized. There are, thus, significant restrictions on what individual models in population biology can achieve. For example, if one tailors a model exactly to a particular ecosystem, by including in detail all operative causal mechanisms (e.g., the various predator-prey relationships) as well as precise measurements of significant parameters (e.g., of reproduction rates and the nutritional needs of each species), this will inevitably restrict the generality of the model – if successful, it will pick out one, and only one, real target system in the world. This suggests that the theoretical desiderata of generality, precision, and realism 'trade off' against one another. This forces some tough choices on population biologists, who must choose between, for example, constructing a model that is detailed and realistic, yet lacks generality, or trading either detail or quantitative precision for an increase in the model's range of applicability.

Levins's claim that not all three desiderata can be optimized simultaneously leads to a natural classification of modelling strategies into three types, depending on which desideratum 'loses out' in the process of optimizing the other two. Thus, in 'Type I' cases, the biologist tailors his model to the specific empirical detail and causal mechanisms of a particular system, thereby sacrificing generality for realism and precision. In 'Type II' modelling, by contrast, realism is sacrificed for precision and generality; models of this type are characterized by 'general equations that give precise outputs, but rely on unrealistic idealisations and assumptions' (Matthewson 2011). 'Type III' models, finally, sacrifice precision for generality and realism; while such models do not lend themselves to making quantitatively precise predictions, they are thought to be true to the dominant causal relationships that exist in the general class of systems whose behavior they are meant to explain. It is the latter – 'type III' – models that Levins is often thought to have promoted (cf. Justus 2005, 1273); however, as Matthewson points out, precision, realism, and generality are all equally considered to be desiderata of modelling. This suggests that Levins's account gives rise to a genuine *pluralism* about the goals of model-building; any preference Levins himself may have had for 'type III' models may simply have been due to a desire to rehabilitate generality and realism against a perceived overemphasis on precision as the ultimate goal of model-building in Levins's own discipline, population biology.

Why might one expect the simultaneous maximization of precision, realism, and generality to be unattainable in a domain like population biology? For one, such models would involve 'perhaps 100 simultaneous partial differential equations' (Levins 1966, 421), each with numerous parameters, to be obtained from lengthy field studies. Even if it were possible to obtain accurate measurements of the relevant parameters, Levins argues, the resulting equations would be 'insoluble analytically and exceed even the capacities of good computers' (Levins 1966, 421). Furthermore, in those rare cases where solutions might be within reach, interpreting the results might still be beyond the cognitive capacity of finite human reasoners. As Jay Odenbaugh has argued, the fact that we have difficulty making sense of comparatively simple models does not bode well for the interpretation of complex, 'photographically perfect' models (Odenbaugh 2003, 1498-1499).

Whether Levins regards trade-offs in population biology as the result of contingent – though possibly insurmountable – cognitive or computational limitations, or as persisting even if perfect knowledge and unlimited computational resources were available, is a question that has received extensive discussion elsewhere (see Orzack and Sober 1993, Odenbaugh 2003). Here, I simply wish to challenge the view that trade-offs are specific to models of biological systems, as opposed to physics and chemistry, which, it is claimed, are not subject to constraints on the simultaneous maximization of theoretical desiderata.

3. Model construction in condensed-matter physics

In virtually all cases of condensed matter phenomena that have traditionally received attention, what physicists have aimed to explain is the macroscopic behavior of systems consisting of a large number of strongly interacting particles, such as electrons. In order to overcome the complexity of interactions in any real many-particle system, physicists turn to simplified *many-body models*, which typically purport to be partial representations of what goes on at the microscopic level. Alternatively, they may dispense with the illusion of giving a description of microphysical reality altogether, and characterise the macroscopic processes in terms of *phenomenological models*. Whereas many-body models are typically characterised by a quantum Hamiltonian (the equivalent to a classical energy function, which, from the perspective of fundamental theory, should fully determine the system's behavior), *phenomenological* models may be the result of an '*ad hoc* combination of considerations from thermodynamics, electromagnetism, and quantum mechanics itself' (Cartwright 1999, 263). In the present section, I wish to argue that such divergent approaches to model construction are closely associated with the existence of theoretical and methodological trade-offs in condensed matter physics.

3.1. Phenomenological models: The example of the Ginzburg-Landau model

In a series of papers, Nancy Cartwright has defended a view of model construction as a multifaceted process that goes beyond mere application of (or approximation to) fundamental theory. Models add to science in a way that the traditional focus on scientific theories cannot account for; at the same time, this means that, in order 'to get models that are true to what happens we must go beyond theory' (Cartwright 1999, 243). Cartwright rejects the semantic and the syntactic view of models in equal measure, insofar as both are versions of what she calls the 'vending machine view' of how models and theories interrelate in scientific inquiry. On this view, 'the theory is a vending machine: you feed it input in certain prescribed forms for the desired output; it gurgitates for a while; then it drops out the sought-for representation, plonk, on the tray, fully formed, as Athena from the brain of Zeus' (ibid., 247). Such a view, Cartwright claims, is incapable of accounting for the variety of uses to which models are put in scientific practice, since it regards the model user's creative input as limited to two stages: first, the 'eyeballing' of the scientific phenomenon, 'to see what can be abstracted from it that has the right form and combination that the vending machine can take as input'; second, since one can never actually build the machine that 'just outputs what the theory should', the need for 'either tedious deduction or clever approximation to get a facsimile of the output the vending machine would produce' (ibid.). This 'vending machine' view, Cartwright claims, contrasts with real science, which does not limit the role of modelling to the 'eyeballing' of phenomena or ingenious approximations to fundamental theory, but gives it a 'creative and cooperative treatment' (ibid.); as Cartwright sees it, 'models require a cooperative effort', by which 'knowledge must be collected from where we can find it' (ibid., 241).

As an illustration of her argument, Cartwright chooses an example from condensed matter physics, the Ginzburg-Landau model of superconductivity. Superconductivity was first discovered in 1911 by Hans Kamerlingh Onnes, who studied the electrical resistance of pure metals at temperatures only a few degrees above absolute zero and found that, for certain materials, the resistance drops abruptly to zero below a certain critical temperature. This striking behavior Vitaly Ginzburg and Lev Landau set out to explain. Their model is a textbook example of a phenomenological model, in that it does not purport to give a microphysical explanation of the phenomenon of superconductivity, but instead is formulated entirely in terms of macroscopic properties, with the help of general thermodynamic results and relations. (The alternative approach, which models superconductivity as the collective outcome of processes at the microscopic level, resulted in the famous BCS model, named after its proponents Bardeen, Cooper, and Schrieffer; see next sub-section.)

Various macroscopic theories were initially proposed to explain superconductivity, most notably the homogeneous theory of brothers Fritz and Heinz London in 1935 (which accommodated measurable regularities such as the sudden expulsion of magnetic flux from a superconductor, known as the Meissner effect), the nonlocal theory of Brian Pippard (1950), and finally the Ginzburg-Landau theory, proposed in 1950. The latter was itself based on a more general theory, developed by Landau in 1937, which explained second-order phase transitions in fluids in terms of the minimization of the Helmholtz free energy. Drawing on these earlier electromagnetic and thermodynamic theories, Ginzburg and Landau conceived of the conducting electrons as constituting a fluid that could appear in two phases, a superconducting phase and a normal (non-superconducting) phase. To this they added certain quantum-mechanical considerations, in order to account for the observation that the motion of the 'electron fluid' is affected by the presence of magnetic fields.

Insofar as Ginzburg-Landau theory builds on earlier theoretical descriptions of the phenomenon (the classical London equations) and unites them with new theoretical concepts (the quantum wave function), it is indeed the product of a ‘creative and cooperative’ approach that Cartwright sees as characteristic of phenomenological models. At the same time, by drawing on such disparate realms as thermodynamics, (classical) electromagnetics, and quantum mechanics, it helps itself to theoretical knowledge wherever it is to be found. This last point, especially, brings out the *constructive* character of the model – which, strictly speaking, cannot be ‘derived’ from any one of the theories involved: Models such as the Ginzburg-Landau model ‘are not *models of* any of the theories that contribute to their construction’ (Cartwright 1999, 244; italics added).[2]

In addition to their heterogeneity in terms of ingredients from different fundamental theories, what characterizes phenomenological models is a concern for *accurate prediction* and *empirical adequacy*. At one level, this merely restates the platitude that ‘it is the job of any good science to tell us how to predict what we can of the world as it comes and how to make the world, where we can, predictable in ways we want it to be’ (Cartwright 1999, 243); at another level, however, it acknowledges an important observation about scientific practice: namely that, when it comes to predictive accuracy, carefully fine-tuned phenomenological models often fare better than more ‘fundamental’ models that have been derived from what is thought to be the ‘underlying’ theory. For example, in the case of high-temperature superconductivity, physicists ‘are still very far from having a generally agreed microscopic model’ (Leggett 2006: 134), yet ‘substantial progress has been achieved’ in using macroscopic Ginzburg-Landau theory ‘to calculate the observable electromagnetic properties’ (ibid.). Whereas microscopic models attempt to answer the question ‘How does it work?’, phenomenological models start from questions such as ‘What are the sets of phenomena that naturally occur?’ or, more generally, ‘What are the world’s possible manifestations?’.[3] Just as, on Cartwright’s interpretation, phenomenological (rather than fundamental) *laws* are ‘the complicated, messy laws which describe reality’ (Cartwright 1983, 129), so phenomenological *models* – not their fundamental counterparts which purport to explain phenomena as the collective effect of microscopic constituent parts – capture reality in its full empirical detail.

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[2] There has been considerable debate about whether the case of superconductivity supports Cartwright’s claims, or whether it can be accommodated by theory-driven accounts of modeling. (For a defense of the latter claim, see French and Ladyman 1997.) At the same time, as Cartwright points out in a joint paper with Mauricio Suárez, the position she and her collaborators defend has sometimes been misinterpreted as an outright rejection of any constraining role of theory, when in fact it only asserts ‘that theories function as tools, not as sets of models already adequate to account for the startling phenomena that reveal their power’ (Suárez and Cartwright 2008, 66).

[3] I am borrowing this way of contrasting phenomenological and mechanism-based models from (Krieger 1981, 427).

3.2. Microscopic many-body models: The BCS model of superconductivity

An electric current is the result of charge carriers moving through a medium. In metals, conductivity is due to the mobility of conduction electrons that move freely through the periodic crystal lattice of (inert) positive ions. One might think, then, that superconductivity should be the sole result of interactions between electrons. Indeed, this idea is what earlier theories of superconductivity were based on. However, the later discovery of the *isotope effect*, according to which the critical temperature at which electrical resistance vanishes depends strongly on the isotopic mass of the substance, showed that the crystal lattice must somehow be involved in bringing about the superconducting state. Two theoretical ideas preceded the formulation of the microscopic BCS model of superconductivity.

First, it was shown that electrons, which would normally repel each other, may, when in a crystal, under certain conditions experience an attractive force; second, as Cooper demonstrated, electrons with opposite momentum can form correlated pairs (now known as 'Cooper pairs'), allowing them to interact with each other via the vibrations of the crystal lattice (phonons), thereby changing their individual momenta without varying the total (zero) momentum of the electron pair. This mode of interaction, in the presence of the lattice potential, may lead to an overall *decrease* of total potential energy that is greater than the increase in *kinetic* energy associated with the electrons' moving about in the crystal (thus carrying an electric current). In other words, the ground state of a system – that is, the state in which the system is most 'energy-efficient', as it were – may correspond to a situation in which some electrons move about freely in Cooper pairs, rather than each being bound to individual atoms in the crystal lattice. If this is the case, the substance will display superconducting behavior.

The BCS model of superconductivity reflects this microscopic picture of how electrons behave in a superconductor, by stipulating that the behavior of the system as a whole can be modeled as the collective effect of a small number of separable mechanisms. In the case of the BCS model, there are three major additive terms, each representing a posited microscopic process, which together make up the system's Hamiltonian. Thus, one finds contributions that represent the movement of all electrons through the crystal potential field, the Coulomb repulsion between electrons (partially 'screened' off by the positive lattice ions), as well as the phonon-mediated electron-electron interaction. The first two contributions are not specific to the BCS model of superconductivity: all conduction electrons in a metal ('Bloch electrons') have a certain kinetic energy, associated with their movement, and experience a periodic lattice potential. Likewise, all electrons in close proximity to one another will experience some degree of mutual Coulomb repulsion; how much of it is 'screened off' depends on the geometry of the lattice. The geometry of the crystal lattice is itself an important, though sometimes overlooked, ingredient of the model: after all, each of the additive terms of a quantum many-body Hamiltonian is itself the sum over all entities involved in the process in question; however, which particles we need to sum over, depends partly on the stoichiometry of the crystal lattice.

Where genuinely new content enters the BCS model, is in the last term of the Hamiltonian, which specifies an effective (indirect) electron-electron interaction. Unlike the screened Coulomb potential, this electron-electron interaction does not arise from any properties the electrons have either intrinsically or because of immersion into a uniform crystal; rather, it arises from dynamic interactions between electrons and the quantized lattice vibrations (‘phonons’). The fundamental idea is that an electron passing through a crystal deforms the lattice in its immediate neighborhood. In the formalism of quantized lattice phenomena, deformation is represented microscopically as the absorption or emission of phonons (corresponding to the intensity of particular normal modes of vibration). A second electron passing by may then ‘register’ this lattice deformation and react to it. This results in an effective – indirect, phonon-mediated – electron-electron interaction, which is independent of the usual Coulomb interaction and, therefore, need not be repulsive. Indeed, it is the emergence of an *attractive* (indirect) electron-electron interaction that is credited with bringing about the formation of Cooper pairs which, on the BCS model, are the microscopic basis of the phenomenon of superconductivity.

The piecemeal character of Hamiltonians – the fact that they consist of additive terms, each of which represents one supposedly fundamental microscopic mechanism or process – is a general feature of the construction of quantum many-body models. Thanks to mature mathematical formalisms, such as the formalism of creation and annihilation operators (which ‘simulate’ the addition and removal of individual particles to or from a collective many-body quantum state), theoretical physicists can model individual processes, such as the movement of an electron (which may be modeled as its ‘annihilation’ at one lattice site, together with its re-emergence, or ‘creation’, at another point in the crystal).[4] While this constructive character of many-body models sits well with Cartwright’s defence of the element of creativity in model-building, it poses a challenge to the view that model-building must ultimately be closely tied to empirical phenomena. Recall Cartwright’s defense of the Ginzburg-Landau model, which drew on empirical regularities, combined them with considerations from electrodynamics, thermodynamics, and quantum mechanism, and issued in the empirically meaningful prediction of two characteristic lengths: the *correlation length*, which describes the scale of thermodynamic fluctuations in the superconducting phase, and the *penetration depth*, which describes the typical distance to which an external magnetic field can penetrate into a superconductor. By contrast, the BCS model, like other quantum many-body models, offers little to go on in terms of easily accessible empirical content. Its component parts are abstract representations of posited fundamental mechanisms, which

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[4] On the notion of a ‘mature mathematical formalism’, see (Gelfert 2011: 284-285).

may or may not be related to macroscopically observable quantities. This would suggest that one should be careful not to put too much weight on the theoretical interpretations of a model Hamiltonian's constituent parts.

Cartwright tries to preempt such criticism by emphasizing that, when it comes to constructing microscopic many-body Hamiltonians, it would be wrong to think that any choice of constituent terms is as good as any other. In particular, there are strict limits to how we may assign Hamiltonians to specific mechanisms or processes:

> When the Hamiltonians do not piggyback on the specific concrete features of the model – that is, when there is no bridge principle that licenses their application to the situation described in the model – then their introduction is *ad hoc* and the power of the derived prediction to confirm the theory is much reduced. (Cartwright 1999, 264)

Specific knowledge of the target system – of the target situation which the model is meant to describe – thus always trumps whatever other reasons one might have for positing specific mechanisms and modelling them separately, as contributions to the system's overall Hamiltonian. Cartwright is explicit in her injunction 'not to think of the models linked to Hamiltonians as picturing individually isolatable physical mechanisms', as this might mislead us in several ways. First, Cartwright argues, 'it could dispose one to a mistaken reification of the separate terms which compose the Hamiltonians'. Sometimes, as in the BCS example, one may be justified in interpreting separate terms of a Hamiltonian as representing distinct physical mechanism, but more often than not 'the break into separable pieces is purely conceptual' (ibid., 261). All modeling, for Cartwright, is subject to the constraint that we 'need ways to link the models to the world'. Merely 'guessing' a Hamiltonian, even if we are intrigued by the microscopic processes that we seem to be able to attribute to its constituent parts, does not ensure that it is linked to the world of phenomena in the right way, for 'it is not enough to count a description as a correct representation of the causes that it predicts the right effects; independent ways of identifying the representation as correct are required' (ibid., 262).

Yet, the construction of the BCS model – which Cartwright approves of – does appear to involve a fair amount of legitimate guesswork, based on intuitions about what kinds of microscopic mechanisms might be expected in the materials that display superconductivity. How might one deal with this apparent tension in Cartwright's account? First, one might point out that the BCS model is part of essentially the same overall research programme as the Ginzburg-Landau model, and as such operates under the same empirical constraints as its phenomenological predecessor; indeed, it is widely regarded as one of the successes of the BCS model that it reproduces some of the very findings (such as the Meissner effect) that also guided the initial construction of the phenomenological Ginzburg-Landau model. Furthermore, it gives theoretical meaning, albeit retrospectively, to the Ginzburg-Landau model, insofar as it offers a microscopic interpretation to the order parameter $\Psi(\mathbf{x})$ involved in the transition to the superconducting state. Whereas in the Ginzburg-Landau model, $\Psi(\mathbf{x})$ plays a largely auxiliary role, as a complex quantity whose functional is the free energy, in the BCS model $\Psi(\mathbf{x})$ can be interpreted more directly as a measure of the presence of Cooper pairs moving about in the superconductor.

Interestingly, Cartwright's own defense of the BCS model adopts a different strategy. What makes the BCS Hamiltonian acceptable, despite its piecemeal character, is that each contribution to the Hamiltonian corresponds to one of a set of 'basic interpretative models' that have been studied independently and are well-understood, both on theoretical grounds and in *other* empirical contexts (ibid., 264). These basic interpretative models are the textbook examples of the central potential, scattering, the Coulomb interaction, the harmonic oscillator, and kinetic energy; indeed, according to Cartwright, quantum theory itself 'extends to all and only those situations that can be represented as composed of central potentials, scattering events, Coulomb interactions and harmonic oscillators' (and possibly a small number of others that may in due course be added to our 'catalogue of interpretative models'; ibid., 265). The use of those five or so stock examples, Cartwright argues, is licensed by 'bridge principles' which help 'make the predictions about what happens intelligible to us' (ibid., 246). The main point is that 'with each new case it is an *empirical* question whether these models, or models from some other theory, or no models from any theory at all will fit' (ibid., 266; italics added). Only when we construct phenomenological models 'bottom up', in a way that incorporates empirically observable regularities, *or when we rely on the very small number of independently licensed 'stock interpretative models' as building blocks for more complex models*, do we have any assurance at all that our attempts at model construction will be true to the world of phenomena.

There is some tension between Cartwright's goal of granting phenomena primacy over theories, and her insistence that 'if we wish to represent a situation within quantum theory – within the very quantum theory that we prize for its empirical success – we must construct our models from the small stock of features that quantum theory can treat in a principled way' (ibid., 279). If, as evidenced by scientific practice, actual scientific models do not usually fit the traditional philosophical account of theoretical models as ideal models (of theories) standing in need of subsequent 'de-idealization', then one may very well ask why, in the case of quantum Hamiltonians, one should be limited to those component models that are licensed by fundamental theory. In the remainder of this section, I shall focus on an alternative view of what should guide the construction of many-body models, before arguing that the contrast between the two viewpoints raises the spectre of a more fundamental trade-off affecting model construction.

3.3. Empirical detail vs. mechanism-based understanding

On the alternative viewpoint I have in mind, models are regarded primarily as mathematical structures (or, if one wants to avoid overtones of talk about 'mathematical entities', as *possible outputs of a mature mathematical formalism*, such as the formalism of second quantization; see Gelfert 2011: 282). One may then construct a 'many-body model' by imagining an abstract (infinite, *n*-dimensional) lattice of a certain geometry, with certain (well-formed) mathematical expressions associated with each lattice point, and define a 'Hamiltonian' as the sum of all those individual contributions. Whether or not this 'Hamiltonian' is indeed the Hamiltonian of a real physical system, or an approximation of it, is not a consideration that enters at this stage of model construction. One might even define higher-dimensional ($d>3$) models in this way, fully recognizing that such models could not possibly describe any physical arrangement in real space. There are many reasons why one may find it useful, in the context of scientific inquiry, to construct such models: for example, one may wish to test the reliability of numerical methods for various geometries, before applying them to models of actual systems; or one might investigate abstract models with an eye toward 'customizing' them for future representational uses. As Sang Wook Yi has argued, mathematical models that lack immediate physical interpretations may nonetheless have important exploratory uses:

> One of the major purposes of this 'exploration' is to identify what the true features of the model are; in other words, what the model can do with and without additional assumptions that are not a part of the original structure of the model. (Yi 2002, 87)

Exploring the intrinsic features of a model thus 'helps us shape our physical intutitions about the model', well before these have acquired the status of 'canonical' intuitions, supported by the 'successful application of the model in explaining a phenomenon' (ibid.). A good example of this is the Lenz-Ising model, whose gradual journey 'from relative obscurity to a prominent position in modern physics' (or, more starkly, 'from irrelevance to relevance') has been explained in terms of changing perceptions 'of the model's ability to yield physical insight' (Niss 2009: 243) – perceptions that were heavily shaped by the study of intrinsic (e.g., mathematical) features of the Lenz-Ising model, irrespective of its empirical accuracy.[5]

At the same time, empirical success at describing a phenomenon, irrespective of the underlying microscopic mechanisms that drive it, is not usually something that, by itself, inspires confidence in the truth of the model. As Yi points out, '[t]here are many cases in [condensed matter physics] where physicists hesitate to claim "understanding" of a controversial phenomenon by a certain model despite the impressive empirical success of the model' (ibid., 89). For example, physicists usually discount the value of the Gaussian model (which models couplings between neighboring spins as following a Gaussian distribution) in spite of its excellent empirical adequacy in describing system behavior in the high-temperature limit, on the ground that 'the interactions of the model are "unphysical" and that the model becomes "meaningless" (not just empirically

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[5] For an extensive historical survey that illustrates this point at virtually every turn of the Lenz-Ising model's career in theoretical physics, see (Niss 2009).

inadequate) in the low temperatures' (ibid., 84f.). The reverse, it should be added, also holds: a model may be regarded as being of explanatory value, and as conferring understanding, even when it is unclear whether it is capable of reproducing in detail the phenomenon it was initially meant to describe. This is the case with other many-body models, such as the Hubbard model. Initially introduced in 1963 in order to explain ferromagnetic behavior in systems with itinerant (electron) spins, it was not until the advent of quasi-exact computer simulations in the 1990s, that the Hubbard model was shown to be capable of representing ferromagnetic behavior, at least in limiting cases. (See, for example, Gelfert 2009, 514-17.) In the 30 years in between, the Hubbard model continued to drive research in condensed matter physics, because the component parts of its Hamiltonian – a kinetic term allowing for tunneling ('hopping') of particles from one lattice site to another, and a potential term representing an on-site interaction – were seen as a minimal representation of the most likely processes at the microscopic level. The case of the Hubbard model is by no means an extreme example: often, the 'exploratory' phase of understanding a proposed many-body model, and cultivating intuitions about the interplay of the microscopic mechanisms it is designed to represent, is drawn out over many years; whether the model will in the end match an empirical phenomenon in many cases remains an open question.

The two accounts I have sketched, of how to think about model construction, are not necessarily incompatible. Perhaps the exploratory phase of model construction is simply what it takes, on Cartwright's account, for a new 'basic bridge principle' to be established. After all, Cartwright concedes that we may expect more basic stock models (licensed by bridge principles) 'to be added as we move through the theory net from fundamental quantum theory to more specific theories for specific topics' (1999: 265). Similarly, Yi's claim that even when faced with 'fantastic predictive success by a model, physicists usually hesitate to claim they understand the phenomenon without understanding the model itself' (Yi 2002: 84), is simply a roundabout way of acknowledging that physicists do not always take too seriously the microsopic processes they posit in their Hamiltonians. However, it seems hasty of Cartwright to dismiss as 'a mistaken reification' the tendency of many-body physicists to interpret different components of their models as 'picturing individually isolatable physical mechanism' (Cartwright 1999: 261); for, while such interpretations are necessarily tentative, they need not be naïve: in many cases, it is *because* Hamiltonian parts can be interpreted literally, drawing on the resources furnished by fundamental theory as well as by (interpreted) domain-specific mathematical formalisms, that they generate *understanding*. This applies both to the mathematical model itself and, more often than not, to the target system it may eventually be used to describe.

What this suggests is that the desiderata of *empirical detail* – whether a model adequately describes the empirically accessible aspects of a phenomenon – and *explanatory value* (that is, our ability to identify, and understand, the fundamental mechanisms that drive the phenomenon in question) often come apart in model construction.[6] If one is interested in describing the precise behavior of a complex system, a phenomenological model may be more easily 'fitted' to empirical data and, given the right choice of parameters, may turn out to be more predictively successful. However, if what one is after is a microphysical explanation of macroscopically observable phenomena, then a many-body model that conjectures the existence of certain isolatable causal mechanisms and processes may be preferable, even when it lacks the empirical success of a more phenomenological model.

Acknowledging that empirical success (prediction) and explanatory success (understanding) may come apart is, of course, one thing, asserting that there is a trade-off between them is quite another. There are certainly *prima facie* reasons why one might expect a trade-off to occur. Explanatory models, such as microscopic many-body models, are often deployed in order to account for poorly understood phenomena (such as specific phase transitions); a premature focus on empirical success (e.g., the exact value of the transition temperature) might lead one to add unnecessary detail to a model, before one has developed a sufficient understanding of which microscopic processes influence the macroscopically observable variables. As Robert Batterman puts it (citing a condensed matter theorist, Nigel Goldenfeld):

> On this view, what one would like is a good *minimal model* – a model 'which most economically caricatures the essential physics' (Goldenfeld 1992: 33). The adding of details with the goal of 'improving' the minimal model is self-defeating – such improvement is illusory. (Batterman 2002: 22)

The view is certainly a wide-spread one among those working in the physics of complex systems. Thus, Alexander Rueger and David Sharp, in their analysis of nonlinear dynamics, make a similar observation:

> There is thus a trade-off between a theory's explanatory power and its (potential) truth: the more efficient a theory is in explaining or organizing a large variety of different phenomena, the less can it be true or state the facts. (Rueger and Sharp 1996: 96)

A major difference lies, of course, between 'adding details' to a microscopic model (thus improving its empirical adequacy at the expense of its explanatory power), and eschewing microscopic many-body models altogether, in favor of phenomenological models. Indeed, the existence of a trade-off between explanatory power '*en gros*' and empirical adequacy '*en détail*' may well be the best explanation for the continued coexistence of both phenomenological models and microscopic many-body models even in mature areas of condensed matter physics.

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[6] As Darrell Rowbottom notes, empirical accessibility needs to be understood as encompassing 'cognitive accessibility' (Rowbottom 2009: 288) as well as 'experimental accessibility' (Keller 2002: 52).

4. Trade-offs as disciplinary demarcation criterion?

Talk of 'trade-offs', at least in relation to desiderata of theoretical models, is not as widespread in physics as it is, for example, in population biology. A quick database search for occurrences of the term in the physics literature reveals that it mostly refers to trade-offs between accuracy (of computer simulations and other calculations) and ease of computation, due to limited computational resources, not to trade-offs at the level of abstract desiderata of models as such. The relative sparsity of references to theoretical trade-offs in other disciplines has not gone unnoticed by contributors to the debate in population biology. Thus, Steven Orzack and Elliott Sober note:

> It is of relevance that claims about trade-offs similar to Levins's have not, to our knowledge, arisen in physics and chemistry. (Orzack and Sober 1993: 544)

Recently, John Matthewson has suggested that there may be principled reasons why trade-offs should be expected in biological systems. These have to do with the heterogeneous character of biological organisms as historical products of evolution by natural selection. This sets biology, and population biology with its emphasis on relations between (evolved) species in particular, apart from other branches of science:

> The requirement of 'variation that really matters within a population' does not arise in the other natural sciences. So population biology specifically deals with ensembles of entities that must be heterogeneous, in a way that does not arise in chemistry or physics. (Ibid.)

It might seem, then, that the presence or absence of trade-offs in model-building might serve, at least at the descriptive level, as a demarcation criterion of sorts between sciences such as physics and chemistry, which (to borrow a phrase from Orzack and Sober; 1993, 544) have the 'potential for generality', and others such as ecology, population biology, and evolutionary theory, which cannot ignore the evolved, heterogeneous nature of their basic objects of investigation.

It would be wrong, however, to assume that the idea of trade-offs is wholly absent from physics and chemistry. Indeed, scientists in both disciplines are well aware of the theoretical choices that are forced upon them by the existence of unavoidable trade-offs. Daniela Bailer-Jones, in a series of interviews, has attempted to document how scientists think of models. While the sample size is too small to allow for wholesale generalizations, it is nonetheless striking that those of Bailer-Jones's interviewees who hint at 'trade-off'-like characteristics in scientific modeling all have a background in condensed matter physics, broadly construed as comprising both its 'hard' (solid-state physics) and 'soft' (granular media, surface physics) variety. According to Bailer-Jones, John Bolton, one of the solid state theorists among her interviewees, 'correlates the missing predictive accuracy of models with the insights provided by a model – insight compensates for lack of detail' (Bailer-Jones 2002: 286). In his own words:

> [S]ometimes getting, I suppose, a possible match to reality is not everything. What you are looking for is an understanding of what's happening in nature, and sometimes a simple model can give you that, whereas a very large computer program can't. (Quoted after Bailer-Jones 2002: 286.)

The sentiment that Bolton expresses in this quote stems, of course, from precisely the trade-off identified in the previous section: between the empirical success of one's models (as measured, amongst others, by their predictive accuracy) and their explanatory role as sources of understanding. The idea that accuracy – especially in situations where access to knowledge and (computational) resources is limited – may trade off against the explanatory goal of identifying the fundamental mechanisms that drive the system under investigation, is echoed by other interviewees. Thus, Nancy Dise observes that 'because you are limited by time and money and by your knowledge of the system you take what you believe are the most important drivers of that process', which are then included in the model. (Quoted after Bailer-Jones 2002: 285.)

Turning to scientific practice, however, does not settle the question of whether trade-offs arise merely as the result of technical limitations (or lack of knowledge) that may, in principle, be overcome, or whether there is a deeper reason for their occurrence. Perhaps scientists simply settle for 'mere' explanatory uses of models whenever accuracy is beyond their reach. However, there is at least one general trade-off that, it seems, cannot be blamed on mere issues of feasibility and lack of information. This is the trade-off between precision and generality.[7] If, in a mathematical model, one specifies the parameter values more precisely, then, trivially, it picks out fewer possible target systems; conversely, by relaxing one's standards of precision, a larger set of possible target systems may be accommodated by the model. In this sense, precision and generality are inversely linked to one another. Of course, precision and generality are themselves closely associated with empirical and explanatory success, respectively; if we accept that generality is a desideratum of scientific explanation, then we would prefer an explanatory model to be applicable to a range of target systems, not just one particular system (even if it succeeded in reproducing the latter's behavior in great detail and with great accuracy). This suggests that more than merely practical limitations may be responsible for the widely perceived trade-off between accuracy and explanatory power in model-building.

What are the conditions under which trade-offs between precision and generality, or between accuracy and explanatory power, acquire practical significance? In his discussion of qualitative models in chemistry, Michael Weisberg (2004) argues that, strictly speaking, the inverse relation between precision and generality (as discussed in the preceding paragraph) only holds if generality is measured by how many *logically possible* target systems a model picks out. Such '*p*-generality' is conceptually distinct from '*a*-generality', which is measured by how many *actual* target systems a model applies to. It is obvious that the two can come apart: many *logically* possible

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[7] My presentation in this paragraph mainly follows Weisberg (2004: 1075-79).

target systems can be excluded on the basis of background knowledge about what the world is like, and any loss of *p*-generality that is due to the exclusion of such 'unphysical' (or otherwise uninstantiated) possibilities is not going to manifest itself in any actual process of empirical inquiry. The extent to which the 'intuitive' trade-off between *p*-generality and precision translates into an actual trade-off between *a*-generality and precision, Weisberg argues, depends on the *homogeneity* of the set of target systems the model is intended to apply to (basically, a measure of their qualitative similarity), as well as on the *scope* of inquiry (i.e., which aspects of the target system, or target systems, are deemed relevant).

In recent work, Matthewson and Weisberg (2009) have refined this picture by distinguishing between *strict* trade-offs – where an increase in the magnitude of one desideratum necessitates a decrease in the magnitude of another – and 'increase trade-offs', which merely mean that the magnitude of both desiderata cannot both be increased simultaneously. Through a formal analysis, they are able to show that, regardless of how a system is modeled – provided all other attributes are held fixed – it is impossible to increase both precision and *a*-generality; that is, precision and *a*-generality do not *strictly* trade off against each other, but exhibit an increase trade-off. (See Matthewson and Weinberg 2009: 185.) In contexts of actual scientific inquiry, the 'costliness' of the (otherwise largely abstract) trade-off between precision and generality is thus determined by the degree of heterogeneity within the set of intended target systems: the more heterogeneous a class of target systems, the more difficult it will be to simultaneously increase precision and generality (e.g., by subsuming a range of target systems under one and the same model-based account). Conversely, when dealing with highly homogeneous sets of target systems, increases in precision need not greatly affect generality, since the systems are similar in all relevant respects. Nothing in science, of course, is more similar than identical elementary particles. Hence, Matthewson argues:

> [i]t is possible to model the behaviour of electrons very precisely and generally, because they all have the same properties. But it is not possible to model the behaviour of any particular type of ecosystem both precisely and generally, because ecosystems vary with respect to many of their important properties. (Matthewson 2001: 331.)

The relatively 'docile' nature of trade-offs in physics, as opposed to their salience in ecology and population biology, on this interpretation is partly a by-product of the general tendency of physicists to focus on comparatively homogeneous systems which can be characterized by the same small number of parameters across a wide range of situations. By contrast, Matthewson argues, 'population biology specifically deals with ensembles of entities that must be heterogeneous, in a way that does not arise in chemistry or physics' (Matthewson 2011: 332). While I agree with the first part of this statement, I shall suggest in the next section that a new class of phenomena in physics, especially in the area of mesoscopic physics, casts doubt on the strictness of this demarcation between biology and physics, in terms of their susceptibility to trade-offs.

5. Trade-offs at the nano-scale: The case of mesoscopic physics

Given that heterogeneity among target systems is a significant factor in the emergence of trade-offs, it is worth reflecting briefly on its sources. It is instructive in this regard to look at some relevant contrasts between physics (traditionally regarded as largely unaffected by trade-offs) and the paradigmatic case of trade-offs in population biology. (A more complete discussion would need to do full justice to the manifold contrasts between physics and biology as scientific disciplines, which lies beyond the scope of the present article.) A first, albeit imperfect, contrast concerns the status of law-like generalizations. While physics and, to a large extent, chemistry rely heavily on law-like generalizations, most biological 'laws' describe overall empirical patterns that typically allow for exceptions. To be sure, there are some biological regularities – such as the Hardy-Weinberg principle (which states that, absent specific disturbances, the allele and genotype frequencies in a population remain constant), or the Fundamental Theorem of Natural Selection (first proposed by R.A. Fisher and later made precise by George R. Price), which identifies the genetic variance in fitness at a given time with the rate of increase (attributable to changes in gene frequencies) in the mean fitness of any organism – which hold universally, for life as we know it. However, such biological laws typically either supervene on factors, such as evolved genetic mechanisms, that are themselves contingent (in ways that relevantly contrast with, say, physics), or apply at the systems level (e.g., ecoystems or idealized populations) rather than, with nomic force, at the object level of individual organisms.[8] Whereas, say, an electron always responds to an external magnetic field in precisely the same way, organisms are complex adaptive systems that often exhibit a range of possible reactions to external stimuli. What a modeler includes as relevant elements of her scientific model, will depend on her theoretical background assumptions about the ontology of the target system, and the model's generality will in this sense be shaped (though usually not fully determined) by the underlying theory. Thus, a many-body model that 'includes' Bloch electrons, will inherit some of the background assumptions about the behavior of electrons in a perfect crystal – namely, that all electrons will behave in qualitatively identical ways; the very choice of constituents (electrons) and situations to be modeled (perfect crystal lattices) means that, a fortiori, homogeneity among the target system is assured. By contrast, models in population biology need to abstract away all sorts of complex (and potentially significant) properties that organisms have, apart from their being, say, a predator or prey. A second contrast that sets biological systems apart from

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[8] Spelling out exactly how biological and physical 'laws' contrast with respect to universality, nomic force, or scope is far beyond this paper. It is the persistence of the debate about biological laws, and the desire for demarcation which it conveys, that is relevant to our present purpose. (For a review of the debate about biological laws, see Hamilton 2007.)

most physical systems, is their status as *evolved* objects; most systems traditionally studied by physics do not exhibit the sort of trajectory dependence that characterizes biological evolution.

Recent scientific developments, however, point to the emergence of a new methodological pluralism in condensed matter physics. Whereas traditional solid-state physics revolved around the analysis of highly homogeneous systems – describing macroscopic phenomena basically as perturbations (of various sorts) of ordered systems that lend themselves to description in terms of law-like generalizations – contemporary condensed matter physics has increasingly looked at systems that are heterogeneous in ways that resemble the situation in biology. In the preceding paragraph, I identified as features that contribute to the heterogeneity of biological systems in particular their resistance to law-like generalizations as well as their status as historical (evolved) objects. At the biological level, these two factors are intimately connected, given that evolution by natural selection gives rise to a diversity *of* species, as well as to diversity *within* each species. Indeed, one of the important realizations associated with the synthetic theory of evolution is that not only is no species quite like any other, but no two subpopulations of the same species will typically behave in quite the same way. Ultimately, it is the uniqueness of particular systems – the fact that they can only be described imperfectly, if at all, by law-like generalizations – that gives rise to the overall heterogeneity among them. It is only when the world 'cooperates' in ways that render the target systems homogeneous that the issue of uniqueness can be sidestepped; sometimes such homogeneity can be artificially induced, as when a biologist creates a population of clones (which lack the diversity of genotypes found in real populations), sometimes it may occur naturally, as it does in lattice systems such as crystals, where symmetry allows for the rare *macroscopic* expression of the underlying *microscopic* uniformity among constituent parts.

The almost exclusive focus in traditional condensed matter physics on solid-state systems with a high degree of macroscopic homogeneity has in recent years been broadened to also include systems such as granular media, quasi-crystals, or colloids, where the symmetries that assure homogeneity among target systems are broken. Granular media are known to exhibit phenomena such as *hysteresis*, whereby the behavior of the system is path-dependent: it then becomes impossible to predict, at a macroscopic level, a system's future behavior without knowledge of its past history. As a result, systems that appear to be in the same macroscopic state may well behave quite differently, depending on the trajectories by which each arrived in this state. While this is still a far cry from the evolutionary 'path dependence' of biological systems, it does introduce an 'historical' element into the study of physical systems, thus increasing their heterogeneity. As a result, increases in the ability to model and predict the specific behavior of such systems may come at the expense of generality, given that models now need to be individuated by, and tailored to, their initial conditions and causal histories, not merely in terms of their macroscopic properties or a set of basic mechanisms that are thought to drive their behavior. It may, of course, still be possible to find satisfactory models for various pragmatic goals of inquiry; yet, the assumption – common in traditional solid-state physics – that descriptively accurate phenomenological models and explanatorily successful microscopic models will eventually be 'patched together', in a way that guarantees validity across whole classes of systems, looks remote: the existence of trade-offs between generality and empirical accuracy is all too real.

Whereas path dependence means that systems can no longer be classified on the basis of their macroscopic descriptions alone, the final example I wish to discuss appears to dispense with the idea of classes of target systems altogether – at least to the extent that it no longer regards it as the primary goal to explain the actual behavior of a specific system *as an instance of* a general class of target systems. The example I have in mind concerns so-called 'fingerprint effects' in mesoscopic systems, which manifest themselves as 'time-independent stochastic magnetoresistance patterns', which 'vary between samples but are reproducible (at a given temperature) within a given sample' (Lee, Stone & Fukuyama 1987: 1039). It is thought that such 'magneto-fingerprints' arise as the joint effect of, on the one hand, disorder and impurities and, on the other hand, the fact that quantum interference in mesoscopic systems acts over a characteristic length much larger than the size of an atom. As a result, each sample of a material will have its own – experimentally reproducible, yet theoretically unpredictable – 'fingerprint-like' behavior. (See Imry 2008: 171.) It is important to emphasize the experimental reproducibility of these 'magnetic fingerprints', since – unlike in the case of thermal fluctuations – the seemingly chaotic behavior does not 'average out' over time but is 'frozen in time', as it were. Magneto-fingerprints are unusual – and quite unlike traditional statistical features of complex systems – in that their shape and form is determined not, as it were, by a given system's membership in a larger reference class of like systems, but instead by the brute atom-by-atom particularity of the specific sample in question.[9]

Due to this novel combination of empirical replicability with sample-specificity, magnetic fingerprints are genuinely new characteristics that can also be exploited technologically. Thus, physicists have developed techniques to capture the magnetic 'fingerprints' of nanostructures that are buried within the boards and junctions of an electronic device (see Wong et al. 2009). Other researchers, exploiting similar quantum effects, have created nano-scale transistors that bear 'unique fingerprint-like device-to-device differences attributed to random single impurities'; the same group emphasizes the 'critical need' to model such fingerprint-like behavior (Lansbergen et al. 2008: 2). It is clear that no model of the unique 'fingerprint-like' behavior of a *specific* target system can possibly generalize to another sample (unless the two are microscopically

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[9] In this respect, magneto-fingerprints (and like phenomena) contrast markedly with conventional ways of characterizing samples of complex materials, e.g. in terms of their relative material composition. Whereas a traditional analysis of, say, a sample of steel might allow one to infer which foundry it came from, based on the different levels of impurity associated with that foundry's production process, magneto-fingerprints would allow one to distinguish between specific samples from the same foundry.

one and the same). Of course, the same modeling strategies that were used in one case may again be deployed to successfully model another sample, but the outcome in each case will be tailored to the specific atomic constitution of the particular sample in question. The case of magneto-fingerprints is but one example of a broader trend in physics, as condensed matter physics (along with neighboring disciplines) is beginning to make good on its promise to explain the vast variety of complex systems and their macroscopic behavior around us. Thanks to technological and computational advances, it is increasingly becoming possible to analyze matter at ever greater resolution, allowing researchers to identify highly localized, sample-specific – yet individually reproducible – regularities that previously would have been either dismissed as 'noise' or regarded as idiosyncracies of a given experimental setup, standing neither in need of explanation nor much chance of being accounted for by one's models.

Thus, we here have a case where predictive accuracy of a model is strongly tied to the individual characteristics of the target system – its atom-by-atom constitution – in a way that does not easily generalize. This indicates a departure from traditional ways of approaching solid-state systems, which revolved around treating individual systems as instantiations of general model situations (e.g., of Bloch electrons in crystals), with at best some minor perturbations or 'de-idealizations' added at a later stage, in an attempt to 'apply' the general model to a specific case. While this approach has been extraordinarily successful in furnishing unified explanations across a range of phenomena in condensed matter physics, technological and computational advances are now beginning to make it possible to reliably measure and model macroscopic materials at the level of their atomic constitution. Not just biological organisms, but matter itself – at least in its condensed form – may thus come to be recognized as ultimately consisting of individually unique and collectively heterogeneous assemblages, whose behavior it may only be possible to predict accurately on a case-by-case basis, by trading-in the generality of explanatory models for the ability to describe (and eventually exploit) the material constitution and sample-specific characteristics of systems at the nano-scale.

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