Review of Mark Wilson, *Physics Avoidance*[1]

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"Physics avoidance" is a term coined by Mark Wilson, the meaning of which he illustrates with an anecdote about Mormon leader Brigham Young. Wilson recounts that Brigham Young would instruct his followers to "travel two hundred miles directly south and plant cotton," and they would dutifully and literally head *directly* south through coulees, hills, and any other obstacles (51). While Brigham Young's followers refrained from taking the indirect but easier path to their destination, physics avoidance entails adopting indirect but easier strategies for achieving ends in physics. Wilson defines physics avoidance as recognition of "the fact that nature rarely arranges its affairs for our calculational convenience but forever forces us into seeking clever work-arounds for improving our calculational lot in life" (364). However, philosophers reading this book find themselves in a position more akin to Brigham Young's followers than physics avoiders: the direct and difficult path through the complex examples cannot be avoided. The book is full of detailed historical examples of applied mathematics that are thoroughly analyzed. This degree of intricate detail is needed to appreciate the theses and the arguments. Indeed, a suitable slogan for the book is "the road to perdition is paved with inadequately examined examples" (366). (The religious language is a nod to

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Paul Bunyan's *Pilgrim's Progress*, which is the literary model for the "Second Pilgrim's Progress" narrated in Chapter 9.) For the reader who is only interested in some case studies, the chapters are relatively self-contained. When details from other chapters are relevant detailed summaries are supplied.

It is not possible to adequately examine all of the examples or even all of the theses in a book review. Wilson characterizes the book as "largely a work within philosophy of science," but states its "chief ambition" is to free philosophers in a range of sub-fields (e.g., philosophy of mathematics, language, and analytical metaphysics) from "the shackles of pseudo-scientific philosophy" (xii). I will focus on themes that are of interest to philosophers of physics and historians of physics. For those who are put off by the anti-philosophical polemics and are interested in general philosophical conclusions, I will argue that (even though it is not advertised this way in the book) the final chapters sketch a valuable philosophical account of applied mathematics.

On the theme of freeing from shackles, one of the main theses of the book is familiar from Wilson's earlier book *Wandering Significance*: "Theory T thinking" by philosophers rests on an inaccurate portrayal of scientific methodology. Theory T thinking is marked by generalized talk of a theory T that abstracts from the particularities of the concrete context of application to a specific domain. A paradigm example of errors to which Theory T thinking is prone is the failure to distinguish between the types of explanations afforded by evolutionary equations and equilibrium equations. Consider the problem of modeling the effects of placing a weight on a metal bar that is oriented vertically and fixed at its base (64-67). An evolutionary model uses hyperbolic partial differential equations and imposes initial and boundary conditions with the goal of describing the bending of the bar as a function of time. Models of this type tend to be calculationally intractable and unreliable, especially over long time frames. Alternatively, one can practice physics avoidance by constructing an equilibrium model using elliptic equations and boundary conditions. This type of model does not deliver a description of the time evolution, but can predict phenomena such as the critical weight at which the bending bar will break. Wilson's main moral is that Theory T thinking assimilates these two types of models and their methodologies, resulting in mistaken accounts of explanation and causation in science. However, absorbing this moral is not enough to save oneself from eternal damnation. The rich details about the difficulties encountered in constructing models for different types of systems and the strategies that can be used to avoid these obstacles are also relevant to understanding how science works. Wilson relates key historical episodes in the development of applied mathematics from Descartes' pre-calculus models through ordinary and partial differential equations to Schwartz distributions and Cauchy sequences to motivate and illustrate successful and failed strategies. *Why* these modeling strategies work when they do work is an even more interesting—-and more fraught—philosophical question. The focus throughout is on models that apply classical mechanics to systems more complex than a handful of point particles (e.g, bars, fluids, strings, and friction).

This is a tale of heroes and villains. The prominent villains are twentieth century philosophers who heedlessly engage in Theory T thinking (Hempel, Railton, Lewis, Paul, Benacerraf, ...). The heroes are primarily applied mathematicians. Wilson opines that "functional analysts have often served as subtler philosophers than most of us 'professionals'" (360). But even some of the heroes get only a partial endorsement. Duhem, the subject of the longest chapter in the book, only rates two cheers out of three. This chapter offers a detailed historical account of Duhem's pioneering role in the development of thermomechanics, including the historical context of Lagrange's *Analytical Mechanics*. Duhem set himself the goal of modeling systems in which both energy and heat exchange need to be taken into account, such as those subject to friction or a bar that is both being heated and struck with a hammer. Duhem struggles to integrate the "Old Mechanics"—epitomized by Lagrange's virtual work-based framework—into a "New Mechanics" that also incorporates thermal properties. The resulting thermomechanics is the beginning of the contemporary subject of non-equilibrium thermodynamics. Wilson's account of Duhem's difficulties will resonate with contemporary philosophers of physics. The central foundational issue is the extent to which thermodynamic systems can be adequately modeled as isolated and evolving autonomously in time, independent of external control from the environment.

The chapter on Duhem (as well as the chapter on Leibniz and discussions of Lagrange's mechanics scattered throughout the book) will also be of interest to historians, particularly those who are taking a fresh look at the development of classical mechanics after Newton. However, it should be borne in mind that the goal of this historical narrative is to supply a case study of the (partial) success of physics avoidance techniques—and the corresponding inadequacy of Theory T thinking. This can only be accomplished with the benefit of hindsight, so more recent texts by Truesdell and Noll are interwoven with passages from Duhem and Mach. Historians should also heed the explicit warnings such as the stipulation that "it will be a thoroughly sober and dry-cleaned Duhem who sallies forth in this essay, whereas our real life protagonist could be an obnoxious pugilist, fond of exaggeration and xenophobic rant" (141). The censored views include sweeping anti-realist and phenomenalist commitments. In a footnote, Wilson likens his account of Duhem to Disney's account of Davy Crockett. Historians will benefit from the detailed account of Duhem's contributions to thermomechanics, but will no doubt wish to address these acknowledged gaps.

Duhem's two cheers review is not due to his anti-realist pronouncements. The third cheer is withheld because Duhem sides with Cauchy, Green, and Stokes in advocating a "top down" from the macrolevel, axiomatic approach to model-building. Post-Duhem, multiscalar modeling techniques were developed. We now recognize, with the benefit of hindsight, that the better modeling strategy is neither top down nor bottom up, and that the improved models that result are scale-dependent (194). But Duhem cannot exactly be faulted for his lack of prescience in foreseeing that top down and bottom up were not the only practical modeling strategies (195). According to Wilson, the people who are blameworthy are twentieth century philosophers who ignore recent developments in applied mathematical methods and persist in endorsing Theory T positions. The passages that discuss philosophers who commit this error give the impression that Wilson's goal is to root out Theory T thinking wherever it is found. But Theory T thinking is a large package of commitments. It seems to me that Wilson's Theory T counterexamples leave room for some dimensions of Theory T thinking to be respectable in some cases. And, indeed, Wilson's treatments of the views of physicists in discussions of the case studies are more nuanced than the discussions of views of philosophers.

The permissibility of axiomatic methods is a good example. Wilson is critical of the presumption of Quine (and others) "that all of science's vast menagerie of useful reasoning practices can be neatly codified as instances of logical reasoning from clearly enunciated premises" (420). Dropping the unwarranted generalization and the restriction to formal logic leaves room for selective deployment of mathematically-stated axioms. Wilson is also adamant that we refrain from assuming that *eventually* fundamental physics will take a neat, axiomatized Theory T form. But what about the use of axiomatic methods in the midst of theory development? A point on which Duhem and his rival Hertz agree is that an axiomatic framework has the virtue of allowing traditional, intuitive concepts (e.g., mechanistic concept of force in Newton's mechanics) to be set aside in favour of new concepts introduced by implicit definition (151-152; Appendix to Chapter 7). Wilson allows that this is not entirely bad: "Real-world scientific development being as it is, such a perfected T pinnacle is rarely obtained, but it represents the syntactic goal to which we should aspire. Although I am often critical of modern Theory T thinking in these pages, I also recognize that its doctrinal origins trace to these commendable efforts to outfit science with a wider array of conceptual liberties."[2] (151).

While this is not exactly a ringing endorsement, a more relaxed attitude towards the aspirational use of axiomatic approaches as a possible strategy for model-building in cases in which new concepts are needed does fit well with Wilson's brand of pragmatism and emphasis on applied mathematics as strategic thinking, discussed below. Of course, in the case of thermomechanics, implicit definition from general, top down axioms turned out to be inadequate because it is not compatible with the highly context sensitive, patchwork nature of concepts from both mechanics and thermodynamics. But, as Wilson emphasizes, the devil is in the details of the context of application, and we should be alert for different applicational contexts requiring different methods. The development of electromagnetism post-Maxwell is an instructive case study for comparison with thermomechanics. That was a case in which the use of Lagrangian mechanics as a sort of

[2]Wilson even allows that "[f]or similarly commendable reasons, formal axiomatics became a central plank within logical empiricist thinking as well" (193).

abstract axiomatic framework did turn out to be fruitful for the introduction of the classical electromagnetic field concept.

For Wilson, avoiding perdition involves sticking closely to the details of each individual case study, and refraining from generalizing to other cases, drawing inferences about future success, or trusting apparent explanations of the success of the method. Is there a positive philosophical thesis that emerges from the detail-oriented, context-sensitive analysis of case studies? Wilson professes “I am not striving to supply a ‘general theory’ of anything” (57). Univocal philosophical accounts of explanation and causation are disavowed, as are accounts of modality and laws of nature. But there is one philosophical position that Wilson repeatedly endorses: he declares himself a “stout scientific realist” (146; see also 79, 361). This is a longstanding theme in Wilson’s thinking. Wilson (2000, 305) argues that Cartwright’s case studies in *How the Laws of Physics of Lie* support (at worst) mathematical opportunism, not anti-realism. Mathematical opportunism is the attitude that “it is the job of the applied mathematician to look out for the *special circumstances* that allow mathematics to say something useful about physical behaviour” (Wilson 2000, 297). Mathematical opportunists are skeptical about the scope of applicability of mathematical models, not about representative capacity within the scope of application. Similarly, Duhem’s anti-realist pronouncements are criticized on the grounds that Duhem falsely assumes that idealizations are necessary to model his target systems (140-141, 200). In *Physics Avoidance*, Wilson emphasizes that he accords theoretical entities the ontological status of real entities even though he rejects Boyd’s and Putnam’s theories of reference as inadequate in the face of the ‘wandering significance’ of key theoretical terms (146, 383-384). And, indeed, a patchwork, context-senstive theory of reference is compatible with the metaphysical commitment of scientific realism, that theories genuinely refer to mind-independent entities.

However, there are other morals of the case studies that are in tension with stout scientific realism. A moral of the Greediness of Scales chapter is that multiscale techniques for modeling materials produce models at different scales with incompatible descriptions of the fundamental components of the material (e.g., continuous medium vs. discrete particles). Perhaps this moral could be accommodated in a less stout version of scientific realism, but semantic mimicry seems more difficult to reconcile with even a more modest variant of scientific realism. Semantic mimicry is the phenomenon that (3), numerical calculations, *appear* to correspond to (2), a solution, when in fact there is some different and more complicated (2*), the true solution, that better represents (1), the target in the world (375-376). (Bracketed numbers track these representational levels.) A historical example in which semantic mimicry occurred is the modeling of the stress distribution on airplane wings by decomposing the wing into square elements and then calculating the stresses on the middle of the neighbouring edges, ignoring the corners (333-335, 356). The method works as an approximation, but a literal interpretation of the representation employed does not explain its success: an airplane wing cannot be physically composed of squares welded together and subject to the same stresses as in the model because the corners of the squares have infinite stresses. The corrective is to adopt a more sophisticated mathematical model that accommodates solutions of type (2*). For the airplane wing model, the fix involves Schwartz distributions, virtual work manipulations, and the Rayleigh-Ritz method.

Wilson endorses Hadamard's insight that the representation of the the target system by this more sophisticated model is not accomplished by the equations alone, but by the combination of equations with initial and boundary conditions (341). But Wilson emphasizes that this correction should be regarded as temporary; mutual adjustment between mathematical models and the world is an ongoing process. The "background world/word pictures" on which a model relies are provisional and defeasible (419). This is a departure from the epistemic dimension of scientific realist commitment—that we are justified in regarding our successful models as supplying approximately correct representations of the world. As Wilson points out, semantic mimicry actually undercut the justification for such a belief in the case of the mechanical models constructed by Kelvin and others at the end of the nineteenth century. Wilson argues that subsequent developments in mathematics vindicate Duhem's "complain[t] that these extraneous 'imaginative pictures' provide little evidence for the existence of discrete molecular structures" (360). Furthermore, in Wilson's bigger picture view of applied mathematics, the main goal of the activity is to improve our strategic abilities to model the world, particularly on the calculational side. The main method for achieving this goal is to turn away from positing an improved physical description of the target system and towards Greater Mathematicsland. This approach to developing improved mathematical models is not incompatible with scientific realism, but the issue of whether one is committed to the approximate truth of the mathematical model seems to be of secondary importance.

In my view, the most important positive contribution of this book is not a thesis about representation, such as scientific realism, but a thesis about methodology. In the latter part of Chapter 8 ("Semantic Mimicry") and Chapter 9 ("A Second Pilgrim's Progress") an account of applied mathematics emerges. The account is based on the case studies carefully examined in the book, but Wilson does permit himself to generalize. And it does not feel like the gates of Hell are opening to swallow him up. The prompt is the desire to articulate a variety of naturalism that is inspired by Maddy's Second Philosophy. Wilson disagrees with Maddy on some issues, but he follows her in rejecting Quine's account of mathematics and in his conviction that higher mathematics occupies an important place within science and empiricist epistemology. Wilson's naturalism is based on a loose, "quasi-biological" analogy between frogs' strategies for fly catching and humans' mathematical strategies for solving computational problems. In the course of setting out this account of naturalism, Wilson also sketches an account of applied mathematics. From an applied mathematics perspective, physics avoidance involves finding effective mathematical strategies for improving our computational abilities when we run up against a recalcitrant world. This is the empirical kernel of this empiricist account: "the real-life struggles we confront in dealing with a largely uncooperative natural world...are the strongest empiricist rationales for developing higher mathematics" (365). Set theory is held up as a prime example of this phenomenon. In general, mathematics is a useful tool (386) for devising work around strategies because it facilitates the transfer of inferential templates between domains: "Notable advances in science often occur when someone notices a nifty reasoning technique employed in field A and decides to try out similar moves within field B, despite the lack of any evident connection between A and B. Sometimes, after a bit of corrective tinkering, these coarse inferential borrowings open the doors to bountiful new results within B..." (382). The formal structure for reasoning that is supplied by mathematics is relevant, not the representation of physical properties particular to target systems in domain A (394). Wilson's slogan for this is that "mathematics supplies the basic science of why strategies work" (416). Another slogan of this account of applied mathematics is that "'mathematics' work is never done' in ongoing science" (364, 383): as semantic mimicry illustrates, mathematical methods can be successfully used without understanding why they work; however, understanding why mathematical strategies work when they work often requires introducing higher mathematics that antecedently may not have been expected to have this application.

This general, philosophical account of applied mathematics that emerges in sketched form in Chapters 8 and 9 is in itself an important contribution to the philosophy of applied mathematics, but it also raises some interesting unresolved issues. After computational obstacles are encountered and the empirically-motivated leap into Greater Mathematicsland is made, how does higher mathematics help in the formulation of generally applicable new mathematical methods? The case-specific details that Wilson emphasizes throughout the book are less relevant to this aspect of the account of how new mathematics gets developed for eventual applicational purposes in new cases. Computational power is one factor driving the development of new mathematics, but presumably there are also other factors. Since we are encountering a situation in which abstracting from intuitive physical pictures is helpful, does axiomatization play a role here? Since mathematical frameworks that can be easily transferred from one domain to another are of strategic importance, what about the role of general physical principles that are hypothesized to hold across domains and also to be preserved in future theories (e.g., conservation principles)? What about the role of development pressures from goals and practices internal to (pure) mathematics, which are foregrounded in Maddy's account of applied mathematics (Maddy 2007, 2008)? In this spirit, the book does conclude with the line "[a]nd our greatest tool for understanding the mysterious byways of effective strategy is mathematics, of a rather purist cast" (422).

Finally, what does this book contain for philosophers of physics who are primarily interested in contemporary physics? Wilson's case studies are rooted in problems that first arose in classical mechanics prior to 1900. He traces developments in applied mathematical methods for solving these problems into the twentieth century, and their implications for foundational issues in materials science. Philosophers of physics who are interested in more recent developments in physics will find it fruitful to continue investigating whether Wilson's morals are applicable. (Hancox-Li (2015) and Batterman (2010) are two examples of existing work in this vein.) While calculational intractability and the use of higher mathematics have continued to be prominent features of recent physics, philosophers should also be alert for creative, novel uses of strategic mathematical thinking in recent cases. A prime example is renormalization group methods, which are a species of multiscalar technique. This is a rich example of applied mathematics with a very broad scope of application that will repay further study in a Wilsonian vein. Furthermore, renormalization group methods are the next chapter of Wilson's historical narrative about the development of the calculus from ordinary differential equations through treatments of partial differential equations. Pioneer Kenneth G. Wilson presents renormalization group methods as being based on a new type of derivative that is defined using a new type of continuum limit (Wilson 1975). Kenneth G. Wilson's own reflections on renormalization group methods as a species of applied mathematics are as perceptive as Hadamard's on partial differential equations.

The twentieth and twenty-first centuries have been an era in which mathematical methods for physics avoidance (broadly speaking) have been transferred back and forth between condensed matter physics and particle physics. Wilson's cautionary morals about carefully distinguishing between interpretations appropriate to equilibrium and evolutionary contexts, and attending to differences between causal, modal, and nomological notions that are applicable in these contexts, are of enduring relevance. Inattention to these basic differences—an instance of Theory T thinking—is a live danger in contemporary philosophy and physics. (An example: care is needed in assessing naturalness as a criterion for Beyond the Standard Model candidates.) The use of analytic functions as a physics avoidance strategy also has a history that continues after the case studies examined by Wilson. Wilson explains that analytic functions are suitable as solutions for elliptic equations of equilibrium models, but not for hyperbolic equations of evolutionary models due to analytic continuation: "they secretly embody a reproducibility property allied to that of a flatworm: from any little piece their behavior everywhere can be constructed by so-called analytic continuation" (74). This is unsuitable for evolutionary models because initial conditions are taken to be freely specifiable and correlations between data in different spacetime regions are taken to be limited by finite signal speed. There is an interesting twentieth century twist to this story. Analytic continuation has been adopted as a creative applied mathematics technique for constructing models associated with evolutionary hyperbolic equations. The strategy is to analytically continue a whole set of solutions for an evolutionary model (e.g., QFT with a scalar ϕ^4 interaction) into a whole set of solutions for an equilibrium model (e.g., classical statistical mechanical Ising model for a ferromagnet) (Fraser 2017; Hancox-Li 2017). The technique is new, but the underlying physics avoidance strategy is familiar from Wilson's case studies: equilibrium models tend to be easier to construct than equilibrium models. And these are just a few of the examples that came to mind. Philosophers and historians of physics have much to learn from Wilson's adequately examined examples and also from his more general reflections on applied mathematics.

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