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Applying Unrigorous Mathematics: Heaviside's Operational Calculus

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ABSTRACT

Applications of unrigorous mathematics are relatively common in the history and current practice of physics but underexplored in existing philosophical work on applications of mathematics. I argue that perspicuously representing some of the most philosophically interesting aspects of these cases requires us to go beyond the most prominent accounts of the role of mathematics in scientific representations, namely versions of the mapping account. I defend an alternative, the robustly inferential conception (RIC) of mathematical scientific representations, which allows us to represent the relevant practices more naturally. I illustrate the advantages of RIC by considering one such case, Heaviside's use of his unrigorous operational calculus to produce and apply an early generalization of Ohm's law in terms of "resistance operators."

1. Introduction

From the early calculus in Newtonian physics to the Dirac delta function and ill-defined path integrals in quantum theory, physicists have leaned heavily on mathematical tools that fall well short of the standards of rigor of present-day pure mathematics. Such tools have facilitated important physical results despite the mathematics' not clearly sufficing on its own to pick out well-defined mathematical structures. The success of these applications and their relationship to applications of more rigorous mathematics are among those features we might expect a philosophical account of the applicability of mathematics in science to explain. However, in such work, little attention has been paid to applications of unrigorous mathematics.

The most prominent accounts of mathematical scientific representations are mapping accounts, according to which mathematical representations posit a structure-preserving mapping between the structure(s) picked out by the relevant mathematics and the structure of the target system (e.g., Pincock, 2004, 2012; Bueno and Colyvan, 2011; Bueno and French, 2018). An alternative, the robustly inferential conception (RIC), takes a piece of mathematics to represent a physical target system in virtue of shared patterns of inference being licensed in reasoning about both the mathematics and the target system (McCullough-Benner, 2020). The aim of this paper is to show that applications of unrigorous mathematics give us good reason to adopt RIC. Central to applying unrigorous mathematics is the use of inference strategies that restrict the use of incoherent, underdeveloped, or otherwise problematic concepts so that undesirable results cannot be derived. Because such strategies typically involve local inferential restrictions that do not naturally correspond to neat divisions of a mathematical structure, mapping accounts don't naturally capture them. In contrast, they can be represented and explained straightforwardly in terms of RIC.

I substantiate these points by examining Oliver Heaviside's application of his operational calculus, which was notoriously unrigorous. His cavalier approach led the Royal Society to take the unprecedented step of subjecting his work to peer review and ultimately refusing to publish his work on the subject.[1] Rather than leading Heaviside to pursue greater mathematical rigor, this seems instead to have strengthened his convictions:

> Shall I refuse my dinner because I do not fully understand the process of digestion? No, not if I am satisfied with the result. Now a physicist may in like manner employ unrigorous processes with satisfaction and usefulness if he, by the application of tests, satisfies himself of the accuracy of his results. (Heaviside, 1899, §224, pp. 9f)

Heaviside's operational work was remarkably successful. There have been no credible challenges to his physical results, and his unrigorous techniques underpinned real conceptual (not just computational) advancement. His "resistance operators," central to his operational techniques, generalized the concept of resistance so that he could extend Ohm's law to time-varying circuits with reactive elements, anticipating the later concept of generalized s-plane impedance, which did the same work more rigorously.

How is it that Heaviside was so successful in applying his operational calculus despite the highly unrigorous nature of his work? Heaviside employed a number of strategies to restrict the inferences one could make with his incoherent, underdeveloped, or otherwise problematic mathematical concepts. Heaviside often made inferential restrictions in a strikingly local and ad hoc way, and he frequently used the physical interpretation of the mathematics in a given case to inform the mathematical inferences he ultimately took to be licensed. Explaining how and why these strategies worked requires us to represent Heaviside's inferential practices at a finer level of grain than mapping accounts naturally allow for—a task for which RIC is perfectly suited.

In section 2, I compare mapping accounts and RIC, and

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[1] For useful discussions of this episode, see Yavetz (1995, pp. 318–20) and Nahin (2002, pp. 222f).

I present an initial argument for RIC on the grounds of its ability to represent applications of unrigorous mathematics. The rest of the paper is devoted to substantiating these points through an examination of the application of Heaviside's operational calculus. In section 3, I introduce the operational calculus. In section 4, I discuss several ways in which it fails to meet the standards of rigor of pure mathematics, as well as the inference strategies Heaviside adopts in order to manage this lack of rigor. I argue that RIC is better suited to explaining the contributions of these strategies to the success of applications of the operational calculus. In sections 5 and 6, I consider two cases in which mapping accounts might be thought to provide explanatory benefits beyond those available to RIC. In section 5, I consider Heaviside's appeals to the physical interpretation of operational expressions to inform his mathematical reasoning. In section 6, I consider retrospective explanations of Heaviside's success in relation to later methods, particularly the Laplace transform. I argue that neither case favors mapping accounts over RIC. As a result, there is good reason to favor RIC as an account of mathematical scientific representations.

2. The problem of mathematical scientific representation and mathematical rigor

2.1. Accounts of mathematical scientific representation

Accounts of mathematical scientific representation are generally intended to perform two tasks. The first is to provide an answer to what Nguyen and Frigg (2017, p. 3) call "the general application problem." This is the problem of explaining how mathematics can represent target systems in general, with emphasis on the question of how any piece of mathematics could "hook on" to the systems it represents. This problem arises because the subject matter of mathematics is prima facie distinct from that of the scientific representations in which it is used. The second task is to serve as a meta-level device used by philosophers of science to represent particular episodes of scientific practice in which mathematics is applied with an eye to bringing out its philosophically significant features. For instance, Bueno and French (2018) emphasize the utility of their account—a combination of the inferential conception of applications of mathematics (Bueno and Colyvan, 2011) with the partial structures approach (e.g., da Costa and French, 2003)—as such a device, spending much of the book applying this framework to episodes from scientific practice to illustrate philosophical problems that arise in connection with applied mathematics.

By far the most common approach to both is to adopt some version of the mapping account. The common core of these views is that scientists use mathematics to represent a target system by positing a relation—usually a structure-preserving mapping—between the structure(s) picked out by the relevant mathematics and the structure of the target system (e.g., Pincock, 2004, 2012; Bueno and Colyvan, 2011; Bueno and French, 2018). Such representations thus represent their target systems as bearing a structural similarity to the structures picked out by the relevant mathematics. This structural relation is central to performing the first task described above; mathematics "hooks on" to the non-mathematical world in virtue of the shared structure entailed by the existence of such a relation.[2] It also provides a framework for understanding applications of mathematics in practice (the second task above). The mathematically mediated inferences scientists make in using such representations are licensed by that structural similarity; the existence of the right sort of mapping is what makes it possible to draw inferences about the target system on the basis of mathematical reasoning. Many heuristic moves can also be explained in terms of interpreting surplus mathematical structure (via a more extensive mapping) or relating the relevant structures to other structures via further mappings.[3]

An alternative is to appeal not to shared structure but to shared patterns of inference. According to the robustly inferential conception (RIC) of mathematical scientific representation (McCullough-Benner, 2020), all we can say in general in response to the first task is that a piece of mathematics is relevant to physical target systems because some of the patterns of inference appropriate for reasoning about the mathematics are also appropriate for reasoning about the physical systems. Mathematics places constraints on what the target system of a representation must be like by helping to specify inferences about the target system that must preserve truth if the representation is (perfectly) accurate. Such representations have three ingredients:

- **(RIC1)** a *physical interpretation* of the language of the mathematical theory sufficient to provide at least some sentences in this language with physical truth conditions,
- **(RIC2)** an *initial description of the target system* in the language of the mathematical theory, given this interpretation, and
- **(RIC3)** a *collection of privileged inference patterns* from those licensed by the original mathematical theory.

The commitments of the representation are the physically interpreted claims in the language of the mathematical theory that are in RIC2 or can be derived from these via the inference patterns in RIC3. The informational content of the representation is then given by the conjunction of these commitments.

An important similarity between RIC and mapping accounts is that both ultimately explain the success of a mathematically mediated physical inference in terms of the informational content of the representation that licenses it; if the world is as the representation says it is, the mathematically mediated inference must be truth-preserving. The difference is in how that informational content is spelled out.

[2] That said, more would seem to be required to provide an adequate response to this problem, namely a means of specifying the structure of the target system, like a "structure-generating description" (Nguyen and Frigg, 2017).

[3] See, for example, Bueno and Colyvan (2011, pp. 364f) and Bueno and French (2018, pp. 141ff) on the reasoning that led Dirac to posit the positron.

Mapping accounts do so by positing that mathematical and physical systems themselves stand in a structural relationship. RIC does so by positing shared syntactic structure between mathematical inferences and truth-preserving inferences about physical systems. In either case, the scientists' mathematically mediated inferences are ultimately justified by the supposition that the representation is in fact accurate, and this is where explanations in either case bottom out.

As a result, mapping accounts can be recovered as a special case in which RIC1 is provided by the relevant structure and mapping, RIC2 by a "structure-generating description" (Nguyen and Frigg, 2017) or something similar, and RIC3 by the collection of inference patterns that preserve truth when interpreted in terms of the relevant mathematical structure. In such cases, the informational content is spelled out in terms of exactly the same features that mapping accounts appeal to. This means that RIC can appeal to the full resources of the mapping account in representing particular applications of mathematics. But these ingredients should not be spelled out in this way in every case. Most importantly for the purposes of representing applications of unrigorous mathematics, RIC3 need not correspond to the inference patterns that preserve truth in reasoning about any particular structure but may be specified independently. The result is a high degree of flexibility in representing scientists' mathematical inference strategies.

2.2. Adjudicating between mapping accounts and RIC: Unrigorous mathematics

Because RIC recovers mapping accounts as special cases, adjudicating between them comes down to the question of whether RIC's generality has philosophical benefits that outweigh its costs; if RIC had no such benefits, mapping accounts would be preferable on the basis of their greater specificity. This paper explores one such benefit: RIC, I argue, is a better tool for representing episodes in which unrigorous mathematical techniques have been applied in the history of science.

For the sake of this argument, I am happy to concede that some versions of the mapping account successfully explain how unrigorous mathematics can in principle be used to represent a target system, the first task for accounts of mathematical scientific representation. It is enough for my purposes to show that they have shortcomings in relation to the second task, as meta-level devices to help philosophers represent philosophically salient features of episodes in which scientists apply unrigorous mathematics.

While such episodes raise a number of philosophical questions, here I focus on the question of how to explain the success of these techniques. We might approach this question in two ways. First, we might be interested in how the techniques scientists used to manage the epistemic shortcomings of unrigorous mathematics contributed to the success of the resulting representations. Second, we might be interested in explaining the success of these techniques retrospectively in terms of their relation to other, more rigorous techniques. These approaches are complementary. The former shows why unrigorous techniques are epistemically respectable in a way that is accessible to those engaged in the practice; it explains why it was *reasonable* for them to reason as they did. The latter shows why the *results* of that practice happened to be correct, regardless of their epistemic status at the time.

Most of the paper will be devoted to supporting the claim that RIC facilitates more perspicuous explanations of the first kind. This on its own is a significant theoretical benefit, and it is here that the advantages of RIC are clearest. However, I will return to the second kind of explanation at the end of the paper, arguing that RIC allows for a significant, albeit more modest, improvement to explanations of this kind as well.

2.3. Why unrigorous mathematics favors RIC

Crucial to using a mapping account as a meta-level device is identifying an appropriate structure or collection of structures to represent the relevant mathematics. This is simple when applications involve well-understood mathematical theories. Even if scientists skip a few steps or rely on unarticulated assumptions, we philosophers of science can straightforwardly represent their practice in terms of a well-defined mathematical structure associated with the theory.

However, when a mathematical theory or technique involved in an application is not a well-understood piece of pure mathematics, more work must be done. In the extreme, the mathematics involved might be inconsistent, as in the case of the early calculus[4] or the Dirac delta function, both of which found—and in the case of the Dirac delta function, continue to find—widespread use in physics. Since no classical, model-theoretic structure satisfies an inconsistent theory, additional work must be done to understand such cases in terms of the mapping account. Similarly, a piece of mathematics might fail to pick out a well-defined structure because it appeals to inchoate or incoherent concepts. If a mathematical theory appeals to such concepts, there will be a degree of indeterminacy in its global mathematical commitments—even if those concepts are well-behaved and well-understood in more local contexts—due to there being multiple ways to flesh out these concepts or resolve their incoherence.

One option is to appeal to a more liberal kind of structure built to accommodate inconsistency and indeterminacy. The most promising account to do so is the partial structures approach (da Costa and French, 2003; Bueno and Colyvan, 2011; Bueno and French, 2012, 2018). A unary relation R in a partial structure partitions the domain into three blocks: R_1, those items of which R holds; R_2, those of which R does not hold; and R_3, those for which R is undefined. (n-ary relations and functions are treated similarly.) Total structures are a special case in which the R_3 block of every relation is empty. A statement ϕ is partially true in a partial structure if ϕ is true in a total structure that extends the partial structure (by moving elements from the R_3 to the R_1 and R_2 blocks of its relations). This means ϕ and $\neg\phi$ can both be partially

[4] For extended treatment of this case, see McCullough-Benner (2020). For arguments against thinking of the early calculus as inconsistent, see Vickers (2013), but note that even if Vickers is right, it is still unrigorous in the sense articulated below.

true in the same partial structure, provided that the structure can be extended in one way to make ϕ true and in another to make $\neg\phi$ true. The same device allows us to represent conceptual indeterminacy in addition to inconsistency. When a mathematical theory is not clearly inconsistent but also does not clearly pick out a determinate (total) structure, the theory can be represented as a collection of partial structures in which propositions to which it is not clear whether the theory is committed are partially true.

Another option is to explain applications of inconsistent and otherwise unrigorous mathematics in terms of classical structures that stand in a more complex relationship to the practice in question. For instance, one might represent scientists as reasoning about different classical structures in different local contexts, even within the same argument.[5] Alternatively, one might appeal to a structure picked out by a later, more rigorous successor to the mathematical theory. For instance, we might understand early applications of the infinitesimal calculus in terms of modern calculus, or we might understand applications of the Dirac delta function in terms of Schwartz's theory of distributions.

Why expect such strategies to produce less perspicuous explanations of the success of unrigorous mathematical techniques than RIC? Because crucial to these explanations are the *inferentially restrictive methodologies* (Davey, 2003) scientists adopt when using these techniques. Such a methodology is one in which not just any concept or classically valid inference may be used at any point in an argument. It is because such restrictions are in place that physically persuasive arguments can incorporate problematic mathematical arguments as components. In typical cases, this involves a patchwork of local inferential restrictions (rather than the more general restrictions that might be involved in adopting a particular non-classical logic, say). Such restrictions allow scientists to quarantine mathematically ill-defined concepts to contexts in which they behave in the desired way, making it impossible to use them to derive undesirable (or, in the limit, absurd) results. For example, the properties of the Dirac delta function[6] are inconsistent. But it can be useful provided that one adopts the inference strategy, explicitly avowed by Dirac (1967), of using it only as a factor within an integrand.

Regardless of how local or global a set of inferential restrictions is, RIC can directly represent it via the specification of the RIC3 component of the representation, the set of privileged inference patterns; disallowed inferences may simply be excluded from RIC3. With these inferences excluded, the result of applying the inference patterns in RIC3 to the initial specification of the target system (RIC2), given the interpretation of the mathematical vocabulary (RIC1), needn't be inconsistent or contain otherwise undesirable propositions meant to be avoided via the inferential restrictions. The inferential restrictions may be specified in an entirely piecemeal fashion or at a very high level of generality, but in applications of unrigorous mathematics an intermediate level of grain will almost always be called for. Consider Dirac's inference strategy. We can very naturally specify RIC3 in this case by including all inference patterns licensed in real analysis given the assumption that the Dirac delta is an extended real-valued function, apart from those in which it doesn't appear as a factor in an integrand. Provided the problematic concepts really do behave in the desired way in these restricted contexts, we then have a quite straightforward explanation of the success (and epistemic legitimacy) of their application: thus restricted, the behavior of those concepts was sufficiently well understood for scientists to judge whether the *physical* inferences they licensed under a given physical interpretation (RIC1) were in accord with their understanding of the target system. In Dirac's case, the restrictions allowed him to do so by showing that the delta function could ultimately be dispensed with altogether.

In contrast, representing such inferential restrictions in terms of a mapping account is considerably less straightforward. Consider, for example, how the partial structures approach would treat the Dirac delta function. Because the properties $\int_{-\infty}^{+\infty} \delta(x)dx = 1$ and $\delta(x) = 0$ for $x \neq 0$ are inconsistent, each must be merely partially true in the partial structure representing the mathematical theory as a whole. This structure is directly mapped to the target structure.[7] But the important properties of this structure are those it shares with structures in which one or the other is strictly true. When one reasons as if $\int_{-\infty}^{+\infty} \delta(x)dx = 1$, one reasons about a partial structure in which that sentence is strictly true. This reasoning is connected back to the structure in which it is merely partially true by means of a partial morphism. The restriction of the delta function to where it appears as a factor in an integrand can be reflected only indirectly in which structures are mapped back to the one that is interpreted physically and by which morphisms.

Compared to the RIC-based explanation sketched above, the partial structures explanation is positively baroque. It requires a proliferation of new resources—at least four structures and three mappings, even in this relatively simple case—to indirectly represent surface features of Dirac's practice that RIC can capture by appealing directly to syntactic restrictions that Dirac makes explicitly. And this divergence in complexity becomes more pronounced when scientists' inference strategies are themselves more complex, resulting in a spider's web of structures and morphisms.

This in itself needn't be a problem. A reconstruction of

[5] This is the essence of the "chunk-and-permeate" strategy (Brown and Priest, 2004), even if Brown and Priest don't explicitly present it in terms of the mapping account.

[6] That is, $\int_{-\infty}^{+\infty} \delta(x)dx = 1$ and $\delta(x) = 0$ for $x \neq 0$.

[7] This is a simplification. As Dirac saw it, this inferential restriction meant that the delta function could be dispensed with entirely, albeit at the cost of expressing quantum mechanics in a more cumbersome way. If we wished to represent this in terms of the partial structures approach, we would do so by adding a delta-free structure mapped to both the target structure and the partial structure just described. In terms of RIC, we would note that the restriction of the delta function to contexts in which it appears as a factor in an integrand, together with properties connecting such expressions to expressions in which the delta function does not appear, means that we could achieve an equivalent representation by replacing all inference patterns in which the delta function appears in RIC3 with inference patterns in which it doesn't appear.

complicated scientific reasoning should itself be expected to be complicated. The question is whether the additional layer of complexity introduced by mapping accounts adds any value, and it's not at all clear to me that it does in this case. I think it's telling that Bueno and French (2018, pp. 131ff), who treat this case at length, never explicitly appeal to partial structures, apart from writing, "we would speculate—although we shall not go into it here—that our framework of partial homomorphisms could quite naturally capture both the open-ended nature of Dirac's theory and the manner in which it can be related to Schwartz's" (pp. 136f). Instead, they write directly about the restriction of the delta function to contexts in which it is a factor in an integrand and how that together with Dirac's algebraic rules for manipulating the delta function in such contexts ensures that the delta function is dispensable. These features of Dirac's practice are at the center of his successful use of the delta function, but they can only be represented very indirectly in terms of partial structures. Explicitly representing the case in terms of partial structures would have distracted from these more important features. In contrast, they could be much more directly represented in terms of the set of inference patterns RIC3. And so this complexity arguably hinders the partial structures approach in its role as a meta-level device for representing philosophically significant features of scientific practice.

Note that the problem here isn't the move from classical to partial structures. The first type of classical structure-based approach, appealing to multiple classical structures to capture different aspects of the mathematical reasoning (cf. Benham, Mortensen and Priest, 2014), yields a similar proliferation of structures and morphisms without introducing any further resources for representing inferential restrictions. And an approach based on later, rigorous structures has even fewer resources for representing such restrictions. The right diagnosis, I think, is that the inferential restrictions used to successfully apply unrigorous mathematics in practice rarely correspond to neat divisions of some mathematical structure—or even divisions between several such structures. Even when a structure and mapping that correctly capture the informational content of the representation can be found, these don't lend themselves to a neat account of how they relate to the relevant inferential restrictions. It's in this sense that I claim that RIC facilitates more perspicuous representations of applications of unrigorous mathematics. Both RIC- and mapping-based explanations of the success of such applications, particularly of why it was reasonable for scientists to reason as they did, must make essential appeal to strategies of inference restriction used by those scientists. RIC directly represents such information, while mapping accounts can represent it at best very indirectly, placing more emphasis on the structural gymnastics required to accommodate such episodes.

Still, one might think that the mapping-based explanations are deeper or more substantial than their RIC-based counterparts in that they provide an explanation in terms of structures and mappings of the restricted inferences that RIC directly appeals to. Here, it is worth recalling the important similarity between RIC and mapping accounts discussed in §2.1. A mapping account's structures and mappings do the same work as the three components of RIC; they represent the informational content of the relevant mathematical scientific representation, which in turn is used to explain mathematically mediated inferences licensed by the representation. In either case, scientists' mathematically mediated inferences are ultimately justified by the supposition that the representation is in fact accurate. And so explanations of why particular mathematically mediated inferences are justified ground out in either case in the informational content of the relevant mathematical representation.

But what about the explanation of Dirac's *success* in using the delta function? Dirac's inferential restrictions are at the foundation of the RIC-based explanation of this success: by choosing the appropriate inferential restrictions, Dirac ensured the delta function behaved as desired, so that the resulting representations had the desired content. A proponent of a mapping account should flesh this out as follows: By making the appropriate inferential restrictions, Dirac ensured that he was reasoning about mathematical structures related in the right way to the relevant quantum mechanical structures. But it might seem that a mapping-based explanation achieves greater depth than the RIC-based explanation by going one step further, explaining those inferential restrictions in terms of structures and mappings. While the mere fact that mapping accounts represent such restrictions in terms of structures and mappings doesn't entail that it *explains* them in those terms, there is a feature of these restrictions that can be profitably explained in terms of structures and mappings: their appropriateness. Why are the restricted inferences appropriate? Because they are licensed in reasoning about a collection of structures and morphisms that collectively support using the delta function to reason about quantum mechanical structures. But this is just to say, in terms of the mapping account, that they are appropriate because they ensure that the representation has the desired informational content. And as before, this informational content can be specified (very often with greater ease) in terms of RIC to yield a parallel explanation. And so I see no reason to think explanations given in terms of structures and mappings are in virtue of that deeper than other explanations formulated in terms of RIC.

The rest of the paper is devoted to substantiating the points made here through an examination of Heaviside's operational calculus. In contrast to Dirac's, Heaviside's inference restriction strategies are strikingly local, piecemeal, and ad hoc (§4). I argue that these features put further stress on mapping accounts. I then consider two ways in which mapping accounts may be thought to have an explanatory benefit outweighing these shortcomings: explaining the role of the physical reasoning in informing Heaviside's mathematical reasoning (§5) and explaining Heaviside's success retrospectively in terms of later rigorous techniques (§6). I argue that neither case favors mapping accounts.

3. Heaviside's operational calculus and resistance operators

3.1. The operational calculus

As a bare mathematical device, Heaviside's operational calculus was a method for solving differential equations algebraically by treating differentiation as an operator. As a toy example, consider the equation $dx/dt = f(t)$. The first step was to reformulate the equations in terms of differential operators. In our example, this would yield $px(t) = f(t)$, with p the operator corresponding to d/dt. Next, Heaviside treated these operators as ordinary algebraic quantities, allowing him to solve the reformulated equations algebraically in terms of functions of these operators, construed as algebraic quantities. Call this the "operational solution." In our example, solving for $x(t)$ in the operationalized equation yields the operational solution $x(t) = p^{-1}f(t)$. Heaviside would then "algebrize" this solution to eliminate all reference to functions of differential operators, yielding the desired solution to the original differential equations. He most often achieved this by expanding functions of p in his operational solution in ascending or descending powers of p and applying rules for replacing particular expressions containing p with expressions for functions of t, though he occasionally used substitutions that did not require such power series expansions.[8] In our simple example, no power series expansion is required, but the expression p^{-1} requires an interpretation. Heaviside usually interpreted p^{-1} as the definite integral $\int_0^t du$, which in this case gives us $x(t) = \int_0^t f(u)du$.

Heaviside is far from the originator of methods treating differential operators as algebraic quantities independent of the functions they operate on. This was made possible by Leibniz's d/dx notation for differentiation and subsequently developed by many of the biggest names in late-eighteenth- and nineteenth-century mathematics, including Lagrange, Laplace, Fourier, Cauchy, and Boole, among others.[9] Heaviside is known to have studied the relevant work of Fourier and Boole in particular (Cooper, 1952, p. 12), on the basis of which he likely developed his own version of the operational calculus. While the novelty of Heaviside's approach, construed purely as a piece of mathematics, is up for debate,[10] the *primary* contribution of Heaviside's operational calculus was not that it taught mathematicians anything they did not already know about differential equations. Indeed, these mathematicians were able to prove more general results—and with greater rigor—than Heaviside. Rather, Heaviside's primary contribution in this work was, as Nahin (2002, p. 218, emphasis in original) puts it, to show "*how to apply to real, physical problems of technological importance* analytical techniques that had up till then been symbolic abstracts."

3.2. Resistance operators

Central to this task were what Heaviside called "resistance operators." These he used to present an early generalization of Ohm's Law:[11]

> If we regard for a moment Ohm's law merely from a mathematical standpoint, we see that the quantity R, which expresses the resistance, in the equation $V = RC$,[12] when the current is steady, is the operator that turns the current C into the voltage V. It seems, therefore, appropriate that the operator which takes the place of R when the current varies should be termed the resistance-operator. To formally define it, let any self-contained electrostatic and magnetic combination be imagined to be cut anywhere, producing two electrodes or terminals. Let the current entering at one and leaving at the other terminal be C, and let the voltage be V, this being the fall of potential from where the current enters to where it leaves. Then, if $V = ZC$ be the differential equation (ordinary, linear) connecting V and C, the resistance-operator is Z. (Heaviside, 1894, p. 355)

While Ohm's Law could be used only for the analysis of DC circuits in steady state or time-varying circuits without reactance, the concept of resistance operator generalized it to circuits with reactive elements and arbitrary time-varying voltages or currents: $V = ZC$ with Z the resistance operator for the circuit rather than its total resistance. While some work had been done to generalize Ohm's law to time-invariant AC circuits in the years immediately preceding Heaviside's work,[13] Heaviside's resistance operators were a significant step forward in that they could be used to analyze the behavior of a much wider range of systems.

Heaviside's operational calculus was central not just to how resistance operators played these roles, but also to the very possibility of expressing them. Resistance operators for circuit elements were derived from the usual equations describing their relation to voltage and current by solving for V/C. In the case of a resistor with resistance R, the relevant equation is just Ohm's Law, yielding $Z_R = V_R/C_R = R$. For reactive elements, the operational calculus becomes

[8] Cf. the schema presented by Lützen (1979, §I.5, pp. 170–2).

[9] For detailed treatment of these historical antecedents, albeit without reference to Heaviside, see Koppelman (1971). For a discussion that more explicitly ties this history to Heaviside, see Cooper (1952).

[10] For instance, Cooper (1952) argues that Heaviside's results are not very novel at all, Lützen (1979) responds by citing a number of distinguishing features of Heaviside's techniques. Petrova (1987) argues in turn that these distinguishing features were anticipated by Cauchy, Gregory, and Boole, but there is no consensus on this (see, e.g., Yavetz, 1995, p. 310, note 4).

[11] Prior to introducing this concept in 1887 in "On Resistance and Conductance Operators" (Heaviside, 1894, pp. 255–74), Heaviside used these techniques extensively in "On the Self-Induction of Wires" (Heaviside, 1894, pp. 168–323), first published in 1886–7. Aspects of them appear as early as "The Induction of Currents in Cores" (Heaviside, 1892, pp. 353–416), first published in 1884–5.

[12] Heaviside deviates from modern usage here in using C rather than I for current. I follow him in this throughout this paper for the sake of consistency.

[13] The first to do so seems to have been Wietlisbach (1879), and these techniques were subsequently refined by Oberbeck (1882) and popularized in Britain by Lord Rayleigh (1886a; 1886b; 1891). For a useful discussion of this history, see Kline (1992, pp. 77ff).

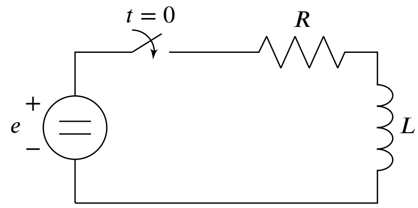

Figure 1: A DC circuit with a resistor and inductor in series, as treated in (Heaviside, 1899, §283, pp. 129f).

crucial, as the relevant equations are differential equations relating inductance or capacitance to voltage and current. The equation for an inductor with inductance L is $V_L(t) = L\frac{dC_L(t)}{dt}$, derived from Faraday's law. Heaviside derived its resistance operator by substituting his p operator for d/dt, yielding $V_L = LpC_L$. Solving for V_L/C_L yields $Z_L = V_L/C_L = Lp$. The resistance operator for the circuit as a whole is then calculated from those of its parts in much the same way as the total resistance of a configuration of resistors. For circuit elements with resistance operators $Z_1, \ldots, Z_n$ in series, $Z_{\text{total}} = Z_1 + \ldots + Z_n$. For circuit elements in parallel, $\frac{1}{Z_{\text{total}}} = \frac{1}{Z_1} + \ldots + \frac{1}{Z_n}$.

3.3. A simple example: Step response of an RL circuit

For a simple example of how this worked in practice, consider a circuit consisting of an ideal resistor with resistance R and an ideal inductor with inductance L in series—or, equivalently, a coil with resistance R and inductance L—with a constant external voltage e applied at $t = 0$. One thing we might want to know about this circuit is the resulting current C as a function of time. Heaviside discusses this problem in §283 of the second volume of *Electromagnetic Theory* (Heaviside, 1899, pp. 129f).

Since the resistor and inductor are in series, the resistance operator for the whole circuit is just the sum of the resistance operators of these elements—i.e., $Z = Z_R + Z_L$. And so, making the appropriate substitutions, we have $Z(p) = R + Lp$. To represent the external voltage's being applied at time $t = 0$, we represent the voltage by that external voltage e multiplied by the unit step function[14]:

$$\mathbf{1}(t) = \begin{cases} 0 & \text{if } t \leq 0 \\ 1 & \text{if } t > 0 \end{cases}.$$

Making the relevant substitutions in $V = ZC$ and solving for C then yields $C = e\mathbf{1}/(R + Lp)$. This is the operational solution of the problem.

Heaviside's next step was to "algebrize" this solution to yield an expression for C as a function of t, rather than p. Heaviside expands the right-hand side of the operational solution in descending powers of p, yielding

$$\begin{aligned} C &= \frac{e}{R + Lp}\mathbf{1} = \frac{e}{Lp(1 + R/Lp)}\mathbf{1} \\ &= \frac{e}{R}\left(\frac{R}{L} \cdot \frac{1}{p} - \left(\frac{R}{L}\right)^2 \cdot \frac{1}{p^2} + \left(\frac{R}{L}\right)^3 \cdot \frac{1}{p^3} - \ldots\right)\mathbf{1}. \end{aligned}$$

Heaviside then had to give meaning to the expression $1/p$ (or p^{-1}). In cases like this, Heaviside interpreted this as the inverse operator of $p = d/dt$—since algebraically we should have $p \cdot p^{-1} = 1$, and multiplication by 1 should correspond to the identity operator—and took this inverse operator to be $1/p = \int_0^t du$. $1/p^n$ then comes to represent n-fold integration.

So we have

$$\frac{1}{p} \cdot \mathbf{1} = \int_0^t \mathbf{1}\, du = \begin{cases} 0 & \text{if } t \leq 0 \\ t & \text{if } t > 0 \end{cases}$$

and so

$$\frac{1}{p^n} \cdot \mathbf{1} = \begin{cases} 0 & \text{if } t \leq 0 \\ t^n/n! & \text{if } t > 0. \end{cases}$$

So the power series can be rewritten as

$$\begin{aligned} C &= \frac{e}{R}\left(\frac{R}{L} \cdot \frac{1}{p}\mathbf{1} - \left(\frac{R}{L}\right)^2 \cdot \frac{1}{p^2}\mathbf{1} + \left(\frac{R}{L}\right)^3 \cdot \frac{1}{p^3}\mathbf{1} - \ldots\right) \\ &= \frac{e}{R}\left(\frac{R}{L}t - \left(\frac{R}{L}\right)^2 \frac{t^2}{2!} + \left(\frac{R}{L}\right)^3 \frac{t^3}{3!} - \ldots\right), \end{aligned}$$

which Heaviside recognized as the power-series expansion of

$$C = \frac{e}{R}\left(1 - \epsilon^{-(R/L)t}\right)$$

for $t \geq 0$, the correct result.[15]

4. Failures of rigor and inferential restrictions

4.1. Layered local inferential restrictions

We are already in a position to observe several ways in which these techniques failed to live up to the standards of mathematical rigor—in the hands of Heaviside at any rate. In each case, the failures of rigor do not involve the derivation of any straightforwardly incorrect result, but rather, as Yavetz (1995, p. 317) puts it, "the use of terms and procedures that are seldom fully defined". As in other examples of unrigorous mathematics, the result is a degree of indeterminacy in the global mathematical commitments of the operational calculus, and Heaviside avoided deriving incorrect results by adopting an inferentially restrictive methodology. However, as we will see, these restrictions were more local, piecemeal, and ad hoc than in other cases, and his ultimate justification for them was atypical. As a result, these

[14] In more recent texts, it is more common to use $H(t)$ for the unit step function in Heaviside's honor. Here I use a boldface $\mathbf{1}$ to remain close to Heaviside's own notation, while marking the difference between the unit step function and the integer 1. Heaviside typically left multiplication by the unit step function $\mathbf{1}$ implicit in such cases, much as one normally leaves multiplication by the integer 1 implicit. In the rest of this discussion, I've added in instances of $\mathbf{1}$ where Heaviside leaves them implicit in his treatment of this example in (Heaviside, 1899, pp. 129f) for the sake of clarity.

[15] Here again, I follow Heaviside in using ϵ rather than e for Euler's number.

inference strategies are even more difficult to treat straightforwardly in structural terms that those considered in section 2, while RIC can again represent them with relative ease.

An illustrative problem present in the example in §3.3 was that Heaviside's techniques often involved interpreting the inverse of his time-differentiation operator $p = d/dt$ as $p^{-1} = \int_0^t du$, but, thus interpreted, these are not generally inverse operators. Following Nahin (2002, p. 232), consider again the simple differential equation $dx/dt = f(t)$. As we saw in the previous section, applying Heaviside's operational techniques yields the solution $x(t) = \int_0^t f(u)du$. But this entails that $x(0) = 0$, since $\int_0^0 f(u)du = 0$ for any f we like. Since not every function x has this property, p and p^{-1} are not generally inverse operators. This problem is blatant and easily avoided in this simple case by restricting the use of this reasoning to cases in which we know that $x(0) = 0$ (for all functions x that might be the operand of p or p^{-1}). Heaviside observed this in practice by restricting his attention almost exclusively to circuits in which the voltage and current are enveloped by the unit step function $\mathbf{1}$.

However, further restrictions turn out to be required. If p and p^{-1} are inverse operators, they must also be commutative, and Heaviside ultimately rests his justification of this property on an interpretation of $p\mathbf{1}$ as the (inconsistent!) Dirac delta function:

> Thus $pp^{-1}\mathbf{1} = pt = 1$ but $p^{-1}p\mathbf{1} = [p^{-1}]0 = 0$, unless we say $p^{-1}p\mathbf{1} = p^{-1}\frac{t^{-1}}{(-1)!} = \frac{t^0}{0!} = 1$. This property has to be remembered sometimes. (Heaviside, 1899, §358, p. 298)

$p\mathbf{1}(t)$ must be zero except when $t = 0$, since it jumps from 0 to 1 at $t = 0$ and is otherwise constant. If $p^{-1}p\mathbf{1} = \int_0^t p\mathbf{1}(u)du = 1$, then $p\mathbf{1}$ can only be the Dirac delta function.[16]

We should interpret Heaviside's move here not as an attempt to rigorously justify the commutativity of the p and p^{-1} operators but instead as a way of managing inconsistent demands on the behavior of these operators. In essence, Heaviside is adopting an inferential strategy that makes the line of reasoning that leads to the contradiction in this case—namely, $p^{-1}p\mathbf{1} = p^{-1}0 = 0$—off-limits. Rather, $p\mathbf{1}$ must be reasoned with as if it were the Dirac delta function, so that only the second line of reasoning—$p^{-1}p\mathbf{1} = p^{-1}\frac{t^{-1}}{(-1)!} = \frac{t^0}{0!} = 1$—is allowed. This second line of reasoning is still unrigorous, since it appeals to the Dirac delta function under the guise of $p\mathbf{1} = \frac{t^{-1}}{(-1)!}$. But by limiting oneself to one or the other of these lines of reasoning, one rules out an obvious way of deriving an explicit contradiction from the inconsistent properties of the Dirac delta function.

[16] Recall that the Dirac delta function is defined by the properties $\delta(x) = 0$ for $x \neq 0$ and $\int_{-\infty}^{\infty} \delta(x)dx = 1$. In the presence of the first property, the second is equivalent to $\int_0^t \delta(x)dx = p^{-1}\delta = 1$. This, incidentally, is why the many attempts to provide the operational calculus with a purely algebraic, as opposed to analytic, foundation failed. Making p and p^{-1} commute means living with the Dirac delta function or something very much like it. For a useful historical treatment of such approaches, see Lützen (1979, pp. 188ff).

While this on its own is not enough to guarantee that one won't derive explicit contradictions via the inconsistent properties of the delta function, Heaviside does seem to have implicitly adopted a strategy similar to Dirac's strategy of using the delta function only as a factor in an integrand. In Heaviside's case, it was the step of "algebrization" that allowed instances of $p\mathbf{1}$ to be dispensed with via identification of $p^{-n}\mathbf{1}$ with $t^n/n!$ only after the algebraic manipulation of p-expressions required to derive the operational solution had been completed.

So to ensure that p and p^{-1} were inverse operators while avoiding nasty side effects, Heaviside had to observe several inferential restrictions at different levels of grain: (1) That property could only be appealed to when those operators were applied to functions enveloped by the unit step function, (2) p must be taken to be the ordinary time derivative *except when applied to step functions*, in which case its integral is non-zero, and (3) reasoning as if $p\mathbf{1}$ were the Dirac delta was restricted to a certain part of Heaviside's overall operational procedure, during which those operators are treated merely algebraically prior to the (counterintuitively named) step of "algebrization," during which they were again interpreted analytically.

While Heaviside's practice was far from ideal, RIC allows us to express a natural account of why it was nonetheless reasonable. Each of these local inferential restrictions served to rule out certain ways of working with p and p^{-1} that produced undesirable algebraic behavior by appealing to properties of those operators that were otherwise useful. When these concepts were constrained to contexts in which their algebraic behavior was understood—represented by RIC in terms of excluding the relevant patterns of inference from RIC3—they yielded determinate numerical results, which could be compared with known results or with Heaviside's physical understanding of his target systems via the interpretation given by RIC1. In essence, by restricting those operators to contexts in which they are well-behaved, Heaviside ensured that his representations (at least as far as those operators are concerned) have determinate accuracy conditions and so could be evaluated in terms of their agreement with both theoretical understanding of the target phenomena and experimental results.

Mapping accounts may be able to tell a similar story, but only in a cumbersome way. Consider how the partial structures approach might do so, which can again be naturally adapted to an approach based on classical structures. Directly mapped to the target structure will be a structure in which, for each problematic property of p and p^{-1}, it is merely partially true that they have that property: that p and p^{-1} are inverse operators, that p and p^{-1} *aren't* inverse operators, that p and p^{-1} commute, that p and p^{-1} *don't* commute, that $p^{-1}p\mathbf{1} = 0$, that $p^{-1}p\mathbf{1} = 1$, and so on. In particular contexts, one reasons with partial structures extending this one in which some of these statements are strictly true. Each of these structures is then mapped back to the structure is which all these statements are merely partially true either directly or through mappings to intermediary structures.

Heaviside's inferential restrictions are then represented indirectly in terms of which structures can be reasoned about in which contexts and how they can be mapped to other structures in this family. As in the case of the Dirac delta, I suspect that the inclusion of partial structures gives only a false appearance of explanatory depth here. Each explanation ultimately bottoms out in properties of the inferential restrictions Heaviside observes. Representing those restrictions indirectly in terms of structures adds unnecessary complexity and obscures the features of Heaviside's practice that do the explanatory heavy lifting.

4.2. Ad hoc inferential restrictions

A further striking feature of Heaviside's inference strategies is their often ad hoc nature. Heaviside generally didn't lay them out in advance and expressed comfort with the possibility that such techniques might, if used injudiciously, lead to inconsistent results.

One striking example is that Heaviside frequently treated his operators in general (not just p and p^{-1}) as if they were commutative, though they don't generally have this property. Heaviside explicitly noted this but made no attempt at a general explanation of when such moves were permissible. In a representative passage, he writes,

> The reader may have noticed in the above, and perhaps previously, that we change the order of operations at convenience, as in $f(p)\phi(p)\mathbf{1} = \phi(p)f(p)\mathbf{1}$, and that it goes. But I do not assert the universal validity of this obviously suggested transformation. It has, however, a very wide application, and transforms functions in a remarkable manner. *Reservations should be learnt by experience*. (Heaviside, 1899, §251, pp. 59f, my italics)

The idea seems to be to freely appeal to the commutativity of his operators in contexts where this *works*, determining which contexts these are through experimentation.

Heaviside expressed the same attitude toward one of his most central results, the expansion theorem, which Lützen (1979) calls "Heaviside's most important tool in algebrizing procedures" (p. 170): where $e = ZC$ is an operational solution, e is a constant multiplied by the unit step function, and "the form of Z [is] such as to indicate the existence of normal solutions for C,"

$$C = \frac{e}{Z_0} + e \sum \frac{\epsilon^{pt}}{p\frac{dZ}{dp}}$$

(Heaviside, 1899, §282, p. 127). This was a powerful tool because it worked in such a wide range of circumstances, including for algebrizing operational solutions of partial differential equations describing continuous telegraph circuits. But again it was not true generally. Heaviside insisted that it was actually *undesirable* to state precisely the conditions in which it can be used:

> Now it would be useless to attempt to state a formal enunciation to meet all circumstances. [...] *It is better to learn the nature and application of the expansion theorem by actual experience and practice*. (Heaviside, 1899, §282, p. 128, my italics)

So what do we make of this? What Heaviside seems to be proposing is what we might call an *ad hoc* inference restriction strategy: inferences are restricted not in advance, but only as we discover that certain patterns of inference produce incorrect or undesired results. Until we make such a discovery we must "keep [our] eyes and [our] mind open, and be guided by circumstances" (Heaviside, 1899, §223, p. 3). How do we make sense of the success of such a strategy, and, in particular, to what extent was it reasonable for Heaviside to adopt it?

Ultimately, such a strategy balances the benefits of a mathematical opportunism with a recognition that results thus obtained are more fallible than those obtained by rigorous means. Recognizing this fallibility means continually scrutinizing the results achieved with suspect mathematics on the basis of their agreement with known results and one's independent understanding of the target system. RIC offers considerable flexibility in how we represent this scrutiny.

On one hand, we might think of such a strategy as one in which inferences are added to RIC3 in piecemeal fashion, so that the commitments of the representation never outstrip those results that have actually been derived. When an inference would lead to an inconsistent result or to a result that can otherwise be ruled out—for instance, because it has physical consequences that are known to be incorrect—that inference is simply not added to RIC3. As a result of this extreme conservatism, the representation cannot be committed to anything undesirable. This might be relaxed in contexts in which the problematic mathematics is better understood. Heaviside writes of "numerical groping" only as a technique of last resort "when [physical] intuition breaks down" (Heaviside, 1899, §437, pp. 461f). In less desperate cases, these piecemeal additions might be higher-level inference *types* in which one has gained confidence.

On the other hand, we might think of such a strategy as one in which inferences are *excluded* from RIC3 in piecemeal fashion, so that inferences are only disallowed when they are shown to lead to results that are inconsistent or can otherwise be ruled out. In this case, the representation is very likely to be inconsistent, but its use does not require its users to commit themselves to its accuracy. Instead, a user of such a representation might only commit themselves to the accuracy of those results actually derived and scrutinized (in parallel fashion to the previous case), reserving judgment about other commitments of the representation or at least keeping their fallibility well and truly in mind.

Ultimately, a combination of the two approaches is likely to be most useful, with the former accounting for Heaviside's own commitments and the latter accounting for his opportunistic heuristic use of less well-understood techniques. Inference patterns from the latter are incorporated into the former only after they have been suitably scrutinized. This allows epistemically suspect results of unrigorous techniques to be quarantined even when one does not yet have a good understanding of the contexts in which they can safely be applied.

Now consider how the partial structures approach would treat this inference strategy.[17] In this case, the move corresponding to the addition of an inference to RIC3 is extending the existing morphisms between the partial structures at work in the representation in a way that licenses the inference and, when the representation cannot be made to license the inference otherwise, adding a further partial structure and morphism. In this way, the partial structures approach represents ad hoc reasoning in terms of ad hoc choices of structure and mapping. Now, as the addition of inferences becomes more and more piecemeal, it becomes less clear to me that the partial structures approach has the resources to license exactly those inferences without licensing further inferences not licensed by the corresponding RIC representation. But more importantly, this again introduces more complexity to do the same explanatory work. Because the relevant structures will often in practice be several morphisms away from the one that is directly mapped to the target structure, particularly when other strategies of inference restriction are also in use, the requisite additions will often be less than straightforward. And it is again unclear that the formal apparatus used to represent these inferential restrictions adds depth or substance to the explanation. What seems to do the work in explaining why such techniques were epistemically justifiable is simply the practice of withholding judgment about certain mathematical inferences until further support for their conclusions is found. And this practice is again more straightforwardly represented in terms of RIC.

5. Failures of rigor and "physical mathematics": The physical demand for fractional differentiation

Rather than the failures of rigor considered so far, it was Heaviside's treatment of fractional differentiation and divergent series that most upset his contemporaries. The latter ultimately led the Royal Society to begin subjecting his submissions to peer review and justified the rejection of his final submission (Cooper, 1952, p. 14).

The second volume of Heaviside's *Electromagnetic Theory* (Heaviside, 1899) begins with a spirited, but sometimes bitter and defensive, justification of his unrigorous techniques in the wake of this rejection. In addition to presenting a number of practical virtues of his unrigorous techniques, he presented an approach to mathematics that is deeply grounded in physical reasoning. In physics, the physical interpretation of the mathematics is to be kept in mind at all times, so that physical knowledge and intuition can guide one's mathematical work. (Heaviside, 1899, §224, pp. 4f; see also §437, pp. 460f) We've already seen one way in which it can do so: serving as means of checking results derived mathematically. But mathematics, according to Heaviside, is also answerable to physics in that, if a physical representation requires us to use a mathematical expression that appears to be mathematically meaningless, we can conclude that that piece of mathematics is indeed meaningful and use the physics to elucidate its behavior (Heaviside, 1899, §224, pp. 6f). Even if we reject the idiosyncratic empiricist philosophy of mathematics Heaviside used to bolster these claims, we can make sense of them in a context where the needs of physics outstrip existing mathematical resources. In such cases, physics can legitimately guide the development of new mathematical theories and techniques tailored to particular kinds of physical problem. This sort of inferential move from physics to mathematics is common in Heaviside's practice.

Heaviside was driven to the topics of fractional differentiation and divergent series by his representation of semi-infinite, continuous systems, particularly the representation of a semi-infinite transmission line via the telegraph equations

$$-\frac{\partial V(x,t)}{\partial x} = RC(x,t) + L\frac{\partial C(x,t)}{\partial t}$$
$$-\frac{\partial C(x,t)}{\partial x} = KV(x,t) + S\frac{\partial V(x,t)}{\partial t},$$

where R, L, K, S are resistance, inductance, "leakance" (conductance between the signal and return wires), and capacitance per unit length of the telegraph wire, respectively.[18] Replacing $\partial/\partial t$ with Heaviside's p and doing some algebra yields

$$V = \sqrt{\frac{R+Lp}{K+Sp}}C.$$

Producing numerical solutions or indeed any solutions expressed in terms of functions of t rather than p, even for special cases in which we ignore one or more of R, L, K, and S, requires making mathematical sense of expressions like '$p^{\frac{1}{2}}\mathbf{1}$'. Heaviside's confidence in the representation of such systems given by the telegraph equations grounded his conviction that this operational equation must have numerical solutions (if it is to adequately represent these systems) and thus that there was indeed sense to be made of such expressions.[19]

[17] It can again be adapted to an approach positing multiple classical structures in a straightforward way.

[18] Heaviside's treatment of this case can be found in (Heaviside, 1899, chapter 7).

[19] Heaviside made a similar move in interpreting the divergent series expansions of various operational solutions of the telegraph equation. He made no reference to the then-burgeoning theory of divergent series, but he rightly recognized certain divergent series expansions as what we would now call "asymptotic expansions" of the relevant functions on the basis of their physical meaning (i.e., the meaning of the physically interpreted mathematical expression). He even correctly conjectured that the divergent parts of these asymptotic expansions are meaningful, carrying information about the exact value of the function that they approximate, again based on a physical interpretation of the components of the series. Nonetheless, as John R. Carson observed, "the precise sense in which the expansion asymptotically represents the solution cannot be stated in general, but requires an independent investigation in the case of each individual problem" (Carson, 1926, p. 78). This relied on a notion of "equivalence" between convergent and divergent series that Heaviside left undefined (e.g., Heaviside, 1899, §340, p. 250).

Heaviside did so via the equation $p^{\frac{1}{2}}\mathbf{1}(t) = (\pi t)^{-\frac{1}{2}}$, a result known at least as early as 1819 by Sylvestre Lacroix (1819, pp. 409f), but derived independently by Heaviside by more "experimental" means.[20] One such derivation is the following. If we ignore leakage and inductance, we can derive the equation

$$C = (Sp/R)^{\frac{1}{2}}e\mathbf{1}$$

for the current at $x = 0$ where $e = V(0,0)$. One way to algebrize this operational equation is by considering the case in which the wire has finite length l, producing a Fourier series expansion for the finite case (via Heaviside's expansion theorem), and taking the limit as $l \to \infty$. Using the result to calculate the current at $x = 0$ yields a new expression for the current at $x = 0$:

$$C = \frac{2e}{R\pi}\int_0^\infty \epsilon^{-s^2t/RS}ds = (S/R\pi t)^{\frac{1}{2}}e.$$

Comparing our two formulae for current at $x = 0$ and doing a little more algebra yields $p^{\frac{1}{2}}\mathbf{1} = (\pi t)^{-\frac{1}{2}}$ (Heaviside, 1899, §350; cf. §240). After deriving this, he writes, "The above is only one way in a thousand. I do not give any formal proof that all ways properly followed must necessarily lead to the same result" (Heaviside, 1899, §350, p. 288). Despite the lack of assurance that this is the unique possible result and despite its reliance on a particular special case (the telegraph equation without inductance or leakance), in the rest of the same chapter Heaviside uses this equation to provide an account of more general fractional differentiation, including half-integer differentiation and cases in which polynomials in p occur under the radical, as well as the operational solution of the telegraph equations in their full generality.

Here Heaviside moves from a physical system represented in operational terms, a semi-infinite telegraph cable with no inductance or leakage, to a conclusion about the mathematics apparently used to represent this very system. The thought seems to be that, *whatever the underlying mathematics*, if it is to play the right role in representing this particular physical case, then it must interpret $p^{\frac{1}{2}}\mathbf{1}$ as $(\pi t)^{-\frac{1}{2}}$ (at least in this instance). And if it does that, it must interpret $p^{\frac{n}{2}}\mathbf{1}$ for odd n as $p^{\frac{n-1}{2}}p^{\frac{1}{2}}\mathbf{1}$. Consideration of other cases determines a value for $p^{\frac{1}{4}}\mathbf{1}$. Using these values that are necessitated by the physics (in its operational representation), Heaviside then can test possible extensions of p^x for arbitrary $x \in \mathbb{R}$. It is agreement with these results, rather than reasoning from first principles, that justifies his choice of extension of p^x to arbitrary real x.[21] (Heaviside, 1899, ch. 7)

It might seem at first that this is a case in which mapping accounts can provide a significant explanatory benefit even in the absence of a well-understood mathematical theory, contrary to my claims in section 2 and in contrast to the cases considered in the previous section. Such a benefit might in turn justify the greater complexity with which mapping accounts must represent Heaviside's strategies of inference restriction. According to this line of thought, Heaviside learned about the relevant mathematical structure by making inferences about that structure on the basis of its structural relation to its target system. So even if Heaviside didn't start with a well-understood mathematical theory that neatly picked out a particular structure, mapping accounts can nonetheless explain how he came to understand some of the properties of the mathematics needed to represent his chosen target systems.

But closer analysis of the case doesn't bear this out. In this case, the only conclusion that Heaviside used considerations about the physical system to directly support is that fractional powers of p must be able to be used meaningfully. The operational version of the telegraph equations, which Heaviside took to accurately represent the relevant physical systems, can only be algebrized if such expressions can be manipulated, and only the algebrized equations, expressed in terms of functions of time rather than p, can be used to derive numerical solutions, which, when interpreted, yield determinate predictions about the behavior of the target system. That is, if the operational representation can be used at all, fractional powers of p must be able to be manipulated. So far, this means only that inferences in which such expressions appear can't be ruled out wholesale by any inference restriction strategy Heaviside might adopt for reasons like those discussed in the previous section.

When Heaviside determined how $p^{\frac{1}{2}}\mathbf{1}$ should be reasoned with, it was by comparing an operational expression for a given physical quantity—$C = (Sp/R)^{\frac{1}{2}}e\mathbf{1}$—with an expression for the same quantity that can be derived by extending existing algebrization procedures to this new context—$C = (S/R\pi t)^{\frac{1}{2}}e$. The result can be interpreted as telling us how $p^{\frac{1}{2}}\mathbf{1}$ must be reasoned with provided that the expansion theorem can be extended to this context in this way. But, as we saw earlier, Heaviside made a point of not committing to the uniqueness of his interpretation of $p^{\frac{1}{2}}\mathbf{1}$. A simple extension of his algebrization strategies to this new case might yield inconsistent results, in which case some of the inferences involved in deriving these inconsistent results might need to be restricted, perhaps even those that allowed him to derive $p^{\frac{1}{2}}\mathbf{1}(t) = (\pi t)^{-\frac{1}{2}}$. So the inference of the properties of fractional powers of p shouldn't be represented as a simple inference of the properties of one (mathematical) structure on the basis of another (physical) structure and a morphism between them. Instead, the same sorts of strategies of inference restriction discussed in the previous section must still be at the heart of an explanation of why Heaviside's treatment of fractional differentiation was epistemi-

[20] For an extended treatment of the history of fractional differentiation, see Ross (1977).

[21] This reasoning from first principles would likely involve the gamma function, which extends the factorial function to the complex numbers. In the end, Heaviside does much of the work of defining the gamma function for arguments with non-positive real parts via analytic continuation (albeit restricting his attention to the reals), but this again is used because it is the most expedient way to define p^x for negative x to suit the physical cases at hand. (Heaviside, 1899, §425, p. 435)

cally respectable—at least from a physical, rather than mathematical, perspective. And so the arguments from that section apply here as well.

6. The Laplace transform in heavy disguise?

So far, I have largely ignored the possibility of making sense of Heaviside's success in terms of a structure picked out by later, more rigorous alternatives to his techniques. This is because I have focused on why his techniques were epistemically justifiable at the time he used them, and I see no reason to think that a mapping account appealing to later rigorous structures would be any better situated to provide that sort of explanation than the versions of the mapping account I've considered so far. But there is another sort of explanation of Heaviside's success that explains why the *results* of those techniques were correct, regardless of their epistemic status at the time, in terms of later mathematics.

We can appreciate the importance of such explanations regardless of whether we feel the pull of mapping accounts. For instance, Wilson (2006, ch. 8) uses the case of Heaviside's operational calculus to argue against a classical view of concepts according to which their meaning must be grasped once and for all at the outset. This presents a difficulty for mapping accounts, as it means that many of scientists' mathematically mediated inferences aren't explicitly grounded in the existence of structural relations, as scientists' concepts don't suffice to pick out the needed structures. But Wilson makes sense of Heaviside's inferences in a local way through what he calls "correlational pictures", "generic stories that speakers tell themselves with respect to how their predicate's usage matches to worldly support within normal circumstances of application" (p. 516). Heaviside's contemporaries gave him grief because "he was unable to supply orthodox mathematical underpinnings for his procedures in terms of an adequate associated picture" (p. 521). But Heaviside was vindicated by subsequent rigorous work that provided such a picture. And mapping accounts have a neat story to tell about how this later work did so. The question is then whether this gives us good reason to favor mapping accounts over RIC, and in this section I argue that it doesn't.

This later mathematics was largely built on the Laplace transform and its inverse.[22] Heaviside biographer Paul J. Nahin goes as far as to write, "in fact, Heaviside's operational calculus is just the Laplace transform in heavy disguise" (2002, p. 218). The Laplace transform maps functions in the time domain to functions in the s-domain, where s is a complex number whose imaginary component represents the function's periodic behavior (frequency response) and whose real component represents its non-periodic behavior (e.g., its decay). The Laplace transform $F(s)$ of a function $f(t)$ is given by the integral $F(s) = \int_{0^-}^{\infty} f(t)e^{-st}\,dt$. The result typically looks a lot like Heaviside's operational solution of the same problem, but with s taking the place of p. Consider again the example in §3.3. Just as Heaviside started by calculating the resistance operators for the components of the circuit, a contemporary electrical engineering student could start by calculating the s-plane impedance of each circuit component by applying the Laplace transform to the differential equation characterizing it, yielding $Z_R(s) = R$ where Heaviside has $Z_R(p) = R$, $Z_L(s) = Ls$ where Heaviside has $Z_L(p) = Lp$, and so on. Corresponding to Heaviside's operational solution $C = e\mathbf{1}/(R + Lp)$, they would arrive at the Laplace transform for the circuit $I(s) = 1/(R + Ls)$ (using the modern notation, I instead of C, for current) and multiply it by the Laplace transform of the input signal, in this case V_0/s (the Laplace transform of the unit step function multiplied by the value of the voltage source).[23] Corresponding to Heaviside's "algebrization", they would translate this back to the time domain via the the inverse Laplace transform, given by $f(t) = \frac{1}{2\pi i} \lim_{T\to\infty} \int_{\gamma-iT}^{\gamma+iT} F(s)e^{st}\,ds$. In this case, applying the inverse Laplace transform to both sides yields $i(t) = \frac{V_0}{R}\left(1 - \exp(-\frac{R}{L}t)\right)$, the same result Heaviside achieved.[24]

Now, the relationship between Heaviside's operational calculus and the Laplace transform cannot be one of simple identification, as Nahin suggests. Heaviside himself certainly didn't think so.[25] More importantly, significant differences arise in practice between the two techniques. For one thing, it is certainly not the case that we can simply substitute s for each instance of p. Note that even in this simple case, we cannot do this for Heaviside's operational solution ($C = \frac{e\mathbf{1}}{(R+Lp)}$) and its equivalent in the s domain

[22] As Wilson (2006, p. 531) points out, the Laplace transform on its own doesn't suffice to vindicate all of Heaviside's techniques. In particular, those involving his use of $p\mathbf{1}$, interpreted as the Dirac delta, should be understood in terms of Schwartz's theory of distributions. Here I focus on the Laplace transform, but the points I make can be naturally extended to other pieces of mathematics used to retrospectively vindicate Heaviside.

[23] Strictly speaking, one should take the Laplace transform of each side of the differential equation characterizing the circuit as a whole, and most treatments work directly with this equation. But this can be done in terms of the Laplace transforms of the circuit elements thanks to the linearity of the Laplace transform, which allows for an approach closer to Heaviside's.

[24] For a representative recent treatment of this example, see Salivahanan et al. (2000, pp. 157f).

[25] In fact, he wrote to Bromwich, the first to rigorize the operational calculus via the inverse Laplace transform, "I never could stomach your complex integral method" (letter to Bromwich on 7 April, 1919, quoted in Nahin (2002, p. 230)). For a useful summary of Bromwich's (1916; 1928) approach, see Lützen (1979, pp. 176–180, 184–7).

Similarly, Heaviside's operational treatment of AC looks similar to Steinmetz's phasor method, the basis of the current treatment of impedance in terms of the complex plane, but Heaviside again resisted any such identification. In such cases, Heaviside interpreted p as ni, where n was angular velocity and i a differential operator that *behaved like* the imaginary unit. Heaviside explicitly contrasted this approach with approaches like Steinmetz's, which take the use of complex numbers seriously. In the latter case, one "[assumes] a complex form of solution at the beginning. It comes out complex at the end. [...] The algebra is that of the real imaginary." (Heaviside, 1899, §284, p. 132) In contrast, the i in Heaviside's $p = ni$ "must be finally interpreted correctly, as a differentiator, of course" (Heaviside, 1899, §284, p. 132). Putting it more strongly later, he wrote, "if i be used at all, it is only a spurious imaginary" (Heaviside, 1899, §437, p. 459). There is interesting historical and philosophical work to be done to make sense of Heaviside's claim not to be working with complex numbers in the same way as the likes of Steinmetz. Regrettably, space constraints mean I cannot discuss this further here. For a useful survey of early approaches to AC in terms of complex numbers and their relation to Steinmetz's phasor method, see Kline (1992, pp. 77ff).

(in modern notation, $I(s) = \frac{V_0}{s(R+Ls)}$) as a result of the additional factor of $1/s$ in the latter.[26] Calculations involving the Laplace transform were also often more cumbersome than the corresponding calculations in the operational calculus. Harold Jeffreys, who harshly criticized Heaviside's lack of rigor, nonetheless wrote

> [A]s a matter of practical convenience there can be no doubt that the operational method is far the best for dealing with the class of problems concerned. [. . .] [I]t is certain that in a very large class of cases the operational method will give the answer in a page when ordinary methods take five pages, and also that it gives the correct answer when ordinary methods, through human fallibility, are liable to give a wrong one. (Jeffreys, 1927, p. v)

But if we're only interested in a retrospective explanation of Heaviside's success, regardless of the epistemic status of his techniques at the time, no such identification is required. For example, in response to Wilson (2006), Pincock (2012, ch. 13) concedes that a scientist might not grasp concepts sufficient to pick out a determinate mathematical structure, but he suggests that we adopt a kind of semantic externalism according to which such scientists can be understood to articulate claims that go beyond the features of their concepts (both mathematical and physical) that they explicitly grasp. If so, we can understand Heaviside as unknowingly appealing to the sort of structure picked out with Laplace transform techniques. We then have a new explanation of Heaviside's success: Heaviside's mathematically mediated physical inferences succeeded because, unbeknownst to him, their mathematical part correctly characterizes structures picked out by the theory of Laplace transforms, and those structures stand in the right relationship to his target systems.

I am happy to concede that this is a perfectly good retrospective explanation of Heaviside's success, but I don't think it gives us reason to favor a mapping account. For one thing, retrospective explanations are also available in terms of RIC. Because RIC recovers mapping accounts as a special case, one option is to simply coopt the explanation just considered. But this is unnecessary. Why are Laplace transform techniques appropriate to do the work of the operational calculus? Because the inferences licensed by applications of the operational calculus, under the inference restriction strategies discussed so far, are a subset of those licensed by applications of Laplace transform techniques. And so the informational content of the former, treated in terms of RIC, is a subset of the informational content of the latter. Due to the rigor of Laplace transform techniques, we can be about as sure of their consistency and coherence as we can of any mathematical theory. As a result, experimental and theoretical agreement with representations involving Laplace transform methods confers a higher degree of epistemic support than similar agreement with representations appealing only to the operational calculus. When we can't be so sure of a mathematical theory's consistency and coherence and must therefore adopt flexible inference restriction strategies, we have less assurance that any such agreement won't be undermined by the derivation of problematic results that necessitate further inferential restrictions. In this way, the relationship between Heaviside's techniques and subsequent rigorous ones retrospectively bolsters the epistemic standing of applications of the former.

Is there reason to favor the mapping-based explanation on the grounds that it further explains the informational content of representations using Laplace transforms in terms of a mathematical structure? I don't think so. As I argued in section 2, RIC provides an alternative account of representations' informational content to explain how it licenses mathematically mediated inferences. We therefore don't need to appeal to later rigorous theories to explain how Heaviside's representations came to have informational content that justified his inferences. We only need to appeal to such theories to explain why these representations enjoyed a stronger epistemic status than he or his contemporaries could have appreciated.

Moreover, RIC has more to say about the relationship between applications of unrigorous techniques and their unrigorous counterparts than this sort of mapping account allows for. For example, different uses of the operational calculus bear remarkably different relationships to more rigorous mathematics, which must be understood in terms of different inferentially restrictive methodologies. Like Jeffreys, Bromwich recommended working with Heaviside's operational calculus rather than more rigorous mathematics as a matter of practical convenience. But he suggested an alternative inferentially restrictive methodology to Heaviside's "experimental" one: the operational calculus should not be used to derive any result that could not be derived independently via the method of Laplace transforms.[27] While Heaviside's inferentially restrictive methodology may have been appropriate in the absence of more rigorous alternatives, Bromwich's approach seems entirely more appropri-

[26] What is more, the means of reaching these two equations will generally differ. See footnote 23.

[27] Bromwich suggested such an approach in a letter to Heaviside:

> After coming back to these questions after 2½ years of war-work, I found myself able to work more readily with operators than with complex-integration. [. . .] I at once saw that I must make the operator-method take the leading place: and complex-integrals have accordingly been pushed into footnotes. I still regard the complex-integral as a useful method for convincing the purest of pure mathematicians that the *p*-method rests on sound foundations: but I am sure that the *p*-method is the working-way of doing these things. (Letter to Heaviside on 5 April, 1919, quoted in Nahin (2002, p. 229))

Bromwich was not rewarded for his kindness. Heaviside responded,

> I rejoice to know that you have seen the simplicity and advantages of my way [. . .]. Now let the wooden headed rigorists go hang, and stick to differential operators and leave out the rigorous footnotes. It is easy enough if you don't stop to worry. [. . .] I never could stomach your complex integral method. (Letter to Bromwich, 7 April, 1919, quoted in Nahin (2002, pp. 229f))

ate once Laplace transform methods have been shown to do rigorously what Heaviside's methods could only do unrigorously. Once one can calculate inverse Laplace transforms via the Bromwich integral, one has a reliable, general means of checking results derived via the operational calculus, so that more ad hoc inferential restrictions serve little purpose.

Finally, even limiting our attention to "correlational pictures" in Wilson's sense, the costs of limiting ourselves to retrospective explanations is high. *Pace* Wilson, the explanation of Heaviside's success is not simply "because he was lucky" (Wilson, 2006, 528) to have picked out algebraic rules that both were useful and could be vindicated by later rigorous work, but rather that he took great care to calibrate his techniques to the physical problems in which he used them. As I argued in section 4, Heaviside's inference restriction strategies served to ensure that his representations had determinate accuracy conditions in restricted contexts and so could be evaluated in terms of their agreement with theoretical and experimental results. His ad hoc restrictions ensured that he could continually submit his results to theoretical and experimental scrutiny when he ventured out onto shakier ground. This certainly seems to allow him to tell a convincing story about how the usage of his operational techniques "matches to worldly support within normal circumstances of application" (Wilson, 2006, p. 516), albeit not one with all of the epistemic benefits of a more rigorous approach. An approach to supplying correlational pictures to justify Heaviside's inferences that is limited to retrospective explanations in terms of rigorous theories therefore misses an important part of the justification of Heaviside's practice.

7. Conclusion

Central to how unrigorous mathematics can be successfully applied are the inferentially restrictive methodologies scientists use to manage the risks of working with an unrigorous theory. This is particularly clear in the case of Heaviside's operational calculus, which required him to make largely piecemeal inferential restrictions and to appeal, among other things, to the physical interpretation of the mathematics to determine how his mathematical tools ought to behave. These practices are naturally represented in terms of RIC, but at best in a cumbersome and indirect way in terms of mapping accounts. As a result, RIC can be used to formulate better explanations of the success of physical inferences based on unrigorous mathematics—both why it was reasonable for Heaviside to adopt such techniques and why, in light of later developments, the results of these techniques were correct. Since RIC can recover the mapping account as a special case, and so does at least as well as mapping accounts in contexts where the mapping account succeeds, this gives us good reason to favor RIC.

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