Conditions on the Use of the One-dimensional Heat Equation

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Abstract. This paper explores the conditions under which scientists are warranted in adding the one-dimensional heat equation to their theories and then using the equation to describe particular physical situations. Summarizing these derivation and application conditions motivates an account of idealized scientific representation that relates the use of mathematics in science to interpretative questions about scientific theories.

Introduction

Almost all of our best, current scientific theories use mathematics. Philosophers who have been preoccupied about this have generally been philosophers of mathematics wrapped up in some species of indispensability argument and the realism/nominalism debate about mathematics. Setting this debate aside, what is left for the philosopher to worry about when it comes to the application of mathematics in science? I believe that philosophers of science should pay greater attention to the role of mathematics in our scientific theories and the constraints that this places on admissible interpretations of these theories. Suppose, for example, that I use the real numbers to represent some physical magnitude like temperature. There is no greatest real number, the real numbers are continuous and there are continuous but nowhere differentiable functions of a real variable. Which, if any, of these three mathematical truths play a role in using real numbers to represent temperature? Certainly the fact that the real numbers are continuous by itself does not imply that temperatures themselves are continuous. Field seems to make this inference, though, when he says that

> temperature and other scalar fields used in physics are assumed to be continuous, and this guarantees that if point x has temperature $\psi(x)$ and point z has temperature $\psi(z)$ and r is a real number between $\psi(x)$ and $\psi(z)$, then there will be a point y spatio-temporally between x and z such that $\psi(y) = r$ ([8], 57).

Even if we are skeptical of this sort of claim, the burden seems to be on the skeptic to say which properties of the mathematics cannot be transferred to the physical magnitudes and why.

I believe some progress can be made by focusing not on the general question of representing physical magnitudes by mathematical objects, but rather on particular equations that scientists themselves accept (or at one time accepted).[1] I take as my focus here one of the simplest equations that involves a modicum of advanced mathematics: the one-dimensional heat equation. For $u(x,t)$ representing the temperature of point x at time t, we can derive the partial differential equation:[2]

$$\alpha^2 u_{xx} = u_t \tag{6}$$

where $\alpha^2 = \kappa/\rho s$, κ is the thermal conductivity of the material, ρ its density and s the specific heat of the material. Throughout subscripts indicate partial differentiation with respect to that variable, e.g. $u_t = \frac{\partial}{\partial t} u(x,t)$.

Now our question is this: under what conditions are scientists warranted in adding this equation to their scientific theory of heat and under what conditions are scientists warranted in using this equation to describe a particular physical system? I call the former question the search for the derivation conditions of our equation and the latter question the search for the application conditions. As we will see, even in the simple case of the one-dimensional heat equation, the derivation and applications conditions can be usefully distinguished. Furthermore, there are two attitudes that we can take to (6), only one of which I believe to be warranted by the evidence that scientists typically have available. I will conclude by relating this attitude to some broader interpretative questions about mathematical scientific theories.

Derivation conditions

To repeat, by the derivation conditions of (6) I mean to indicate the assumptions we must make to reasonably add (6) to our scientific theory. Field may believe that the derivation conditions include that temperatures are continuous. I do not believe this is true, and will review one derivation of (6) to show this.

To add (6) to their theories scientists appeal to two experimentally determined laws. These laws connect heat, considered as a physical magnitude Q, to temperature in two different ways:

- Newton's law of cooling: the amount of heat per unit of time that passes from a warmer plate 2 to a cooler plate 1 is

$$H = \frac{\Delta Q}{\Delta t} = \frac{\kappa A |T_2 - T_1|}{d} \tag{7}$$

[1] Cf. Steiner on descriptive applicability ([15], ch. 2).
[2] I closely follow the presentation of [4], 514-584.

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where T_2 and T_1 are the respective temperatures of the plates, A is their area, d their distance from each other and κ is the thermal conductivity of the material.

- A claim about the average change in temperature in an element: Δu increases in proportion to $Q\Delta t$, but is inversely proportional to $s\rho A\Delta x$, where s is the specific heat, ρ the density, A the cross-sectional area, Δx distance, and so $\rho A\Delta x$ is the mass of the element:

$$\Delta u = \frac{Q\Delta t}{s\rho A\Delta x} \tag{8}$$

To derive (6) we consider a perfectly insulated, homogeneous circular rod with endpoints $x = 0$ and $x = l$, and consider what (7) implies about how heat will be conducted in an arbitrary element of the rod, stretching from x_0 to $x_0 + \Delta x$. From left to right, across the x_0 boundary, the heat transfer at instant t, $H(x_0, t)$, will be

$$\begin{aligned} H(x_0,t) &= -\lim_{d\to 0} \kappa A\,[u(x_0+\frac{d}{2},t) - u(x_0-\frac{d}{2},t)]\;/\;d \\ &= -\kappa A u_x\,(x_0,t) \end{aligned} \tag{9}$$

Similarly, at the other boundary, the heat transfer from left to right is

$$H(x_0+\Delta x,t) = -\kappa A u_x(x_0+\Delta x,t) \tag{10}$$

So, the net heat flow Q into our element will be $H(x_0, t)$ minus the amount flowing out given by $H(x_0 + \Delta x, t)$:

$$\begin{aligned} Q &= -\kappa A u_x(x_0,t) - (-\kappa A u_x(x_0+\Delta x,t)) \\ &= \kappa A(u_x(x_0+\Delta x,t) - u_x(x_0,t)) \end{aligned} \tag{11}$$

Over a short time Δt, (11) requires the net heat flow to be

$$Q\Delta t = \kappa A(u_x(x_0+\Delta x,t) - u_x(x_0,t))\Delta t \tag{12}$$

The other quantity we are interested in is how much heat is absorbed by our element over this time and the resulting change in temperature Δu. Here we appeal to (8). Crucially, we assume that this Δu is identical to some actual temperature change at a point $x = x_0 + \theta\Delta x$ (where $0 < \theta < 1$), which allows us to equate the temperature change at this point with (8):

$$u(x_0+\theta\Delta x, t+\Delta t) - u(x_0+\theta\Delta x,t) = \frac{Q\Delta t}{s\rho A\Delta x} \tag{13}$$

Rearranging (13) yields another expression for $Q\Delta t$,

$$Q\Delta t = [u(x_0+\theta\Delta x,t+\Delta t) - u(x_0+\theta\Delta x,t)][s\rho A\Delta x] \tag{14}$$

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Combining (12) and (14), we must have

$$\kappa A(u_x(x_0+\Delta x,t)-u_x(x_0,t))\Delta t = [u(x_0+\theta\Delta x,t+\Delta t)-u(x_0+\theta\Delta x,t)][s\rho A\Delta x] \tag{15}$$

Here the perfect insulation of the rod is crucial, as otherwise we could not identify the heat absorbed with the net heat flowing into the element across the x, $x+\Delta x$ boundaries. Dividing both sides by $\Delta x\Delta t$ and taking the limit of $\Delta x\to 0$, $\Delta t\to 0$ gives

$$\kappa A u_{xx} = s\rho A u_t \tag{16}$$

And (16) is just (6).

Now the interpretative question is what someone who adds (6) to their scientific theory of heat on the basis of (7) and (8) commits herself to and, more specifically, whether or not it commits someone to the continuity of temperatures. According to what I will call a mapping account of applied statements, (7) is a claim about the numbers that are in the range of a series of mappings whose domains involve temperatures, areas, distances and the thermal conductivity of the materials involved. And, to the extent that (7) is empirically verified, it will not involve assignments of these magnitudes to particular real numbers, but only ranges or intervals of real numbers reflecting the imprecision of our measuring devices. By contrast, in deriving (6) we can only proceed by drawing on the full structure of the real numbers and functions on real numbers. Already in obtaining (9) and (10) we assumed that certain limits were well defined. Again, in moving from (8) to (13), we exploited a fact about continuous functions of real numbers. There is thus an apparent gap between our evidence for the premises of the derivation and the assumptions needed to warrant the steps of the derivation.

There are two attitudes we can take to these inferences which lead us from (7) and (8) to (6). We can take what I will call the *physical attitude* which insists that throughout we are talking about physical systems and physical magnitudes. If we take the physical attitude towards the derivation, then in accepting (6) we have indeed committed ourselves to a domain of continuous temperatures. This is the only way to make sense of several steps in the derivation. The problem with the physical attitude is that it is unjustified by our available evidence. No experiment leads us to think that the limits, interpreted physically, will be well defined or that there will be some $x_0+\theta\Delta x$ where the actual change in temperature equals the average change in temperature in our bar element. In fact, our best theory of material bars goes against these assumptions and directs us to view an iron bar, say, as an arrangement of iron atoms (and impurities) related by chemical bonds. So, if we adopt the physical attitude towards these derivations, we undermine our confidence in the scientific theory that has (6) in it, contrary to how scientists actually operate.

The second attitude, which I want to defend, I will call the *mathematical attitude*. It takes seriously the steps in the derivation leading from (7) and (8) to (6) but views these steps as involving only mathematical objects, here the real

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numbers and functions on the real numbers. The signal that we are no longer considering physical situations comes right at the beginning of the derivation where we turn our focus on "a perfectly insulated, homogeneous circular rod". Here we are not picking out an actual physical rod or even a physically possible rod. Instead, we are describing a wholly mathematical model of a physical rod whose features are entirely mathematical and that outstrip the properties that any physically possible rod could have. Taking the mathematical attitude towards the derivation has some immediate benefits, but also some serious risks. The benefits are that we can view the derivation of (6) from (7) and (8) as entirely warranted. Viewed as a mathematical truth, "(7) and (8) $\rightarrow$ (6)" is not open to doubt. So we can rescue our ordinary confidence in the steps that scientists take when constructing their scientific theories.

The risk, of course, is that if we just think of the derivation as entirely mathematical, without any further constraints, then we will end up with a mathematical theory that is no longer a theory about the physical world. We want (6) to be about some physical situations, just as (7) and (8) are, but the mathematical attitude seems to undermine this. Here we have a specific instance of the more general problem of how an idealized mathematical model can represent a physical situation. Generally, idealizations are assumptions about the mathematical model that we know to be false of the physical situation we are trying to represent. It must be possible to use idealizations to construct mathematical models that represent physical situations because we do this throughout science all the time. Still, it is hard to say when an idealizing assumption is consistent with having a genuine representation, and at what point an idealizing assumption deprives our model of its representational powers.[3]

In [13] I criticized [6] for overgenerating idealized models that represent. Their relation of partial isomorphism proved too easy to find. A more restrictive approach would insist that representation must involve an isomorphism between some mathematical model and the physical situation represented. The innovation that I suggest here is that the model described in the derivation (the derivation model) be distinct from the model that is isomorphic to the physical situation (the matching model). The derivation is directly about a wholly mathematical model M_1 and indirectly about a physical system P when (i) there is a mathematical model M_2 that *matches* P and (ii) there is an *acceptable* mathematical transformation from M_1 to M_2. The idea behind a mathematical model matching a physical situation is fairly simple, at least assuming what I have above called the mapping account of applications. Matching models require an isomorphism between the model and entire physical situation in all its details and complexities. Models can fail to match a physical situation in at least two ways. A model will *lack* (*have excess*) resolution if its constituents and processes are of a *larger* (*smaller*) scale than the constituents and processes of the physical situation mod-

[3] [9] offers an important survey of these issues, but unfortunately appeared after this paper was largely completed.

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Figure 7: Matching Model

eled. In our example, the bar in the derivation model has excess resolution as its point elements are of a smaller scale than the iron atoms that make up an actual bar.

It remains to clarify exactly when a transformation between a non-matching and a matching model is acceptable. This is the crucial step in the proposal as making acceptable transformations too easy to obtain will lead back to an over-generation of representing models, while too narrow a definition will leave out models that are used in science. My definition is a generalization of Batterman's proposal in [3]. Batterman has investigated a wide range of cases where the mathematical models lack resolution compared to the physical situations the models represent. Batterman shows how this mismatch in resolution is consistent with genuine representation because no matter what the details of the situation, the mathematical model and the physical situation will agree for certain phenomena. His simplest example is the equation for the load weight of a strut:[4]

$$P_c = \frac{\pi^2 EI}{L^2} \tag{17}$$

The model which we use to derive (17) ignores the details of the physical strut that lead to buckling, but this model still represents the strut because the details that it ignores are not relevant to the load that the strut can bear. What is especially important about this case and others that Batterman discusses is that we can prove that the derivation model will agree with the matching model in relevant respects. Giving this proof requires a workable description of the matching model and the necessary transformation. Batterman groups these sorts of arguments under the term "asymptotic reasoning".[5]

It would be unfair to ascribe the full account of representation that I develop here to Batterman, although his talk of ideal and minimal models in [2] is very suggestive of my matching and derivation models. Still, from our perspective we can see that Batterman has isolated just one type of acceptable transformation between a matching model and a model that lacks resolution. Others exist, as do acceptable transformations between matching models and models with excess resolution. We can say, then, that a mathematical transformation between a non-matching and a matching model is acceptable just in case the non-matching and the matching model agree on the relevant features of the situation. This

[4] [3], 12.

[5] Since Batterman's book has appeared, Barenblatt has published an accessible treatment of much of the relevant mathematics. See [1].

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introduces a significant degree of relativity into our account of representation. For it is only fixed whether or not a mathematical model represents a situation once the features of interest are determined. Our model involving the heat equation may be judged adequate to represent the temperature properties of certain iron rods over small stretches of time, but would of course fail to represent any actual rod's composition out of iron atoms. A further appeal to context is in fact already needed to distinguish accurate from inaccurate representations. What I have offered so far is, strictly speaking, an account of when a mathematical model will accurately represent a physical situation. I add that a model represents (full stop) when the agents involved *believe* that the right sort of transformation exists between their model and a matching model. An inaccurate representation is then a representation where this belief is false, i.e. where the needed transformation does not exist.

An important feature of my proposal for idealized representation is that it does not require that we have any useful description of the matching model or the transformation from the non-matching model, but only that this matching model and transformation exist. This is essential as the matching model and transformation will generally be incredibly complicated and beyond the scope of our current mathematics and science to directly describe. Still, through experimentation and insight scientists can provide good indirect evidence for what is required. In our iron bar case, we have no access to the matching model, but know that the model that we describe directly with the heat equation is not the matching model. This disconnect and the resulting empirical success of the heat equation strongly suggest that there is an acceptable transformation between our model and a matching model. Such indirect investigation of the matching model is of course open to further correction and revision as the experimental data may accumulate. In many cases in the history of science further investigation revealed that no appropriate matching model existed after all because the physical situation turned out to be radically different than the scientists had assumed.

Much of the most interesting work on idealized representation, e.g. [11] and [10], presents a series of increasingly realistic models that connect what I am calling the derivation model and the matching model. Such cases are in between the fully worked out cases that Batterman gives and the more trial and error process just described. This should reinforce the flexibility of my proposal and explains the diversity of levels of confidence in different idealized representations that we find in scientific practice. In each case, scientists directly describe a derivation model that they understand fairly well, but what varies is the degree to which they have access to the matching model and the transformation from the derivation model to the matching model.

Mark Wilson would most likely criticize the assumption here that every physical situation has a corresponding matching model as "lazy optimism".[6] While his objections are subtle, and I must reserve a full response to them for

[6]See, e.g., [16] and [17], esp. 308-311.

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future work, his concerns are ambiguous between what we could call a metaphysical and an epistemological interpretation. On the metaphysical interpretation, Wilson denies the existence of sufficiently many mathematical structures to ensure the existence of a mathematical model. Here a full response would require an appeal to mathematical practice and the naturalistic assumption that this practice is our best guide to mathematical ontology. On the epistemological reading, which fits more with what Wilson explicitly says, he is concerned with appealing to models that we have no workable understanding of. While I agree with him that this is often illegitimate, I fail to see what is wrong with the appeal I make here where an *indirect* investigation of the matching model remains possible.

Application conditions

Once we add an equation like (6) to our theory there can still be further substantial questions about which systems fit the equation. In our case, if we have certain restricted initial and boundary conditions, we can prove that (6) yields an exact solution: for boundary conditions

$$\begin{aligned} u(0,t) &= 0, \\ u(l,t) &= 0, t > 0 \end{aligned} \tag{18}$$

with initial conditions

$$\begin{aligned} u(x,0) &= f(x), 0 \leq x \leq l \\ f(x) &= \sum_{n=1}^{\infty} b_n \sin(n\pi x/l) \end{aligned} \tag{19}$$

that solution is

$$u(x,t) = \sum_{n=1}^{\infty} b_n \exp(-n^2\pi^2\alpha^2 t/l^2) \sin(n\pi x/l) \tag{20}$$

That is, (19), reflecting the initial temperature distribution, must be expressible as a Fourier series. What implications does this have for when a solution to (6) found using (20) is applicable to a physical situation?

We get from (6) to (20) by noting that (6) and the boundary conditions (18) are linear and homogeneous. This implies that we can use the separation of variables technique:

$$u(x,t) = X(x)T(t) \tag{21}$$

Substituting (21) into (6) and rearranging, we see that both sides of

$$[X''/X] = [1/\alpha^2][T'/T] \tag{22}$$

must be equal to some constant, call it σ. Rearranging the resulting two equations leads to

$$X'' - \sigma X = 0 \tag{23}$$
$$T' - \alpha^2 \sigma T = 0 \tag{24}$$

So we have transformed our original partial differential equation into two ordinary differential equations. Appealing to the boundary conditions requires

$$X(0) = 0 \tag{25}$$
$$X(l) = 0 \tag{26}$$

After a detour where we replace real σ by complex $-\lambda^2$, we get that nontrivial solutions require that $\sigma = -n^2\pi^2/l^2$ and

$$X(x) = \sin(n\pi x/l), n = 1, 2, \ldots \tag{27}$$

Similar considerations require that

$$T(t) = \exp(-n^2\pi^2\alpha^2 t/l^2), n = 1, 2, \ldots \tag{28}$$

But we know that any linear combination of the product of (27) and (28) will be a solution to (6) satisfying (18), so we can write the general solution as (20). To see why (19) must take on the form it does, note that initial conditions (18) and general solution (20) imply that, replacing t by 0,

$$f(x) = \sum_{n=1}^{\infty} b_n \exp(-n^2\pi^2\alpha^2 0/l^2) \sin(n\pi x/l) \tag{29}$$

which reduces to (19).

We can isolate two steps in finding (20), both of which border on the incoherent if we take the physical attitude. First, we opted for (21) and the separation of variables. Now, what physical considerations suggest that there are such functions X and T? If we take the physical attitude towards this solution technique it seems that, like our attitude towards u, we must think that there are physical magnitudes governed by X and T whose product equals u. While in some cases we can take a physical attitude towards a pair of functions and their product, there is no reason to do so in this case. For example, if L is a function yielding the length of an object and W yields the width of the same object, then $A = WL$ does pick out another physical magnitude, namely the area covered by that object. In our heat equation case, though, we have no grip on what physical magnitude X or T are meant to represent and so the physical attitude is unwarranted.

The second step to (20) was to go from (23), (25) and (26) to (27) and, by a similar route, to arrive at (28) from (24). While I have skipped the details,[7] the

[7] Though see [5], §4.4.1 and [12].

physical attitude towards these considerations is called into question simply by pointing out that it involves replacing real σ by complex $-\lambda^2$. Such a substitution has no direct physical significance for our situation of heat conduction. While some physical magnitudes can be directly represented by complex numbers, none of the magnitudes involved here can. Taking a physical attitude towards these sorts of substitutions makes the results look like magic.

The mystery is removed by taking the mathematical attitude towards both steps. We have a general theory of partial differential equations involving real valued functions, conceived of now as wholly mathematical functions. This theory tells us that we can always derive solutions to equations like (6) with boundary and initial conditions (18), (19) using separation of variables and a detour through the complex plane. Properly understood, these claims are applications of mathematics within mathematics to our idealized mathematical models, i.e. non-matching models. We find that such models will be correctly described by some specific function of type (20) whenever it is also correctly described by (6) and conditions (18), (19). And we know that we can represent an actual case of heat conduction by this sort of solution because we have the right kind of relationship between a model of excess resolution and a matching model.

While our discussion of applications conditions has been much briefer than the discussion of derivation conditions in the last section, it is sufficient to show that, even in this simple case, the derivation and application conditions diverge. In our case, even when (6) is part of our physical theory we need additional application conditions to be satisfied to solve it. The same equation can have associated with it different, conflicting application conditions and there are probably also cases where the application and derivation conditions conflict, requiring three distinct models. In line with the account of representation developed in the last section, I suggest that we view the reasoning concerning application conditions as being directly about a wholly mathematical model and only indirectly about the physical situation represented. Thus, we distinguish three different mathematical models, derivation, matching and application, and have the scope to investigate the relations between all three. In the case of (6), the application models are just a subset of the derivation models, but the most general situation is represented in figure 2.

Conclusion

In our review of how the heat equation is derived and applied I have emphasized four conditions:

(D1) Various limits are well defined.
(D2) Given the average change in temperature Δu in the bar element, there is some point in the bar x whose temperature change is identical to Δu.
(A1) Separation of variables is appropriate, i.e. there are functions X and T such that $u(x,t) = X(x)T(t)$.

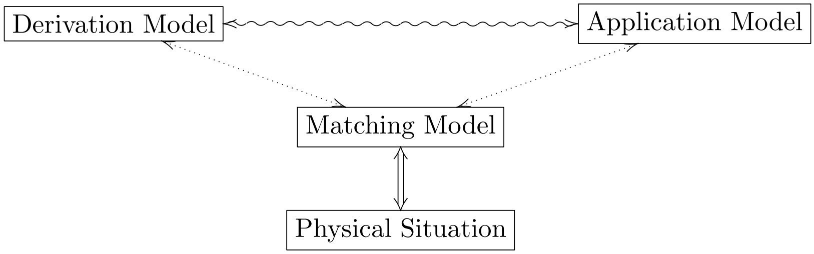

Figure 8: Derivation and Application Models

(A2) Given a function on a real variable, there are additional parallel functions on a complex variable.

All four conditions are unwarranted if we take a physical attitude towards them. A mathematical attitude, combined with an account of idealized representation, is necessary in order to make sense of how scientists reason.

I end with some tentative conclusions which summarize this paper and set out further topics of investigation. First, there are two attitudes that we can take to (6), only one of which is warranted by the evidence that scientists typically have available. Given the obvious criticisms I have raised against the physical attitude, it is worth asking why Field adopts it. The short answer is that this is the only way for Field to justify the axioms needed for his nominalistic physics. Recall that in his nominalistic physics for scalar magnitudes like temperature, Field presents axioms whose only non-logical vocabulary are relations like 'Temp-Bet' and 'Temp-Cong'. These axioms formulate claims about the way temperatures are distributed, including the strong claim about instantiated temperatures quoted above. If we grant these axioms, then Field is able to prove that his nominalistic theory of temperature N is an adequate version of a mathematical theory of temperature M in the following precise sense: every semantic consequence of M statable in the language of N is also a semantic consequence of N. While there is no point in denying this logical result, Field does little to motivate the axioms of N beyond invoking what I have called the physical attitude. In the case of temperature, the resulting axioms are not only unwarranted, but conflict with our theory of the material constitution of objects like iron bars.

Adopting the mathematical attitude and the associated two stage account of mathematical representation overcomes these problems. In addition, it suggests a role for mathematics in science: we use mathematics when we are unable to construct a matching model. This lack of access to the matching model may be due either to its complexity or to our ignorance of some of the details of the physical situation under study. In both cases, we can construct a derivation or application model which we understand mathematically and can investigate by experimental means the degree to which our non-matching model captures the relevant features of the physical system.

Second, this account of idealized representation can give us insight into some interpretative questions about scientific theories. If the use of mathematics is tied to a lack of understanding of the matching model, then we can see why highly mathematical theories pose the greatest interpretative challenges. The nature of temperature, heat and their connection remained a controversial issue long after the heat equation was discovered and successfully applied. This is because it is much easier to establish that an acceptable transformation between the non-matching and matching models exists than it is to decide if heat is a fluid-like substance. The latter interpretative question was of course eventually resolved scientifically, but only by appeal to broader theoretical considerations and experimental data.

The lesson I draw, then, is that mathematics plays a useful function in generating idealized scientific representations, even when scientists are ignorant of many of the features of the situation represented. Philosophers of science engaged in interpretative debates about successful scientific theories should pay attention to this feature of applied mathematics if they wish to understand why mathematics is used in science and how its use goes hand in hand with interpretative uncertainty.

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