What the justification of idealizations in science tells us about the laws and language of nature.

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Abstract

Describing a physical system in idealized terms involves making literally false claims about the system. Given this, it is puzzling that justified beliefs about physical systems can be formed by starting with idealized descriptions and then performing mathematical calculations. I argue that this puzzling aspect of idealizations cannot be easily removed by introducing talk of approximations. I go on to develop an account of how this curious feature of idealizations is to be understood. My account requires us to reassess what precisely we take the laws of physics to be saying, and also has consequences concerning the kind of evidence we can have for thinking that mathematics is the 'language of nature'. Finally, some critical comparisons are made with the so-called *model-based* account of scientific laws developed by Cartwright and Giere.

1 The Problem Of Idealizations.

To motivate the main problem of this paper, we start with an example. Suppose a small charged particle is fired at an angle into a long cylindrical tube around which a current carrying wire has been tightly wrapped. Such a particle will travel in a helical path, as depicted below. Suppose a physicist is interested in calculating the radius of this helical motion.

There are many ways to proceed – I sketch the most elementary approach, though I will argue that its most salient features apply generally. In this approach, the physicist assumes that the wire is infinitely thin and tightly wound around the cylinder, that the current is completely constant throughout the wire, and that the cylinder is infinitely long – in effect, he treats the solenoid as an infinite, homogeneous cylindrical 'sheet' of circulating charge. He then argues (with the help of Ampere's law) that the magnetic field inside the cylinder is uniform, and calculates its strength.

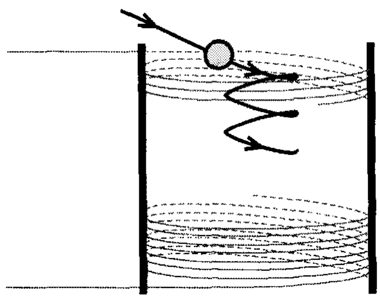

Figure 1: A charged particle in a solenoid.

Given all this, the parameters associated with the helical motion are then completely determined by applying the Lorentz force law to the case of the motion of a charged particle in a uniform magnetic field of given strength. If there is nothing especially irregular about the actual experimental setup, the physicist is then justified in concluding, for instance, that the radius of the observed helical motion should be approximately equal to that given by the numerical result of this calculation.

Even though the calculation gives an exact numerical value for a certain radius of motion, the conclusion (let us call it C) that the physicist draws is of the form 'the actual radius is approximately r'. This statement C about the *approximate* value of the radius is the correct conclusion to draw from the chain of reasoning sketched above.[1]

Abstracting away from the details of the example, the physicist has a mathematical argument from a set of premises $\{P_1, ...P_n\}$ (containing, amongst other things, the highly idealized description of the system), whose conclusion C is a statement about the approximate radius of motion of the particle. The physicist's argument from $\{P_1, ...P_n\}$ to C persuades him that C is true, and I shall assume that the physicist, having seen this calculation through, is *justified* in believing C.

But this is puzzling. If the physicist proceeds in the way just outlined, he is *not* justified in believing all the premises $\{P_1, ...P_n\}$ of this argument. The wire is not infinitely thin, the current is not constant throughout the wire, the wire and the cylinder have inhomogeneities and other imperfections – in fact, the solenoid is *known not* to be an infinite, homogeneous cylindrical sheet of circulating charge. Nevertheless, in virtue of the mathematical argument that takes these premises to the conclusion C, we take the physicist to be justified in believing C. That is to say, in virtue of

[1]One should not think of the physicist as drawing a conclusion C' about the *exact* value of the radius, and then trivially inferring a proposition C about the approximate value of the radius. No such conclusion about the exact value of the radius is drawn at all.

having performed a calculation involving assumptions in which he does not have justified belief, the physicist becomes justified in believing C. For justified beliefs to be generated from propositions in which one does not have justified belief is puzzling.

I take this to be a general feature of reasoning involving idealizations. Such reasoning generates justified beliefs from premises in which one does not have justified belief. The main question is how this is possible. In other words, our question is how reasoning with idealizations can be *justified.* This is the question that I shall try to answer here, at least for a specific, more manageable type of idealization.

One might expect that the problems posed here can be answered easily by talking about approximations. If one takes the premises $\{P_1, ...P_n\}$ to be only approximately true, then with a little mathematical effort one might hope to be able to prove that the conclusion C (which is already only a statement about the approximate radius) is also true. In this way, one might hope to reformulate the main mathematical argument so that it involves an inference to the conclusion C from just the *approximate* truth of the premises. Because one is justified in believing in the approximate truth of the premises $\{P_1, ...P_n\}$, the argument for C only invokes justified beliefs as premises, and the problem is solved. In §2 of this paper, however, I argue that this quick attempt at a solution will not work.

Given this, one might argue that our belief in C is justified instead by the simple fact that we have good *inductive* evidence for thinking that particles in everyday, regular solenoids behave *as if* they were charged particles fired into ideal solenoids. In this way, given our knowledge of how ideal solenoids are supposed to behave, our belief in C can be given a fairly straightforward inductive justification, regardless of whether we have worked out the intricate mathematical details of how slightly less than ideal solenoids behave. I will also criticize this line of argument in §2 (even though, on my view, a close relative of this proposal turns out to be correct.) This pair of criticisms developed in §2 then defines the challenge to be met in the remainder of the paper: in virtue of what can facts about ideal solenoids justify beliefs about slightly less than ideal solenoids, if not through a detailed mathematical study of approximations, and if not by simply relying on the inductive evidence we have that such idealizations work? What middle ground is possible?

In §3, I develop my account of why reasoning with idealizations is justified. The rough idea is that all the so-called 'basic laws' invoked in a typical physics calculation should be taken as having something like the following form:

> *A physical system of type X may be treated as if it were a mathematical structure of type Y, for purposes of deriving con-*

clusions of the form Z.

For instance: a physical collection of microscopic charges may be treated as if it were a set of mathematical point charges mutually interacting under Maxwell's laws, for the purposes of deriving specific types of conclusions about specific types of macroscopic systems. Claims of this form are knowable on experimental grounds. A typical calculation in physics then involves exploring the mathematical consequences of assertions such as these. In this way, we can justify the use of idealizations in physics, and can take mathematical arguments that use idealizations to give us genuine *grounds* for believing in their conclusions.

This way of thinking requires us to take a different point of view as to what precisely we are doing when we invoke things such as Maxwell's laws in a calculation. In §4 I discuss the picture of laws saddled on us by my account. I also discuss the question of whether we are forced to posit a type of law that does not involve idealizations and that explains those laws that do invoke idealizations. This has consequences for the question of whether and in what sense we may claim to have inductive grounds for thinking that mathematics in the language of nature.

In §5, I conclude by contrasting my account of idealizations and laws with the so called 'model based' approach of Cartwright and Giere. Although there are similarities between my account and the model based account, there are also crucial differences that need to be stressed.

2 Is There Really a Problem?

In this section, I will consider and reject two ways of thinking about how justified belief in generated in our main example.

The example of the solenoid is supposed to be an example of an argument A with premises $\{P_1, ...P_n\}$ and conclusion C in which one is not justified in believing all the premises, even though the argument itself justifies belief in the conclusion C. (Recall that C is only a statement about the approximate radius of the particle.)

It is natural to suggest that the argument A as presented is *not* the argument that delivers justified belief in C. Instead, the argument A should be thought of as 'shorthand' for a more complicated argument A' that only takes justified beliefs as premises, and delivers the same conclusion C. The purpose of the argument A is not to generate justified belief itself – instead, the purpose of argument A is to point us towards a more complicated argument A' that *does* deliver justifed belief in C.

Whether this suggestion works depends on whether a candidate for A' can be found. I will argue that, in general, no such A' exists, and thus this suggestion does not work.

In this section, I will consider and reject two proposals for such an A'. The first proposal is what I will call a *deductive* proposal, while the second proposal is what I will call an *inductive* proposal.

2.1 A Deductive Proposal.

Let start with the more obvious proposal for A'. From the laws of electrodynamics and the claims that the wires are *almost* negligibly thin, the current *almost* constant, and so on, it should be possible to argue for an approximate value for the radius of the particle. This refined argument is our A'. Roughly speaking, the argument A' has three steps:

1. State the laws of electromagnetism (Maxwell's equations), and any other laws to be used in the remainder of the argument.
2. Argue that if the usual ideal assumptions are made about the solenoid, then the resulting radius of motion will be r.
3. Argue that adding small but bounded width to the wires, small but bounded inhomogeneities in the current (and so on), one gets a small but bounded difference in the ultimate radius of motion. Calculate the largest size δr of an 'error' that might be introduced in this way to the previous calculation of the radius, and infer that the radius of the particle lies in the range $(r - \delta r, r + \delta r)$.

This is our candidate for A'. Because it only involves the premises that the wires are *almost* negligibly thin (and so on), it looks like it only relies on justified beliefs as premises. I call the proposal *deductive*, because it allows us to calculate the approximate radius r by applying only valid mathematical and deductive forms of argument to statements about the approximate initial state of our system.

But it is perhaps relevant to note that no-one has ever produced such an argument A'. The technical difficulties are immense. It is certainly possible to determine the behavior of systems that depart from the ideal in very simplistic ways. For instance, suppose one introduces a small, semicircular 'bump' into some portion of the wire in our ideally described system:

Figure 2: A ideal deviation from ideality.

The effect that an idealized inhomogeneity of this sort has on the resulting helical motion can be calculated, and an error bound for the radius calculated. But this is a very simple case. It is still assumed, for instance, that the wire is infinitely thin; or at least that the cross section of the wire has circular symmetry. The usual calculation of the magnetic field around a wire assumes circular symmetry of the wire, so once this assumption is dropped (as it must be if unjustified claims about the system are to be avoided), the effect that more significant inhomogeneities in the wire has on the magnetic field will have to be calculated. Technically, this is extremely difficult, except for the most 'ideal' inhomogeneites. But even more serious problems quickly ensue. The 'current' is being viewed as something flowing uniformly through the wire. This is inaccurate – a current is the flow of a swarm of electrons, each of which may be moving in a different direction, but which collectively move in the direction of the current. It must somehow be argued that this deviation from ideality only has a certain bounded effect on the radius of the subsequent helical motion. That is to say, it must somehow be argued that the result that follows if the current is treated accurately as a swarm of electrons is not significantly different from the result that follows from treating the solenoid in the idealized ways suggested. At some point all the substance of the cylinder and wire will also have to be treated from a molecular point of view.

There are rough ways to perform some parts of some of these steps. But rough arguments are not what is needed — any calculation that, for instance, assumes that terms of higher order in a Taylor series can be ignored is making an idealized assumption that must be removed at some point, if the strategy in question is to be followed to completion. If this strategy were to work, it would at the very least have to involve a rigorous derivation of the macroscopic behavior of an enormous set of point charges in some initial macroscopic configuration, evolving solely in accordance with Maxwell's equations. It is far from clear what this argument would even look like, and it is by no means obvious that it can be performed at all (more on this below.)[2] The moral is that arguments of the form A' can be written down only when very 'ideal' departures from ideality are involved. Such arguments are clearly not good enough for our present purposes.[3]

It is interesting to ask whether such an argument A' nevertheless exists, even if the technicalities may be too much for any mortal to deal with. Suppose one had grounds for thinking that such an argument A' existed. One could then claim that A was simply shorthand for this argument A'

[2]One would expect the difficulties to be at least as great as the difficulties involved in justifying the assertions of macroscopic, equilibrium thermodynamics (for instance); and the conceptual and technical difficulties involved in a rigorous formulation of this branch of physics are tremendous.

[3]Note that this difficulty is quite distinct from the difficulty that one may not have enough knowledge about the initial state of the system to perform this calculation.

(whose details are beyond mortal comprehension) but whose conclusion is C. If one was justified in thinking that A' existed, and one knew that one was justified in believing all the premises of A', one would then be justified in believing C, even if one did not know how the details of the argument A' actually went. In this way, one might not *need* to know all the details of A' to be justified in believing C.

I will argue, however, that one is not even justified in believing that such an A' *exists*. More specifically, one does not have grounds for thinking that such an A' exists whose premises involve only laws of nature in which one already has justified belief.

To see why, let us suppose first that the only laws of nature one wants to invoke are Maxwell's equations. Maxwell's equations cannot account for the stability of the atom, and thus any calculation of the form gestured at in A' *must* have as its conclusion that no stable matter exists shortly after the system is set up. Such a conclusion is not very helpful. (For further discussion of questions about the stability of matter in classical electrodynamics, see [3] and [1].) The lesson is that Maxwell's equations are tremendously useful if one is interested in physical systems about which one has made a host of idealized assumptions and on which a host of idealized constraints have been imposed – but they are not useful at all if one is interested in applying them literally to an unidealized description of matter.

So no argument of the form A' exists that invokes only 19th century physics. What about if fancy modern theories are allowed? Quantum mechanics, of course, resolves the problem of the stability of the atom, so the calculation of A' should be expected to be quantum mechanical. But if one is to account for all idealizations in the system in question, one must also include an account of gravity; for if A' is to do its job, it must include a subargument to the effect that, for instance, the gravitational attraction of all the nuclei in our system only has a certain bounded effect on the radius of the resulting helical motion. Such an argument is impossible to make using laws of nature in which one currently has justified belief, because no sufficiently well developed theory of quantum gravity capable of doing this job exists.

I will not dispute here that, abstractly speaking, an argument of the form A' with conclusion C *could* exist, if one had justified belief in a very different set of physical laws. The question, however, is whether one has any reason to believe that such an argument A' exists that only invokes laws of nature in which one *currently* has justified belief. I have argued for a negative answer.[4]

[4]In order for such an argument A' to exist and do its job, one must have justified belief in a set of laws of nature which, although they need not turn out to be absolutely correct,

2.2 An Inductive Proposal.

In this section, I wish to consider and criticize what I will call an *inductive* proposal for an argument A'.

Let us suppose that after having conducted a host of experiments on different solenoids, the physicist discovers that the actual trajectory of a charged particle in a carefully constructed solenoid is very much like the mathematically predicted helical trajectory of an idealized charged particle in an ideal solenoid. If this is confirmed in a sufficiently large number of instances, the physicist becomes *inductively* justified in believing that actual charged particles in carefully constructed solenoids have trajectories very much like the mathematically predicted helical trajectories of idealized charged particles in ideal solenoids.

When presented with a carefully constructed solenoid and a charged particle inside it, the physicist can then proceed as follows:

1. Point out the inductive evidence that suggests that charged particles in carefully constructed solenoids behave like ideal particles in ideal solenoids.

2. Argue that in the present case, if the usual ideal assumptions are made about the solenoid, then the resulting radius of motion will turn out to be r.

3. Conclude that the radius of motion in the actual case is approximately r.

This is the proposal for A'. I call this proposal *inductive*, because it revolves around an inductive argument for treating actual carefully constructed solenoids as if they were ideal solenoids.

Unlike the deductive proposal, I think this proposal does justify belief in the desired conclusion C. The problem, however, is that it justifies belief in C *for the wrong reasons.* According to this proposal, we discover that reasoning as if our system were ideal turns out to be quite accurate. We therefore take ourselves to be justified in believing C because we have found a certain reliable method of inference that yields C. But this is not what is wanted. We should want to say that we are justified in believing C *because a certain mathematical argument, applied to the physical system, tells us that we ought to believe* C. But the present account does not allow

must be able to completely account for the stability and basic properties of microscopic and macroscopic matter, when that matter is described without idealizations. Although such a set of laws need not be a 'final theory', it must, in a sense, be very close to such a theory. Because of these constraints on the sorts of laws that could function in an argument A' of the sort that is needed, the discovery of new but imperfect theories are unlikely to justify belief in the existence of arguments such as A'.

us to say this. This should become clearer as I flesh out the worry in a little more detail.

Suppose someone has a friend who is a very competent mathematician. This person often asks the mathematician whether certain facts are true, and the mathematician very reliably gives the right answer. Suppose the mathematician's friend then asks him whether some T is true, and the mathematician tells him that it is. The mathematician's friend is justified in believing T is true, because he has strong inductive grounds for trusting the mathematician. The mathematician, however, is justified in believing T because he has constructed a proof.

When the physicist gives an argument for the expected trajectory of the charged particle, his conclusion is justified because he has seen a certain mathematical argument through, and appreciated its validity. I would like to say that he is justified in roughly the same way that the mathematician is justified. But using the inductive proposal just given, the only reason the physicist is justified in believing C is because he has discovered a reliable process for generating true beliefs, and this procedure tells him to believe C. His justification is merely like that of the mathematician's friend.

Still, one might claim that the physicist's belief in C *is* a result of seeing a certain mathematical argument through – namely, the argument concerning the ideal system. But although it is true that his belief in C is the result of a mathematical argument, it is not the result of the *right* mathematical argument. The physicist is justified in believing C because he has argued that applying Ampere's Law *to the actual physical system* tells him that the magnetic field inside the solenoid should be more or less uniform, and that, applying the Lorentz force law *to the actual physical particle* and *to the actual magnetic field inside the solenoid* reveals a trajectory which is approximately helical. The proposal in question gives the helical trajectory as the result of a mathematical calculation – but it is a mathematical calculation on a counterfactual system, and as such, is not what is wanted.

In some ways, the inductive proposal fares better than the deductive proposal. However, if we want to hold on to the idea that the physicist is justified in believing C because a certain mathematical argument applied to the physical system before him tells him that he ought to believe C, then this proposal cannot be right.

The challenge that we face should now be clear. If we demand that we be able to prove that all our idealizations and approximations are harmless, we raise the standards too high. On the other hand, if we settle for an inductive justification in which we merely argue for the reliability of our idealizational procedures, and give up on the idea that the validity of our calculations form the true justificatory basis for our empirical beliefs, we let our standards drop too low. The problem is how to account for the use of idealizations in such a way that we avoid these unattractive extremes.

3 Justified Idealizations

We are justified in believing that the particle in the solenoid will exhibit the helical motion exhibited in our calculation. But it is not just on brute inductive grounds that we are justified in believing this – we are justified in believing this because we have a mathematical argument that shows us that this is how the particle must behave. The problem we encountered in the last section is that it is difficult to say exactly what this mathematical argument is supposed to be, insofar as it is an argument that genuinely *justifies* the belief in question.

In textbook electrodynamics, a straight current carrying wire will often be treated as if it were a line of charge with constant charge density $\rho(\vec{x})$ and current density $\vec{j}(\vec{x})$ obeying Maxwell's equations. Although the textbooks do not say so, this is a substantive assumption, even if we grant (contrary to known fact) that the classical Maxwell's equations correctly describe the electromagnetic field. Implicit in the textbook treatment of electrodynamics then is the following claim:

> (1) *For the purpose of simple calculations, a long, straight current carrying wire may be treated as if it were as if it were an infinite line of charge with constant charge density $\rho(\vec{x})$ and current density $\vec{j}(\vec{x})$ obeying Maxwell's equations.*

In order to apply Maxwell's equations to real world situations, principles such as (1) must be assumed.

It will be important to make a couple of comments about (1). There are two ways in which the statement (1) is imprecise. The first occurs with the use of the phrase 'for the purposes of simple calculations'. The idea here is that our treatment of the wire as an infinite line with constant charge density and constant current density is only something we will trust if we want to draw sufficiently modest conclusions. If we are interested in the fourth significant figure of the magnetic field at some specific point near the wire, it will not do to treat the wire as an infinite line of charge with constant $\rho(\vec{x})$ and $\vec{j}(\vec{x})$. There are some inferences we may confidently draw from our idealized description of the wire, and others we may not. It is difficult, however, to draw a sharp line between the inferences that are permitted and those that are not permitted. Some inferences will be very safe, some utterly unreliable, and the remainder somewhere between. Experiment can give us a decent sense of the category in which any given inference lies, but only in a rough and ready way. Although the proviso 'for the purposes of simple calculations' can be made a bit more precise, it certainly cannot be made completely precise.

Another vagueness in statement (1) occurs when mention is made of 'long, straight current carrying wires'. How long is long, and how straight is straight? Obviously, no precise answer can be given. Moreover, insofar as any sort of answer is possible, it will depend on the use to which the idealized description is being put. If the idealized description is used to only draw rough conclusions, then there is more leeway as to what counts as 'straight', while if very precise conclusions are being drawn, the term 'straight' must be used more strictly. Even if we fix the precise use to which the idealized description is being put, it is unlikely that we can be especially precise about which systems may be treated using the idealized description in question, and which cannot. Again, only experiment can give us a sense of how straight and long the wire must be if a given calculation is to be trustworthy.

In both of these ways, there is considerable imprecision in (1). But this should not distract us from the fact that claims such as (1) are absolutely indispensable if we want to apply electromagnetic theory to everyday systems. Provided that we have a considerable mass of experimental evidence on which to draw, we should have a clear enough sense of the content of (1) in order to know whether the idealization in question is applicable in any familiar situation. In situations that are less familiar, we may simply lack enough evidence to decide whether the use of a given idealization for a given purpose is justified.

Most importantly, belief in (1) is straightforwardly justifiable on empirical grounds. In fact, the mass of experimental evidence we have for classical electrodynamics is really just experimental evidence for claims such as (1) and its relatives. This is not to deny that we might also be able to justify belief in a claim such as (1) on more fundamental grounds; I will say more about this later. But even if this is so, we are also justified in believing (1) on empirical grounds.

Now there are many other principles similar to (1) that are also assumed in the textbooks. For instance,

> (2) *For the purpose of simple calculations, a closed current carrying wire loop without sharp kinks may be treated as if it were a charged 1-dimensional loop with constant charge density* $\rho(\vec{x})$ *and current density* $\vec{j}(\vec{x})$ *of constant magnitude (tangent to the curve), obeying Maxwell's equations.*

> (3) *For the purpose of simple calculations, a long, regularly shaped solenoid may be treated as if it were a tightly packed*

pile of rings of charge none of which have any electromagnetic interaction with each other, and each of which has charge density and current density of fixed magnitudes.

(4) *For the purpose of simple calculations, a small charged particle moving through an electromagnetic field may be treated as a charged point particle being acted on (but not acting on) the electromagnetic field.*

All of these principles have the same character. They all involve treating a system in an idealized way. They all involve imprecisions of the same sort – they are imprecise in that they do not specify exactly what counts as a 'simple calculation', and they do not specify precisely which physical systems fall under them. Nevertheless, all of these principles are justified on experimental grounds (at the very least), and all of these principles are indispensable for the application of classical electrodynamics to a wide variety of textbook and real-life situations.

I would like to furthermore urge that (1)-(4) are genuine laws of nature, insofar as they describe deep and very general regularities in natural phenomena. One might think that their unusual form

A physical system of type X may be treated as if it were a mathematical structure of type Y, for purposes of deriving conclusions of the form Z.

speaks against their being laws; but I see no positive reason for thinking this. In fact, essentially all the laws of physics that we know have precisely this form. This is even true for the 'fundamental' laws of attraction and repulsion between particles. We do not know, for instance, that tiny charges interact in accordance with the laws of QED – in fact, insofar as QED posits a fixed spatial background metric, rather than something compatible with general relativity, we know that tiny, light charges do *not* interact in accordance with the laws of QED. (Because of this, it does not even make sense to say that the *electromagnetic* part of the interaction is governed by QED.) The best we can say is that for certain calculations, we may treat tiny, light particles as if they were point particles or localized field excitations interacting in accordance with QED. But this claim has precisely the form just indicated. Insofar as any of the laws of physics are known to us, then, they have the form in question. This is not to deny that there could be laws of physical interaction without this form (laws that told us that such and such an equation is literally true, for instance) – it is just to say that there are no such laws in which we presently have justified belief.

One might still resist this, and say that (1)-(4) should not be called laws, because they can be explained (somehow) in terms of laws *not* involving idealizations. It might be urged that it is these later laws – those not involving idealizations – that are the bona fide laws, and that (1)-(4) are merely 'shadows' of laws – things that owe their existence and can be explained in terms of the real laws, without being real laws themselves. But this at most seems a reason for not calling any of (1)-(4) (or any assertion about the applicability of an idealization) a *fundamental* law. I certainly do not think that any of (1)-(4) are fundamental, and so there is no real disagreement here. (Actually, I do not agree with this general argument against calling an assertion about the applicability of an idealization a fundamental law, but I will postpone this discussion.)

Armed with laws (1)-(4), we can now justify the claim that the charged particle fired through the solenoid will have an approximately helical motion with some particular radius. For assuming that the solenoid and the particle are regular enough, these principles allow us to treat the solenoid as a tightly packed pile of rings of charge with given charge and current densities. At this point, we can invoke the textbook calculation to determine the approximate radius of the helical motion. Insofar as the determination of the approximate radius of the helical motion of the particle counts as a 'simple calculation', our principles tell us that the resulting belief about the helical motion is justified.

First of all, note that this justification is not a brute inductive justification. As a result of this calculation we can say that the reason the helical motion has the radius it does is *because* a particular idealized solenoid produces a particular electromagnetic field, with the property that a particular idealized particle passing through it moves in a particular way. Consequently, we do not just understand *that* the motion will be helical with some particular radius – we also understand *why* the motion will be helical with some particular radius. (In the example of the previous section, the mathematician's friend understood *that* the given proposition was true, but not *why* is was true.) In fact, our explanation for the given motion is a straightforward covering law explanation – we calculate the approximate radius by invoking laws that cover the given situation.[5]

Next, note that this justification is nothing like the deductive justifications attempted in the previous section. There is no attempt to derive the claim about the radius of helical motion from claims about *unidealized* systems, only from claims about idealized systems. As such, the justification I have proposed here is different in character from the deductive justifications offered in the previous section.

Before moving on, it will be helpful to address some worries, which I

[5] Of course, not all calculations involving subsumption under laws are explanatory – consider the well-known case in which we derive the height of a flagpole by a calculation involving the length of its shadow. Our main example, however, is nothing like this.

will pose in the form of objections.

Objection 1: The justification suggested here is no better than that of the mathematician's friend in the previous case. We have no real idea why the idealization works, and so we cannot say that we understand *why* the approximate value of r given by the calculation is correct. ■

Response: It is not true that we have no idea why our overall idealization works. We know that the system consisting of a charged particle being fired through a long solenoid may be treated in a certain way *because* the charged particle may be treated in a certain way, the solenoid may be treated as a tightly packed pile of rings of charge, where rings of charge may in turn be treated in a certain way, and so on. In this calculation, we paste together different idealizations to obtain a new idealization. Our final idealization works because the antecedent idealizations work, the system is sufficiently simple, and the calculation we wish to perform is sufficiently modest.

Objection 2: Let us focus on (1)-(4) then. These seem at best to be known inductively – we cannot claim to understand *why* these idealizations work. Because of this, our justification for believing any of (1)-(4) is like the justification of the mathematician's friend. Insofar as our calculation builds on (1)-(4), our conclusion is something we are only justified in believing in some unattractively modest sense.

Response: First of all, one *can* say more about why (1)-(4) are justified. For instance, one can justify (1) by doing an analysis of how electrons flow in a conducting material. But of course, this analysis will involve its own set of idealizations. The worry my opponent has, I think, is that at some point an idealization must be introduced that is *not* subsequently justified, and that this is bad.

But do not think that this poses a real problem. Insofar as there is a worry here, it stems from the thought that in order for an explanation to count as a genuine explanation, it must reduce everything to 'brute facts', so that nothing in the explanans could possibly call for further explanation. For unless one has this concern, it is difficult to see what is wrong with our explanation going no deeper than merely citing facts (1)-(4) and using them in a calculation. The idea that explanations must reduce everything to brute facts, however, imposes an absurdly high standard on scientific explanation, and is implausible as it stands. For instance, the fact that Snell's law is not a 'brute fact' does not make explanations of optical phenomena in terms of Snell's law incomplete. It may be that explanations of such optical phenomena in terms of Maxwell's laws or QED are better explanations (in some sense), but that is different from saying that the explanations in terms of Snell's law are incomplete. ■

The main claim of this section is that we can justify the results of calculations obtained from idealizations by viewing those results as ordinary consequences of *laws* that tell us that for certain purposes, certain physical systems may be treated in a certain idealized ways. This sort of justification, I have argued, avoids the problems discussed in previous sections. I have also claimed that this is actually the prototype of essentially all explanation in physics. Laws (1)-(4) are not merely useful for certain sorts of calculations in physics – rather, they form the prototype for the only substantive lawlike facts in physics with any sort of explanatory power.

4 The Laws and Language of Nature.

4.1 Idealizations and the Laws of Nature.

I have argued that the only laws of physics in which we currently have justified belief are of the form:

> *A physical system of type X may be treated as if it were a mathematical structure of type Y, for purposes of deriving conclusions of the form Z.*

For instance, we are justified in believing:

> (1) *For the purpose of simple calculations, a long, straight current carrying wire may be treated as if it were as if it were an infinite line of charge with constant charge density $\rho(\vec{x})$ and current density $\vec{j}(\vec{x})$ obeying Maxwell's equations.*

It is natural to ask whether we have grounds for positing the *literal, unqualified* truth of Maxwell's equations (or QED, or some other theory) as the *best possible explanation* of (1). That is to say, it is natural to ask if we are justified in believing:

> (1′) *Maxwell's equations are true.*

by an argument involving the claim that (1′) is the best possible explanation of (1). If so, then although we cannot argue for the literal truth of Maxwell's equations by a straightforward enumerative inductive inference, we can argue for the literal truth of such laws by an abductive inference. Would such an abductive inference be justified?

There are several questions here that need to be distinguished. The first is whether some *specific* theory – say, Maxwell's electrodynamics – is the best possible explanation of (1). Here, the answer is an easy negative. We have good reason to think that Maxwell's electrodynamics (and QED, for that matter) are *not* literally true, because as they stand, these theories presuppose a classical spatiotemporal background. No theory constructed on a classical spatiotemporal background can be the literal truth about the electrodynamic field in the presence of quantum gravity, so far as anyone can tell.

There is a second question that it is natural to ask here. Are we at least entitled to assume there is *some* set of physical laws that may be stated without any talk of idealizations (in the way that the statement of ($1'$) does not involve idealizations), that may be taken as literally and unqualifiedly true, and that explains (1)? Specifically, does an inference to the best explanation tell us that there is *some* explanation of (1) involving general laws that may be taken as literally and unqualifiedly true – even if this inference does not tell us what exactly those general laws *are*?

I do not see how even this more modest sort of inductive inference is supposed to work. It may well be true that laws like (1) tend to have explanations, but as a matter of empirical fact, these explanations are always in terms of other laws that invoke idealizations. Because we do not have *any* examples in physics of laws like (1) being explained by laws *not* involving idealizations, it is difficult to see how any sort of straightforward inductive inference is capable of telling us that there must be (or even, more modestly, are likely to be) laws not involving generalizations that explain facts such as (1).

In cases in which we abductively infer that some X must be true because it is the best explanation of Y, we always have antecedent grounds for thinking that Y has an explanation of a certain sort. We then claim that there is something about X that makes it most likely to be the correct explanation. For instance, suppose we find someone with a bloodied knife running from a house in which a murder has been committed. An inference to the best explanation justifies the belief that this person likely committed the murder. The permissibility of this inference, however, depends on the fact that when a person has a bloodied knife in their hand, this tends to be explainable in one of a certain number of ways. (Either they have committed a murder, or have been preparing a fresh meat dinner, or something like that.) In this particular case, we think that one explanation is more likely. In order for abduction to work, we must have prior reasons for thinking that the thing to be explained most likely has an explanation of a particular sort. But what sort of prior reason do we have for thinking that a claim like (1) must be explainable by a claim that looks something like ($1'$)? I do not see what sort of answer can be given.

Taking a different approach, one might try to argue that physics has produced a stream of increasingly accurate claims, and therefore we have inductive grounds for thinking that there must be laws of physics that are absolutely accurate – i.e., *literally* true – and that perhaps even explain laws like (1). But the capacity to come up with mathematical equations that may be relied upon in broader and broader contexts does not give us grounds for thinking that it is possible to come up with equations that may be relied on in *all* contexts, any more than the discovery that matter can be divided into smaller and smaller units gives us grounds for believing in point particles. The fact that it is possible for some quantity to be made arbitrarily small does not on its own give us any reason for thinking that the quantity can be made to vanish, and so this line of argument strikes me as weak.

One might try to argue on more general metaphysical grounds that a claim like (1) must be explainable by something that looks like (1′). For instance, one might try to argue from the Principle of Sufficient Reason that (1) has an explanation, and that an explanation of (1) involving idealizations is not a *bona fide* explanation. But it is unclear why we should accept the Principle of Sufficient Reason, and equally unclear why an explanation of (1) involving idealizations is not a bona fide explanation. I am not aware of any other vaguely plausible ways of arguing on general metaphysical grounds that (1) must be explainable by something like (1′).

To summarize the main point of this section – it is nothing more than an article of philosophical faith that laws involving idealizations are consequences of deeper, underlying laws not involving idealizations. We have no ordinary inductive grounds for believing that such laws exist, or even for believing that if they do exist, they explain laws that involve idealizations. This is not to deny that there might be such laws, nor is it to deny that such laws might explain laws involving idealizations. It is just to say that natural science neither presupposes nor gives us reason to believe in the existence of such laws. Insofar as we are inductively justified in believing laws of nature, those laws involve idealizations, and insofar as we believe in laws that do not involve idealizations, those beliefs are not inductively justified.

4.2 Idealizations and the Language of Nature.

It is sometimes said that the language of nature is mathematics. For instance, we find Kepler saying:

> *'The chief aim of all investigations of the external world should be to discover the rational order and harmony which has been imposed on it by God and which He revealed to us in the language of mathematics.'*

This sentiment is also found in the writings of Gauss, Galileo, Einstein, Wigner and others.

It is impossible to understate the importance of mathematics for modern science. There is obviously a very deep sense in which nature is mathematical. But precisely because of this, it is worth getting clearer about what it means to say that mathematics is the language of nature. Having done this, we can then try to figure out exactly what sort of evidence we have for this claim.

Suppose it is the case that the physical objects in nature can be characterized at any point of time by a set of numerical parameters. (Perhaps, for instance, a physical body can be characterized by specifying the position, momentum, charge and mass of all the particles composing it.) Suppose that the way in which these parameters evolve over time can be captured exactly by a set of equations involving those parameters. In such a case, we might say that mathematics is the language of nature.

It way well be true that there is such a list of parameters and a set of such equations. But as I have argued in the previous section, we do not have *inductive* grounds for thinking that such things exist. In this sense, we do not have inductive evidence for thinking that mathematics is the language of nature.

But this does not mean that Kepler was entirely wrong. Consider again the lawlike claim:

> (1) *For the purpose of simple calculations, a long, straight current carrying wire may be treated as if it were as if it were an infinite line of charge with constant charge density* $\rho(\vec{x})$ *and current density* $\vec{j}(\vec{x})$ *obeying Maxwell's equations.*

Think of an infinite line of charge obeying Maxwell's equations as a *mathematical structure.* Our law then says that in certain contexts (e.g., in the context of calculating the first significant figure of some particular quantity), we may treat our physical system as if it were the given mathematical structure.

The accomplishment of Maxwell's electrodynamics is not that of coming up with a set of literally true claims about charged objects. Maxwell's equations do not do that at all. The accomplishment of Maxwell's electrodynamics is that of identifying a class of physical systems and questions about those systems that can be answered by treating the system as a mathematical structure. Moreover, the strategy of identifying classes of physical systems and questions about those systems that can be answered by treating those systems as mathematical structures has proven to be a good strategy in the expansion of human knowledge. This strategy has shown us how to solve (in principle, and sometimes in practice), a large variety of problems about the natural world. It is in this sense that the language of nature is mathematics. That is to say, it is in virtue of the fact that there are many physical systems and questions about those physical systems that can be answered by treating those physical systems as if they were mathematical structures, that we say that the language of nature is mathematics.

Insofar as we continue to find more physical systems and questions about those systems that may be answered by treating those systems as mathematical structures, we may say that we have *evidence* that the language of nature is mathematics. But insofar as we mean something more ambitious when we say that mathematics is the language of nature – that is, insofar as we mean that objects may be characterized by parameters whose time evolution can be captured exactly by a set of equations – we have no evidence at all that mathematics is the language of nature. When Kepler expressed admiration at the role mathematics played in articulating the orbits of the planets, he was presumably expressing admiration at the fact that the questions he asked about the orbits of the planets could all be answered by treating the planets and their orbits as a given mathematical structure. Kepler was amazed at the fact that simple, harmonious models could be found that enabled him to make accurate predictions about planetary orbits. Presumably, he was not amazed at the fact that he was able to uncover a set of literal truths about planetary motion – for even he realized that his laws were not exactly true. So when Kepler took mathematics to be the language of nature, he did so in the more modest sense that I have outlined. In this sense, Kepler was right to be struck by the mathematical language of nature. But he would not have been justified to say that mathematics was the language of nature in any stronger sense.

5 The Model Based Approach to Laws.

This section needs to be rewritten (and, more importantly, rethought.)

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