Mathematics, Science and Confirmation Theory

Christopher Pincock (pincock@purdue.edu)

March 26, 2010

4896 words

Abstract

This paper begins by distinguishing intrinsic and extrinsic contributions of mathematics to scientific representation. This leads to two investigations into how these different sorts of contributions relate to confirmation. I present a way of accommodating both contributions that complicates the traditional assumptions of confirmation theory. In particular, I argue that subjective Bayesianism does best accounting for extrinsic contributions, while objective Bayesianism is more promising for intrinsic contributions.

1. Introduction. One can distinguish between two different sorts of contributions that mathematics makes to our scientific representations. These are intrinsic and extrinsic contributions. The contrast between these two kinds of contributions will structure my discussion of how mathematics makes a contribution to the confirmation of scientific representations. Above all, I want to draw attention to the difficulties in understanding these contributions using our standard ways of thinking about scientific confirmation. Given the space available, the options that I will discuss are subjective Bayesianism and objective Bayesianism. It will turn out that subjective Bayesianism has a promising strategy for explaining the extrinsic contributions, but it has more difficulties accounting for the intrinsic contributions. Conversely, while objective Bayesianism seems able to ground the principles necessary to explain the intrinsic contributions, this approach faces further problems with extrinsic contributions. This leaves us with no comprehensive approach to confirmation that is sufficient to explain how mathematics contributes to the confirmation of scientific representations.

As with most of the literature on confirmation, I will put my discussion in schematic terms. Consider first a dynamical representation D of some type of physical system. I include in the specification of D a suitable dose of initial and boundary conditions so that D has a unique solution P_D. But I will stipulate that the typical route for arriving at P_D from D involves a mathematical derivation whose assumptions M outstrip the mathematics necessary to formulate D. This can happen in several different ways, but for our purposes it is sufficient to think of D as a system of differential equations involving real-valued functions and the additional mathematics M as involving the complex numbers and complex-valued functions.[1] To continue our example, it may happen that the solution P_D is checked against the results of several experiments E_i for a range of relevant systems. I will take it for granted than any account of confirmation should help us to understand how the agreement of P_D with E_i provides us with a reason, other things being equal, to believe that D is correct. Or, to put the point more in the terms of confirmation theory, such a result should raise our degree of confirmation for D.

We can distinguish two places in which mathematics appears to be making a contribution to the confirmation of D. First, there is the mathematics *intrinsic* to the formulation of D. What I mean by this is that there is some body of mathematics M_D that an agent must believe if they are to understand the content of D. An explanation of exactly why mathematics might have this central role requires some discussion of what the content of D and other such representations actually is. I would suggest that we think of the content as involving a claim that there is a structural relation between some of the physical properties and relations of the physical system and the mathematical properties and relations of some mathematical structure. If this is right, we can see why an agent would have to believe some mathematics M_D in order to contemplate D. Understanding D requires understanding what the system must be like if it stands in the specific structural relation at issue to the mathematical structure. A belief in this mathematical structure and some of its features is thus part of the background against which D acquires the content that it does.

[1]See Colyvan 2001, p. 83 and Pincock 2004, §5.

Second, we have the mathematics that is *extrinsic* to the formulation of D. This is any part of mathematics that contributes to the scientific investigation of D that falls outside of the intrinsic mathematics of D. For our purposes, the only extrinsic mathematics in play is M, the mathematics that appears in the derivation of P_D from D. On a first pass, at least, it seems obvious that an agent would not be in a position to confirm D using the experimental results E_i unless they believed M. For M was essential to the derivation of P_D from D and it is only this derivation that established a link between D and the E_i.

A reasonable question to ask at this point is where the belief in the intrinsic and extrinsic mathematics that is presupposed in this simple sort of confirmation comes from. Confirmational holists about mathematics will insist that any mathematical beliefs be confirmed just like any other beliefs as part of the confirmation of our best scientific theories. I do not see, though, how such a holistic approach can make sense of either sort of contribution from mathematics that I have introduced. If there actually are intrinsic contributions from mathematics, then an agent must believe some mathematics in order to even formulate a given scientific representation. Similarly, the extrinsic contributions require significant mathematical assumptions in order to link a given representation to some testable predictions. If all mathematics were confirmed as the holist suggests, then it seems to me that scientific confirmation would never get off the ground. While what I have said here is not much of an argument against holism, it at least explains why I am inclined to assume that some parts of mathematics can be confirmed independently of its role in science. I will take this for granted in what follows.

I urge holists to try to redescribe the sort of simple cases that I discuss in their own terms and see if the difficulties that they encounter might suggest some reconsideration of holism.

2. Extrinsic. We can distinguish several different approaches to confirmation. In the interests of space, I will focus on theories that employ the probability calculus to represent degrees of confirmation or relative measures of confirmation. The most popular deployment of this option is of course Bayesianism.[2] In its subjective variant, Bayesianism associates degrees of belief with competing scientific hypotheses and explains under what circumstances these degrees of belief should go up or go down in light of learning new information about the world. These degrees of belief, in turn, are grounded in what sorts of odds an agent would accept on a certain kind of bet. It is then argued that any rational agent should try to assign their degrees of belief using the rules of the probability calculus, and that they update or conditionalize their degrees of belief over time using Bayes' Theorem. One distinctive feature of subjective Bayesians is that they have a permissive conception of rationality which allows different agents to start with different prior probability assignments. This seems to many to undermine the link between degrees of belief and what is reasonable. The subjectivity is removed by various attempts to make a stronger link between degrees of belief and objective features of the world. Objective Bayesians insist that prior probabilities must be fixed by genuine probabilities in the world, for example, the objective chance that a given belief will turn out to be true. While objective Bayesians seem more in line with our ordinary conception of rationality as

[2] See Hawthorne 2010 for a very helpful overview of Bayesianism.

objective, it remains a challenge for them to justify their prior probability assignments.

In this section I want to consider the problems for accounting for extrinsic contributions of mathematics to scientific confirmation. These problems can be traced to a well-known problematic feature of the rules of the probability calculus. This is that the rules require that logically equivalent propositions be assigned the same probability. When probabilities are interpreted as degrees of belief, this assumption can easily be seen to be unrealistic. As typically understood, all logical and mathematical truths are logically equivalent, but agents do not know which sentences written in logical and mathematical terms express true propositions. That is, agents are not logically and mathematically omniscient. As a result, they do not know which propositions are logically equivalent, so it is impossible for them to follow this rule of the probability calculus. This point seems especially difficult for the objective Bayesian as she insists that degrees of belief track objective chances. In any viable sense, the objective chance of all mathematical truths is 1 and the objective chance of all mathematical falsehoods is 0, but not even the best mathematicians are in a position to make the right assignments for all but a few cases. A subjective Bayesian may seem to have more options as she can permit an agent to violate some rules of the probability calculus based on their subjective limitations. Still, subjective Bayesians typically have nothing to say about degrees of belief for mathematical propositions, and it is unsatisfying to allow massive violations of the rules of the probability calculus in such a central part of science.

The consequences of the assumption of logical and mathematical omniscience are easy to see if we note that a boost in confirmation upon learning some new information E is represented as a change from $Pr(H)$ to $Pr(H|E)$ via conditionalization. However, $Pr(H|E) = Pr(H\&E)/Pr(E)$. If E is any proposition with degree of belief of 1, then $Pr(E) = 1$ and $Pr(H\&E) = Pr(H)$ (assuming E is consistent with H). As a result, $Pr(H) = Pr(H|E)$. This means that if every mathematical and logical truth is assigned a degree of belief of 1, they make no contribution to the confirmation of scientific representations. A rational agent would already have factored these truths into their probability assignments and so is precluded from deploying these beliefs in the practice of confirmation. And if we simply relax logical and mathematical omniscience, then it is no longer clear what systematic story we can tell about how mathematical beliefs help out with scientific confirmation.

More sophisticated Bayesians are aware of this sort of problem, and tend to offer two responses. The first is to restrict their focus to contingent propositions. While this may seem reasonable, it automatically excludes from the Bayesian framework a whole host of scientific cases where mathematical beliefs play a central role. More ambitious Bayesians can offer a second response. This is to attempt to revise the Bayesian framework to accommodate failures of logical and mathematical omniscience in some systematic way. Independently of my motivations to do this, there has already been an extensive discussion of these issues in connection with what is known as the problem of old evidence. The problem arises when we think of evidence that is believed with degree of belief 1 prior to the formulation of a new scientific hypothesis. Based on the argument from the previous paragraph, if the evidence is consistent with the hypothesis, it cannot lead to any change in the degree of belief in the hypothesis over the prior probability that the agent assigns to it. This seems contrary to actual scientific practice. It is often the ability of a hypothesis to accommodate evidence that was already accepted that provides a decisive reason to accept the hypothesis.

While formally similar, I would suggest that the correct solution to the problem of old evidence should be quite different from our difficulties with logical and mathematical omniscience. When the evidence is contingent, it is possible to consider an agent A' quite like the agent A, but who does not believe the evidence with degree of belief 1. We can then consider how A''s degrees of belief in the relevant hypothesis would change upon learning the evidence. This can give us some insight into whether or not A has a reason to believe the hypothesis based on the evidence.[3] This shows that it is possible to factor out the contribution of the evidence to the rational degree of belief in the hypothesis even when it is old evidence. This promising strategy is not possible for the case of mathematical truths. Consider again our case of the extrinsic deployment of some body of mathematics M in moving from a dynamical representation D to its solution P_D. It is metaphysically necessary that $D \rightarrow P_D$. As a result, it looks like $Pr(P_D|D) = Pr(P_D|D\&M) = 1$. So, here the fact that the agent believes M is not relevant to the probability assignment for P_D. There thus appears to be no solution to the problem based on some attempt to factor out the agent's belief in M by considering similar agents. Something different is required to handle the problem.

[3]Howson and Urbach 1989, p. 274: "the support of h by e is gauged according to the effect which a knowledge of e *would* now have on one's degree of belief in h, on the (counter-factual) supposition that one does not yet know e."

My suggestion is that we scrutinize more carefully the relation between logically equivalent propositions and metaphysically necessary truths. Suppose we found a principled way to maintain that, even though $D \rightarrow P_D$ is metaphysically necessary, this proposition is not logically equivalent to a tautology like $A \vee \neg A$. Then we would at least have room to insist that $Pr(P_D|D) < 1$ and $Pr(P_D|D) < Pr(P_D|D\&M)$. This would allow us to make sense of someone who came to deploy M in moving from D to P_D and their resulting boost in confirmation.

My proposal is to insist that even though correct mathematical claims are true in all possible worlds, they are not logically necessary, and so in particular metaphysically necessary conditionals like $D \rightarrow P_D$ do not correspond to genuine logical entailments. Consider the simple inference from "The number of fish is greater than the number of cats" and "The number of cats is greater than the number of dogs" to the conclusion "The number of fish is greater than the number of dogs". It is true in all possible worlds, or metaphysically necessary, that if $a > b$ and $b > c$, then $a > c$. So, there is no possible world in which the premises are both true and the conclusion is false. Still, a standard paraphrase of the argument into a formal logical language would give it the form "$Nx(x$ is a fish$) > Nx(x$ is a cat$)$, $Nx(x$ is a cat$) > Nx(x$ is a dog$)$, therefore $Nx(x$ is a fish$) > Nx(x$ is a dog$)$". The operator "Nx" takes a formula "$F(x)$" and yields a name for an object, namely the number of Fs. So, in terms of its logical form, the sentences of this argument look like they just involve a two-place predicate symbol and two names. As a result the argument can be further simplified as "Gab, Gbc, therefore Gac". This paraphrase signals that there are no logical terms in the premises or the conclusion and so none of the terms has a fixed interpretation. Taking this approach, then, leads us to conclude that the argument is not logically valid. The same result would obtain for the inference from D to P_D if we treated its mathematical terms as non-logical terms. In both cases we could add new premises which would make the argument logically valid. For example, in the fish, cat and dog argument it would sufficient to add the claim that greater than is a transitive relation. My point is that these additional claims involve non-logical assumptions.

The core of this proposal is that there is a sharp line between logical and mathematical terms. This division can be questioned by many different philosophical approaches to mathematics, but the clearest objection comes from logicism and its successors like the neo-Fregean philosophy of arithmetic championed by Hale and Wright (Hale and Wright 2001). They take "$NxFx$" to be defined using Hume's principle:

$$NxFx = NxGx \leftrightarrow F \approx G$$

where $\approx$ is a relation that obtains between the concepts F and G when the Fs and the Gs are one-one correlated. This sort of "contextual" definition links numbers to facts involving concepts. So, if this definition is adopted, then it might seem like the final paraphrase of our argument as "Gab, Gbc, therefore Gac" ignores certain logical relationships between the premises and the conclusion. For example, Hume's Principle and further mathematical definitions could ensure that the greater than relation is transitive. More generally, any broadly logicist approach to mathematics threatens to restore a tight link between premises and conclusion and so force a return to mathematical omniscience and its consequences.

While there is insufficient space to engage with this objection here, my preferred line of response would be to insist on a distinction between logical truths and those that arise from definitions given in non-logical terms. It would be difficult for the advocates of the neo-Fregean definition of numbers to insist that Hume's principle is a logical truth for the simple reason that in the context of second-order logic it is sufficient to prove that infinitely many things exist. The derivation of the Peano axioms from Hume's principle is known as Frege's Theorem. It was used to motivate the claim that Hume's principle is the correct definition of number. Hale and Wright do not insist that it is a logical truth, but prefer to articulate a sense of "analytic" according to which Hume's principle is analytic. Even if this proposal is accepted, it would still be possible to carry through a strict division between logical and non-logical terms. Along the same lines, one can adopt the definition of "bachelor" as "unmarried man" and still regiment the sentence "All bachelors are men" as "$\forall x(Fx \rightarrow Gx)$".

According to the strategy just defended, all metaphyscial necessities involving mathematical entities are removed when we regiment an argument into logical notation. This arguably goes a bit too far, though. To see why recall the role of the intrinsic mathematics D_M in specifying the content of D. This suggests that some minimal features of D_M, or what I will call the *core conception* of D_M, should be tacitly included as premises in the inference from D to P_D.[4] Still, if the mathematics used in getting from D to P_D is genuinely extrinsic to D_M, then these tacit premises will not suffice to

[4]One way of clarifying what the core conception of a given mathematical domain is can be found in Peacocke's discussion of implicit conceptions (Peacocke 1998).

constrain the interpretation of D to deliver the entailment relation to P_D. The only thing that can do this is the addition of enough additional premises associated with M. This will further constrain the premises and restore the entailment.

Recall that our original problem was to make sense of both $Pr(P_D|D) < 1$ and $Pr(P_D|D) < Pr(P_D|D\&M)$. $Pr(P_D|D) = 1$ seems to be required by the axioms of the probability calculus whenever D logically entails P_D. But we have seen how our mathematical cases need not correspond to genuine logical entailments, and so the Bayesian is free to set their $Pr(P_D|D)$ to some number less than 1. However, when sufficiently many mathematical beliefs are brought to bear on D, then at some point the conjunction $D\&M$ will logically entail P_D. So, the Bayesian can explain why it is rational to require that $Pr(P_D|D) < Pr(P_D|D\&M)$. The extrinsic contribution from the mathematics M has been accounted for in terms of a boost in confirmation.

There does not seem to be any barrier to adopting this proposal for a subjective Bayesian. But for the objective Bayesian there are severe obstacles. The basic problem is that objective Bayesians constrain their priors using objective features of propositions like the chance that the proposition is true. In our case, the conditionals in question are metaphysically necessary and so there is no room for the distinction that I have made between a logically necessary statement and a mathematical truth. Here, it seems, the flexibility of the subjective Bayesian approach has proved a virtue. In the next section I consider the intrinsic contributions from mathematics where this virtue turns into a vice.

It is worth briefly noting an alternative strategy for accounting for these extrinsic contributions which is available to the objective Bayesian. This is to insist that the mathematics is playing a merely epistemic role in helping us to recognize what our degrees of belief should be. On this view, an agent should always set $Pr(P_D|D) = 1$, but without the use of M the agent is not able to realize this. It would be a delicate matter to decide whether this "recognition" approach is superior to the account of confirmation which I have just described.

3. Intrinsic. The positive contribution of intrinsic mathematics is harder to discern for the simple reason that we do not typically have representations in any sense analogous to D but also lacking mathematics. It is true that we can sometimes come up with what are arguably non-mathematical versions of D, as with Hartry Field's attempt to present a nominalistic version of our best scientific theories (Field 1989). The problem with such approaches, even if they are actually non-mathematical, is that they result in representations that we have no reason to believe are actually correct. As I have argued this point in detail elsewhere (Pincock 2007), here I stick to a comparison of different representations where a difference in the intrinsic mathematics is at least partly responsible for a difference in confirmation. If there are such cases, I think it is fair to attribute the variation in confirmation to the variation in intrinsic mathematics.

Suppose that an analysis of the dynamical representation D leads an agent to posit a sub-class of target systems of D and a new steady-state representation S of these systems. As the term "steady-state" suggests, S represents such systems as unchanging within certain spatial and temporal bounds.[5] It may happen, for example, that some of the systems covered by D undergo a qualitative change if some parameter exceeds a critical threshold, after which they move into a steady-state. S will not represent the dynamical processes that lead to this transition and may even omit the crucial parameter that is responsible for it. Just as with D and its intrinsic mathematics D_M, S will have its own intrinsic mathematics S_M. There is no guarantee that the intrinsic mathematics of the two representations will be the same or even overlap to some degree.

Scientists often shift from D to S, and I want to suggest that one of the reasons that they do this is that S is typically easier to confirm and disconfirm when compared to D. As far as I can tell, there is not much discussion of this sort of shift in confirmation theory.[6] The reason seems to be that the differences between D and S relate most directly to their relative prior probability and it has proven difficult to make any systematic claims about how relative prior probabilities should be constrained. My proposal is to focus on the range of constraints that a representation places on a system for that representation to be accurate. In certain cases, like the relation between D and S, we can have pairs of representations that are related in the following way. D includes S as a special case or at least as a case that results from D through the specification of certain parameters or other simplifications of this sort. As a result, there is a sense in which S says less than D. This diminished content shows itself in two ways. First, the

[5] Briefly, a steady-state model represents no net changes in the magnitudes of a system. A special case of this is an equilibrium model according to which there are no net changes because all the relevant contributions are zero.

[6] But see Sober 1983.

number of systems that S purports to capture is less than what D purports to capture. This follows as a natural consequence of the simplifications that produce S from D. Second, even when the two representations have the same target system, S says less about it than D. That is, S imposes conditions on the system that are easier to meet than D. When both of these tests are met, then I will say that S has *less content* than D.[7]

With this notion of the amount of content in mind, we can articulate three principles that I would argue any confirmation theory must include. I am not optimistic that every approach to confirmation theory can find a way of motivating the principles I give, but I will not argue this point here. The principles are

(P1) Other things being equal, if R' has less content than R, then R' starts with a higher relative degree of confirmation than R.

(P2) Other things being equal, if R' has less content than R and a piece of evidence E supports both R' and R, then R' will receive a larger boost in confirmation than R.

(P3) Other things being equal, if R' has less content than R and a piece of evidence E undermines both R' and R, then R' will receive a larger drop in confirmation than R.

It is worth seeing how these principles help us to reconstruct our central case of intrinsic mathematics contributing to the confirmation of a scientific

[7] There are of course other notions of content according to which S has more content than D or where it makes no sense to compare representations with respect to their content. The skeptical reader can think of "less content" as an abbreviation for the sort of relationship between representations just described.

representation. If D is a dynamical representation of some kind of system and S is a steady-state representation that results from D via certain simplifications, then it is plausible to think that S has less content in my sense than D. First, almost all of the systems of the given kind are not in a steady-state, and so the targets of S are a proper subset of D. Second, when we consider what S says about systems that have reached a steady-state, in so far as D is a dynamical representation, it will include details of how the system stays in that steady-state for an extended period of time. Other things being equal, S will not include that information as it will just represent what the system is like while it stays in the steady state. So, our principles should be in force.

(P1) is the most debatable of our principles and has some similarity to principles of simplicity that some might use to fix priors in an objective Bayesian framework. I do not see how one could make it plausible as a quantitative principle, but in the ordinal way that I have presented it here, it seems plausible, and tends to accord with judgments of scientists. (P2) and (P3) come into play after we have collected evidence relevant to both representations for a system that they both represent. As R' has an impoverished content as compared to R, R' is more susceptible to being confirmed or disconfirmed by a single relevant piece of evidence. For example, if a steady-state representation S predicts that the system will be stable in that state, and it turns out that the system is unstable in that state, then it is more or less completely disconfirmed and should be discarded. By contrast, a failure of D to capture this feature may be explained away more easily based on the large number of simplifications necessary to cash out stability from a dynamical perspective. Even if D gets this stability feature wrong, it can still get the dynamics largely correct, and so can remain a viable representation.

In the more positive case, where (P2) is in force, we have evidence that a steady-state representation S gets some feature, e.g. stability, correct. This gives the representation a larger boost in confirmation than what is received by a corresponding dynamical representation D. D, while it has gotten things right, involves a wide range of other commitments about the system. Just because we have seen that it gets this part correct, we are not entitled to raise its confirmation as much as we did for S. Additional tests concerning the dynamics itself are needed.

It might seem like (P2) is vulnerable to the following sort of counterexample.[8] Suppose that the evidence E is a conjunction $E_1 \wedge E_2$. Furthermore imagine that while E_1 supports both R' and R, E_2 supports only R. Then isn't it possible that E will provide greater support to R than it does to R'? This problem suggests that we need to impose a fairly strict reading of what it means for a piece of evidence E to support both R' and R. The strict reading requires that E and all its parts support both R' and R. Adhering to this strict reading blocks the counterexample. Of course, this does not yet provide a positive argument for (P2) or clarify exactly why the strict reading is warranted.

While I have just scratched the surface of these principles and their defense, it should be clear that the objective Bayesian will have a much easier time arguing for these principles than the subjective Bayesian. The subjective Bayesian has a permissive conception of rationality which allows rational agents to assign vastly different priors as long as they conditionalize using the

[8]Similar problems arise for (P3).

same rules as the evidence comes in. This means that it is hard for the them to defend (P1). The objective Bayesian, by contrast, typically tries to justify more general principles for assigning priors which could have (P1)-(P3) as a consequence. Whether this further grounding of our principles is plausible would of course depend on the specifics of the objective Bayesian position. Even at this stage of the discussion, though, it is clear that the task of accounting for the intrinsic contributions from mathematics is quite different than what we saw in the extrinsic case. Here it is the objective Bayesian that has a clear strategy, while the subjective Bayesian is left with few viable options. It is always open to the subjective Bayesian to supplement their account of rationality with additional principles like our (P1)-(P3). But the burden is on them to say how these principles follow from their conception of rationality.

4. Conclusion. I have distinguished two different ways in which mathematics can play a crucial role in the confirmation of our scientific representations. It is premature to conclude that none of the extant approaches to confirmation theory can be adjusted to handle both of these contributions. At the same time, the difficulties for subjective and objective Bayesianism in handling both contributions suggest that this is a daunting challenge for theories of confirmation. Further work is needed to help us to develop models of how mathematics makes the contributions that it does to our actual scientific knowledge.

Acknowledgments. An earlier version of this paper was given in a symposium with Robert Batterman, Stathis Psillos and Mark Wilson. I would like to thank them for a productive session as well as for their help with this project. Comments from the audience, Paul Draper, James Hawthorne, Jonah Schupbach and two anonymous referees were also very useful in revising this paper.

References

Colyvan, Mark. 2001. *The Indispensability of Mathematics*. New York: Oxford University Press.

Field, Hartry. 1989. *Realism, Mathematics and Modality*. New York: Blackwell.

Hale, Bob and Wright, Crispin. 2001. *The reason's proper study: Essays towards a neo-Fregean philosophy of mathematics*. New York: Oxford University Press.

Hawthorne, James. 2010. "Bayesian confirmation theory." To appear in Steven French and Juha Saatsi (eds.), *Continuum Companion to the Philosophy of Science*.

Howson, Colin and Urbach, Peter. 1989. *Scientific Reasoning: The Bayesian Approach*. Chicago: Open Court.

Peacocke, Christopher. 1998. "Implicit conceptions, understanding and rationality." *Philosophical Issues* 9: 43–88.

Pincock, Christopher. 2004. "A revealing flaw in Colyvan's indispensability argument." *Philosophy of Science* 71: 61–79.

Pincock, Christopher. 2007. "A role for mathematics in the physical sciences." *Nous* 41: 253–275.

Sober, Elliot. 1983. "Equilibrium explanation." *Philosophical Studies* 43: 201–210.