Mathematical Explanation Beyond Explanatory Proof

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Abstract

Much recent work on mathematical explanation has presupposed that the phenomenon involves explanatory proofs in an essential way. I argue that this view, 'proof chauvinism', is false. I then look in some detail at the explanation of the solvability of polynomial equations provided by Galois theory, which has often been thought to revolve around an explanatory proof. The paper concludes with some general worries about the effects of chauvinism on the theory of mathematical explanation.

Near the beginning of 'Mathematical Explanation'—the founding document of the recent literature on the subject—Mark Steiner writes: 'Mathematical explanation exists. Mathematicians routinely distinguish proofs that merely demonstrate from proofs which explain' ([Steiner 1978a], p. 135). Judging from his treatment of the subject in the rest of the paper, Steiner seems to intend the second claim as something like an elaboration of the first, rather than as an example of one sort of mathematical explanation among others.[1] That is, Steiner appears to endorse approximately the following view:

> Proof Chauvinism: All or most cases of mathematical explanation involve explanatory proofs in an essential way.

Steiner's contemporaneous paper on mathematical explanations in science ([Steiner 1978b]) is perhaps even more explicitly committed to proof chauvinism. In that essay, Steiner claims that we have a mathematical explanation of a physical phenomenon just in case 'when we remove the physics, we remain with a mathematical explanation—of a mathematical truth' (p. 19). A few lines later, Steiner seems to suggest that 'remaining with a mathematical explanation' amounts to obtaining an explanatory proof of the relevant theorem.[2] On this view, then, an explanation of a mathematical fact just is an explanatory proof of that fact.

Much has been written on the subject since Steiner's work appeared, and his theory of mathematical explanation is no longer popular.[3] For one reason or another, however, Steiner's proof-chauvinist assumptions have taken deep root. Subsequent papers on mathematical explanation have almost invariably dealt with some issue in the theory of explanatory proofs—for instance, whether and why a particular proof explains its result ([Sandborg 1998]; [Mancosu 2001]; [Lange 2015]), whether certain general types of proof are explanatory or not ([Cellucci 2008]; [Lange 2009]; [Baker 2010]), or the factors that contribute to a proof's being explanatory ([Mancosu 1999]; [Lange 2014]; [Pincock 2015]). Some

[1] Near the end of the paper, Steiner also mentions explanations 'of the "behavior" of a mathematical object' (p. 148), and explanations obtained by regarding one mathematical object as another. It's not clear what Steiner takes to be the *explanantia* in these cases. As best I can tell from Steiner's discussion, the *explanans* in the first case involves a proof or at least some sort of argument. I'm unsure what the *explanans* in the second case is supposed to be. Steiner characterizes these cases as non-examples of 'explanation by proof', but it's unclear how to interpret this remark in light of the surrounding discussion and the commitments he expresses elsewhere.

[2] The comment about explanatory proof is made in the context of Steiner's analysis of a particular example, and he doesn't make it totally clear whether this feature of the example is supposed to generalize. But the whole discussion suggests that Steiner does endorse the generalization. ([Baker 2012]), for instance, certainly reads Steiner this way.

[3] For criticisms of Steiner's view, see for example [Resnik & Kushner 1987], [Hafner & Mancosu 2005], and [Pincock 2015]. [Baker 2012] argues against Steiner's account of mathematical explanations in science. Baker's line is that some mathematical results contribute to scientific explanations even though no explanatory proofs of the results are known. (For instance, the perimeter-minimizing optimality of the hexagonal tiling of the plane partly explains why honeybees build hexagonal cells. But the only available proof of the optimality theorem is not at all explanatory.) As will become clear below, I agree with the idea that theorems without explanatory proofs can nevertheless be explanatory themselves. (Thanks to an anonymous referee for reminding me about [Steiner 1978b] and [Baker 2012] and their relevance to the chauvinism issue.)

authors admit that there might be other kinds of mathematical explanation, but they almost never say what these kinds might be or discuss specific examples. And it's still common to see mathematical explanation in general run together with explanation-by-proof in particular, as in passages like the following.:

> The topic of mathematical explanation seems to me a central one for an accurate understanding of mathematics. Mathematicians, as we have seen, do not simply struggle to obtain rigorous and compelling proofs. They often look for new proofs or consider old proofs unsatisfactory on account of their lack of "explanatoriness". ([Mancosu 2001], p. 102)

The impression given by all this is that mathematical explanations not involving explanatory proof are vanishingly rare, or not very interesting, or both. I aim to show that this view is mistaken, and importantly so. Proof chauvinism has already led to missteps in the theory of mathematical explanation, and its continued hegemony threatens to make things worse. (Hence it's not merely, as one might have thought, that philosophers have focused on explanatory proofs just because they're the most distinctively mathematical sort of mathematical explanation. Such an attitude might have led to an imbalanced distribution of research efforts, but it probably wouldn't be so bad on the whole. Rather, I'll argue that proof chauvinism has had more insidious effects—that it's led to theoretical errors and misdiagnoses of important cases, for instance.)[4]

I'll begin by considering the epistemic status of chauvinism. What sorts of reasons could there be for accepting it, as many philosophers apparently do? Neither Steiner nor anyone else, as far as I'm aware, has defended the principle explicitly, but its possible justifications seem to fall into two general categories. The first kind is something like this: We should accept chauvinism because it faithfully reflects mathematical practice. When mathematicians pursue explanations or make claims about what explains what, according to this line of thought, explanatory proofs are generally their targets. Hence, as philosophers striving to ground our theories in the realities of working mathematics, we should be chauvinists. The second sort of possible justification runs as follows: We should accept chauvinism because it has a compelling philosophical rationale. Whatever mathematicians might or might not say, the theory of explanation presents us with good reasons—perhaps emanating from general philosophy of science, or from elsewhere in the philosophy of mathematics—to think that explanatory proofs must be central to mathematical explanation. Hence, as philosophers striving to achieve a unified picture of the theoretical landscape, we should accept chauvinism.

The next section discusses several arguments of the above kinds. Its conclusion is that philosophical considerations offer no support for proof chauvinism, and mathematical practice strongly suggests its falsity. The rest of the paper provides some reasons for caring about the chauvinism issue. I look in some detail at the explanation of the solvability of polynomial equations provided by Galois theory, which has often been thought to revolve around an explanatory proof. I argue that this diagnosis is mistaken. The paper concludes with some general worries about the effects of chauvinism on the theory of mathematical explanation.

1 Why I Am Not a Proof Chauvinist

1.1 Proof chauvinism and mathematical practice

As suggested above, one reason for embracing proof chauvinism is that it might be thought to reflect mathematical practices of seeking and offering explanations. It's not hard to see why this view might seem attractive. After all, mathematicians certainly do talk often about the explanatory virtues of various proofs (and proof methods)—the recent literature on mathematical explanation has shown at least that much beyond doubt. Generalizing from these cases, one might form the impression that proofs are the only sort of explanation that mathematicians care much about.

But this impression is erroneous. Mathematicians often cite items other than proofs as the *explanantia* of mathematical explanations: for example, theories, diagrams and other sorts of pictures, and particular facts of various descriptions. Examples of each kind of attribution are easy to find. Here are just a few:

[4] Thanks to an anonymous referee for prompting me to make this point more clear.

- ‘The following theorem, which we give here without proof, provides a necessary and sufficient condition for a number to be the sum of two squares: *An integer a is the sum of two squares if and only if any prime congruent to* 3 (mod 4) *in the prime factorization of a appears an even number of times in the factorization.* This theorem explains why 12 is not the sum of two squares.’ ([Maor 2007], p. 224; emphasis in original)
- ‘The next lemma explains why certain degenerate cases, singled out by the condition $\check{\chi} = \chi$, sometimes simplify [. . .] *Let* χ *be a character on a group* G *such that* $\check{\chi} = \chi$. *If* G *is generated by its squares, and in particular if* G *is* 2*-divisible, then* $\chi = 1$.’ ([Stetkær 2013], p. 31; emphasis in original)
- ‘The fact that $x^m - 1$ is reducible over $\mathbb{Q}$... explains why its roots do not all have the same algebraic properties over $\mathbb{Q}$.’ ([Newman 2012], p. 92)
- ‘The central limit theorem is the reason why the Gaussian distribution is the limit of the binomial distribution.’ ([Keshav 2012], p. 40)
- ‘Complex analysis explains why the Taylor series for the function $f(x) = 1/\left(1 + x^2\right)$ converges in the finite domain $-1 < x < 1$ even though $f(x)$ is smooth for all real x.’ ([Bender et al. 2009], p. 1)
- ‘An arithmetic quotient of a bounded symmetric domain has several compactifications, that is, Satake-Baily-Borel’s, Mumford’s toroidal and Looijenga’s compactifications. A general theory explaining the relations between these types of compactifications and those of a geometric nature [. . .] has been developed by Looijenga.’ ([Artebani & Kondo 2011], p. 1445)
- ‘Let F be the field of rational numbers or the Galois field $\mathrm{GF}(p)$, where p is a prime. Let A be the underlying additive group of F. Pick an abelian group X [. . .] [Then] $\mathrm{Hom}(A, X)$ is A-cellular. Also, the following diagram explains why the “evaluation at one” map is an A-equivalence,

$$\begin{tikzcd} & X \\ A \arrow[ur, "\forall\tau"] \arrow[r, "\check{\tau}:\alpha\to\{1\to\alpha\tau\}"'] & \mathrm{Hom}(A, X) \arrow[u, "ev"'] \end{tikzcd}$$

  where $1 \to \alpha\tau$ is the unique homomorphism taking 1 to $\alpha\tau$.’ ([Farjoun et al. 2007], p. 66)

In these and many similar cases, we’re confronted with mathematical explanations[5] in which proofs seem to play no obvious role. I think this impression is basically right. Hence I think mathematical practice lends no support to proof chauvinism. The chauvinist, however, has a number of options for trying to resist this conclusion. Let me present what I take to be the most pressing concerns; answering them will help clarify just what is and isn’t shown by examples like the above.

An initial worry is whether diagrams can count as proofs, as some have argued that they can (for instance [Brown 1997]). If so, then explanatory diagrams may just be explanatory proofs, so that these cases aren’t counterexamples to chauvinism after all.

It may well be true that some diagrams are proofs. But the claim that *every* explanatory diagram is a proof is much stronger, and there’s no obvious reason to accept it. For instance, it’s doubtful that the commutative diagram above, taken by itself, proves anything about the map ev. (Compare the way in which, e.g., an appropriate dot diagram might be thought to prove that the sum of the first n odd numbers is n^2.) At any rate, even if it’s true that all explanatory diagrams are proofs, this doesn’t put any pressure on the other types of example.

Similarly, it’s unclear whether claims about explanatory theories might not ultimately depend on facts about explanatory proofs. At the very least, it’s hard to deny that true claims about explanation-by-theories are sometimes grounded in facts about explanation-by-components-of-theories. In the empirical sciences, these components are sometimes facts or laws falling under the purview of the theory

[5] Or apparent mathematical explanations, anyway. I don’t mean to take a stand on whether each of the above explanatory claims is actually correct.

in question. Hence we might think that biological theory explains why the fox population increased at time t in virtue of the fact that the Lotka-Volterra equations explain why the fox population increased at t. (Compare van Fraassen's proposal that the claim 'theory T explains fact F' is equivalent to the claim 'there are facts which explain F relative to T'; [van Fraassen 1980], p. 101.) In the mathematical setting, couldn't the explanatory components of T just as well be proofs?

I don't deny that some claims about explanation-by-theories in mathematics are true in virtue of facts about explanation-by-proofs. But why think this is the typical case? Assuming that, say, theorems are often explanatory in their own right, we should expect many explanations-by-theory to rest instead on explanations-by-constituent-theorems. And perhaps there are cases in which theories are 'irreducibly' explanatory—that is, explanatory in a way that isn't grounded in the explanatoriness of any of their components.

Maybe so. Still, I admit that it's usually hard to tell exactly what's behind a given claim of the form 'theory T explains why p'. In the above cases, and in most others, there isn't any straightforward way to determine whether T is supposed to be explanatory in virtue of containing an explanatory proof, or an explanatory theorem, or an explanatory something-else, or in its own right.

Since the cases of diagrams and theories involve the complications just discussed, and since they're probably less important anyway, I'll mostly set them aside in what follows. My focus for the rest of the paper will largely be on the explanatoriness of *theorems* and other mathematical facts. (But I continue to maintain that explanatory diagrams and theories can't be generally assimilated to explanatory proofs.)

Another objection: why assume that claims like the above should be taken literally? Mathematicians' talk about explanations and reasons in such contexts, one might think, could just as well be understood as innocent *façons de parler* with no deep philosophical meaning. For instance, claims like 'theorem T explains why p' may just be stylized ways of expressing the thought that T implies p.

As before, there's no point in denying that some authors may sometimes use explanatory language in this way. But neither is there any reason to think this is always or usually the case. More to the point: if this suggestion is supposed to help the proof chauvinist, then she'd have to maintain that mathematicians generally speak literally when they refer to explanatory proofs, but non-literally when they make other sorts of explanatory claims. This idea seems quite ad hoc, and I can't see how anyone would try to defend it.

I've saved the next concern for last because it's weightier than the others, and the response it requires is relatively involved. The worry is this: given only a list of decontextualized claims like those above, it's difficult to tell whether proofs are in fact making some important explanatory contribution which the quoted text doesn't make obvious. For instance, when mathematicians say things like 'theorem T explains why p', this might be a loose or elliptical way of pointing to the explanatoriness of some proof of p from T—one that's offered or indicated elsewhere in the text, perhaps.

Certainly this is possible, and perhaps it's the right way to understand some cases. But typically there's no evidence that anything like this is going on. If the explanations in the above cases involved proofs in some significant way, then one would expect the explanatory features of those proofs to be highlighted at some point during the proof presentations or in the surrounding discussions. Otherwise it's hard to see what the point of the explanatory claims would be: if the intention is to communicate something about a proof, why say something about a theorem instead, and then fail to draw the reader's attention to the features of the proof that one had in mind? This would be a strange and unhelpful expository choice, to say the least.

Thus, if nothing about any proof is flagged as explanatory anywhere in the relevant text, I take this to be good evidence that no claim about explanatory proofs is intended. And this is in fact the situation in cases like the ones I've offered. There's just no suggestion in these cases that an explanatory proof is somehow lying behind an assertion about an explanatory proposition, or diagram, or theory. Indeed, in some cases a proof isn't so much as gestured at. This suggests that the examples in question are just what they appear to be: instances of mathematical explanation that don't involve explanatory proofs.

But perhaps this is too quick. Marc Lange has recently proposed a way of thinking about this issue that's congenial to chauvinism, but which doesn't seem to require that an explanatory proof be flagged in the way I've suggested. Lange's view isn't crudely chauvinist in the sense that it denies explanatory power to anything other than proofs. In fact it deserves credit for being, as far as I know, the only account of mathematical explanation which acknowledges that some theorems are genuinely explanatory.

Regarding d'Alembert's theorem on roots of polynomials, for instance, Lange writes:

> Why is it that in all of the cases I have examined of polynomials with real coefficients, their nonreal roots all fall into complex-conjugate pairs? Is it a coincidence, or are they all like that? [. . .] In fact, as I have shown, d'Alembert's theorem is the explanation; any polynomial with exclusively real coefficients has all of its nonreal roots coming in complex-conjugate pairs. Here we have a mathematical explanation that consists not of a proof, but merely of a theorem. ([Lange 2016], pp. 241-2)

So far I'm happy to agree. As it turns out, however, Lange's theory about the nature of explanation-by-theorems is still chauvinist at heart. For Lange, '[a] theorem can explain [one of its instances] only if the theorem is no coincidence and hence only if it has a certain kind of proof' ([Lange 2016], p. 345). The kind of proof in question is what Lange calls a 'common proof' of all the theorem's cases—that is, a proof that exploits some respect in which the cases are alike, and which 'proceed[s] from there to arrive at the result by treating all of the (classes of) cases in exactly the same way' ([Lange 2016], p. 287).[6]

Such proofs, Lange thinks, are necessarily explanatory, at least in the appropriate context.[7] So even though he recognizes that some theorems genuinely explain, his proposal implies that the explanatory power of a theorem derives from the explanatory power of a proof. (If this is the case, why are the relevant features of the relevant proofs not always explicitly pointed out? Perhaps Lange would say that his theory is tacitly understood by mathematicians, and assumed to be understood by their readers. So when someone writes 'theorem T explains why p', it's supposed to go without saying that this means 'T has a "common proof" that exploits some respect in which its instances, including p, are all alike'.)

There are a few things to say in response to Lange's view. First of all, his account only applies to theorems which explain their instances by subsuming them under a more general fact. But not all explanatory theorems work this way.[8] For instance, in the third example given above, the fact that the roots of $x^m - 1$ don't have all the same algebraic properties over $\mathbb{Q}$ isn't an instance of the fact that $x^m - 1$ is reducible over $\mathbb{Q}$. And yet the latter fact is supposed to explain the former. So even if Lange is right, his account only applies to a limited class of explanations-by-theorems, and there's no obvious way to generalize the proposal to cover other cases. So the strategy leads only to a partial vindication of chauvinism at best.

Second, it's doubtful whether Lange's view is in fact correct. Consider that mathematicians also make claims about what a theorem would explain if it were true, even though its truth value is actually unknown, and hence even though no proof yet exists. For instance, in his discussion of the current state of evidence for $P \neq NP$, William Gasarch writes:

> If a theory explains much data, then perhaps the theory is true. ...Are there a set of random facts that $P \neq NP$ would help explain? Yes. [. . .] [For example,] Chvatal in 1979 showed that there is an algorithm for Set Cover that returns a cover of size $(\ln n) \times OPT$ where OPT is the best one could do. Moshkovitz in 2011 proved that, assuming $P \neq NP$, this approximation cannot be improved. Why can't we do better than $\ln n$? Perhaps because

[6]See [Lange 2010], or Chapter 8 of [Lange 2016], for more on Lange's notions of mathematical coincidences and common proofs.

[7]The type of context in question is one in which we find it noteworthy, or salient, that the theorem identifies some property that all its instances have in common.

[8]Relatedly, an anonymous referee suggests that it may be useful to distinguish between explanations of specific facts and explanations of general theorems for the purposes of evaluating the plausibility of proof chauvinism. One might think, for instance, that we can often explain a specific fact just by citing a general theorem under which it falls. But when it comes to explaining a general theorem itself, our only recourse is usually to point to some explanatory features of its proof. Hence chauvinism might be false as a thesis about explanations of specific mathematical facts, but at least mostly true as a thesis about explanations of general results.

Maybe something in the neighborhood of this view is right. But it's impossible to say unless we've got some means to tell a specific fact from a general theorem, and I don't know that there's any good way to do this. For, of course, what seems to be a general result in one context often looks like a narrow special case from another viewpoint. Consider, for instance, the fact that all circles in the plane intersect the x-axis at most twice. Is it specific or general? On the one hand, it's about *all* circles, not any one in particular. That seems to indicate generality. On the other hand, the result about circles can be viewed as a very special case of the Fundamental Theorem of Algebra. (And for that matter, the FTA is a particular case of Bézout's theorem, which in turn 'has many strengthenings, generalisations, and variants' ([Tao 2012]).) This sort of case seems very ordinary. So I seriously doubt that enough sense can be made of the specific/general distinction for it to do useful work in clarifying the scope of proof chauvinism.

$P \neq NP$. If this was the only example it would not be compelling. But there are many such pairs where assuming $P \neq NP$ would explain why we have approached these limits. ([Gasarch 2014], p. 258)

This sort of case makes it hard to see how chauvinism, or Lange's account in particular, could be right. We have no proof of $P \neq NP$. Nor does anyone have much idea what a proof of $P \neq NP$ might look like. And yet it seems perfectly natural to say that $P \neq NP$ would explain all sorts of results in computability theory if it turned out to be true.

Of course, this particular example doesn't involve a case of explanation-by-subsumption, but similar remarks apply to cases that do. Consider for instance the famous Hadwiger conjecture in graph theory, which says that, for all $t \geq 0$, a graph is t-colourable if it has no K_{t+1} minor.[9] The conjecture is equivalent to the four-colour theorem when $t = 4$, but the general case is considered extremely difficult, and again nobody has much idea what to expect from a proof. Nevertheless, many mathematicians feel that the Hadwiger conjecture, if true, would explain the four-colour theorem. As Paul Seymour writes:

> The four-colour conjecture (or theorem as it became in 1976) [. . .] was the central open problem in graph theory for a hundred years; and its proof is still not satisfying, requiring as it does the extensive use of a computer. (Let us call it the 4CT.) We would very much like to know the "real" reason the 4CT is true; what exactly is it about planarity that implies that four colours suffice? [. . .] By the Kuratowski-Wagner theorem, planar graphs are precisely the graphs that do not contain K_5 or $K_{3,3}$ as a minor; so the 4CT says that every graph with no K_5 or $K_{3,3}$ minor is 4-colourable. If we are searching for the "real" reason for the four-colour theorem, then it is natural to exclude K_5 here, because it is not four-colourable; but why are we excluding $K_{3,3}$? What if we just exclude K_5, are all graphs with no K_5 minor four-colourable? And does the analogous statement hold if we change K_5 to K_{t+1} and four-colouring to t-colouring? That conjecture was posed by Hadwiger in 1943 and is still open. ([Seymour 2016], pp. 417-8.)

Seymour's discussion strongly suggests that, if the Hadwiger conjecture turned out to be true, then it would explain (i.e. give 'the real reason' for) the four-colour theorem. But neither Seymour nor anyone else is in a position to guess whether the conjecture has a 'common proof' in Lange's sense. And even if there turned out to be no such proof, there's no reason to think this would change anybody's assessment of the situation.

So it seems that the question whether a Langean common proof exists is irrelevant to the question whether the conjecture would be explanatory. Moreover, this is precisely the type of case that Lange's account is designed to handle, since the four-colour theorem is a particular case of the Hadwiger conjecture. Hence Lange's account appears not to work even for cases of explanation by subsumption under a theorem.

I conclude that the verdict reached earlier was the right one. Many theorems are explanatory in a way that doesn't involve or depend on their having explanatory proofs.

So the first strategy for defending proof chauvinism seems to fail. Mathematical practice offers no support for the claim that explanatory proofs are at the centre of most instances of mathematical explanation. On the contrary, mathematicians regularly identify particular facts (as well as things like theories and diagrams) as *explanantia*, and they do so in ways that leave proofs entirely out of the picture.

[9]Definitions: A graph G is t-colourable if, given a collection of t distinct colours, G's vertices can be assigned colours in such a way that adjacent vertices never share the same colour. A graph H is a minor of a graph G if H can be obtained from G by deleting edges and vertices and by merging adjacent vertices. The notation K_t denotes the complete graph on t vertices (i.e. the graph where every vertex is adjacent to all of the remaining $t-1$ vertices) $K_{m,n}$ denotes the complete bipartite graph on m and n vertices (i.e. the graph consisting of two vertex sets of sizes m and n, where each of the m vertices is adjacent to all and only the n vertices, and vice versa).

1.2 Proof chauvinism and philosophy

1.2.1 An argument from philosophy of science

I turn now to the second possible strategy for defending chauvinism. According to this line of thought, there are compelling philosophical reasons to think that proofs must be central to mathematical explanation—reasons that don't directly involve mathematical explanation-seeking practices, but which hinge on general considerations about the nature of explanation, the role of proofs in mathematics, and the like. Here I'll focus on two fairly natural ways that such an argument might go.

The first argument appeals to the history of philosophy of science. As everyone knows, the most important early works on scientific explanation in the modern tradition were [Hempel & Oppenheim 1948] and its sequels, which defended a view according to which explanations are arguments. (More specifically—according to the 'deductive-nomological' version of the theory, which is the version that's relevant here—an explanation is a deductive argument whose premises are antecedent conditions and general laws, and whose conclusion is the fact to be explained.) If one wanted to adapt this sort of view to the mathematical setting, the natural thing would be to identify explanations with proofs. And why not give this a shot? The Hempel-Oppenheim theory was enormously popular in philosophy of science for decades, after all, and in some ways it seems even more promising as an account of mathematical explanation.

I'll consider the merits of this idea shortly. But first, why do I say that the explanations-as-arguments view seems better off in the mathematical setting? Well, recall the main objections that eventually led many philosophers of science to abandon the view. Salmon's philosophical history *Four Decades of Scientific Explanation* ([Salmon 1989]) describes three such problems. The first is that irrelevant information is 'harmless to arguments but fatal to explanations' (p. 102)—hence cases like that of 'John Jones' birth control pills' are fine as deductive inferences, but unacceptable as pieces of explanatory reasoning. The second problem is that the explanations-as-arguments paradigm seems unable to account for explanations of low-probability events. The third problem is that legitimate explanations exhibit 'temporal asymmetry'—meaning that the events or conditions appearing in the *explanans* always occur before those appearing in the *explanandum*—whereas arguments are subject to no such constraint. Salmon's preferred solution to these problems, which has since become thoroughly mainstream, is to view scientific explanation as a matter of identifying causes rather than simply giving deductively valid arguments.

In the mathematical context, however, things look quite different. Salmon's second and third problems are no trouble at all for the view that mathematical explanations are arguments (that is, proofs), since the relevant issues about probability and temporality simply don't arise. The first problem is a little more complicated, but perhaps one could say that arguments containing irrelevant information don't qualify as fully satisfactory proofs in the first place. (A good proof, after all, is a piece of reasoning that actually is or would be accepted by the mathematical community, and mathematicians aren't fond of arguments packed with irrelevancies.) In any case, even if we had doubts about the adequacy of the explanations-as-arguments model in mathematics, there's no obvious move available corresponding to Salmon's turn to causation. One could perhaps try to replace causal relations with some species of metaphysical grounding or dependence, but it's not at all clear how to do this in a plausible way. ([Pincock 2015] explores a view of this sort, but I think the proposal can be shown not to work.)

Prima facie, then, the Hempel-Oppenheim approach may look pretty promising as an account of mathematical explanation. And since it identifies explanations with arguments, such an approach would vindicate a type of proof chauvinism. Let me now explain why we shouldn't be convinced by this line of thought.

The first thing to note is that the Hempel-Oppenheim model isn't primarily motivated by the idea that explanations are arguments of some sort. Hempel and Oppenheim didn't start with this conviction, and then proceed to investigate what sort of argument an explanation might be. The real motivation for the view is rather the 'covering law' conception of explanation. This is the thought that

> [an event] is explained by subsuming it under general laws, i.e., by showing that it occurred in accordance with those laws, by virtue of the realization of certain specified antecedent conditions. [. . .] Thus [. . .] the question "*Why* does this phenomenon happen?" is

> construed as meaning "according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?" ([Hempel & Oppenheim 1948], p. 136).

The identification of explanations with (deductive-nomological) arguments is offered, then, as a way of making the covering law picture more precise. But the latter is really the heart of the view.

Why is this point relevant to the chauvinism issue? In the first place, because the covering law conception has no plausibility whatsoever as a theory of mathematical explanation. Even if we limit our attention to proofs—and even if we assume we can make sense of 'antecedent conditions' and 'general laws' in mathematics—the covering law picture gives plainly wrong conditions for explanatoriness. Subsumption under a law isn't necessary, because many uncontroversially explanatory proofs just don't have this form. Consider for instance the proof of Desargues' theorem discussed in [Lange 2015], whose explanatoriness seems rather to lie in its 'exploit[ing] [a] feature of the given case that is similar to the remarkable feature of the theorem' (p. 438). (One could also consider the many explanatory proofs that succeed by transferring a problem to a geometric setting, or by making it otherwise visualizable.) And subsumption isn't sufficient, either. If it were, then countless proofs like the following would presumably be explanatory: 'All odd numbers are divisible by some prime number; 9 is odd; hence 9 is divisible by some prime.'[10] I take it we'd like to avoid this result.

Perhaps contrary to first appearances, then, the motivation for the Hempel-Oppenheim view fails to transfer in any meaningful way to the mathematical setting. The 'covering law' conception of explanation, which the Hempel-Oppenheim theory is supposed to formalize, is a non-starter as an account of explanatory proof. And the fault doesn't lie just with the deductive-nomological style of argument in particular. In fact, there seems to be no interesting formal structure of any kind that's common to all explanatory proofs. So nothing even in the spirit of the Hempel-Oppenheim view looks promising.

For these reasons, even if such a view was wildly successful as a theory of scientific explanation, this would provide no obvious support for the notion that mathematical explanations are proofs. But of course the Hempel-Oppenheim view is far from wildly successful. The current consensus is rather that it 'jams an alien picture of explanation onto the scientific phenomena [. . .] fails to fit swathes of explanations in the sciences, and has been consequently widely rejected in the philosophy of science as the truth about scientific explanation' ([Gillett 2016], p. 48). Indeed, it's worth remarking that—along with the objections due to Salmon described above—a longstanding complaint is that the deductive-nomological picture wrongly precludes singular facts from serving as *explanantia*. As Michael Scriven wrote more than fifty years ago:

> If you reach for a cigarette and in doing so knock over an ink bottle which then spills onto the floor, you are in an excellent position to explain to your wife [. . .] why the carpet is stained (if you cannot clean it off fast enough). You knocked the ink bottle over. This is the explanation of the state of affairs in question, and there is no nonsense about it being in doubt because you cannot quote the laws that are involved [. . .] ([Scriven 1962], p. 68)

I think this is right, and I think the same point is well taken in the mathematical setting. Not all legitimate explanations, whether mathematical or scientific, involve the deduction of a special case from a general law. In light of these issues, it's very hard to see how the Hempelian paradigm could provide the proof chauvinist with any encouragement.

[10] I don't claim to know what a 'general law' of mathematics is, or what it would be if there were such things. One plausible idea is something like this: a general law is a true universal statement in which only sufficiently natural mathematical predicates appear. Since oddness and divisibility by a prime seem quite natural, the example I give would seem to involve a general law by this criterion.

Another approach, suggested by an anonymous referee, is to identify general laws with axioms. This is an appealing idea, since axioms, like laws, are a special class of (typically general) truths that play a distinguished foundational and organizational role in the theories to which they belong. But this approach has problems too. One issue, noted by the referee, is that *all* proofs might be thought to rest ultimately on axioms, and hence to have the form of covering-law arguments. If this were right, then a covering-law theory of mathematical explanation would (wrongly) predict that all proofs are explanatory. I'm not so sure myself that all proofs should be viewed as proofs from axioms, but I do doubt that there's any good principled way to distinguish the axiom-based proofs from the rest. Another obvious worry is that the distinction between axioms and non-axioms is relative to a choice of axiomatization. Since lots of mathematical theories can be axiomatized in more than one way, whether or not a given proof is explanatory would often turn out to depend on which statements we happened to be taking as axioms. This seems quite counterintuitive.

1.2.2 An argument from philosophy of mathematics

I now turn to the second philosophical argument for chauvinism. The argument comes from philosophy of mathematics, and it appeals to the supposed epistemological primacy of proofs (as compared to axioms, theorems and the like). This type of proof-centred viewpoint is defended with vigor by [Rav 1999]. Rav writes, for instance, that

> [*P*]*roofs rather than the statement-form of theorems are the bearers of mathematical knowledge*. Theorems are in a sense just tags, labels for proofs, summaries of information, headlines of news, editorial devices. The whole arsenal of mathematical methodologies, concepts, strategies and techniques for solving problems, the establishment of interconnections between theories, the systematisation of results—the entire mathematical know-how is embedded in proofs. (p. 20; emphasis in original)

On this sort of picture, chauvinism looks almost inevitable, since a mere theorem is presumably too slender a reed to bear the weight of being explanatory. Only proofs seem epistemologically robust enough to handle the job.

In defense of his view, Rav points out that some theorems (or conjectures) have generated proofs (or proof-attempts) which are much more interesting, useful and deep than the original statements themselves, such as Fermat's last theorem and the Goldbach conjecture. Similarly, there are statements like the Continuum Hypothesis that turned out to be independent of the relevant axioms (and hence neither provable nor disprovable), but whose would-be proofs led to important kinds of progress.

True enough, but such examples hardly vindicate Rav's view. All that these cases show by themselves is that *some* theorems are less epistemologically valuable than their proofs. It's quite another thing to claim that theorems in general occupy a lower rung on the epistemological ladder than do proofs in general. And indeed there's plenty of reason to deny this. Mathematicians routinely identify theorems as profound, beautiful, powerful, deep, essential, and the like. High praise for mere tags! And just as some run-of-the-mill theorems have interesting proofs, the reverse is also true. The Fundamental Theorem of Arithmetic, for instance, is a result worthy of its name: the unique prime factorization property of the natural numbers is a highly nontrivial feature[11] that provides a foundation for much of what goes on in number theory. But the proof of FTA doesn't seem noteworthy in any way. At least, it isn't especially deep or revelatory, and its development didn't lead to the invention of any groundbreaking new machinery: FTA could have been (and practically was) proved by Euclid, using just the modest number-theoretic resources at his disposal. (Euclid's lemma, which is *Elements* VII.30, is the key ingredient of the proof.) Plenty of other results fit the same pattern. One could mention, for instance, Ramsey's theorem in combinatorics, the intermediate value theorem in analysis, the binomial theorem in algebra, Brouwer's fixed-point theorem in topology, and the five lemma in category theory as results which are more important and interesting than their proofs. (See also the discussion of Morley's triangle theorem in [Lange 2016], pp. 252-3; this result has been called 'spectacular', 'amazing', and 'the marvel of marvels', but its known proofs are all notably unenlightening.)

So I don't think we can get to proof chauvinism by way of a Rav-style view about the epistemological primacy of proofs. Rav is right to insist on proof's distinctive value, and to remind us that mathematics wouldn't get very far without the creative stimulus provided by the search for new problem-solving methods. But it doesn't follow that proofs are the only things worth knowing. Far from being mere tags or labels, many theorems also have great epistemological significance in their own right. Indeed, it's puzzling why anyone would go to the trouble of devising proofs in the first place if the statements to be proved had no intrinsic intellectual worth. This thought is corroborated by the section on the goals of mathematical research in the recent *Princeton Companion to Mathematics*:

> The object of a typical paper is to establish *mathematical statements*. Sometimes this is an end in itself: for example, the justification for the paper may be that it proves a conjecture that has been open for twenty years. Sometimes the mathematical statements are established in the service of a wider aim, such as helping to explain a mathematical phenomenon that is poorly understood. But either way, mathematical statements are the main currency of mathematics. ([Gowers et al. 2008], p. 73; emphasis in original)

[11] For an interesting discussion of the non-triviality and non-obviousness of FTA, see ([Gowers 2011]).

Indeed. Theorems are epistemologically valuable; more specifically, they have explanatory value, as I've been arguing.

2 An Example: Galois Theory and Explanatory Proof

I've now finished making the case that proof chauvinism is false. But it's not yet clear that its falsity should be any great concern. It would be useful to know how, if at all, chauvinist assumptions have actually done harm in the theory of mathematical explanation, and how they might do more in the future.

In order to get clearer about the perils of chauvinism, then, let me now consider a particular case in somewhat more detail. One of the standard examples in the literature has long been Évariste Galois's explanation of the solvability of polynomial equations. The case deserves the attention it gets; Galois's work was a major explanatory achievement, and a watershed moment in the history of mathematics. I'll argue, however, that explanatory proof plays no role in this case. What's more, the prejudice in favor of identifying mathematical explanation with explanatory proof has led to some confusions about Galois's work.

First, a brief review of what Galois theory accomplished. The oldest and most famous problem in the theory of equations involves finding the set of solutions to a given equation 'in radicals', that is, as a function of the equation's coefficients involving only basic arithmetical operations and nth roots. (The quadratic equation $x = \frac{-b \pm \sqrt{b^2-4ac}}{2a}$ is a familiar example of such a solution formula.) Solving a linear equation $ax + b = 0$ in radicals is easy; the single root is given by $x = -\frac{b}{a}$. The quadratic formula, or something essentially equivalent, was known already in antiquity. But the cubic and quartic cases are much harder. General solution formulas for these equations were found only in the sixteenth century, by a laborious trial-and-error process involving complicated substitutions.

Following these discoveries, mathematicians sought to understand whether and why an arbitrary polynomial (of any degree) is solvable in radicals. After centuries of limited progress, Niels Henrik Abel proved the unsolvability of the quintic in 1824, and Galois put the general problem to rest shortly thereafter. The approach he developed, now known as Galois theory, involves the study of a certain algebraic object associated with a given polynomial (namely its 'Galois group', a group of permutations of the roots). Galois's major breakthrough was the discovery that, if the Galois group of a polynomial has a certain property (also known as 'solvability'[12]), then the polynomial is solvable in radicals; if not, then no solution formula exists. This result is 'Galois's criterion for solvability'.

To obtain a complete picture of which equations are solvable and which aren't, Galois combined this criterion with two other results. The first is that the Galois group of an nth-degree polynomial is a subgroup of the 'symmetric group' S_n, i.e. the group of all permutations of an n-element set. (Moreover, S_n itself is the Galois group of the 'general' polynomial of degree n.) The second fact is that S_n and all its subgroups are solvable for $n \leq 4$, but not for $n \geq 5$. Together, these facts imply that an arbitrary equation of degree ≤ 4 is always solvable in radicals, but not so for equations of higher degrees.

Many mathematicians and philosophers view Galois's accomplishment as an important case of mathematical explanation. And so it is. Unfortunately, the chauvinist preoccupation with explanatory proof has led to some mistakes in the philosophical understanding of the case. For instance, [Pincock 2015] presents a theory of what he calls 'abstract mathematical explanation' motivated largely by Galois theory. I won't go into the details of Pincock's account here. For now I only want to note that, although little is said to justify either of these assumptions, Pincock takes it for granted that the case involves an explanatory proof, and he fixes on Galois's proof of the unsolvability of the quintic as the relevant item. (Pincock then suggests that this proof is a paradigmatic example of an abstract mathematical explanation, that is, a proof that explains 'by appeal to an entity that is more abstract than the subject-matter or topic of the theorem' ([Pincock 2015], p. 2).)

I think this is a mistake. Galois theory isn't explanatorily successful by virtue of containing an explanatory proof. Instead, I claim that the *explanantia* in this case are theorems, notably Galois's criterion itself and his results about the solvability of symmetric groups.

[12]A group G is solvable if it has subgroups $\{1\} = G_0 < G_1 < \cdots < G_k = G$ such that G_{j-1} is normal in G_j, and G_j/G_{j-1} is a commutative group, for $j = 1, \ldots, k$.

There are a couple of reasons to think so. For one, my proposal reflects how mathematicians actually talk about the case—they simply don't ever, as far as I can tell, mention any proof (or any aspect of any proof) in their discussions of Galois's explanatory achievements. Instead, they identify Galois's theorems, or sometimes Galois theory as a whole, as the bearers of explanatory value. The following sorts of remarks are typical:

- '[Galois] showed that the group of the general nth degree equations is the symmetric group S_n and that solvability of an equation by radicals is reflected in a group-theoretic "solvability" property, possessed by S_3 and S_4 but *not* by S_5. This explains why the general cubic and quartic are solvable by radicals and the general quintic is not' ([Stillwell 1995], p. 58).

- 'The group of the general equation of the nth degree is the symmetric group $[S_n]$ [. . .] whose factors of composition... [for $n = 3$] are 2, 3; for $n = 4$ they are 2, 3, 2, 2... all of them prime; this is the reason why the general equations of the third and fourth degree are solvable by radicals' ([Bolza 1891], pp. 138-139).[13]

- 'In Sect. 2.1, we show that if the group G is solvable, then the elements of the field P are representable by radicals through the elements of the invariant subfield K of G [. . .] When P is the field of rational functions of n variables, G is the symmetric group acting by permutations of the variables, and K is the subfield of symmetric functions of n variables, this result provides an explanation for the fact that algebraic equations of degrees 2, 3 and 4 in one variable are solvable by radicals' ([Khovanskii 2014], p. 48).

- 'An algebraic equation of the form $[a_n x^n + \cdots + a_1 x + a_0 = 0]$ is solvable by radicals if and only if its Galois group is solvable [. . .] In view of [this theorem], the nonsolvability by radicals of the general polynomial equation of degree ≥ 5 is then explained by the following result [. . .] *The symmetric group S_n is solvable if and only if $n < 5$*' ([Pantea et al. 2014], p. 425; emphasis in original)

- 'The quadratic formula for polynomials of second degree [. . .] holds that an equation of the form $ax^2 + bx + c = 0$ has two solutions [. . .] Galois theory explains why there is no comparable formula for equations of degree 5 and higher' ([Nadis & Yau 2013], p. 170).

So the mathematical literature provides no support for a proof-chauvinist interpretation of the Galois theory case. But it provides ample support for the idea that Galois's theorems are explanatory. (Pincock claims that there's 'considerable evidence that practitioners consider [Galois's unsolvability proof] to be more explanatory than its predecessors' ([Pincock 2015], p. 1), but none of the sources he cites mentions anything about the proof; all of them talk only about Galois's ideas or Galois theory in general.)

In any case, the suggestion that a proof was Galois's main explanatory contribution is problematic by itself. For one, as Leo Corry observes, 'Galois's writings were highly obscure and difficult, and his proofs contained many gaps that needed to be filled' ([Corry 2004], p. 26). Presumably, an argument as problematic and incomplete as the one Galois actually gave is a poor candidate for an explanatory proof. Presumably, also, this is why Pincock presents a modern field-theoretic version of the proof of Galois's criterion, even though the reasoning is substantially different from what Galois himself could have had in mind.

But this line of thought leads in some troubling directions. If we accept chauvinism, and if we also admit that Galois's original proof was unexplanatory, then we apparently have to say that Galois's work contained little or nothing of explanatory value. This is an unappealing view, and I doubt that Pincock would want to accept it. On the other hand, if we're willing to bite the bullet and deny that Galois explained anything, it follows that a genuinely explanatory proof of Galois's criterion appeared at some point after the 1830s and before the present day. Which version of the proof was this, exactly? (Or, if we think of explanatoriness as a matter of degree, what was the first reasonably explanatory version of the proof?) Although perhaps not unanswerable, these questions are at least uncomfortable, and Pincock says little to suggest how he might either respond to or avoid them.

[13] The condition that a group G is solvable is equivalent to the condition that G's composition factors all have prime order. Earlier presentations of Galois theory, including e.g. Jordan's influential *Traité des Substitutions*, tended to use the composition factor condition.

By contrast, the idea that Galois's achievement was to prove a number of explanatory theorems suffers from none of these difficulties. For one, as we've seen, this proposal is squarely in line with what mathematicians actually say about the case. Those who identify specific *explanantia* almost invariably point to Galois's results, rather than his proofs—in particular, they identify Galois's criterion, and the theorems about the solvability of symmetric groups, as the bearers of explanatory value. Additionally, the view vindicates the consensus opinion that Galois achieved something of explanatory significance. After all, Galois was certainly the first to conceive of and state the theorems in question, but it's much less clear that he discovered adequate (let alone explanatory) proofs for these theorems.

Let me conclude by considering another suggestion in the chauvinist spirit, due to Lange. Like Pincock, Lange thinks that Galois theory explains by virtue of containing an explanatory proof. He writes:

> I suggest that the distinction between an explanation and a mere proof of the quintic's unsolvability is grounded in this result's most striking feature: that in being unsolvable, the quintic differs from all lower-order polynomial equations (the quartic, cubic, quadratic, and linear), every one of which is solvable. In the context of this salient difference, we can ask why the [quintic] is unsolvable. Accordingly, an explanation is a proof that traces the quintic's unsolvability to some other respect in which the quintic differs from lower-order polynomials. Therefore, Galois's proof (unlike Abel's, for example) explains the result. ([Lange 2016], p. 440)

The main novel claim here is that Galois's proof of the unsolvability of the quintic, though not Abel's, works by identifying and exploiting some difference between fifth-degree and lower-degree equations. Hence only Galois's proof is explanatory. This is, I think, mistaken. I can't discuss Abel's proof in detail here—for that, see [Pesic 2013]—but the general idea is as follows. First Abel shows that any solution formula for an equation of degree m must have the form

$$x = p + R^{\frac{1}{m}} + p_2 R^{\frac{2}{m}} + \cdots + p_{m-1} R^{\frac{m-1}{m}},$$

where '$p, p_2, \ldots$ are finite sums of radicals and polynomials and $R^{\frac{1}{m}}$ is in general an irrational function of the coefficients of the original equation' ([Pesic 2013], p. 90). (The already-known formulas for lower-degree equations can easily be seen to have this form.) Hence, if there were a solution formula for the general quintic, it would look like

$$x = p + R^{\frac{1}{5}} + p_2 R^{\frac{2}{5}} + p_3 R^{\frac{3}{5}} + p_4 R^{\frac{4}{5}}.$$

The rest of the proof shows that this can't occur. Abel's main tool here is a theorem of Cauchy, which implies that $R^{\frac{1}{m}}$ can have only two or five distinct values when the roots of the quintic are permuted. Abel shows that each of these possibilities leads to a contradiction; it follows that the general quintic is unsolvable.

Pace Lange, this argument strikes me as 'a proof that traces the quintic's unsolvability to some other respect in which the quintic differs from lower-order polynomials'. The respect in question is the satisfiability of the equation $x = p + R^{\frac{1}{m}} + p_2 R^{\frac{2}{m}} + \cdots + p_{m-1} R^{\frac{m-1}{m}}$ for $m < 5$, as compared to its satisfiability for $m \geq 5$. By Lange's criterion, then, it's unclear why Galois's proof should count as any more explanatory than Abel's.

3 Conclusion

The Galois theory case is interesting and important in its own right, and I think the above observations would be worth making even if nothing further was at stake. But in fact proof chauvinism has some more general philosophical risks. Let me close by mentioning a couple of them.

First, theorizing about mathematical explanation under the influence of chauvinism is likely to lead to a distorted picture of the phenomena. If one is bent on finding explanatory proofs, and there are no such proofs on offer in a particular case, then one is liable to conclude that the case contains nothing of explanatory interest. But if chauvinism is false and many mathematical explanations don't involve explanatory proofs, this approach will yield frequent false negatives. This is especially worrisome in a field in which progress still largely depends on assembling a representative stock of examples. As Mancosu suggests, the theory of mathematical explanation will advance only when we've managed to 'carefully analyze more case studies in order to get a better grasp of the varieties of mathematical explanations' ([Mancosu 2015]).

Second, proof chauvinism threatens to draw attention away from the broader epistemological contributions of other elements of mathematics. Explanation, after all, is closely linked to understanding, theory change, conceptual development, various cognitive benefits, and other phenomena of epistemological interest. So if one is convinced that proofs alone are explanatory, then one is a short step away from concluding (mistakenly) that only proofs play a meaningful role in these other phenomena.

The theory of mathematical explanation is still in its infancy. But infancy is a crucial developmental phase, and an early mishap can lead to more serious problems later on. The philosophy of mathematics would do well, then, to inoculate itself against an unhealthy fixation on explanatory proofs.

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