# Is Mathematics Unreasonably Effective?

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**Abstract**

Is mathematics unreasonably effective when applied to the natural sciences? Many mathematicians, physicists, and philosophers have argued that it is. I evaluate an argument for this conclusion by Mark Steiner, according to which (i) mathematics is good only insofar as it possesses certain aesthetic virtues, and (ii) the fact that such mathematics is of great use in applications results in a puzzling and problematic anthropocentrism. I respond by arguing that the aesthetic virtues used to evaluate and motivate mathematics are far less straightforwardly anthropocentric than one might presume, and (with reference to case-studies within Galois theory and probabilistic number theory) I show that these virtues are themselves epistemically relevant or at least track generally recognized theoretical virtues, such as explanatory and unifying power, fruitfulness, and importance.

## 1. Introduction

It is a striking fact about contemporary mathematics that it is so frequently and so successfully applied to the natural sciences. One kind of philosophical question that arises concerns the implications for large-scale philosophical theorizing about (pure) mathematics. Many philosophers – most notably, in the tradition inaugurated by the so-called indispensibility arguments due to Quine and Putnam – have taken the applicability of mathematics to be of considerable significance in resolving, among other things, debates between platonists and nominalists about the existence of mathematical objects.[1] But the applicability of mathematics raises a different kind of question, one that arises less from an interest in pre-existing philosophical disputes and more from a sense that its success is puzzling in its own right. The fact that mathematics – a discipline carried out more or less entirely in the armchair and with an apparently exclusively *a priori* methodology – can be applied to the physical world can be made to seem striking, mysterious – even inexplicable. Puzzlement of this kind has been

[1] See e.g. Quine [1960], Putnam [1975], Field [1980], Colyvan [2003] for just a small sample of this literature.

expressed by many. Perhaps the canonical formulation is in Eugene Wigner's famous essay "The Unreasonable Effectiveness of Mathematics in the Natural Sciences", where he writes that:

> [T]he miracle of appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it, and hope that it will remain valid for future research, and that it will extend, for better or for worse, to our pleasure even though perhaps also to our bafflement, to wide branches of learning.[2]

Adding to the air of mystery surrounding the puzzle is a widely-shared sense that *aesthetic* judgements play a fundamental role in the development of mathematics. Wigner characterizes "[m]ost advanced mathematical concepts" as being "so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty" – and is explicit that this conception of mathematics is responsible for much of the force of the puzzle. How could it be that mathematical concepts – devised not only in the armchair, but also partly out of an aesthetic impulse – can be put to such spectacular use in empirical applications? Similar puzzlement is expressed by Steven Weinberg:

> It is very strange that mathematicians are led by their sense of mathematical beauty to develop formal structures that physicists only later find useful, even where the mathematician had no such goal in mind. [ . . . ] Physicists generally find the ability of mathematicians to anticipate the mathematics needed in the theories of physics quite uncanny. It is as if Neil Armstrong in 1969 when he first set foot on the surface of the moon had found in the lunar dust the footsteps of Jules Verne.[3]

In this paper, I hope to provide a resolution of the puzzle of applicability. The first task (§2) involves getting a more precise formulation of the puzzle into view. There I distinguish a number of issues in the philosophy of applied mathematics, and consider an interpretation of the puzzle – following Mark Steiner – that presents it as an argument to the effect that mathematical and scientific practice is problematically *anthropocentric* (in a sense to be explained). The rest of the paper contains the resolution. In §3 I discuss the popular aesthetic conception of mathematics – responsible for much of the puzzle's appeal – as a discipline whose development is significantly attributable

[2]Wigner [1960, p.14]. A similar sentiment can be found in Feynman [1967, p.171]: "I find it quite amazing that it is possible to predict what will happen by mathematics, which is simply following rules which really have nothing to do with the original thing."

[3]Weinberg [1993, p.125].

to the aesthetic judgements of mathematicians; in §4 I challenge (or at least, provide a significant supplementation to) the aesthetic conception of mathematics by arguing that there are mathematical virtues other than aesthetic virtues; and, in §5, I appeal to two case studies from mathematical practice (within Galois Theory and Probabilistic Number Theory) in order to reject the crucial inference from the aesthetic conception of mathematics to anthropocentricism. I argue that these case studies show that the applicability of mathematics, and in particular the use of aesthetic criteria in the development of mathematics, is far less mysterious than has previously been supposed. Indeed, a stronger thesis still falls out of my discussion: that correctly understanding of the use of aesthetic criteria in mathematics will itself *substantially contribute* to a satisfying resolution of the puzzle. By the end of the paper, I hope that the use of aesthetic criteria in mathematics will have been substantially demystified, the puzzle of applicability will have been resolved, and new light will have been shed on the success of mathematics in scientific and other applications. But before we get to that, we had better get clear on what the problem is supposed to be in the first place.

## 2. What is the Puzzle of Applicability?

How can Wigner's puzzle of applicability – "the problem of the unreasonable effectiveness of mathematics in the natural sciences" – be precisely formulated? As a way of clearling the ground, it will be helpful to mention a number of issues in the philosophy of applied mathematics that do *not* appear to be especially promising candidates. Some are what we might call *technical* questions; for instance:

- (1) Semantic questions. What is the logical form of statements of applied mathematics? Can we give a uniform semantics for "mixed" sentences, containing both mathematical and physical vocabulary, and if so, how?

- (2) Questions about the relationship between pure and applied mathematics. Given one's preferred overall philosophical view of pure mathematics, how is applied mathematics to be integrated into this picture? (For instance, suppose we adopt a platonist views of pure mathematics, according to which it concerns a realm of abstract, mind-independent mathematical objects. A number of pressing philosophical questions arise when applied mathematics is brought into view: e.g. what relations are there between abstract mathematical objects and concrete objects; and how do these relations sustain the sort of uses to which mathematics is put in natural science? Of course, we need not be platonists; but other views of mathematics will face analogous questions).

- (3) Questions about the applicability of *particular* mathematical tools. Why are particular mathematical concepts, theories, or structures apt for application to particular physical phenomena? (For instance, why is the mathematical operation of addition over the natural numbers apt for applications having to do with aggregation of physical objects; or why are analytic functions over the complex numbers apt for applications in fluid dynamics and relativistic field theory?)

I call these technical questions not to disparage them – they are certainly deserving of philosophical attention, and it would be desirable to have a systematic set of answers to each – but just to emphasize that they do not appear (at least on first blush) to be capable of giving rise to the puzzlement of the kind evinced by Wigner and others.[4]

A more promising candidate interpretation is presented by Mark Steiner, in an insightful discussion in his book *The Applicability of Mathematics as a Philosophical Problem*.[5] In Steiner's view, Wigner's puzzle (properly understood) does not fundamentally involve the utility of mathematics in *formulating* physical theories. Rather, it is fundamentally a problem about the way in which mathematics informs the *discovery* of such theories. It will be helpful to briefly present an example used by Steiner to make the case, taken from the development of the theory of electromagnetic radiation from Maxwell's laws.[6]

As Maxwell realized, at the laws of Coulomb, Faraday, and Ampere – laws that enjoyed a large degree of empirical confirmation – imply that the fundamental principle of the conservation electrical charge is violated. In order to restore the conservation principle for charge, Maxwell modified Ampere's law by adding a "displacement current". Unlike the orthodox understanding of current (according to which it consists of moving charge), displacement current consists of the rate of change of the electrical field. The modified version of the law then states that the curl of the magnetic field is the sum of the displacement current and the "orthodox" current:

$$\nabla \times \mathbf{B} = \frac{4\pi}{c}\mathbf{J} + \frac{1}{c}\frac{\partial \mathbf{E}}{\partial t}$$

Now, although magnetism is caused by an electric current, Maxwell asserted that his newly revised law held in *arbitrary* cases – even cases where there "orthodox" current is zero – and thereby extrapolated far beyond the empirical evidence available to him at that moment. This extrapolation produced a new prediction, namely that a changing magnetic field would interact in some circumstances with a changing electric

[4]Wigner sometimes writes as if the problem arises from the fact that mathematical language can be used to formulate physical theories; but it is not clear, on reflection, why there is any puzzle here that goes beyond the cluster of issues surrounding (3) above – issues which do not seem to me, at least, to generate any deep *puzzle*.

[5]Steiner [1998].

[6]My exposition of the case is *very* brief. For more details, see Steiner [1998, p.77] and Colyvan [2001].

field to produce a wave that would propagate through a vacuum. Thus was electromagnetic radiation predicted, which was later bourne out by experimental results of Hertz.

Steiner takes this example as a paradigmatic case of a *Pythagorean analogy*: that is, "a mathematical analogy between physical laws (or other descriptions) not paraphraseable at $t$ into non-mathematical language".[7] In particular, he argues, it is a case of a strategy where some "Equation E has been derived under assumptions A. The equation has solutions for which A are no longer valid; but *just because they are solutions of E*, one looks for them in nature."[8] A closely related notion is what Steiner calls *Formalist* reasoning. A piece of mathematical or physical reasoning is said to be Formalist if it relies on transitions sustained by the syntactic or orthographical modification of statements of pre-existing laws: in other words, reasoning that is "based on the syntax or even orthography of physical theories, rather than what (if anything) it expresses."[9] As an example of this kind of reasoning, Steiner discusses Heisenberg's strategy of predicting the energy levels of the hydrogen and heliom atoms using the – purely formal – trick of substituting matrices for the variables of the Hamiltonians of the system.

With that background, we are in a position to give a brief reconstruction Steiner's formulation of Wigner's puzzle. Firstly, an extensive catalogue of cases of Pythagorean and Formalist (P/F) reasoning employed in the (recent) history of physics is presented, drawing particularly (but not exclusively) in the early development of quantum mechanics in the hands of practitioners such as Dirac, Heisenberg, Pauli, and Schrodinger.

Although it might be possible to question the details of Steiner's reading of these historical episodes, I do not propose to do that here. To my mind, he makes a compelling case that Pythagorean and Formalist reasoning was used extensively during these periods of scientific discovery; furthermore, it is striking that the historical examples raised by Steiner as raising puzzlement are precisely those to which Wigner, Feynman, and Weinberg advert in their own treatments of the puzzle. So let us not contest the point, which we can construe as the first premise of an argument:

**(1)** Pythagorean and Formalist analogies play a crucial role in scientific discovery.

The second premise is this:

**(2)** If Pythagorean and Formalist analogies play a crucial role in scientific discovery, then scientific practice is anthropocentric.

What does this mean? Steiner calls a theory or investigative practice *anthropocentric* if it affirms or presupposes that "the human race is in some way privileged, central to the

[7] *ibid*, p.54.
[8] *ibid*, p.76, italics in original.
[9] *ibid*, p.54.

scheme of things."[10] He goes on to draw further distinctions between what we might call *theoretical* and *behavioural* anthropecentricism. A *theory* is anthropocentric if it is committed to claiming that the human race is privileged. Commitment of this kind can be either explicit (the canonical example being biblical creationism) or implicit, via unexpressed assumptions or presuppositions (the canonical example being geocentricism of a kind that ascribes the planet Earth a privileged location – at the center of the universe – for nothing more than the reason that it is the planet on which humans first arose). A related notion of anthropocentricism attaches, not to theories, but to certain kinds of *behaviour*. Say that a person's or group's behaviour is anthropocentric if that behaviour can be rationalized only against the background of an anthropocentric theory. Thus it is possible for one's actions to be covertly anthropocentric even if it is not the case that one has ever accepted, endorsed, believed, or even ever entertained the truth of any anthropocentric doctrine; all that it requires is that one's behaviour make sense only if an anthropocentric assumption is granted.

The astonishing conclusion of the argument is then:

**(3)** Scientific practice is anthropocentric.

The conclusion is astonishing, in part, because it would have, if true, immense consequences for – to put things somewhat grandly – our understanding of our place in the universe: it seem to attribute to the practice of science the view that the universe is, in Steiner's phrase, "user friendly." It is also astonishing in part because it is opposed to a claim that might plausibly go under the heading of *naturalism*, namely the view that scientific practice and our best scientific theories are *not* anthropocentric, i.e. *not* reliant (for their rationality) on anthropocentric assumptions or presuppositions. Of course, naturalism is a complex and heavily contested notion; but – if Steiner's argument is correct – on at least one perfectly reasonable understanding of the term, naturalism is undermined by scientific practice itself.

It might appear as though we have gone very far from Wigner's original puzzle; and in one sense, that is true, since Wigner's paper does not at all formulate the problem in terms of naturalism (indeed, the word "naturalism" does not appear once in his essay). But in another sense, that is not so clear. It is very plausible to take Wigner as gesturing towards an explanatory puzzle, roughly as follows: certain kinds of mathematical reasoning (what Steiner would call Pythagorean and Formalist reasoning) plays a key role in scientific discovery; but it's inexplicable why such reasoning is as strikingly effective as it is, given its source in the human aesthetic impulse. If Steiner is right, then this explanatory puzzle rests on an additional premise: the claim that scientific practice is naturalistic, that is, not anthropocentric. If – a conjecture, but I think a

[10] *ibid*, p.55.

plausible one – Wigner held this premise implicitly, and if Steiner is right about the role it plays in motivating the explanatory challenge, then there is a better case to be made that this is a faithful reconstruction of Wigner's argument than it might at first appear.

## 3. Resisting Anthropocentric Conclusions

### 3.1 Can Pythagorean/Formalist Strategies be Rationalized?

Steiner's argument, as I have presented it above, is valid. Of course, it does not yet follow that we should accept the conclusion that scientific practice is anthropocentric; the rest of the paper is an attempt to show exactly why not. But before undertaking this task, let me briefly address two related lines of response that have been pressed against Steiner in early reviews by Liston and Simons and explain why I take neither to be convincing.[11] Both authors attempt, in different ways, to reconstruct the behaviour of scientists who use Formalist and Pythagorean reasoning in such a way as to rebut the claim that doing so is thereby tantamount to anthropocentricism. Simons, firstly, notes that in certain contexts, scientists face "desparation" in attempting to solve the problems they have set themselves. In such contexts, he argues, there is no way of, so to speak, reading off the metaphysical commitments of scientists from the strategies they adopt: desperate times call for desperate measures, and if one implements desperate measures, one can hardly be held fully or even partially committed to the underlying principles that might justify their effectiveness.[12] As he puts it, scientists "had no thought to pause and consider whether their heuristic leaps were naturalistically proper and would almost certainly not care if they were not."[13] Liston, secondly, offers a different rationalization: he ascribes to scientists something like an inductive inference to the effect that P/F reasoning is successful: [the scientist] "can admit that no one knows how P/F reasoning works and argue that the very fact that similar strategies have worked well in the past is already reason enough to continue pursuing them hoping for success in the present instance. Past success and the absence of any plausible account of how or why a strategy works appear to be adequate reasons for continued employment of that strategy."[14] On this view, the past track-record of P/F reasoning is enough to justify a continued belief in its methodological viability without any (naturalistically objectionable) committment to anthropocentricism.

[11] Liston [2000], Simons [2001].

[12] We might compare "pragmatic" treatments of induction: crudely put, if reasoning inductively has a chance of being truth-conducive, and if none of the alternatives to reasoning inductively have any such chance, then it is rational to reason inductively regardless of whether one actually believes that induction is valid.

[13] Simons [2001, p.183].

[14] Liston [2000, p.200].

However, as plausible as both of these responses may be, to my mind they are too concessive from the very beginning. For both authors offer responses with the same structure: they concede (for the sake of argument?) that P/F reasoning is commonly employed in science and yet anthropocentric in nature, but they attempt to rationalize the attitudes of scientists in such a way to make it plausible that the scientists themselves are not committed to anthropocentricism. But it is hard to see how this can succeed: once it is admitted that P/F reasoning is naturalistically suspect, one has given the game away, for at best, all that can be sustained if these arguments are successful is the conclusion that anthropocentric tendencies are *justified* or *rational* when employed by scientists in the grip of desperation or who face a good inductive argument for the usefulness of those methods. No doubt, this conclusion may serve to rationalize certain scientific attitudes and practices; and may even absolve the *scientists themselves* from the charge that they have explicitly adopted anthropocentric beliefs; but they do nothing to undermine Steiner's key point, namely, that the practice of science is frequently covertly anthropocentric. This is hardly a resolution that will comfort adherents of naturalism, and hardly one that dispels the puzzle with which we began.

### 3.2 Denying Anthropocentricism

My strategy, by contrast, is going to be to reject Steiner's second premise to the effect that employing Pythagorean and Formalist reasoning in science is tantamount to embracing anthropocentricism. The first issue, then, is getting clear on the motivation for that premise. Surprisingly, given the strategic role that it plays in his overall argument, it is defended only briefly in his book. His defence proceeds via the articulation of a certain conception of the development of mathematics, in which its concepts are developed for what are, primarily, *aesthetic* reasons. I summarize his argument here as follows:

**(A1)** A concept or structure is a *mathematical* concept or structure only because it comports with certain aesthetic judgements (judgements which are largely shared within the mathematical community);

**(A2)** Aesthetic judgements reflect only species-specific preference, and are, consequently, anthropocentric;

**(A3)** Therefore, employing mathematical reasoning in applications is anthropocentric.

As a preliminary skirmish, the argument needs to be refined, for **(A1)** misdiagnoses the role of aesthetic judgements within mathematics. The reason for this is that there is – and that within the mathematical community there is recognized to be – a distinction between two distinctions: *mathematics/non-mathematics* and *good* mathematics/*bad* mathematics.

To see this, one can consider that even systems of axioms that have nothing but demonstrably trivial consequences count, nevertheless, squarely as mathematics – that is, they are mathematical theories, and their study is recognizably mathematical activity – even if the theories themselves are universally acknowledged to be dull, ugly, or uninteresting. A nice example of this is provided in Douglas Hofstader's *Godel, Escher and Bach.* In it, he describes a formal system as follows.

The atomic strings of the theory are **I**, **M**, **U**, and the formation rule for well-formed strings is as follows: if $s$ and $t$ are strings then $\hat{s}t$ – the concatenation of $s$ and $t$ – is a string. The axioms are:

**I** If $x$**I** is a well-formed string, then so too is $x$**IU** (if a string ends in an **I**, you can add on a **U**)

**II** If **M**$x$ is a well-formed string, then so too is **M**$xx$ (the part of a string after an **M** can be repeated)

**III** If $x$**III**$y$ is a well-formed string, then so too is $x$**U**$y$ (occurances of **III** can be replaced by **U**)

**IV** If $x$**UU**$y$ is a well-formed string, then so too is $xy$ (occurances of **UU** can be dropped).

Hofstader then goes on to consider several meta-theorems about this system (most notably, he asks whether **MU** is derivable). Hofstader's aim, as is clear from the context of the book, is *to introduce the reader to a simple axiomatic mathematical system.* But it is clear that very few mathematicians would regard this system as interesting, or serious, or fruitful, or beautiful, or worthy of study (except perhaps for pedagogical purposes). Probably one reason for this verdict that the system is uninteresting is related to what Azzouni has called (in another context) "implicational transparancy": it is, with a little reflection, obvious what the theorems of the system are (or at least obvious that there will be no radical surprises); and obviousness does not make for interesting mathematics.[15] Nor, presumably, would the opposite property of what we might call implicational *intractability*, i.e. immense proof-theoretic difficulty or complexity: we can conceive (in structural detail, at least, if not of any concrete examples) of "intractable" systems with no proofs shorter than a trillion lines and no easily digested proof-theory allowing the recognition of short-cuts. For a mathematical structure to be interesting, it must lie somewhere happily between the extremes of implicational transparancy and intractability.

[15] Azzouni [2000].

Of course, it is much harder to say what is mathematically interesting than what is not. But for present purposes, it's only necessary to note that if Steiner is correct, then both Hofstader's trivial system and an intractable system of the kind just mentioned are not *bad* or *boring* mathematics, but *non-mathematical* altogether. This, I submit, is the wrong result. Steiner's proposed example of chess fares similarly: he asks "Why is chess a game; but Hilbert spaces, mathematics?" He expects the answer to make reference to the aesthetic judgements of mathematicians. But, again, it is not so clear that "theorems" of chess (e.g.: it is impossible to checkmate with only two knights and a king) are *not* mathematical in nature, even if they are deficient in certain other respects.[16]

How much damage do these reflections inflict upon the argument as presented? Not a great deal. Even granting that the relevant distinction here is between *good* mathematics and *bad* mathematics, and not between mathematics and non-mathematics, the argument might seem to go through with only the modified first premise and an additional, but plausible claim:

**(B1)** A concept or structure is a *good* mathematical concept or structure only if it comports with certain aesthetic judgements (that are largely shared within the mathematical community);

**(B2)** Aesthetic judgements reflect only species-specific preference, and are, consequently, anthropocentric;

**(B3)** It is predominantly only *good* mathematics that is of use in applications;

**(B4)** Therefore, employing mathematical reasoning in applications is anthropocentric.

The additional premise, **(B3)**, is reasonably easy to motivate: after all, the mathematics that is actually developed is presumably mainly interesting mathematics, and it is presumably only actually developed mathematics that is applied in science to great success.

Soren Bangu attempts to rebut the conclusion of something much along the lines of this argument by denying that mathematics and mathematical reasoning is anthropocentric, which he attempts to show by reference to certain historical episodes in the development of mathematics.[17] In particular, he examines the rejection of definabilism – the view that mathematical objects must in order to exist be definable in an explicit and uniform way – and, more generally, the displacement of orthodox constructivist

[16] There is of course a distinction between chess (the game) and the meta-theory of chess. It is clear from context that Steiner means to deny that the meta-theory is mathematical in nature.

[17] Bangu [2006].

tendencies from the mainstream of the discipline.[18] He gives two examples: (1) the dispute (among e.g. Euler, D'Alembert, Bernoulli, Fourier, etc) over whether a function is to be understood (as in modern terms) as an arbitrary assigment, or whether it must satisfy additional, definabilist, conditions – for instance, whether it must be continuous, or uniformly definable in terms of analytic functions; and (2) the dispute (among e.g. Lebegue, Borel, Baire, Hadamard, Zermelo, etc) over the adoption of the axiom of choice – whether for any family of non-empty sets $\{A_i\}_{i \in I}$, there exists a choice function $F$ such that $F(A_i) \in A_i$ – the controversy here arising from the fact that the axiom is non-constructive, since it claims that such a choice function exists even if it cannot be constructed or otherwise exhibited. Bangu believes that in each case, anthropocentricism was rejected, and in each case for the same reason: because the (anti-anthropocentric) view prevailed that mathematical objects do not depend for their existence on their cognitive accessibility or susceptibility to definition.

In my view, Bangu's response does go some way in responding to Steiner, and, moreover, is an instance of what is surely the right strategy: looking to mathematical practice as a source of resistance to the idea that mathematical reasoning is implicitly anthropocentric. Nevertheless, what he has traced is, at best, the historical rejection of *one particular source of anthropocentricism*: namely, a certain sort of constructivist/definabilist tendency. It is still open to someone sympathetic to Steiner to respond that what has not been shown is that *other anthropocentric* tendencies, overt or covert, implicit or explicit, were nevertheless at play both in these cases and in the development of mathematics more generally. In particular, there is no avoiding a direct confrontation with the conception of mathematical practice – responsible for much of the power and mystery of the considerations raised by Wigner and Steiner – as driven and governed by aesthetic or otherwise anthropocentric criteria.

This is the task taken on in the remainder of the paper. My response to (the reconstruction of) Steiner's argument comes in two parts. The first (Section 4) is a critical examination of premise **(B1)**, that is, the claim that mathematics is good only if it comports with certain aesthetic standards. In particular, I offer a kind of counter-narrative for what it takes for a concept or structure to be mathematically interesting: I will argue that the structures actually studied in contemporary mathematics departments and considered mathematically interesting have their roots, if we are willing to look back far enough, in abstractions or generalizations of physical or otherwise empirically gen-

[18]"Orthodox" here is supposed to pick out revisionary constructivism, that is, views in the Brouwerian tradition that deny the suitability or even the very coherence of non-constructive reasoning in mathematics. By contrast, what we might call "pluralist" constructivism still flourishes, in that constructivism and intutionism are regarded as legitimate modes of mathematics, if not the only such ones. As John Burgess puts it: "[T]here is no sole legitimate form of mathematics. A mathematician may work now in intuitionistic, now in classical mathematics, just as a painter may work now in a representational, now in an abstract style." Burgess [2008, p.273].

erated concepts. The subject matter of much pure mathematics is thus, in a good sense, still "about" the empirical world, even if the connection can only be seen at a very high level of abstraction.

Nevertheless, Steiner's concern that aesthetic properties play a (naturalistically) objectionable role in mathematics may persist. The second part of my response (Section 5) is to challenge the *inference* in the refined argument as represented above. To do this I will concede (for the sake of argument) that whether or not a piece of mathematics is good is governed by anthropocentric standards; nevertheless, by considering two case studies of what are widely received to be beautiful mathematics, I will show that the aesthetic virtues used to evaluate and motivate mathematics are far less straightforwardly anthropocentric than one might presume. In particular, the aesthetic virtues enjoyed by the theories are either themselves epistemically relevant or at least track generally recognized epistemic good-making features such as explanatory and unificatory power, fruitfulness, and importance. Consequently, it is fallacious to conclude that reliance on these aesthetic virtues is tantamount to anthropocentricism.

These two points reinforce one another: if the ultimate source of many mathematical structures is to be found in the physical world, and if mathematical activity and its fruits are evaluated or pursued (in part) in virtue of, or in such a way that correlates with, their ability to explain, unify, or develop our understanding of these structures, then – the hope is! – we will have a picture of mathematics on which the mystery of the applicability of mathematics has been dispelled.

## 4. The Complex Relationship Between Mathematics and the Physical Sciences

Those – like Steiner and Wigner – who conceive of mathematics as placing serious weight on aesthetic considerations often turn to G.H. Hardy's *A Mathematician's Apology* for support. For in a famous passage, he writes that

> The mathematician's patterns, like the painter's or the poet's must be beautiful; the ideas like the colours or the words, must fit together in a harmonious way. Beauty is the first test: there is no permanent place in the world for ugly mathematics.[19]

But elsewhere in the very same book, Hardy appeals to another criterion distinguishing good and bad mathematics, namely, its *seriousness*:

[19] Hardy [1992, p.85].

> A chess problem is genuine mathematics, but it is in some way 'trivial' mathematics. However ingenious and intricate, however original and surprising the moves, there is something essential lacking. Chess problems are unimportant. The best mathematics is serious as well as beautiful – 'important' if you like, but the word is very ambiguous, and 'serious' expresses what I mean much better... The 'seriousness' of a mathematical theorem lies, not in its practical consequences, which are usually negligible, but in the *significance* of the mathematical ideas which it connects. We may say, roughly, that a mathematical idea is 'significant' if it can be connected, in a natural and illuminating way, with a large complex of other mathematical ideas.[20]

And indeed, for Hardy the two notions – *beauty* and *seriousness* – are related:

> The beauty of a mathematical theorem depends a great deal on its seriousness, as even in poetry the beauty of a line may depend to some extent on the significance of the ideas which it contains.[21]

For Hardy, seriousness is thus an additional constraint upon good mathematics. Note that there is no requirement that beauty and seriousness need line up: it is possible for an item of mathematics to have significant and illuminating implications to a large complex of other mathematics without its being aesthetically appealing in the slightest (although, as the discussion in the next section will reveal, I doubt that the two notions are entirely unrelated.) I would, however, like to take issue with one aspect of Hardy's notion of seriousness and consequently offer a friendly amendment that I think better captures the spirit of the idea. For Hardy, it is enough that a certain piece of mathematics sustain connection with a *suitably large* number of other pieces of mathematics. But surely it is not the sheer number of connections that matters. For as Hardy is at pains to point out, there exists much "trivial" or unimportant mathematics; probably (depending on how exactly the relevant count is performed) there will be as much unimportant mathematics as important mathematics. Surely we would not want to say that results that somehow connect up with a large quantity of *trivial* mathematics are thereby important. Rather, the importance of a piece of mathematics is more plausibly a function of *the importance of the mathematics with which it is connected*. To borrow an analogy from epistemology, the correct picture of "seriousness" looks more foundationalist than coherentist; and as that analogy suggests, what will be needed are the analogues of foundationally justified beliefs – mathematics that enjoys, so to speak,

[20] *ibid*, p.88.
[21] *ibid*, p.90.

the status of "foundationally serious". How might a piece of mathematics attain such status?

The great algebraic topologist Saunders Mac Lane begins his book *Mathematics: Form and Function* as follows:

> Mathematics, at the beginning, is sometimes described as the science of Number and Space – better, of Number, Time, Space, and Motion. The need for such a science arises with the most primitive human activities. These activities presently involve counting, timing, measuring, and moving, using numbers, intervals, distances, and shapes. Facts about these operations and ideas are gradually assembled, calculations are made, until finally there develops an extensive body of knowledge, based on a few central ideas and providing formal rules for calculation. Eventually this body of knowledge is organized by a formal system of concepts, axioms, definitions, and proofs. [...] Mathematics deals with a heaping pile of successive abstractions, each based on parts of the ones before, referring ultimately (but at many removes) to human activities or to questions about real phenomena.[22]

There are two central points made here by Mac Lane: Firstly, the claim that the conceptual roots of *elementary* mathematics grow out of, so to speak, certain basic modes of understanding and interacting with the physical world; and secondly, the claim that the body of *contemporary* mathematical knowledge – in its abstract, axiomatic form – can also ultimately be traced back to these fundamental sources. The picture he offers is a plausible rational reconstruction of the historical development of mathematics according to which – to give the compressed version – a few fundamental axiomatic theories are formulated initially to capture certain natural phenomena – the "intended" model or models – and which then become available for autonomous study without explicit reference to the motivating examples.[23] A similar viewpoint is propounded by Bourbaki, the notoriously formalistically-minded French collective of mathematicians. In their view, mathematics is the study of abstract structures. But not all structures are created equal: the organizing center of the subject, for Bourbaki, consists of a small number of "mother-structures", including the natural numbers, the Euclidean plane, the real line, as well as topological structures, order structures, and algebraic structures. And what is striking about all of these examples is that a plausible case can be made that they originate out of a desire to describe – albeit in a distinctively mathematical, general and abstract way – certain features of the natural world.

[22] Mac Lane [1986, p.6].

[23] See Maddy [2008] for a historically sophisticated and much developed version account along these lines.

My suggestion, then, is that the Bourbakian mother-structures play precisely the role of "foundationally serious" mathematics role adverted to above. Mathematics is important to the extent that it illuminates our understanding of these basic structures, or it illuminates our understanding of mathematics that illuminates our understanding of these structures, or... etc. I take it that this is a plausible explanation of why, say, the Langlands program is considered to be one of the most important research programs in mathematics today: the Langlands conjectures lie at the confluence of algebraic number theory, the theory of automorphic forms, and the representation theory of algebraic groups, all of which are manifestly illuminative of the natural numbers, functions of complex variables, and abstract algebra, all of which in turn inform our understanding of the Bourbakian mother-structures.[24]

Now, one might ask: why is it that these mother-structures – and not other alternatives – are properly considered to be what I have been calling "default serious"? Part of the answer lies precisely in the inherent interest that derives from their physical interpretation; but that is only part of the answer. Some of the reason, I think, is also attributable to the surprising and marvellous fact that the formal systems developed from these modest origins turn out to be extremely intricate, complicated, and beautiful. This was by no means guaranteed: there is no *a priori* reason (in a good sense of *a priori*) why, say, the theory of natural numbers should turn out to be any more intricate than the theory of chess. It is to this topic – and the topic of aesthetic features in mathematics more generally – that we now turn.

To briefly sum up the discussion of this section: the Bourbakian "mother-structures" arise as a formalization of fundamental physical structures, and seriousness – the connection with or ability to shed light upon these structures – is (and is rightly considered to be) a major virtue of mathematics. I think, suitably understood, that this in itself goes a significant part of the way towards rehabilitating a naturalistically acceptable picture of the use of Pythagorean and Formalist mathematical methods in science.

## 5. Aesthetic Judgement and Naturalism

Even granting what I have said above, a challenge still remains: isn't good mathematics characterized as such partly on the basis of aesthetic factors? And doesn't this enforce an anthropocentric view of the good/bad mathematics distinction? Certainly one can find quotes by famous mathematicians suggesting or endorsing the view that aesthetic properties are consitutive (or partially constitutive) of good mathematics (e.g. the Hardy passage cited above). But it would be too quick to take this as dispositive evidence for implicit anthropocentrism, as I will now argue. In order to fully assess this

[24]See Gannon [2006] for a – relatively speaking – readable account of the Langlands program.

line of thought, we would need to clarify what, exactly, we take to be characteristic of (distinctively) *aesthetic* properties; however, this would lead us very far afield. Here are a number of theses that I think proponents of the aesthetic conception of mathematics tend to adhere to, which will render the notion tolerably clear for our purposes.

1. *Prima facie* aesthetic concepts such as beauty, elegance, harmoniousness, cleanliness, and so on (and on the other side of the fence) dullness, clumsiness, and ugliness, etc, are, when deployed by mathematicians in the evaluation of mathematical objects, theories, proofs, or activities, to be taken at face value, i.e. as genuinely aesthetic appraisals (however that mode of appraisal is in general to be understood);

2. These aesthetic appraisals play a significant role in the classification of mathematics as good or bad; and

3. The correct account of aesthetics is a broadly subjectivist or projectivist one, according to which aesthetic appraisal is merely a projection of certain (parochial and species-specific) human subjective attitudes (which is of course compatible with the presence of a great deal of intersubjective agreement).

One way to respond would be to deny the claim that aesthetic properties play a role in mathematical appraisal. However, this would be a desperate response. It is undeniably a well-established part of mathematical practice to use *prima facie* aesthetic terminology in an evaluative way, and there is no clear or compelling reason to think that in so using this terminology, mathematicians are not doing exactly what they appear to be: making aesthetic judgements. Nor would it be plausible to deny that these evaluations are given much weight in classifying mathematics; the literature is replete with examples to the contrary. Another avenue of response, advocated by Chris Pincock, rejects the implicit subjectivist/projectivist account of aesthetic appraisal and attempts to argue that the possession of aesthetic properties is (maybe globally, but at least locally within a mathematical context) a fully objective matter.[25] Although this may in the final analysis be correct, to my mind it does not seem promising. If the only way to respond to the puzzle of applicability is by adopting an objectivist account of aesthetic properties, it might reasonably be thought that the supposed cure of objectivism is worse than the disease of anthropomorphism with which we began.

By contrast, my preferred response to this aspect of the argument is to concede for the sake of argument the subjectivist/projectivist account of aesthetics, while nevertheless claiming that the aesthetic properties relevant to the evalutation of mathematics are correlated with other, non-aesthetic properties that are not merely parochial

[25] Pincock [2012, p.184].

and species-specific – that is, properties that may reasonably be taken to inform a non-anthropocentric distinction between good and bad mathematics. To support these claims, I propose to briefly examine two case studies from mathematical practice.

### 5.1 Probabilistic Number Theory

In a lecture on the importance of mathematics, Fields medallist Timothy Gowers gives an example of what he explicitly considers to be a beautiful result: the Erdos-Kac theorem (also known as the fundamental theorem of probabilistic number theory.)[26] As is well known, every natural number has a unique prime factorization. Let $\omega(n)$ be the number of prime factors of $n$. Then the Hardy-Ramunujan theorem tells us that, for almost all integers[27] and for any real-valued function $\psi$ that tends to infinity as $n$ tends to infinity,

$$|\omega(n) - \log(\log(n))| < \psi(n)\sqrt{\log(\log(n))}$$

The Kac-Erdos theorem can be seen as a natural generalization of this result. What the theorem says, roughly speaking, is that the probability distribution of

$$\frac{\omega(n) - (\log(\log n))}{\sqrt{(\log(\log n))}}$$

is a normal – "Gaussian" – distribution.

One reason this proof is so interesting for my purposes is because Gowers takes the time to explain, explicitly, *why* he finds the result to be so beautiful. He offers five reasons, noted here, and then discussed in §5.3.

Firstly, the very shape of the distribution – a bell curve – is itself aesthetically pleasing.

Secondly, the theorem possesses an appealing *simplicity*: normal distributions, although defined by the relatively complex-looking formula $\frac{1}{\sigma\sqrt{2\pi}}e^{\frac{-1}{2}(\frac{x-\mu}{\sigma})^2}$, arise in fact extremely naturally and are the subject of extensive study in statistics (there is a reason they are called "normal" distributions!). In fact, in a well-defined sense, normal distributions are the simplest possible distributions: the central limit theorem states that in certain (mild but lengthily-specified) conditions, the mean of a sufficiently large number of independent random variables, each with finite mean and variance, will be approximately normally distributed.

Thirdly, the theorem is unexpected. As Gowers puts it,

[26] Gowers [2000].

[27] That is, if $g(m)$ is the number of integers less than $m$ for which the inequality fails, then $g(m)/m \to 0$ as $m \to \infty$.

> Behind the disorder and irregular behaviour of the primes there lies the simplicity and regularity of the normal distribution. This is particularly surprising because the primes are defined deterministically (there is no choice about whether a given number is a prime or not) while the normal distribution usually describes very random phenomena. [28]

Fourthly, the phenomenon uncovered is not one that could have been appreciated by "brute force" means: the sheer computational power that would have been required to calculate large enough $\omega(n)$ to give a reasonable and recognizable approximation to a normal distribution is far beyond feasible limits. Thus, the result is one that is, in a sense, "purely theoretical" – the underlying pattern could only have been appreciated via ingenious theorizing, and not, realistically, via experimental or inductive evidence.

Fifth, and finally, the *proof* of the theorem is, in Gowers' words, "very satisfying". He renders it as follows:

> Step 1. When $n$ is large, most numbers near $n$ have roughly $\log(\log n)$ prime factors. (That is, with a few exceptions, if $m$ is near $n$ then you can approximate the number of prime factors of $m$ by taking its logarithm twice.) This result was proved by Hardy and Ramanujan in 1920, and again in 1934, with a much simpler argument, by Paul Turan.
>
> Step 2. Therefore, most prime factors of most numbers near $n$ are small. This follows because a signicant number of large prime numbers would multiply to a number bigger than $n$.
>
> Step 3. If $m$ is chosen to be a random number near $n$, then the events '$m$ is divisible by $p$', where $p$ is a small prime, are roughly independent. For example, if you know that $m$ is divisible by 3 and 5, but not by 11, it gives you almost no information about whether $m$ is divisible by 7. By a technique known as the Brun sieve, this means that if we think of the events as being exactly independent, then the conclusions we draw from this will be approximately correct.
>
> Step 4. If these events were exactly independent, then a normal distribution would result, because (subject to certain technical conditions that hold here) it always arises when one counts how many of a large number of independent events have occurred.[29]

One reason that the proof is so satisfying is its very susceptability to being described in such simple, shorthand terms. Naturally a fully admissible version of the proof would

[28] Gowers [2000, p.19].
[29] Gowers [2000, p.22].

be much longer and technically formidable. But the presence of vague descriptions such as "$n$ is large" or "roughly $n$ factors" or "most numbers near $n$" – all of which would immediately suggest the appropriate rigorous precisifications to the ears of a trained number-theorist – allows the key ideas of the proof to be easily surveyed and the core strategy of the proof to be easily understood. No doubt, there is the possibility of being fooled into a false sense of understanding if one does not fully realize the complications added by the requirement of rigor. But having worked through the proof, it is hard not to regard this sketch as containing all but the details: all of the essential conceptual moves are there.

### 5.2 Galois Theory

As a second case study of positive aesthetic evaluation in mathematics, consider what is commonly regarded as one of the most beautiful mathematical theories: Galois theory. To briefly substiatiate the sociological claim that the theory is so regarded, I quote the preface to a textbook on Galois theory by Stephen Weintraub:

> Galois theory has a well-deserved reputation as one of the most beautiful subjects in mathematics. I was seduced by its beauty into writing this book.[30]

Similarly, Ian Stewart calls the Fundamental Theorem of Galois Theory one of "the most beautiful theorems in mathematics".[31] I share these judgements, and I submit that I am not especially ideosyncratic in doing so. Let me give a brief exposition of some of the main results and ideas Galois Theory, before trying to draw some lessons.[32]

The roots of Galois theory are to be found in one of the oldest mathematical endeavours: the task of finding solutions to polynomial equations. A polynomial equation is one of the form

$$a_0t^n + a_1t^{n-1} + ... + a_{n-1}t + a_n = 0$$

where $t$ is considered as an independent variable and the coefficients $a_i$ are elements of some field $K$. A field can be thought of as a set of numbers with suitable operations to allow for recognizable analogues of addition, subtraction, multiplication and division. It is a generalization of the structure common to the rational numbers, the real numbers, and the complex numbers. The highest power of $t$ with a non-zero coefficient is called the degree of the polynomial.

[30]Weintraub [2008], preface.

[31]Stewart [2004], introduction.

[32]For a more detailed exposition, see Stewart [2004]. My presentation is much closer to the modern approach than that of Galois, Abel, and Ruffini themselved. See Kiernan [1971] for a comprehensive history of the development of Galois theory within the mathematical canon.

A fundamental and easily asked question is: which polynomials have solutions in radicals – i.e. solutions built up from the coefficients $a_i$ using the operations of addition, subtraction, multiplication, division, and $n^{th}$ roots, and if so, how can these solutions be found? At the time of Galois, the quadratic formula (i.e. for polynomials of order 2):

$$t = \frac{-b \pm \sqrt{b^2 - 4ac}}{2a}$$

(bane of school-children everywhere) was well-known, as were a somewhat more unwieldy cubic formula due to Niccolò Fontana Tartaglia and a much more unwieldy quartic formula due to Lodovico Ferrari. But quintic polynomials and higher remained unsolved, in general, if not in particular cases.

Now, suppose we have a polynomial $f(t) = a_0t^n + a_1t^{n-1} + ... + a_{n-1}t + a_n$. We can construct a new field, $L$ (which is perhaps identical to $K$, but not so in the general case), called the *splitting field* of $f(t)$ with the special property that $L$ contains precisely enough additional elements to allow us to factorise $f$ as the product of linear factors – in other words, the splitting field contains all of the roots of $f(t)$. To illustrate this idea, take $f(t) = t^2 + 1$, considered as a polynomial over $\mathbb{R}$. Since $\mathbb{R}$ doesn't contain any elements whose square is negative, there are no solutions to $f(t)$ in $\mathbb{R}$, and consequently it cannot there be written as the product of linear factors. To do this we need to move to a new field – one adjoining just enough new elements to $\mathbb{R}$ to contain all of the solutions to $f(t)$. As it turns out, this role can be played by the complex field $\mathbb{C}$ (the field obtained by adjoining a complex element $i$ such that $i^2 = -1$) in which we can then factorize $f$ by $f(t) = (t - i)(t + i)$. A similar procedure can be carried out for arbitrary polynomials, and a splitting field can always be shown to exist.[33]

It is this pair of fields – the original field $K$ and the splitting field $L$ that is the focus of our attention. Surprisingly, the information obtained by considering the field extension is precisely enough to obtain a good amount of detail of the structure of the solutions to $f(t)$.

The trick is to think about all of the $K-$automorphisms $\theta : L \to L$. These are maps $\theta$ such that, for all $x, y \in L$ and $k \in K$,

1. $\theta(x + y) = \theta(x) + \theta(y)$
2. $\theta(xy) = \theta(x)\theta(y)$
3. $\theta(k) = k$.

Each $K-$automorphism can be thought of as a permissible permutation or rearrangement of the solutions of $f$. For example, if $f(t)$ is the polynomial $t^2 + 1$ with real coef-

[33]Strictly speaking, the splitting field always exists and is unique *up to isomorphism.*

ficients, then the splitting field for $f$ is $\mathbb{C}$, in which we can write $f(t) = (t-i)(t+i)$. Now, suppose $\alpha$ is an $\mathbb{R}-$automorphism. Then we know (since $\alpha$ fixes $\mathbb{R}$) that $\alpha(f(i)) = 0 = f(\alpha(i))$, so we have either $\alpha : i \mapsto i$ (the identity map) or $\alpha : i \mapsto -i$ (a so-called involution). These are the only two possible such mappings. In fact, for any field extension, all automorphisms of this kind form a group called the *Galois group* $G_{L:K}$. In other words, we can compose or invert any of the permissible permutations of solutions and end up with what is still a permissible permutation. (In our example, note that if we apply the involution twice, the result is the identity map.) Now, one of the core insights of Galois was to notice that there is a one-one correspondence between

1. subgroups of the Galois group $G_{L:K}$ and
2. subfields of $L$ that contain $K$ (i.e. that are "sandwiched" between them thus: $L : M : K$)

This correspondence is specified by associating each subgroup $H \subset G$ with the set $H^\dagger$ of elements $x \in L$ such that $\alpha(x) = x$ for all $\alpha \in H$, i.e. the set generated by all of the elements of $L$ that are fixed by all of the automorphisms in $H$. This set turns out to be a subfield of $L$. Conversely, each subfield $M$ of $L$ is associated with the group $M^* := G_{M:K}$, i.e. the set of automorphisms of $K$ that fix all the elements of $M$. So, we have two maps:

$$\dagger : \mathcal{G} \to \mathcal{F}$$

$$* : \mathcal{F} \to \mathcal{G}$$

(where $\mathcal{F}$ is the set of relevant fields and $\mathcal{G}$ is the set of relevant groups) both of which reverse inclusions in the following sense: if $H \subseteq G$, then $G^\dagger \subseteq H^\dagger$ (since $G$ contains more permutations than $H$, in general it will fix fewer elements of the field and so the subfield it generates will be smaller) and if $K \subseteq M$ then $M^* \subseteq K^*$ (since $K$ contains fewer elements than $M$, in general there will be more automorphisms that fix the elements of $K$). Furthermore, if $L : K$ satisfies certain mild conditions (called normality and separability), then in some sense *no* information is lost by looking at things in terms of the subgroups of $G_{L:K}$, for $*$ and $\dagger$ are inverses: for all subfields $M$ of $L$, we have

$$(M^*)^\dagger = M$$

The details of this correspondence are sometimes called the Fundamental Theorem of Galois Theory.

Once it has been proven, we are in a position to use it to prove the very deep result that there is no general solution in radicals to equations of degree 5 and above.

Recall what it is for a polynomial to have a radical solution: it means a solution can be obtained using only the operations of addition, subtraction, multiplication, division, and $n^{th}$ roots. Translated into field-extension speak, this means that a polynomial $f(t)$ over $K$ is soluble if there is a series of fields

$$K \subseteq K_1 \subseteq ... \subseteq K_{n-1} \subseteq K_n = L$$

and $L$ is a splitting field for $f(t)$. We can say more: each intermediate field $K_i$ must be generated by adjoining an $n^{th}$ root of some element from $K_{i-1}$. With the Fundamental Theorem in hand, the question suggests itself: what can we say about the induced series of *groups:*

$$\{e\}(= L^{\dagger} = G_0) \subseteq G_1 \subseteq ... \subseteq G_{n-1} \subseteq G_n(= K^{\dagger})$$

Now, there is a notion of "division" of groups that is applicable here (for those who know some group theory: because $G_i$ is normal in $G_{i+1}$), and it follows that each of the $G_i/G_{i+1}$ is abelian – i.e. its group operation must be commutative. And conversely, it turns out that if a polynomial generates such a series (with abelian quotients), then it is soluble in radicals! What is going on under the hood, so to speak, is that a soluble polynomial is always obtainable by adjoining $p^{th}$ roots of elements of the base field (where $p$ is prime); and each time such an element is adjoined, the corresponding quotient group is always cyclic, and hence abelian.

The Galois group of a polynomial of degree $n$ is in general isomorphic to a subgroup of the symmetric group on $n$ elements, $S_n$ (i.e. the group of possible permutations of $n$ different things), and it is always possible to find a polynomial of degree $n$ whose Galois group is isomorphic to the whole of $S_n$. Here then is the explanation of the general solubility of polynomials of degree 2,3, and 4: every subgroup of $S_2$, $S_3$ and $S_4$ can be written in the form of a series with abelian quotients. But they are the exception: for $n \geq 5$, it's possible to show that $S_n$ can *not* be written in that form. And for this reason, the quintic (and higher) are, in general, insoluble.

So much for the whirlwind tour of Galois theory. The technical details do not especially matter; more relevant for our purposes are the following features of the theory:

- The core of Galois theory is the correspondence introduced in the Fundamental Theorem: once that is known, it is possible to translate difficult questions about the structure of solutions to a given polynomial into questions about the structure of its Galois group. This simplifies the problem in many ways, some subtle, some obvious. Groups are in many ways simpler algebraic objects than polynomials and field extensions, and much more is understood about them (for instance, groups that possess chains of "divisible" subgroups are easy to classify). Some of the specificity of the polynomial under examination is lost by examining its Galois group, for in general, many different polynomials have the same Galois group. And yet, as the success of the theory shows, the move to its Galois group preserves *precisely enough* information about a polynomial to give an analysis of its solubility. It provides, as is sometimes said, the natural setting for conceiving of the problem.

- Not only does Galois theory allows us to *prove* that the quintic and higher cannot, in general, be solved; it also *explains* why this is the case. It is generally recognized both among mathematicians and philosophers of mathematics that there is a distinction to be drawn between explanatory and non-explanatory proofs, although it has proven difficult to say more about what this distinction consists in and the literature on the topic is still in its early stages. Now is not the time to examine this dispute. But any plausible account, I think, ought to take it as a datum that the Galois theoretic treatment of the insolubility of the quintic is a paradigmatic example of a successful explanation. Unlike many proofs, it provides a kind of "understanding *why*": once it has been worked through and understood, any mystery about the explanandum ("why *quintics*?") is almost entirely dispelled.

- Related to issues of explanation, Galois theory has been called a "showpiece of mathematical unification".[34] There are two senses in which this is true. Firstly, it brings together ideas and machinery from many different parts of the subject, and leads (see below) to many more connections still. But secondly, its very success allows for its subject-matter – polynomials – to be understood in a unified way. This display of unity is not, needless to say, something that is always guaranteed. Contrast, for instance, the study of partial differential equations. While there has been much success in solving particular classes of PDEs, a general theory has proven difficult to come by. Sergei Klainerman [2010] reflects a prevalent view that PDEs are too disparate a class to admit of a general theory: "PDEs, in particular those that are nonlinear, are too subtle to fit into a too general scheme; on the contrary, each important PDE seems to be a world in itself."[35] There is no *a priori* reason that polynomials should have turned out to be so susceptable to a unified treatment; that they do is itself a remarkable fact.

- The theory introduces, in natural ways, ideas which have turned out to be in-

[34] Stewart [2004].
[35] Klainerman [2010, p. 279]

credibly fruitful in later mathematics. David Corfield[36] usefully distinguishes five "degrees" of fruitfulness and importance:

1. When a development allows new calculations to be performed in an existing problem domain, possibly leading to the solution of old conjectures.
2. When a development forges a connection between already existing domains, allowing the transfer of results and techniques between them.
3. When a development provides a new way of organising results within existing domains, leading perhaps to a clarification or even a redrafting of domain boundaries.
4. When a development opens up the prospect of new conceptually motivated domains.
5. When a development reasonably directly leads to successful applications outside of mathematics.

Galois theory, I think unarguably, succeeds in all five respects except perhaps the last ("reasonably directly" is the issue here). Let me say something briefly about the others. (1) The "old" problem – one of the most fundamental in mathematics – about the solubility of polynomials was spectacularly resolved in a theoretically satisfying way. (2) This was done by an assimilation (mentioned in the previous paragraph) of questions concerning the solubility of polynomials to algebraic questions concerning the structure of certain groups, allowing group-theoretic methods to resolve algebraic questions. (3) Galois' work precipitated a radical and widespread reorientation of the very conception of algebra: before it was seen as concerning primarily the solution of equations, but afterwards it in addition encompassed the study of abstract structures such as groups, fields, rings, etc, in their own right. Indeed, the very notion of a group was introduced by Galois himself, and the first steps in group theory were pursued by him for the purpose of developing the theory of solubility. (4) The range of mathematics directly or indirectly motivated by Galois theory is vast – in fact, it constitutes a cornerstone of contemporary algebra. To pick just one striking example, consider the generalization (by Sophus Lie) of Galois theory applied not to *polynomial* equations but to *differential* equations, i.e. equations of the form $a_0D^ny + a_1D^{n-1}y + ... + a_ny$ where $D$ is a differential operator. The groups associated with such equations – Lie groups – are objects that are widely studied in mathematics and extensively applied in physics. Lie groups admit a manifold structure, and in some sense capture the idea of *continuous* symmetry (for

[36] Corfield [2003, p.205].

instance, think of the rotation group of a sphere). Consequently, they are of great interest in many physical applications that involve continuous dynamical systems.[37]

In short: Galois theory is certainly a beautiful theory; but it would be a mistake to end the analysis there, for we can go on to ask *why* it is so beautiful.

### 5.3 Reconsidering the Role of Aesthetic Properties in Mathematics

What is the bearing of all of this on the question of the applicability of mathematics? The primary lesson I want to draw from these case studies is simple: even though mathematicians commonly use the language of "beauty" and "elegance" when describing mathematical theories and results, these judgements are, interestingly, far more closely connected with non-aesthetic features that one might initially be led to expect.

What non-aesthetic features? As we saw, Gowers explicitly includes such considerations as simplicity, unificatory power, explanatory power, and epistemic tractability all as contributing to the beauty of the Erdos-Kac theorem he discusses. And the brief tour of elementary Galois theory makes it plausible that if one looks for the underlying reasons why Galois theory is considered to be attractive or elegant or beautiful, one needs to advert to one or more of the points mentioned above: that is, to the surprising Galois correspondence between fields and groups, to the capacity of the theory to motivate and explain the answer to a deep and central mathematical question via a satisfying, explanatory proof, to its unification of several key mathematical tools and ideas, or to its immensely fruitful consequences in moving mathematics forward.

So, aesthetic judgements in mathematics appear to be intimately connected with a host of non-aesthetic properties. Nor are these just any old non-aesthetic properties, either: it's notable that the ones we've seen crop up – simplicity, unificatory and explanatory power, epistemic tractability, fruitfulness – are precisely the features that are often discussed in confirmation theory and the philosophy of science under the heading of *theoretical virtues*. This is not the place to embark on a detailed discussion of abductive methodology or the nature of theoretical virtues. It will be enough to raise the basic idea: that faced with a choice of theories to believe or develop, it is *epistemically rational* to prefer those that possess the relevant theoretical virtues over those that do not. Needless to say, there are skirmishes about how precisely these virtues are best understood, how they are to be weighted, and perhaps even deeper questions about why it is rational to prefer theories that possess them. But despite all this, there is considerable agreement about the list of candidate properties: and it is striking that it

[37] See for instance Gilmore [2008].

overlaps more or less perfectly with those that we've identified as present in our two case studies of mathematical beauty.

How might the nature of the connection between aesthetic judgements in mathematics and the theoretical virtues be elaborated? I can think of two ways, one stronger and one weaker. The stronger claim is that aesthetic language and judgements in mathematics serve as a way of *expressing* non-aesthetic, theoretically virtuous features of the subject matter. This is not to say that the mode of assessment is not genuinely an aesthetic one; when mathematicians call a proof beautiful, there is no reason to doubt them at their word. Rather we might think of this account as proposing the existence of a distinctive *mode* of mathematical beauty, a mode which is manifested by proofs, theories, and constructions when they are simple, unifying, explanatory, and fruitful. If this claim is right, then it would predict the following bold conjecture: whenever mathematical theorems, theories, or proofs that are widely agreed within the mathematical community to be elegant or beautiful, then they will also either: explain some mathematical fact; provide some understanding of a certain class of phenomena or objects; unify some subject matter; exhibit the presence of connections between disparate areas of mathematics; or lead to fruitful or important consequences. I am not sure that this conjecture is correct; it may be too strong. But even if it is, there is a weaker thesis that will serve our purposes just as effectively: that in mathematics, aesthetic language and judgments at the very least *tend to track* or are *reliably correlated with* the theoretical virtues in a consistent and predictable way.

Once this connection has been elaborated, our original puzzle is cast in a very different light. It is one thing to talk of aesthetic properties like beauty and elegance as reflecting nothing more profound than the contingent predilections bestowed upon our species by evolution. But it is quite another to condemn theoretical virtues – presumptively epistemically valuable properties – such as explanatoriness, unificatory power, fruitfulness, and so on, in the same terms. It certainly seems that it is legitimate, and in particular *naturalistically acceptable*, to want our theories – scientific and mathematical – to possess these properties. Still, it must be conceded that I am in no possession of a knock-down argument for this: indeed, it is open to a defender of Steiner to argue that the theoretical virtues themselves are somehow covertly anthropocentric. This is not the place to respond to such worries, even if I could. I will have to be content with the following disjunctive note: given the weight that theory choice in *science* places upon such virtues, such a reliance had better be naturalistically acceptable; for if it is not, then anthropocentrism in science goes much deeper than even Steiner contends.

## 6. Conclusion

The picture of mathematics responsible for the puzzle – shared by many, and which Steiner explicitly takes to disclose a naturalistically problematic commitment to anthropocentricism – is this: we we formulate and develop mathematics on the basis of our parochial, species-specific, subjective aesthetic judgements; and as a result, it is utterly mysterious how the resulting theories and techniques could describe and predict scientific phenomena with the success that they do. I have not tried to dispute the datum that aesthetic judgements inform mathematics to a considerable extent. What I have tried to do is offer a counter-narrative that shows how this fact might nevertheless be reconciled with the rejection of anthropocentricism: if the starting-point of a large body of contemporary mathematics is the study of Bourbakian "mother structures" inspired by structures in the physical world, and if the aesthetic judgements used to systematize and develop mathematics beyond this starting-point are reliably correlated with theoretical virtues, then I believe much of the mystery with which we began has been dispelled. For the development of mathematics will track much more than merely our parochial, species-specific aesthetic judgements: it will also, by virtue of the ultimate inspiration for our theories, and by virtue of the correlation between these judgements and the theoretical virtues, result in theories and techniques which unify – in fruitful, simple and explanatory ways – facts about structures found in the physical world. It is not so unreasonable, I submit, that such theories find outstanding success in applications.[38]

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