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Is Mathematics Unreasonably Effective?

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ABSTRACT
Many mathematicians, physicists, and philosophers have suggested that the fact that mathematics—an *a priori* discipline informed substantially by aesthetic considerations—can be applied to natural science is mysterious. This paper sharpens and responds to a challenge to this effect. I argue that the aesthetic considerations used to evaluate and motivate mathematics are much more closely connected with the physical world than one might presume, and (with reference to case studies within Galois theory and probabilistic number theory) I show that they are correlated with generally recognised theoretical virtues, such as explanatory depth, unifying power, fruitfulness, and importance.

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1. Introduction

It is striking that mathematics can be so successfully applied to the natural sciences. Many philosophers—following Quine and Putnam—have taken this fact to be significant in the dispute between platonists and nominalists about the existence of mathematical objects.[1] But the applicability of mathematics raises a different question, arising 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, using a seemingly *a priori* methodology—can be applied to the physical world can be made to seem striking, mysterious—even inexplicable. Perhaps the canonical source of this puzzlement is Eugene Wigner [1960: 14], who argued in a famous essay that 'the 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.'

Adding to the air of mystery is a widely shared sense that *aesthetic* judgments play a fundamental role in mathematics. Wigner [ibid.: 3] characterises most '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 the connection with aesthetics is responsible for much of the force of the puzzle. How can mathematical concepts, devised by following aesthetic impulses, be put to such spectacular use in empirical applications? Similar puzzlement is expressed by Steven Weinberg [1993: 125]:

[1] See, e.g., Quine [1960], Putnam [1975], Field [1980], and Colyvan [2001a] for a small sample of this literature.

> 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.[2]

My aim in this paper is to provide a resolution of this puzzle. The first task (section 2) is to get a more precise formulation into view. I argue that the points raised by Wigner et al. generate an explanatory challenge—roughly, that of explaining the applicability of mathematics, given its *apriority* and its connection with aesthetic considerations. The rest of the paper responds to this challenge. Section 3 questions (or at least substantially qualifies) the conception of mathematics responsible for the puzzle by arguing that there are mathematical virtues over and above aesthetic virtues: despite its apparently *a priori* methodology, mathematics is linked by the pursuit of these virtues to the empirical world. Even so, the prevalence of aesthetic criteria in mathematics poses a residual question. This is resolved in section 4, which examines two case studies of aesthetically appealing mathematics—one within Galois theory and one within probabilistic number theory. In light of these examples, I argue that aesthetic virtues in mathematics are highly correlated with more general theoretical virtues—simplicity, generality, explanatory power, etc—of the sort that play an important role within scientific practice.

2. The Applicability of Mathematics as Explanatory Challenge

In presenting his case, Wigner gives a number of examples of mathematics originally developed for internal reasons, but which later emerged as crucial in applications. Moreover, he conceives of these reasons as primarily *aesthetic* in nature. For instance, he writes [1960: 3]:

> if a mathematician is asked to justify his interest in complex numbers, he will point, with some indignation, to the many beautiful theorems in the theory of equations, of power series, and of analytic functions in general, which owe their origin to the introduction of complex numbers.

But, despite their internal mathematical motivation, complex numbers came to play a fundamental role within many parts of physics, including the theory of electromagnetism, fluid dynamics, and quantum mechanics.

Although Wigner's discussion is subtle and contains many suggestive examples, it is not obvious how to extract a sharply formulated philosophical problem from it. I propose to understand the puzzle as an explanatory challenge: facts which, taken together, seem to be 'striking' or 'cry out' for explanation. Arguments with the structure of explanatory challenges have received much attention in recent philosophy of mathematics. For instance, Benacerraf's epistemic argument against realism has been refined as the challenge of explaining the striking fact that our mathematical beliefs are reliable.[3] Unlike these debates, the explanatory challenge here is not directed against any particular philosophical target; I take it to be less a suppressed argument and more a genuine puzzle, confronting anyone with certain (widely held) views about

[2] Similar sentiments can be found in Feynman [1967: 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] See Benacerraf [1973], Field [1989], and Schechter [2018]. See also Warren and Waxman [forthcoming] for a recent explanatory challenge concerning determinacy.

mathematics and its significance to science.[4] I emphasise that I endorse these views only with substantial qualifications, which will emerge as we proceed (chiefly in sections 3 and 4). But, for now, I will motivate them straightforwardly, in order to generate a *prima facie* challenge in the strongest terms.

The first component of the explanatory challenge is a widespread view of mathematics:

(I) Pure mathematics is an *a priori* discipline that appeals essentially to aesthetic considerations.

The relevant notion of *apriority* here is more methodological than epistemic. To be sure, many prominent philosophical accounts of mathematics view it as *a priori*, in the epistemic sense of being justified independently of experience. But, as intended, the claim is consistent even with empiricist views according to which the ultimate justification for mathematics derives from its links with empirical inquiry. The point is simply that the practice of pure mathematics, as the subject is actually carried out, is, methodologically speaking, largely detached from experience. Put crudely, if we observe mathematicians during their working hours, we do not see them conducting empirical investigations, or performing experiments, or measuring physical quantities; rather, their time is spent in 'armchair' activities—proving theorems, investigating mathematical constructions, formulating definitions, etc., primarily using the tools of deductive logic. So, regardless of one's view of the justification of pure mathematics, it seems hard to deny that its day-to-day methodology takes place without substantial empirical input.

(I) also claims an essential role for aesthetic considerations within mathematics. Many observers have noted that apparently aesthetic terminology is prevalent: proofs, techniques, equations, theorems, conjectures, and whole bodies of theory are variously described as 'beautiful', 'elegant', 'neat', 'harmonious', 'clean', and so on, and examples can be multiplied.[5] One option is to dismiss such talk as frivolous or eliminable. But those impressed by the puzzle have tended instead to conceive of aesthetic considerations as essential to fully understanding mathematics or mathematical practice. There are at least three ways in which this might happen.

One is that aesthetic considerations play a *constitutive* role in delineating the boundaries of the mathematical. Steiner [1998: 65] expresses such a view when he claims that 'the aesthetic factor in mathematics is constitutive … concepts are selected as mathematical because they foster beautiful theorems and beautiful theories.' He goes on [ibid.: 66]—mentioning an example of Frege's—to argue that the explanation of the fact that the study of chess is not properly considered to be mathematics, whereas the study of Hilbert spaces is, will 'rely on aesthetics'.[6]

A second possible role for aesthetic considerations is *normative*. Something like this is expressed by Hardy [1940: 85] when he claims that

[4] One option, inspired by Steiner [1998] and pursued in an earlier version of this paper, would be to direct the challenge at a species of *naturalism*, by construing it as an argument that mathematics is tacitly anthropocentric. However, helpful comments from a referee persuaded me that the relevant notion of anthropocentrism was neither entirely clear nor necessary for the dialectic.

[5] See, e.g., the essays in Sinclair et al. [2006].

[6] An immediate issue is that there is a difference between the boundary between mathematics/non-mathematics and the boundary between good/bad mathematics. It is surely a datum that demonstrably trivial or ugly or inelegant or uninteresting theories still count as mathematics, and so, if this view is to be made plausible, something must be said to finesse such counterexamples.

> 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.

I take Hardy not to be claiming here that ugly mathematics is not mathematics altogether, but rather that there is something normatively or evaluatively deficient about it. More generally, on such a view, the full account of what makes a piece of mathematics *good* or *valuable*, or what constitutes *success* in mathematical inquiry, turns upon aesthetic factors.

Finally, aesthetic considerations might be necessary to explain the development of mathematics. Wigner held a version of this view, claiming that the subject was developed, at least in significant part, on the basis of mathematicians' aesthetic judgments. Another proponent is von Neumann [1956: 2062], who claims that

> [the mathematician's] criteria of selection and also those of success are mainly aesthetical … One expects a mathematical theory not only to describe and classify in a simple and elegant way numerous and a priori disparate cases. One also expects 'elegance' in its 'architectural,' structural makeup.

For our purposes, we can remain neutral about the precise relationship between mathematics and aesthetics. What is important for getting the puzzle off the ground is the *prima facie* plausible claim that aesthetic factors play some such significant role in mathematics.

The other component of the explanatory challenge concerns the applicability of mathematics to empirical science:

> (II) Scientific practice draws substantially on mathematical concepts and structures.

Stated at this level of generality, the claim is a truism. In sharpening it, proponents of the puzzle of applicability have focused on two main ways in which science is informed by mathematics. Perhaps the most obvious is that scientific theories are *formulated* by using mathematical vocabulary. This raises deep questions, but, at a sufficiently high level of abstraction, mathematical vocabulary seems to play a kind of representational role, exploiting structural similarities between various mathematical objects or structures and various aspects of the physical world.[7]

A different role of mathematics is in the *discovery* of scientific theories.[8] A nice illustration is Dirac's equation and the prediction of positrons. A major challenge in the early days of quantum mechanics was that of finding a relativistic account of electrons, since prevailing approaches treated space and time in an unrelativistically asymmetric manner. Dirac had the idea of 'factorizing' the Klein–Gordon equation, obtaining:

$$\nabla^2 - \frac{1}{c^2}\frac{\partial^2}{\partial t^2} = \left(A\partial_x + B\partial_y + C\partial_z + \frac{i}{c}D\partial_t\right)\left(A\partial_x + B\partial_y + C\partial_z + \frac{i}{c}D\partial_t\right).$$

Any solutions require $AB + BA = \cdots = 0$ while $A^2 = B^2 = \cdots = 1$, which cannot hold in any of the usual number systems. Dirac realised, however, that this formal relation can be satisfied if $A, B,$ etc are *matrices*. This modification required a

[7] See, for instance, Pincock [2012]. Bueno and Colyvan [2011] emphasise the inferential role of mathematics, but are explicit [ibid.: 352] that they are building on, not repudiating, the representational account (their 'mapping view'). For the purpose of raising the puzzle, we can be neutral on the precise details.

[8] This is hinted at by Wigner [1960], but receives its clearest development by Steiner [1998] (a book that also contains illuminating discussion of many examples).

reconceptualization of the wave function as including four components (as opposed to Pauli's two and Schrödinger's one). This in turn generated novel solutions to the original equation, seemingly corresponding to 'negative' energy levels. On the basis of these solutions, Dirac predicted the existence of positrons ('anti-electrons')—a prediction which, remarkably, was later empirically borne out by Anderson in 1932. So, in this case, as in others like it, mathematics played an important role in the *generation* of a scientific theory, not just in its formulation.

(I) and (II) give rise to a *prima facie* challenge: together, they appear to be striking—in need of explanation. From (I), mathematics is a methodologically *a priori* discipline in which aesthetic considerations are essential—either constitutive of mathematics, or normative for identifying valuable mathematics, or necessary to explain the development of the subject. This in itself raises no obvious puzzle. But it is extremely surprising when combined with (II), for the mathematics that arises from this empirically disconnected methodology proves to be highly significant to natural science.

Consider, first, the role of mathematics in the formulation of scientific theories. One way in which this can come to seem extremely puzzling is that, in many cases, mathematics precedes applications. Take, for instance, Wigner's example of infinite dimensional Hilbert spaces, developed on purely mathematical grounds (seemingly very distant from applications), before becoming of crucial importance to quantum mechanics. This suggests that aesthetic criteria led mathematicians to devise structures that turned out to mirror significant aspects of the physical world. To use an evocative analogy, it would be rather as if artists had, led by their aesthetic sensibilities, conceived of beautiful abstract paintings, only for it to be discovered later that scenes closely resembling these paintings occur in some remote part of the galaxy.

The role of mathematics in scientific discovery is no less *prima facie* puzzling. Consider, again, Dirac's equation. This is a case where, in seeming miraculous fashion, mathematics led to a novel scientific theory, which was later experimentally vindicated. In taking the 'negative' solutions as physically significant, as opposed to mere artefacts of the presentation, Dirac extrapolated far beyond the available empirical evidence: and, what is more, he did so primarily on the basis of the mathematical elegance of the resulting picture.

It is hard to *argue* that the combination of (I) and (II) is striking, since necessary and sufficient conditions for 'strikingness' are notoriously difficult to formulate. But, in addition to the anthropological evidence that many intelligent (indeed, Nobel-prize-winning) commentators have found them to be so, our case shares some important features with paradigm examples of striking facts. One is that each of (I) and (II) renders the other *unexpected*, in the sense that their conjunction would be hard to predict in advance. Suppose that one is told that a certain practice is carried out in the armchair and is largely informed by aesthetic considerations. One is unlikely on that basis to have a high credence that it would prove highly significant to science! The other relevant point is that we have a link between two apparently disparate domains—on the one hand, the physical world, and, on the other, pure mathematics and aesthetic features (beauty, elegance, and the like) *prima facie* detached from the subject matter of science. In light of the disparateness of the domains, it is unsatisfying to accept that the link between them is merely coincidental; again, it cries out for explanation.

The rest of the paper aims to meet the challenge head-on. First, however, it is worth clarifying and responding to a couple of immediate reactions.

It bears emphasis that the challenge depends little on one's underlying view of mathematics. In stating it, no doctrine about mathematical objects, or truth, or realism, or epistemology was assumed—just that mathematics is applicable to science, while methodologically *a priori* and aesthetically informed. One might think that it can be disarmed if—as certain proponents of mathematical fictionalism hold—mathematics can *in principle* be eliminated from scientific discourse. This is a controversial claim, requiring the success of an ambitious technical program.[9] But, even if its success is granted, at best this addresses any challenge arising from the *representational* role of mathematics. Its role in the *discovery* of scientific theories is still salient, and still requires explanation. The challenge is therefore robust across many conceptions of mathematics.[10]

Similarly, the challenge does not obviously rely on any specific view of the nature of aesthetics. It might be thought that it arises only on a roughly subjectivist account, according to which aesthetic judgments are merely the projection of parochial and species-specific human attitudes.[11] But this is not so: even if one holds an 'objectivist' view, according to which aesthetic judgments track objective, mind-independent, aesthetic properties, it still remains to be explained why *these* properties—whose subject matter is *prima facie* distinct from that of natural science—prove relevant to empirical inquiry. The issue is most pressing on non-naturalist versions of the view, according to which aesthetic properties are distinct from any natural properties. But even naturalist versions, on which aesthetic properties are part of the natural order, still bear an explanatory burden: supposing that (say) 'beauty' picks out mind-independent natural property N, why should our tendency to develop mathematics with property N be conducive to its applicability? A specific conception of aesthetics does not obviate the need for an explanation; at best, it might be a starting point for providing one.[12]

Last, it might be argued that the fact that *some* aesthetically inspired mathematics is empirically applicable isn't enough to generate a puzzle; after all, what about all of the mathematics that does *not* find a use in applications?[13] For a real puzzle, the thought continues, we would to somehow quantify just 'how much' mathematics is applicable, and then show that it exceeds the 'quantity' of non-applicable mathematics to some striking degree. While there might be something to this response, it mistakenly conflates considerations that *undermine* the strikingness of our putatively puzzling facts with those that constitute a possible explanation. If the point is right, it doesn't make the initial fact of applicability any less striking; rather, it potentially explains it, if the quantity of pure mathematics produced is large and varied enough to make it likely that at least *some* is applicable by chance alone. With that said, such an explanation would in some sense be unsatisfying: ultimately, the success of mathematics would be explained by sheer chance. By contrast, the explanation that I'll develop in

[9] Field [1980] is the *locus classicus*.

[10] For further arguments that the challenge does not rely on a particular view of mathematics, see Colyvan [2001b].

[11] Steiner [1998] seems to believe that this subjectivist view is responsible for the force of the puzzle; Pincock [2012] attempts to disarm it by denying subjectivism. I believe that both are vulnerable to the point made in this paragraph.

[12] Thanks to the Editor for pressing me to clarify here. The explanation offered in section 4 is in fact neutral between different conceptions of aesthetics: since it relies only on a reliable correlation between aesthetic properties in mathematics and certain non-aesthetic properties concerning theoretical virtues, nothing further needs to be assumed about the nature of the aesthetic properties in question.

[13] Thanks to a referee for prompting clarification here.

the rest of the paper is more satisfying, because it shows why the applicability of mathematics is by no means accidental. It is to that explanation that we now turn.

3. Mathematical Virtues beyond the Aesthetic

The aim of this section is to reconsider the conception of mathematics, responsible for much of the force of the puzzle, as *a priori* and developed on the basis of aesthetic considerations. I'll argue that it is highly misleading to think of mathematical virtues—the features prized by mathematicians in the course of inquiry—as exclusively aesthetic and detached from the empirical world. In particular, I'll consider a virtue that I'll call 'interestingness' or 'seriousness', and I'll argue that many structures actually studied in contemporary mathematics and considered mathematically serious have their roots, if we are willing to look back far enough, in abstractions or generalisations of physical or otherwise empirically generated concepts. The subject matter of 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.

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 the famous passage cited in section 2 he expresses the view that beauty is a necessary condition for mathematics.

But, elsewhere in the same book, Hardy [1940: 88] appeals to another criterion for distinguishing between good and bad mathematics—namely, its *seriousness*:

> 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.

And indeed, for Hardy [1940: 90], 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.

For Hardy, seriousness is an additional constraint upon good mathematics. Note that beauty and seriousness might, but need not, line up with each other: it is possible for some mathematics to have significant and illuminating implications for other mathematics, without 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 to offer a friendly amendment so as to better capture the spirit of the idea.

Hardy claims that it is enough that a certain piece of mathematics sustain connection with a *suitably large* number of other pieces of mathematics. But surely the sheer number of connections does not matter. For, as Hardy points out, there exists much 'trivial' or unimportant mathematics—probably as much as, if not more than, there is important mathematics. Surely we would not say that results somehow connected 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*. Borrowing 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, the status of being 'foundationally serious'. How might a piece of mathematics attain such status?

The great algebraic topologist Saunders Mac Lane [1986: 6] 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.

There are two central points made here by Mac Lane: first, the conceptual roots of *elementary* mathematics grow out of, so to speak, certain basic modes of understanding and interacting with the physical world; and, second, that *contemporary* mathematical knowledge—even in its abstract, axiomatic form—can ultimately be traced back to these fundamental sources. The picture that 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 processes or phenomena arising within the natural world or our interaction with it, and then become available for autonomous study without explicit reference to the motivating examples.[14] A similar viewpoint is propounded by Bourbaki, the notoriously formalistically-minded French collective of mathematicians. In one sense, their view is an orthodox form of structuralism, according to which mathematics is the study of abstract structures. But, for our purposes, the interesting part of their view is that not all structures are created equal: the organizing centre 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 these examples is that a plausible case can be made that each originates out of a desire to describe—albeit in a distinctively mathematical, general, and abstract way—certain features of the natural world.

A natural idea, then, is that these basic structures, arising in a fundamental way from the empirical study of nature, play precisely the role of 'foundationally serious' mathematics, adverted to above. In short, the idea is that there are mathematical virtues other than aesthetic ones, such as importance or seriousness; furthermore, mathematics is viewed as important or serious to the extent that it illuminates our understanding of these basic structures, or that it illuminates our understanding of mathematics that illuminates our understanding of these structures, and so on.

[14] See Maddy [2008] for a historically sophisticated and highly developed account along these lines.

I take it that this is a plausible explanation of why, say, the Langlands program is 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.[15]

If all of that is right, it would be highly misleading to view mathematics as shaped solely by aesthetic considerations; seriousness also plays a significant role. Writers like Wigner and Weinberg tend at times to suggest that the role of pure mathematics is the development of formal descriptions of structures, on purely aesthetic grounds and isolated from empirical concerns, some of which then, mysteriously, turn out to be applicable. But, on the alternative view that I have been urging here, this is a mistake: as Mac Lane emphasises, from its very beginnings, mathematics has been motivated by the desire not only to describe various aspects of the physical world, but also to provide systematic formal theories of various human activities and operations within it. These points in themselves go a considerable way towards responding to our initial explanatory challenge.

4. Aesthetic Judgments, Theoretical Virtues

With all of that said, however, some of the original puzzlement may persist. After all, there are still many cases (for instance, Wigner's example of the application of infinite dimensional Hilbert spaces in quantum mechanics) where physics found a use for mathematics that really did primarily arise on aesthetic grounds, with no apparent motivation stemming from the major empirical structures or human activities just discussed. It is not at all obvious why mathematics of this kind should be relevant to applications.

The aim of this section is to meet this residual puzzle by clarifying the role of aesthetic virtues and judgments in mathematics. To do so, we'll examine two case studies of aesthetically appealing mathematics—Galois theory, and a proof of the Kac-Erdős theorem within probabilistic number theory. The main claim to be defended is that the aesthetic properties to which mathematicians commonly appeal are far less straightforwardly aesthetic than one might presume. More specifically, I will argue that positive aesthetic judgments in mathematics, in a wide range of cases, are correlated with more general *theoretical virtues*—properties like simplicity, internal coherence, surprisingness, unificatory power, explanatory depth, epistemic tractability, fruitfulness, generalisability, importance, parsimony, and so on. In light of this connection, the applicability of aesthetically virtuous mathematics is not so puzzling after all.

4.1. Galois Theory

Galois theory is commonly regarded as one of the most beautiful theories in mathematics. Stephen Weintraub [2008: ix] writes, in a preface, that

[15] The Langlands program is viewed as being of paramount importance within mathematics, wtih dozens of Abel Prizes and Fields Medals having been awarded for progress within it. See Gannon [2006] for a readable account.

> 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.

Similarly, Ian Stewart [2004: 135] calls the Fundamental Theorem of Galois theory one of 'the most beautiful results in mathematics'. Let's briefly examine it, before drawing some lessons.[16]

The roots of Galois theory are found in one of the oldest mathematical endeavours—namely, finding solutions to polynomials, equations of the form

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

with t a variable and the coefficients a_i elements of some field K.[17] The highest power of t is called the degree of the polynomial.

Which polynomials have solutions in radicals—that is, are built up from the coefficients a_i using the operations of addition, subtraction, multiplication, division, and n^{th} roots? At the time of Galois, the quadratic formula—that is, for polynomials of degree 2 (and bane of schoolchildren everywhere),

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

—was well-known, as were much more unwieldy cubic and quintic formulas due to Tartaglia and Ferrari. But polynomials of quintic degree and higher remained, in general, unsolved.

Take a polynomial $f(t) = a_0t^n + a_1t^{n-1} + \ldots + a_{n-1}t + a_n$. We can construct a new field, L (perhaps identical to K, but not always so), called the 'splitting field' of $f(t)$, with the special property that L contains precisely enough additional elements to fully factorize f—in other words, all possible solutions of $f(t)$. Galois theory studies the relation between the original field K and the splitting field L. Surprisingly, this captures a large amount of information concerning the structure of solutions to $f(t)$.

The key is to think about all of the possible permutations or rearrangements of the solutions of f.[18] In fact, for any field extension, all permutations of this kind form a group—the Galois group $G_{L:K}$.[19] One of Galois's main insights was to notice there is a one-one correspondence between

- the subgroups of the Galois group $G_{L:K}$ and
- the subfields of L containing K (that is, the M such that L:M:K).

This correspondence is specified by associating each subgroup H of G with the set $H^\dagger$ of all elements of L fixed by all of the automorphisms in H (which turns out to be a subfield of L). Conversely, each subfield M of L is associated with the group $M^* := G_{M:K}$—that is, the set of automorphisms of K that fix all of the elements of M. Furthermore, if L:K satisfies certain mild conditions (normality and separability), then in some sense no

[16] For a more detailed exposition, see Stewart [2004], and see Kiernan [1971] for a comprehensive history.
[17] A field is a set with recognisable analogues of addition, subtraction, multiplication, and division—a generalisation of the structure common to the rational numbers, the real numbers, and the complex numbers.
[18] Technically: the automorphisms on L that hold K fixed.
[19] 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.

information is lost by looking at things in terms of the subgroups of $G_{L:K}$, for $*$ and $\dagger$ are inverses: $(M^*)^\dagger = M$ for each subfield M of L.

The details of this correspondence are sometimes called the Fundamental Theorem of Galois theory. Using it, we can prove the deep result that polynomials of degree 5 and above cannot, in general, be solved in radicals.

Put in terms of field extensions, a polynomial $f(t)$ over K is soluble if there is a series of fields

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

where 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 following induced series of *groups*?

$$L^\dagger = G_0 \subseteq G_1 \subseteq \ldots \subseteq G_{n-1} \subseteq G_n = K^\dagger$$

Now, there is a notion of 'division' of groups that is applicable here[20], and it follows that each of the G_i/G_{i+1} is Abelian: that is, the order of permutations does not matter. And, conversely, it turns out that if a polynomial generates such a series (with Abelian quotients) then it is soluble in radicals! Fundamentally, the reason 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 that such an element is adjoined, the corresponding quotient group is always 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 (that is, 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 as a series with Abelian quotients. But they are the exception: for $n \geq 5$, it's possible to show that S_n *cannot* be written in that form. And, for this reason, quintics (and higher) are, in general, insoluble.

So much for the whirlwind tour of Galois theory; I hope that I have conveyed its flavour. Several features are worth bringing out in more detail.

The core of Galois theory is the correspondence introduced in the Fundamental Theorem. This allows us to translate difficult questions about the structure of solutions to a given polynomial into questions about the structure of its Galois group. The problem is thus simplified in many ways—some subtle, some obvious. Groups are in many ways simpler algebraic objects than polynomials and field extensions, and are much better understood (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 analyse its solubility. It provides, as is sometimes said, the natural setting for conceiving of the problem.

[20] Because G_i is normal in G_{i+1}, we can form quotient groups.

Not only does Galois theory allow us to *prove* that the quintic and higher cannot, in general, be solved; it also *explains* why this is the case. It is generally recognised, both among mathematicians and among 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. But, on any plausible account, the Galois-theoretic treatment of the insolubility of the quintic is 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* (what is so special about *quintics*?) is almost entirely dispelled.

Related to issues of explanation, Galois theory is described by Stewart [2004: vii] as a 'showpiece of mathematical unification'. There are two senses in which this is true. First, it brings together ideas and machinery from many different parts of the subject, and it leads (see below) to many more connections still. But, second, the theory allows for its subject-matter—polynomials—to be understood in a unified way. Such unity is by no means guaranteed in mathematics. 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 find.[21] There is no *a priori* reason that polynomials should have turned out to be so susceptible to a unified treatment; that they do is itself a remarkable fact.

The theory introduces, in natural ways, ideas that are fruitful when further developed. David Corfield [2003: 205] helpfully distinguishes five 'degrees' of fruitfulness, when a piece of mathematics

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

Galois theory clearly succeeds in all five respects except perhaps the last (the issue here concerns simply the directness of its applicability). 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 assimilating questions concerning the solubility of polynomials to algebraic questions concerning the structure of groups, allowing group-theoretic methods to resolve algebraic questions.

(3) Galois's work precipitated a radical reorientation of algebra: before, it concerned primarily the solution of equations, but afterwards it encompassed the study of 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 in developing the theory of solubility.

[21] Klainerman [2010: 279] 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.'

(4) The range of mathematics directly or indirectly motivated by Galois theory is vast. To pick just one example, consider the generalisation (by Sophus Lie) of Galois theory applied not to *polynomial* equations but to *differential* equations—that is, equations of the form $a_0D^ny + a_1D^{n-1}y + \ldots + a_ny$, where D is a differential operator. The groups associated with such equations—Lie groups—are widely studied in mathematics and extensively applied in physics. Lie groups admit a manifold structure and capture the idea of *continuous* symmetry (for instance, the rotation group of a sphere). Consequently, they are of great interest in many physical applications that involve continuous dynamical systems.[22]

4.2. Probabilistic Number Theory

Our second example is taken from probabilistic number theory. Despite the reputation of number theory as one of the purest branches of mathematics, probabilistic number theory is in fact highly applicable, most notably within cryptography. For example, the effective security of many contemporary cryptographic algorithms depends on the fact that tests for primality and prime factorisation are computationally expensive. In order to obtain results about the expected running time of these operations, accurate methods for estimating the distribution of prime numbers (as well as, for instance, so-called 'smooth' numbers, which possess only small prime factors) are needed. One of the key results in providing such estimates is the Kac-Erdős theorem.

As is well known, every natural number can be uniquely factorised into primes. Let $\omega(n)$ be the number of prime factors of n. The Hardy-Ramunujan theorem tells us that, for almost all integers[23], and for any real-valued function ψ that tends to infinity as n tends to infinity,

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

The Kac-Erdős theorem can be seen as a natural generalisation of this result. Roughly, it states that the probability distribution of

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

is a normal distribution.

One reason why this proof is so interesting for our purposes is because it is explicitly discussed by Timothy Gowers [2000], in a lecture on the importance of mathematics, as a beautiful proof of a beautiful result. Better still, Gowers takes the time to give a number of reasons why he finds the result to be so beautiful. Here are the five reasons that he gives, with some added commentary.

First, perhaps least interestingly, Gowers notes that the very shape of the distribution —a bell curve—is itself aesthetically pleasing.

Second, Gowers suggests that the theorem possesses an appealing *simplicity*. One reason is the role played by normal distributions, which arise extremely naturally within statistics and are the subject of extensive study because they govern a great many statistical phenomena. In fact, there is a well-defined sense in which normal

[22] See, that is, Gilmore [2008].
[23] That is, if $g(m)$ is the number of integers less than m for which the inequality fails, then $\frac{g(m)}{m} \to 0$ as $m \to \infty$.

distributions are as simple and as natural as possible: the central limit theorem states that, in certain (mild but laborious-to-specify) conditions, a sufficiently large number of independent random variables will give rise to a normal distribution. In addition to its simplicity, the theorem is also an example of mathematical unification, by unifying the theory of prime numbers (arising from pure number theory) and the theory of normal distributions (arising from statistics).

Third, the theorem is unexpected. As Gowers [ibid.: 19] puts it,

> 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.

Fourth, the phenomenon uncovered is not one that could have been appreciated by 'brute force' means: the computational power required to calculate large enough $\omega(n)$ to give a reasonable and recognisable approximation to a normal distribution is far beyond feasible limits. Thus, the result is one that is, in a sense, attributable purely to theory alone: the underlying pattern could only have been appreciated via ingenious theorising, and not, realistically, via experimental or inductive evidence.

Fifth, the *proof* of the theorem is [ibid.] 'very satisfying'. He summarises 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.) …
>
> Step 2. Therefore, most prime factors of most numbers near n are small. This follows because a significant 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.

One reason why the proof is so satisfying is its susceptibility to being described in such simple shorthand terms. Naturally, a fully admissible version of the proof would 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 precisifications to the ears of a number-theorist —allows the key ideas of the proof to be easily surveyed and its core strategy to be understood. No doubt, there is the possibility of being fooled into a false sense of understanding if one does not fully realise the complications added by the requirement of rigour. But, having worked through the proof, it is hard not to regard this sketch as encapsulating it: all of the essential conceptual moves are there.

A final reason, I take it, why the proof is satisfying is that it is genuinely explanatory: it shows not just why a normal distribution happens to arise in the foundations of the theory of prime numbers, but furthermore why such a distribution *is to be expected* in light of other known facts about the primes.

4.3. Aesthetic Properties and Theoretical Virtue

With the examples in hand, let us draw some lessons. The first is simple: positive aesthetic appraisals in mathematical contexts are *reliably correlated with* the presence of a number of other properties. As we saw, Gowers explicitly includes such considerations as simplicity, unificatory power, suprisingness, explanatory depth, and epistemic tractability as contributing to the beauty of the Erdős-Kac theorem that he discusses. And the brief tour of elementary Galois theory suggests that, if one seeks to explain why the theory is attractive or elegant or beautiful, the story appeals to similar features—the surprising Galois correspondence between fields and groups, the capacity of the theory to motivate and explain the answer to deep and central mathematical questions via satisfying explanatory proofs, its unification of several key mathematical tools and ideas, the importance of its consequences, or its immensely fruitful consequences in the future development of mathematics. What is more, the correlation is not plausibly a mere accident or matter of luck: as we have seen, when attempting to *explain* or *rationalize* their positive epistemic judgments, reflective mathematicians appeal to properties like those just identified. There is every reason to believe, therefore, that the correlation is robust in a way that would lead us to expect the presence of these properties in a wide range of cases of positive epistemic appraisal in mathematics.[24]

So, aesthetic judgments in mathematics are reliably correlated with a host of non-aesthetic properties. It is highly significant that those we have seen arise—simplicity, unificatory power, explanatory depth, epistemic tractability, surprisingness, the ability to forge connections between seemingly disparate subject-matters, fruitfulness, etc.—are precisely those often discussed in the philosophy of science, confirmation theory, and more recently within metaphysics too, under the heading of 'theoretical virtues'. The relevance to our original explanatory challenge should by now be becoming clear. At the outset, the sense of mystery arose largely from the concern that applicable mathematics essentially involves aesthetic considerations of a sort detached from the physical world and scientific inquiry. But no analogous concern can arise for theoretical virtues, for, although their precise role is contested, it is almost impossible to deny that they play a legitimate and central role in scientific inquiry.

On one prominent cluster of views, associated with a broadly realist perspective, theoretical virtues are both truth-conducive (in that their presence makes a theory more likely to be true) and relevant to issues of confirmation and rational theory choice (in that, when faced with a choice of theories to believe or develop, it is rational—there are 'genuinely epistemic' reasons—to prefer theories possessing these features).[25] But even those who reject this strong realist view nevertheless tend to agree that theoretical virtues are important and desirable within science, even if they

[24] This correlation might itself stand further explanation. One possible, relatively deflationary, approach might look for psychological reasons why we tend to find properties like simplicity, explanatory depth, importance, fruitfulness, to be beautiful or elegant when manifested within mathematics. But perhaps a deeper explanation is possible: for instance, perhaps mathematical beauty or elegance is *grounded in* properties like simplicity, explanatory depth, importance, fruitfulness, etc.; or perhaps aesthetic judgments serve as a way of *expressing* the presence of such properties. While these suggestions are intriguing, evaluating them would require a far more detailed discussion of aesthetic properties and judgments in mathematics than I am able to provide here. For the purpose of resolving the explanatory challenge, the fact of a reliable correlation is enough.

[25] See, for instance, Psillos [2005]. 'Epistemic' here means something like 'truth-directed', as opposed to 'merely pragmatic': it is not meant to cover other notions of epistemic appraisal, such as ones involving epistemic responsibility or deontological considerations (although it is an open question how these relate to truth-directedness: for discussion, see Alston [2005]). Thanks to the Editor for pressing me for clarification on this point.

ultimately play only a pragmatic or heuristic or guiding role in the process of scientific discovery.

I do not mean to deny that many further important questions arise. How are theoretical virtues best understood? How are they to be weighed against one another? How (if at all) are they related to truth? Any attempt to answer these questions would be beyond the scope of this paper. Perhaps they might even generate additional explanatory challenges—to explain why these features are truth-conducive, or relevant to rational theory choice, or even important within science only in a merely pragmatic sense. But even if such questions are felt to persist, we have come a long way from our original challenge. The seeming mystery of the applicability of an *a priori* discipline driven (in part) by aesthetic considerations has, at the very least, been reduced to a more general—and, there is every reason to think, a more tractable—cluster of issues.

5. Conclusion

We began with a puzzle of applicability arising, in large part, from a conception of mathematics according to which it is methodologically *a priori* and largely informed by aesthetic judgments. I have not argued against that conception's central claims. What I have tried to offer are some major qualifications, intended to reconcile it with the empirical origins of many pure mathematical concepts and the ways in which aesthetic judgments systematically track the pursuit of theoretical virtue. If I am right, the resulting account does much to demystify the use of mathematics in the formulation and discovery of scientific theories. Mathematics is not merely an armchair, aesthetic discipline; rather, it provides science with theories, concepts, and techniques that relate—in fruitful, simple, unifying, and explanatory ways—to structures found in the physical world. It is not so unreasonable, after all, that it finds outstanding success in applications.[26]

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