Viewpoint

Solving Wigner's Mystery: The Reasonable (Though Perhaps Limited) Effectiveness of Mathematics in the Natural Sciences

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In 1960 the physicist Eugene Wigner published an influential article on 'The unreasonable effectiveness of mathematics in the natural sciences'. I counter the claim stated in its title with an interpretation of science in which many of the uses of mathematics are shown to be quite reasonable, even rational, although maybe somewhat limited in content and indeed not free from ineffectiveness. The alternative view emphasizes two factors that Wigner largely ignores: the effectiveness of the natural sciences in mathematics, in that much mathematics has been motivated by interpretations in the sciences, and still is; and the central place of theories in both mathematics and the sciences, especially theory-building, in which analogies drawn from other theories play an important role. A major related feature is the desimplification of theories, which attempts to reduce limitations on their effectiveness. Significant also is the ubiquity and/or generality of many topics and notions in mathematics. It emerges that the connections between mathematics and the natural sciences are, and always have been, rationally although fallibly forged links, not a collection of mysterious parallelisms.

Wigner's Thesis

Wigner states as his main thesis 'that the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it'; for example, 'The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand or deserve' [1960, 2,14]. By way of illustration he recalls a story about two friends studying population statistics by means of the normal (or Gaussian) distribution and being bewildered by the presence in the analysis of π: 'surely the population has nothing to do with the circumference of the circle' [p. 1]. He judges this mystery to be 'plain common sense' and does not discuss it again in the article.

Wigner's article has been cited especially by scientists and mathematicians on many occasions, with approval or at least without demur; some related articles have appeared.[1] Philosophers have also considered the article, and some have largely accepted the force of the argument.[2] One should note that most of the established philosophies of mathematics favoured by philosophers have aimed to grasp mathematical theories *already developed* rather than to address theory-building. There [Pólya 1954a, 1954b] is much more promising, with his masterly survey of 'plausible reasoning' and the dynamic relationships between theorems and proofs; however, he focusses largely upon pure mathematics. In my approach, which in general terms follows Pólya, the unreasonableness will largely disappear, but doubts are raised over effectiveness. The discussion is set at the level of formed cognition and theory-building; I

[1]For example, the rather ineffective [Hamming 1980]. In a review of Wigner's article for the *Zentralblatt für Mathematik*, [Kiesow 1960] welcomed a 'brilliantly written essay'. *Mathematical Reviews* did not cover it. I do not attempt a full bibliography of reactions to Wigner's article, but see the Wikipedia online entry on it.

[2][Colyvan 2001] sees Wigner's 'puzzle' as a conundrum for some prevailing philosophies of mathematics, within which mathematics is 'developed primarily with aesthetic considerations in mind' [p. 267]. [Sarukkai 2005] emphasizes the language of mathematics as such rather than mathematical theories, which of course need language for expression. His account of intuitionism is not happy, and both authors misrepresent Hilbert as a formalist.

In a study of the epistemology of questions and answers, [Hintikka 2007] sees Wigner's thesis as exemplifying *a priori* knowledge, and associates mathematics especially with his 'function-in-extension', which plays the central role of linking who/what/where/. . . questions with the proposed answers. While supporting his philosophical enterprise, I am not persuaded that apriority captures Wigner's thesis, nor that the function need be placed in mathematics rather than in the pertaining logic just because functions (and functors) play major roles there.

do not address the interesting subject of the psychology of mathematical creation.

Several of my points have been made in earlier discussions of Wigner's article, but to my knowledge nobody has taken as central the two theses presented in the next section. In general terms I follow the spirit of French [2000], who nicely defends the reasonableness of one particular kind of application. This paper is noted in a section below that is devoted to examples of the approach adopted. These are largely historical ones, as that is my background, but their potency is not thereby lost; for if mathematics *is* unreasonably effective in the natural sciences, then it always has been so, or at least for a long time. In any case, we can surely learn from our past masters. A solution of the mystery about π follows in the final section.

Two Counters

First, in a part of his article called 'What is mathematics?' Wigner asserts that while elementary concepts in mathematics (especially geometry) were motivated by 'the actual world, the same does not seem to be true of the more advanced concepts, in particular the concepts which play such an important role in physics' [1960, 2]. In reply I build upon a large truth coming strongly from the history of mathematics, quite counter to his claim; not only elementary theories and branches of the subject were (and are) motivated by some problems found in the actual world, including on occasion sciences outside the physical ones, but so *equally* were (and are) the more advanced theories. Much mathematics, at all levels, was brought into being by worldly demands, so that its frequent effectiveness there is not so surprising.

It is necessary to emphasise this feature of mathematics, because, especially since the middle of the 19th century, a snobbish attitude developed among substantial parts of the growing mathematical community to prefer pure over applied mathematics ('dirty mathematics' to Berliners, for example). As a consequence the impression has grown that mathematics is always, or at least often, developed independent of the natural sciences, or indeed anything else; thus its undoubted effectiveness is indeed mysterious.

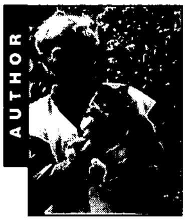
Secondly, in a part of his article called 'What is physics?', Wigner emphasises the role of observing regularities in the world for formulating 'laws of nature' (using Galileo Galilei's law of fall as an example), which nevertheless are subject to 'probability laws' because of our incomplete knowledge [1960, 3–6]. This point about regularities is valuable, and should form part of a wide-ranging analysis of theories *as such*, especially their initial formation and later elaboration. These processes are *central* features of the development of mathematics pure or applied, and indeed of any science, and so they form the basis of my own approach.

The status of theories depends upon whether one subscribes to a philosophy of science that treats theories as mere devices for calculation or prediction (instrumentalism, conventionalism, some kinds of positivism), or to a philosophy that pays attention to the (apparent) explanatory power of theories (inductivism, fallibilism, some kinds of Platonism).[3] These differences matter, because the criteria for (in)effectiveness vary between the two kinds of philosophy. The discussion that follows will apply to both of them, as does Wigner's article.[4]

Developing Theories in the Presence of Other Theories

> In science as in everyday life, when faced by a new situation, we start out with some guess. Our first guess may fall wide of the mark, but we try it and, according to the degree of success, we modify it more or less. Eventually, after several trials and several modifications, pushed by observations and led by analogy, we may arrive at a more satisfactory guess.
>
> Georg Pólya [1954b, 158]

When forming a problem and attempting to solve it, a scientist does not work in isolation: he is operating in various contexts, philosophical, cultural and technical, in some cases con-



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[3]On the different philosophies of science see, for example, [Dilworth 1994].

[4]Wigner ends with a rather strange section on 'the uniqueness of the theories of physics' in which he stresses 'the empirical nature' of laws of nature and considers cases where '"false" theories' give 'alarmingly [sic] accurate descriptions of groups of phenomena' [1960, 11, 13]. Since much of that discussion focuses upon some specific physical phenomena and the possibility of reconciling quantum mechanics with relativity theory, it does not centre on the role of mathematics; so I leave it alone.

sciously recognised but in others intuitively or implicitly adopted. *Thinkers develop theories in the presence of other theories already available as well as by observations of the actual world, and can be influenced positively or maybe negatively by these theories.* My approach complements the theory of 'abduction' of C. S. Peirce, where theory-building is considered largely in terms of reactions to (new) observations. (Wigner notes Peirce in [1960, 2, 4]). In both studies, *it is the world of human theories that is anthropocentric, not the actual world.*

In the discussion that follows, 'notion' is an umbrella term covering not only objects such as function and matrix but also concepts such as convexity, systems of symbols, and proof methods, that occur in mathematical theories; these latter are often called 'topics' when they include individual theorems or algorithms as well as larger-scale bodies of results. The distinction between topic and notion resembles that made by phenomenologists between a part and a moment of a whole; for example, between the third chapter of a certain book and the price of that book [Smith 1982].

Assume that the creator of a new theory S2 was aware of another theory S1 already available and drew upon it in some way; this does not preclude the possibility that he independently recreated S1 on his way to S2. Four categories of relationship may obtain between S2 and S1. Analogies are mentioned here, and analysed in some detail later.

Category 1: Reduction. S1 not only actively plays a role in the formation and development of S2, but the theorist also hopes to *reduce* S2 to the sphere of activity of S1. Analogies now become special cases of S1 in S2; and S1 may be seen as an extension of S2, maybe even a generalisation of it. There are also reductions within a mathematical topic, when it is shown that a particular kind of object may be reduced to a special kind of itself without loss of generality.

Category 2: Emulation. S1 actively plays a role in the formation and development of S2, with resulting structural similarities, but reduction is not asserted or maybe even sought. Analogies are just similarities; for example, S2 uses (close versions of) some of the mathematical notions already deployed in S1.

Category 3: Corroboration. S1 plays little or no role in the formation and development of S2; but the theorist draws upon similarities to S1, maybe including structural ones, to develop S2 further and thereby enhance the measure of analogy between S2 and S1.

Category 4: Importation. S1 is imported into S2 basically intact, to serve as a mathematical tool. Thereby S1 and S2 have certain notions in common, creating analogies; and if some of them are of sufficient generality to surpass the spheres of activity of both S1 and S2, then they are *instantiated* in S1 and S2.

Theory S2 may well have several S1s of various kinds in its ancestry. What relationship does it hold to its principal parents? The word 'revolution' is often used to refer to substantial changes of theory, but in my view excessively and without adequate allowance for the different kinds of relationship that may obtain. I propose the following tri-distinction [Grattan-Guinness 1992a].

Category 5: Revolution. Adoption of S2 means that S1 is replaced, perhaps discarded or at least much reduced in status to a prediction device, with much of its explanatory power abandoned.

Category 6: Innovation. S2 says some quite new things about which S1 was silent, or at most treated only some special cases. Replacement will occur, for example when S2 is preferred over S1 in certain circumstances, but it is not the main feature.

Category 7: Convolution. In its development, S2 exhibits both old and new (sub-) theories; S1 and S2 wind around each other, showing both old and new connections thereby mixing elements of the replacement and innovation.

It seems that convolutions are the most common relationship to be found between old and new theories, with innovations and revolutions as opposite extremes; thus the tridistinction is more of degree than of kind. A very widespread use of convolution occurs when a mathematician takes some existing piece of mathematics (of any kind) and modernises parts of it in some ways before embarking on his new work or while doing so; I call this use of old mathematics 'heritage', to distinguish it from its historical analysis [Grattan-Guinness 2004]. A nice example is the 'genetic approach' to the calculus given by Toeplitz [1963], a heritage that also exhibits historical sympathy.[5]

Some Basic Topics and Notions in Theories

> In the demonstrative sciences logic is used in the main for proofs—for the transmission of truth—while in the empirical sciences it is almost exclusively used critically—for the retransmission of falsity.
>
> Karl Popper [1972, 305]

We consider now some of the main topics and notions that are invoked in the application of mathematics to the natural sciences. They can obtain also within mathematics, between different branches of the discipline and/or parts of the same one; I shall not pursue this feature here, but I note that it increases the content of the mathematical theories involved, and thereby the potential measure of their effectiveness in applications. A significant part of so-called 'pure' mathematics is applic*able*, carried out without any stated applications but with a clear potential there: the various kinds of solution of differential equations are a prominent example.

Table 1 provides some significant topics, notions, and strategies that help in theory-building to produce some sort of convoluted theory out of previous theories. None of the lists in the three columns is meant to be complete (especially not the first one), though every item is noteworthy. Apart from a few groupings in the columns, the order is not significant; and only one connection by row obtains. In several cases the opposite notion is also to be noted (for

[5]However, for Wigner 'It is absurd to believe that the existence of mathematically simple expressions for the second derivative of the position is self-evident, when no similar expressions for the position itself or for the velocity exist' [1960, 11]. Is this strange remark some allusion to Newton's second law?

Table 1. Some topics, notions, and strategies used in mathematics and the natural sciences

Topics from mathematics	Notions from mathematics	Notions from the sciences and/or the actual world
Matrices	Linearity	Space
Determinants	Generalisation	Time
Arithmetic of real numbers	Convexity	Force
Common algebra	Equality, inequality	Energy
Complex numbers, analysis	Ordering	Mass, weight
The calculus	Partitioning	Causality
Functions, functors	Approximation	Continuity
Series	Invariance	Optimisation
Differential equations	Duality	Regularity
Theory of limits	Boundary	Notion of limits
Set theory and the infinite	Recursion	Conservation
Potentials	Operators	Equilibrium, stability
Mathematical statistics	Combinations	Discreteness
Stochastic processes	Bilinear, quadratic forms	Symmetry
Probability	Dispersion, location	Analogy
Topology	Regression, correlation	Periodicity
Mechanics	Nesting	Simplicity, complexity
Theory of equations	Mathematical induction	Generality
Group theory	Proof by contradiction	Randomness
Fields (and other algebras)	Superposition	Identification
(Non-)Euclidean geometries	Structure	Abstraction
Vector algebra, analysis	Axiomatisation	Taxonomy

example, disequilibrium from equilibrium). Those in the third column can be manifest within mathematics also.

Ubiquity and the Role of Analogies

Analogies (and disanalogies) between theories play a very significant role in the reasonable effectiveness of mathematics in theory-building; in particular, in the second way (emulation) of deriving S2 from S1 listed above.[6] Two such theories have some mathematical notion M in common, which therefore is an invariant relative to S1 and S2; for example (which is given a context later), both heat diffusion and acoustics use Fourier series.

A major source of the importance of analogies is that all of these topics and notions are *ubiquitous*, in mathematics and/or in the actual world; hence *lots* of analogies may be tried, and the successful ones help to explain the 'uncanny usefulness of mathematical concepts' [Wigner 1960, 2]. We can also assuage the puzzlement of Steven Weinberg that mathematicians have often produced theories before the physicists [Mathematics 1986, 725–728, mentioning Wigner]: the mathematicians thought up these theories in specific contexts using various ubiquitous topics and notions, which physicists *then* found also to be effective elsewhere.

In addition to analogies between S1 and S2, each theory (I take S1) will have analogies with the pertinent mathematical notion M. A dual role obtains for M: both to be correctly developed as mathematics, and to make sense at some level of detail in S1. The level to which the similarity holds between M and S1 measures their common *analogy content*; for example, it increases if S1 not only uses integrals M but also interprets them as areas or as sums. Analogy content can be modest; for example, when an abstract algebra (lattices, say) M is imported into S1, the analogy content between M and S1 may well be limited to the lattice structure.

Kaushal nicely exhibits ubiquities with lists of scientific contexts in which certain mathematical equations and functions arise: for example, the exponential decay function, and the form $(a-b)/c$ [2003, 60,75; see also pp. 52–57, 67,85]. Pólya presents several simple examples from applications that draw upon analogy [1954a, chs. 9–10]. Knobloch [2000] reviews some cases of analogy from the early modern period.

Examples of Theory-Building

Let us now take some further examples of these seven categories and the table of notions working in harness, not necessarily oriented around analogies. Among importations of elementary mathematical theories, arithmetic has been deployed since ancient epochs, trigonometry and Euclidian geometry for a long while, and common algebra since its innovation by the medieval Arabs. The examples that follow come from more modern times and mostly from more advanced mathematics: I

[6]The philosophy of analogies is not yet well developed. The most extensive account is given in [Kaushal 2003, chs. 3–6]; see especially his synoptic table illustrating 'the contents of a structural analogy' on p. 93. In the rest of his book he considers their use in the humanities and in the Hindu religion. [Pólya 1954a] stresses analogies, mainly in pure mathematics. [Steiner 1998] draws quite a lot on them, partly as a reply to [Wigner 1960]; he also advocates an anthropocentric standpoint. My own approach, based upon 'structure-similarity', is sketched in [Grattan-Guinness 1992b].

have chosen ones with which I am fairly familiar, and which collectively illustrate the variety as much as possible in a limited space. It is impossible to cite the original sources for these examples or give their full contexts in detail: short surveys of all the branches and topics of mathematics involved are to be found in the encyclopaedia [Grattan-Guinness 1994]. The reader will be able to construct lots of further examples from his own knowledge.

In these examples enough of the pertaining science was already available when the mathematicisation described took place, and the mathematics and science were competently handled. Neither property holds in general; in particular, the *simultaneous* development of mathematics and science in a theory-building context is a central feature of mathematical modelling. To reduce complications in the presentation, I have reluctantly avoided cases where major roles are played by notations and notational systems, or by diagrams; they deserve studies of their own. Out of respect for my ignorance, I have not offered examples from the life sciences or medicine.

Among the notions. Inequality has been much underrated as an importation [Tanner 1961]. It is at the centre of theories such as thermodynamics, mathematical economics, (non-linear) programming, certain foundations for mechanics; it also underlies many of the principal definitions and proofs in real- and complex-variable analysis and their uses, in connection with the theory of limits. In contrast, the high status of symmetry is well recognised by, for example, Wigner [1967], and also Weyl [1952] in general, and Mackey [1978] in the context of harmonic analysis.

Simplicity has obvious attractions to reductionists, and it grounds conventionalist philosophies; but when two notions are not close together in kind, the relation 'simpler than' between them requires complicated (sic) analysis.[7] (The use of 'simple' in 'desimplification' *is* of this close kind.) Sometimes it is also used to back up the empiricists, who cut their philosophical throats with Ockham's razor.

Linearity has been of especial importance, even though most of the phenomena observed in the actual world are nonlinear. It covers all manifestations of the linear form $aA + bB + \ldots$, finitely or infinitely. An example of a general kind is forming a problem as a linear differential, or difference, or difference-differential, equation, for many forms of solution are available or may become so; by around 1900 linearisation had become something of a fixation [Grattan-Guinness 2008a]. Linear algebra also brought with it, and to some extent motivated, a further wide range of applications, partly overlapping with that of the calculus.

Perturbation theory. An important example of both strands was initiated by Isaac Newton's innovative insight in celestial mechanics that the planets were 'perturbed' from their basic orbits around the Sun by their mutual attractions. The mathematics to express this situation was not difficult to state but horrible to manipulate, until in the 1740s Leonhard Euler had the superb insight that the distance (and other) astronomical variables could be converted into infinite trigonometric series of appropriate angles, which increased a uniformity of approach [Wilson 1980a]. A major use of this method occurred in *proving* that our planetary system was stable; that is, no planet would ever fly out of the system like a comet, or way off out of the ecliptic plane. Euler (and Newton before him) had been content to rely on God as the guarantee of stability; but in the 1770s J. L. Lagrange secularised the problem by truncating the expansions to their first terms, thereby expressing the motions in a system of linear ordinary differential equations with constant coefficients, which took finite trigonometric series solutions. By a marvellous analysis he made great progress towards establishing stability [Wilson 1980b]. Later work by others (including, surprisingly, A. L. Cauchy and Karl Weierstrass) played major roles in establishing the spectral theory of matrices (the theory of their latent roots and vectors) [Hawkins 1975]—a fine example of the reasonable effectiveness of the natural sciences in mathematics.

This example also exhibits both kinds of generality mentioned previously. First, Lagrange's analysis formed part of his development of analytical mechanics, in which he claimed, controversially, that dynamics could be reduced to statics. Secondly, it hinged on a brilliant transformation of the independent variables that (to use matrix theory, heritage style) reduced the square of the matrix of the terms in the differential equations to an antisymmetric one; the task was then to show that all the latent roots and vectors were real.

Contributions from Fourier. Euler's trigonometric series are not to be confused with Fourier series, which came back into mathematics in the 1800s. The context was heat diffusion, where Joseph Fourier innovated the first large-scale mathematicisation of a branch of physics outside mechanics, in a fine display of convolution [Grattan-Guinness and Ravetz 1972]. Importing the differential and integral calculus in its Leibniz-Euler form, he went for linearisation in forming his differential equation to represent the phenomenon. But in adopting the series as the preferred form of solution for finite bodies, he revolutionised the understanding of a mathematical theory that had been known before him but was disparaged for reasons (especially concerning its manner of representing a function) that he showed to be mistaken. However, he did not apply analogy to carry the periodicity of the trigonometric terms over to a wave-like theory of heat and promote a superposition of basic states, although such a theory was being advocated at that time; for him heat was exchanged with cold, each notion being taken as primitive, and he rejected explaining their nature in other terms such as waves or a substance (caloric). The term 'positivism' can fairly be applied here, as in the late 1820s, his work, was to be a great influence upon the philosopher Auguste Comte. For diffusion in infinite bodies, Fourier innovated around 1810 the integral named after him, where the wave reading does not obtain anyway. The physical interpretation of each term of the Fourier series was due especially to G. S. Ohm in

[7]For example, ponder the question of whether analytical mechanics is simpler or more complicated than Newtonian mechanics, and note that many pertinent points of view are involved. What sort of useful answer would result?

the 1840s, in the context of acoustics; it marks an increase in analogy content relative to Fourier.

Contributions from Thomson. Another of Fourier's early foreign supporters was the young William Thomson, later Lord Kelvin. In his teens in the early 1840s, he not only studied heat diffusion and the series method of solution but also quickly moved on to electricity and magnetism, and then to hydrodynamics [Grattan-Guinness 2008b]. He is a particularly interesting case to note, since he explicitly invoked analogies when passing from one topic to another. The similarities carried over not only at the mathematical level (similar differential equations and methods of solution) but also as physics (for example, from isothermal surfaces to equipotential ones). He was a prominent pioneer in potential theory, not only because of his own contributions but also for popularising George Green's innovative theorem of 1828 relating the internal organization of a solid body to its surface potential. Thanks to these and others' endeavours, potential theory became a massive source of emulation, analogies, instantiations, and importations across many branches of mechanics and classical mathematical physics [Bacharach 1883].

Thomson was also a major figure in the midcentury advocacy of the principle of the conservation of energy in mechanics and physics, or 'energetics', which became another major source of emulation, importation, instantiation, and corroboration across many sciences. But its parent, energy/work principles in mechanics, had already provided a striking example of corroboration, in the wave theory of light. Its main pioneer from the mid 1810s was A. J. Fresnel, whose theories used a variety of emulations from mechanics, such as assuming the simple harmonic motion of the molecules in his punctiform aether and a cosine law for the decomposition of intensities. The corroboration occurred over his 1821 analysis of Huygen's law of double refraction, that a ray of light of unit intensity at incident angle I in crystals such as Iceland spar split into two rays of intensities $\sin^2 I$ and $\cos^2 I$. After carrying out this analysis he realized from the trigonometric version of Pythagoras's theorem that his theory conformed to the principle of the conservation of energy if he presumed the aether to be transparent for its transmission; so he annotated some older manuscripts to this effect [Fresnel 1866, 472, 483, 496].

Quantum mechanics. Thomson died in 1907, just when his empire of classical (mathematical) physics was being replaced by new scientific regimes. One of them was quantum mechanics, especially the emulation by Niels Bohr and others of celestial mechanics with his 1913 planetary-like model of the hydrogen atom as a nucleus surrounded by a charged electron orbiting in a circle (or, for the desimplifying Arnold Sommerfeld, in an ellipse) [Hermann 1971].[8] Given this approach, the governing differential equation, Ernest Schrödinger's, was linear as usual, and for it a wide repertoire of solutions was available. But the physics, especially the notions of atomic states and quanta of light and other phenomena to which Planck's constant had become associated, dictated that analogy should *not* guide the choice of solutions; to be reasonable the mathematics had to follow routes different from Fourier series (although Werner Heisenberg's first theory of the atom drew upon them), special functions and the like. Instead Hilbert spaces, infinite matrices, and integral equations played prominent roles; and as all three mathematical topics were still rather new at the time (the 1910s onwards) to some extent we see again the effectiveness of a natural science upon their development (and conversely, their applicability). Two main forms of quantum mechanics developed in the 1920s, matrix mechanics and wave mechanics; in the latter development Schrödinger closely emulated the analytic mechanics and optics of W. R. Hamilton. Schrödinger and others showed in 1926 that the two versions were mathematically equivalent; however, their physical differences remained rather mysterious. Paul Dirac came up with a third candidate in his quantum algebra; then he embraced all three in his 'transformation theory'.

Another importation into quantum mechanics was group theory, which had developed over the previous 70 years or so, initially in other specific mathematical contexts and then as a general and abstract theory [Wussing 1984]; several basic kinds of groups proved to be effective, especially rotation, unitary, continuous, and permutation [Mackey 1978, 1985]. This example is especially striking to note because a significant pioneer was one Eugene P. Wigner; his book on the matter [Wigner 1931] is surely a fine counterexample to his thesis of 1960 [French 2000].

Statistical mechanics. The relevance of anthropocentrism, mentioned previously, is nicely exemplified in the survey of equilibrium statistical mechanics [Ruelle 1988]. Early on he is willing to 'define mathematics as a logical construct based on the axioms of set theory' (oh Gödel, where art thou at this hour?), and praises Wigner's 'beautiful' article without 'concern[ing] ourselves with this mystery'. Then, to outline his theory of indeed 'human mathematics', he not only invokes equilibrium, but also imports parts of point-set topology, the integral calculus, operator algebra, and mathematical statistics; he even stresses that 'the intrusion of physics therefore changes the historical development of mathematics' [p. 265], and indicates uses of his subject elsewhere. That is, he does much to dissipate the mystery that he claims to be ignoring!

Complex numbers and variables. Like many areas of pure and applied mathematics, quantum mechanics also imported complex-variable analysis. Wigner points to the 'formal beauty' in the mathematics of complex numbers [Wigner 1960, 3: compare p. 7]; they may possess it, but it does not begin to explain their importance. For that we need to distinguish the data, in this case the positive and negative real numbers, from the theories about them, of which the first were the formulae for the resolution of quadratic, cubic, and quartic polynomials.[9] The complex number field has to

[8]Much of [Steiner 1998] is taken up with quantum mechanics, but unaccountably he omits the contributions of John von Neumann. For a desimplified version of my summary history, see [Beller 1983].

[9]The derivation of the formulae depended upon the insight of Scipione del Ferro around 1500 that a cubic polynomial could be reduced without loss of generality to one lacking the quadratic term, and similarly with Ludovico Ferrari on the quartic about 40 years later.

be invoked because the operations of taking square and higher roots are closed in it but not in the real number field: for example, $\sqrt{(a + ib)}$ is always complex for real a and b whereas $\sqrt{a}$ is real only if a is not negative.[10] Complex-variable analysis is a remarkable but reasonable extension, innovated by Cauchy from the 1810s onwards in close analogy with his concurrent exegesis of real-variable analysis based upon the theory of limits [Smithies 1997].

Tweedledum and Tweedledee. Finally, there is the extreme case of analogy, namely identification: this = that, maybe modulo a Gestalt switch. A remarkable instance occurred in October 1947 when John von Neumann and George Dantzig shared their respective interests in economic behaviour and linear programming; they found that they (and, it turned out, a few others) had been using planar convex regions, but for von Neumann against a background of fixed-point theorems, whereas for Dantzig concerning the performance of objective functions [Dantzig 1982, 45]. The effect of the resulting union of theories was a rapid expansion in work in both subjects.

Increasing Effectiveness: Desimplification and the Science of Small Effects

The previous discussion should suggest grounds for finding reasonable the impressive utility of mathematics in the natural sciences. Whether we deem it effective, however, depends in part on the demands we make of the scientific theory involved, or the expectations we hold for it; *how* general, for example, or how numerically accurate?

It is a commonplace but significant observation to notice that the actual world is a complicated place; Wigner himself does so [1960, 4]. Thus the scientist, whether mathematical or not, is forced to simplify the phenomena under study in order to render them tractable: 'the art of the soluble', to quote the artful title of Medawar [1967]. The longrunning preference for linearity noted earlier is a prominent example of such simplifications; in reaction, a notable feature of recent mathematical physics has been a great increase in nonlinear methods and models [West 1985].

Among branches of mathematics, mechanics is notorious for the adoption of light strings and inextensible pulleys, the assumption that extended bodies have constant density, the routine ignoring of air resistance, friction, and/or the rotation of the Earth about its axis, and so on. The assumption is fallibly made that in the contexts under study, the corresponding effects are small enough to be ignored; but part of the reasonableness of theory-building is to check whether or not such assumptions are justified. I called such checks 'desimplification': putting back into the theory effects and factors that had been deliberately left out.

For example, Lagrange consciously simplified the stability problem by taking only the first-order terms in their masses. Thereby he assumed that the terms in higher orders were small enough to be ignored; but should this assumption be checked for reasonableness? In the late 1800s, under the stimulus of a recent analysis by P. S. Laplace, the young S. D. Poisson and the old Lagrange studied the second-order terms and found a mathematical expression that was of interest in its own right. Thus their study of a particular problem led unexpectedly to a much more general one. For once in the history of mathematics the names attached to the resulting theory, in analytical mechanics, are correct: the 'Lagrange–Poisson brackets'.[11] A version of it was to appear in Dirac's algebra noted in connection with quantum mechanics.

The longest-running catalogue of desimplifications of which I am aware concerns the so-called 'simple' pendulum. The adjective seems reasonable, for the instrument consists only of a bob swinging on a wire from a fixed point. However, especially from the late 18th century onwards, pendula were observed very exactly for making precise calculations in connection with the needs of geodesy, cartography, and topography. This was small-effects science par excellence, literally preoccupied with decimal places. Many scientists studied a wide range of properties [Wolf 1889–1891]. Is the downswing *exactly* equal to the upswing? Does the bob make a little angular kick at the top of its upswing or not? What about the effects of Lunar attraction, the spheroidicity of the Earth, air resistance, the possible extension of the wire under the weight of the bob, and the effect of the bob rotating about its own axis? Do possible movements of the supporting frame affect the swinging of large pendula? What special factors attend the use of a hand-held pendulum [Kaushal 2003, 160–172]? These and various other questions made the simple pendulum a complicated instrument! However, all the desimplifications were performed fallibly but reasonably, for they attempted to establish guides on the orders of smallness of the effects upon the motion of the pendulum.

Some of these strategies involved quantitative approximations to the relevant theory. This invoked numerical analysis, which, when taken with numerical linear algebra, forms a branch of mathematics of special pertinence to our theme [Chabert 1999].

Some Comments on Ineffectiveness, Including its Own Possible Ineffectiveness

The account above paints a picture of the development of applied mathematics in sequences of fallible but steadily successful actions. However, it is itself simplified, and needs supplementing with some consideration of types of failure over and above incompetence.

Numerical utility. Some examples are rather slight, even amusing, such as astronomers sometimes calculating values of their variables to ridiculous numbers of decimal places, far beyond any scientific need of their time. However, this action raises the reasonable question, somewhat akin to the considerations of numerical methods just aired: given the instruments available in some scientific context, what is a/the reason-

[10] This reading of complex numbers belongs to the advent of structural algebras during the early 20th century [Corry 1996]. An earlier reading deployed the complex plane; but as it depends upon geometry, it might be seen as more of a heuristic aid than an epistemological ground.

[11] On Lagrange's and Poisson's work see [Grattan-Guinness 1990, 371–386]. On the place of the theory in analytical mechanics see, for example, [Whittaker 1927, ch. 11].

able number of decimal places to aim at in the theory? More generally, which mathematics goes reasonably and effectively with measurement, both in the natural sciences and elsewhere? For Thomson and many others, it should happen as often and as accurately as possible in science; others have been more cautious.[12]

There are also situations where a genuine problem is addressed, but the theories proposed as solutions are of no practical use whatever; I call this type 'notational applications'. A striking example is [Poisson 1823] on the cooling of an annulus in the desimplified situation when the temperature of the environment was *not* constant and so was itself represented in the diffusion equation by a Fourier series. The consequences for the resulting analysis can be imagined; but what was the motivation? He mentioned the predicament of a sailor using a sextant at sea in a (variably) sunny environment, when the rays from the Sun strike the instrument itself and so cause it to distort out of shape. This is a genuine problem; but how do the parades of sines and cosines resolve it, especially in any calculable manner?

Mathematics in economics. One subject where the use of mathematics has been questioned in a fundamental way is economics. In particular, accepting Wigner's thesis, Velupillai [2005] entitled his attack 'The unreasonable *in*effectiveness of mathematics in economics'. The criticisms are wide-ranging: 'the mathematical assumptions are economically unwarranted' and often dependent upon weak analogies with other subjects. For example, several main figures in the early stages of neoclassical economics in the second half of the 19th century emulated mechanics with enthusiasm, especially the notion of equilibrium, and deployed major assumptions such as d'Alembert's principle; but the resulting theories were not very effective [Grattan-Guinness 2007]. What, for example, corresponds in economics to the continuous and uniform force of gravity? There is still a wide spectrum of views on, for example, the effectiveness in economics of the notion of equilibrium [Mosini 2007].

Velupillai specifically finds mathematics in economics 'ineffective because the mathematical formalisations imply non-constructive and uncomputable structures'; as medicine he recommends constructive mathematics, especially in the import of number theory and recursion theory into economics when its data have been expressed as integral multiples of some basic unit. One would certainly have a lot of sophisticated theories to deploy (he explicitly recommends Diophantine analysis); but it is a moot point as to whether the great complications that attend constructive mathematics in general would render economics more effective (or alternatively, whether they can be avoided). There is a widespread practice of mathematicising the proposed theory whatever its content—'bad theory with a mathematical passport', according to Schwartz [1962, 358]—but much less concern for bringing it to test. Some branches do exhibit effective testing; for example, financial data subjected to time series analysis, and not just to find correlations for their own ineffective sake.

Beyond the physical sciences. The failure of the mechanisation of economics shows that the gap between the physical and the social sciences is wide. How about the gap between the physical and the life sciences? Lesk [2000] takes up the matter in connection with molecular biology, copying Wigner's title; so we expect to learn of some more unreasonable effectiveness. However, he is very cautious, stressing disanalogies between the physical and the life sciences, especially over matters concerning complexity; and indeed, in a follow-up letter, Lesk [2001] reports that his hosts asked him to speak of 'effectiveness' rather than 'ineffectiveness' in the lecture of which his paper is the written version! There are topics that can be handled effectively within and without the physical sciences; for example, adap-

Mathematics Pure or Applied

Martin Zerner reports that a few years ago he was scanning a list of mathematical specialties put out by the *Conseil National des Recherches Scientifiques,* and he noticed that one of the categories was

Applications of Pure Mathematics.

With Zerner, we may furrow our brows. Is it an oxymoron? If not, maybe the CNRS ought to have allowed also a category

Purification of Applied Mathematics,

which, if it means anything, surely means something rather inglorious. Like removing from some discipline the stigma of applicability (as if there were such a stigma). Joseph Keller's dismissal of all such anxieties is

Pure mathematics is a subfield of applied mathematics.

Chandler Davis

[12]For a history of the mathematics of measurement, in various scales and many contexts, see [Henshaw 2006].

tation in control engineering and biology when oriented around optimisation [Holland 1975]. However, one is tempted to think that desimplification will not be radical enough, and that the nonphysical sciences—life, mental, social—may need *fresh kinds of mathematics*.[13] Relative to the Table, perhaps we should retain the notions (or most of them) and build different topics around them.

Lesk's remark also draws attention to the limitations of human mental capacities; maybe some phenomena are just too complicated or elusive for effective theorizing, whatever the science. Wigner himself raised this striking point when he noted 'the two miracles of the existence of laws of nature and of the human mind's ability to divine them' [1960, 7; see also p. 5]; I strengthen it by regarding the laws as existing only because of human effort in the first place. There may indeed be limitations on the human capacity to formulate a problem clearly, and/or think up theories to solve it (a possibility that worried philosophers such as Kant, Whewell, and Peirce); but the means for theory-building laid out in this essay suggest that there is still plenty of room for human manoeuvre!

Want of spirit. Let us finally note three kinds of ineffectiveness due to human frailty. The first is vanities such as the generalisation racket, where a mathematician takes a theorem involving (say) the number -2 and generalises it to all negative even integers $-2n$, where however the only case of any interest is given by $n = 1$.

The second kind of ineffectiveness can involve narcissism, where a mathematical theory is applied in a scientific context in an inappropriate form *because* that is the form preferred by its pure practitioners. Then indeed the influence of mathematics upon that science is 'pernicious' [Schwartz 1962]—that is, worse than ineffective. For example, since Cauchy's time in the 1820s, the mathematically superior version of the calculus has been based on a theory of limits; but the older Leibniz–Euler theory using the dreadful differentials often has a better analogy content to the scientific context (especially if the latter involves continua) and so should be given its due [Thompson 1910]. Thomson's career reveals many examples of heresy, including those mentioned earlier.

The third kind of ineffectiveness obtains in any science: oversight! Mathematics has eventually exhibited some nice 'missed opportunities' [Dyson 1972]; what will turn out to be the good ones of today?

Concluding Remarks

It may be that Wigner was drawn to his thesis by his experience with quantum mechanics; he gives some examples from there [1960, 9–12]. Perhaps its first practitioners struck lucky in analogising from the experiential celestial heavens to the highly nonexperiential atom, and enjoying some remarkable later successes; but for those who follow [Popper 1959] in seeing science as guesswork, then sometimes it is bull's-eye time, and quantum mechanics was one of them—for a time, anyway. For a *general* explanation of mathematics, Wigner appeals to its beauty and to the manipulability of expressions [Wigner 1960, 3, 7]: as with the previously mentioned complex numbers, such properties may be exhibited on occasion, but surely they cannot *ground* mathematics or explain its genesis, growth, or importance.

Wigner's thesis about unreasonableness is philosophically ineffective, partly because he neglected numerous clear indications from history of sources of both reasonableness and effectiveness of the natural sciences in mathematics. Yet not only were various histories of applied mathematics available by 1960; some eminent mathematicians had published relevant texts. Pólya [1954a, 1954b] has already been cited; it was followed by Pólya [1963] on 'mathematical methods in science', mostly elementary mechanics, and one could add, for example, Enriques [1906, chs. 5–6] sketching in some detail the history of how physics convoluted out of mechanics, and Weyl [1949, 145–164] providing an historico-philosophical review of 'the formation of concepts' and 'theories' in connection with mechanics.

Wigner also underrated the central place of theories being formed in the presence of other theories, and being desimplified when necessary and where possible. In addition, the ubiquity of the topics and notions elucidated in Table 1, and others not listed there, should be emphasized.

The alternative picture that emerges is that, with a wide and ever-widening repertoire of mathematical theories and an impressive tableau of ubiquitous topics and notions, theory-building can be seen as reasonable to a large extent; however, the effectiveness of the output may need some enhancement through (further) desimplifications, if they can be realised. Instead of 'effective but unreasonable', read 'largely reasonable, but how effective?'. This slogan can also guide appraisals of (un?)reasonable (in?)-effectiveness in contexts overlapping with the one studied here: for example, notations and notational systems (where mathematics meets semiotics[14]), graphical and visual techniques, pure mathematics, numerical methods, logics, and probability theory and mathematical statistics. There are consequences to explore concerning the use of the histories of mathematics and of the natural sciences in theory-building, and the content of mathematics and science education.

By the way, π turns up in the statistical *theory* that is applied to the population *data*, in order to normalize the Gaussian distribution. Wigner does not give this, or any other, explanation of the mystery in his article.

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[13]An example of ineffectiveness is the attempt in [Matte Blanco 1975] to construe the unconscious in terms of set theory, which however is not well handled; for example, paradoxes are admitted, seemingly unintentionally. The *principle* of applying set theory to the mental sciences may be in question, as well as this particular practice.

[14]Of special interest is Peirce's theory of icons, the relationships between (families of) signs, their referents, and the cognitive means of correlating the two.

BIBLIOGRAPHY

Bacharach, M. 1883. *Abriss zur Geschichte der Potentialtheorie,* Würzburg: Thein.

Beller, M. 1983. 'Matrix theory before Schrödinger', *Isis* 74, 469–491.

Colyvan, M. 2001. 'The miracle of applied mathematics', *Synthese* 127, 265–277.

Chabert, J. L. et al. 1999. *A history of algorithms. From the pebble to the microchip,* Berlin: Springer.

Corry, L. 1996. *Modern algebra and the rise of mathematical structures,* Basel: Birkhäuser.

Dantzig, G. B. 1982. 'Reminiscences about the origins of linear programming', *Operational research letters*. I, 43–48. [Slightly rev. ed. in A. Bachem, M. Grotschel, and B. Corte (eds.), *Mathematical programming. The state of the art,* Berlin: Springer, 1983, 78–86.].

Dilworth, C. 1994. *Scientific progress,* Dordrecht: Kluwer.

Dyson, F. 1972. 'Missed opportunities', *Bulletin of the American Mathematical Society* 78, 635–652.

Enriques, F. 1906. *The problems of science,* Chicago and London: Open Court.

French, S. 2000. 'The reasonable effectiveness of mathematics: partial structures and the application of group theory to physics', *Synthese* 125, 103–120.

Fresnel, A. J. 1866. *Oeuvres complètes,* vol. 1, Paris: Imprimerie Impériale.

Grattan-Guinness, I. 1990. *Convolutions in French mathematics, 1800–1840. From the calculus and mechanics to mathematical analysis and mathematical physics,* 3 vols., Basel: Birkhäuser; Berlin: Deutscher Verlag der Wissenschaften.

Grattan-Guinness, I. 1992a. 'Scientific revolutions as convolutions? A sceptical enquiry', in S. S. Demidov, M. Folkerts, D. E. Rowe, and C. J. Scriba (eds.), *Amphora. Festschrift für Hans Wussing zu seinem 65. Geburtstag,* Basel: Birkhäuser, 279–287.

Grattan-Guinness, I. 1992b. 'Structure-similarity as a cornerstone of the philosophy of mathematics', in J. Echeverria, A. Ibarra, and T. Mormann (eds.), *The space of mathematics. Philosophical, epistemological, and historical explorations,* Berlin, New York: de Gruyter, 91–111.

Grattan-Guinness, I. 1994. (ed.), *Companion encyclopaedia of the history and philosophy of the mathematical sciences,* London: Routledge. [Repr. Baltimore: Johns Hopkins University Press, 2003.].

Grattan-Guinness, I. 2004. 'The mathematics of the past. Distinguishing its history from our heritage', *Historia mathematica,* 31, 161–185.

Grattan-Guinness, I. 2007. 'Equilibrium in mechanics and then in economics, 1860–1920: a good source for analogies?', in Mosini [2007], 17–44.

Grattan-Guinness, I. 2008a. 'Differential equations and linearity in the 19th and early 20th centuries', *Archives internationales d'histoire des sciences,* to appear.

Grattan-Guinness, I. 2008b. 'On the early work of William Thomson: mathematical physics and methodology in the 1840s', in R. G. Flood, M. McCartney, A. Whitaker (eds.), *Lord Kelvin: life, labours and legacy,* Oxford: Oxford University Press, 44–55, 314–316.

Grattan-Guinness, I. with the collaboration of Ravetz, J. R. 1972. *Joseph Fourier 1768–1830. A survey of his life and work, based on a critical edition of his monograph on the propagation of heat, presented to the Institut de France in 1807,* Cambridge, Mass.: MIT Press.

Hamming, R. 1980. 'The unreasonable effectiveness of mathematics', *The American mathematical monthly,* 87, 81–90.

Hawkins, T. W. 1975. 'Cauchy and the spectral theory of matrices', *Historia mathematica,* 2, 1–29.

Henshaw, J. M. 2006. *Does measurement measure up? How numbers reveal and conceal the truth,* Baltimore: Johns Hopkins University Press.

Hermann, A. 1971. *The genesis of quantum theory,* Cambridge, Mass.: The MIT Press.

Hintikka, J. 2007. *Socratic epistemology,* Cambridge; Cambridge University Press.

Holland, J. R. 1975. *Adaptation in natural and artificial systems. An introductory analysis with applications to biology, control and artificial intelligence,* Ann Arbor: The University of Michigan Press.

Kaushal, R. S. 2003. *Structural analogies in understanding nature,* New Delhi: Anamaya.

Kiesow, H. 1960. Review of Wigner [1960], *Zentralblatt für Mathematik* 102, 7.

Knobloch, E. 2000. 'Analogy and the growth of mathematical knowledge', in E. Grosholz, H. Breger (eds.), *The growth of mathematical knowledge,* Dordrecht: Kluwer, 295–314.

Lesk, A. 2000. 'The unreasonable effectiveness of mathematics in mo-lecular biology', *The Mathematical Intelligencer* 22, no. 2, 28–36.

Lesk, A. 2001. 'Compared with what?', *The Mathematical Intelligencer*, 23, no. 1, 4.

Mackey, G. W. 1978. 'Harmonic analysis as the exploitation of symmetry: A historical survey', *Rice University Studies* 64, 73–228. [Repr. in [1992], 1–158.]

Mackey, G. W. 1985. 'Herman Weyl and the application of group theory to quantum mechanics', in W. Deppert et al. (eds.), *Exact sciences and their philosophical foundations,* Kiel: Peter Lang, 131–159. [Repr. in [1992], 159–188.]

Mackey, G. W. 1992. *The scope and history of commutative and noncommutative harmonic analysis,* American Mathematical Society and London Mathematical Society.

Mathematics 1986. 'Mathematics: the unifying thread in science', *Notices of the American Mathematical Society* 33, 716–733.

Matte Blanco, L. 1975. *The unconscious as infinite sets: an essay in bi-logic,* London: Duckworth.

Medawar, P. B. 1967. *The art of the soluble,* Harmondsworth: Pelican.

Mosini, V. (ed.). 2007. *Equilibrium in economics: scope and limits,* London: Routledge.

Poisson, S. D. 1823. 'Sur la distribution de la chaleur dans un anneau homogène et d'une épaisseur constante . . . ', *Connaissance des temps* (1826), 248–257.

Pólya, G. 1954a, 1954b. *Mathematics and plausible reasoning,* 2 vols., 1st ed., Princeton: Princeton University Press. [2nd ed. 1968.]

Pólya, G. 1963. *Mathematical methods in science,* Washington: MAA.

Popper, K. R. 1959. *The logic of scientific discovery,* London: Hutchinson.

Popper, K. R. 1972. *Objective knowledge,* Oxford: Clarendon Press.

Roche, J. J. 1998. *The mathematics of measurement. A critical history,* London: Athlone Press.

Ruelle, D. 1988. 'Is our mathematics natural? The case of equilibrium statistical mechanics', *Bulletin of the American Mathematical Society* 19, 259–267.

Sarukkai, S. 2005. 'Revisiting the "unreasonable effectiveness" of mathematics', *Current science,* 88, 415–422.

Schwartz, J. 1962. 'The pernicious influence of mathematics on science', in E. Nagel et al. (eds.), *Logic, methodology and philosophy of science,* Stanford: Stanford University Press, 356–360. [Repr. in R. Hersh (ed.), *18 unconventional essays on the nature of mathematics,* New York: Springer, 2005, 231–235.]

Smith, B. (ed.). 1982. *Parts and moments. Studies in logic and formal ontology,* Munich: Philosophia.

Smithies, F. 1997. *Cauchy and the creation of complex function theory,* Cambridge: Cambridge University Press.

Steiner, M. 1998. *The applicability of mathematics as a philosophical problem,* Cambridge, Mass.: The MIT Press.

Tanner, R. C. H. 1961. 'Mathematics begins with inequality', *Mathematical Gazette* 44, 292–294.

Thompson, S. P. 1910. *Calculus made easy,* 1st ed., London: Macmillans. [Deservedly numerous later eds.].

Toeplitz, O. 1963. *The calculus. A genetic approach,* Chicago: The University of Chicago Press. [German original 1949.].

Velupillai, K. V. 2005. 'The unreasonable *in*effectiveness of mathematics in economics', *Cambridge Journal of Economics* 29, 849–872.

West, B. J. 1985. *An essay on the importance of being nonlinear,* Berlin: Springer.

Weyl, H. 1949. *Philosophy of mathematics and natural science,* Princeton: Princeton University Press.

Weyl, H. 1952. *Symmetry,* Princeton: Princeton University Press.

Whittaker, E. T. 1927. *Analytical dynamics,* 3rd ed., Cambridge: Cambridge University Press.

Wigner, E. P. 1931. *Gruppentheorie und ihre Anwendung auf die Quantenmechanik der Atomspektren,* Braunschweig: Vieweg. [Rev. English trans.: *Group theory and its application to the quantum mechanics of atomic spectra,* New York: Academic Press, 1959.].

Wigner, E. P. 1960. 'The unreasonable effectiveness of mathematics in the natural sciences', *Communications on pure and applied mathematics,* 13, 1–14. [Repr. in [1967], 222–237; and in *Philosophical reflections and syntheses* (ed. G. Emch), Berlin: Springer, 1995, 534–548.]

Wigner, E P. 1967. *Symmetries and reflections: scientific essays,* Bloomington: Indiana University Press.

Wilson, C. A. 1980a, 1980b. 'Perturbation and solar tables from Lacaille to Delambre: the rapprochement of observation with theory', *Archive for history of exact sciences* 22, 53–188, 189–304.

Wolf, C. J. E. 1889–1891. *Mémoires sur le pendule . . . ,* 2 pts., Paris: Gauthier-Villars.

Wussing H. 1984. *The genesis of the abstract group concept,* Cambridge, Mass.: The MIT Press.