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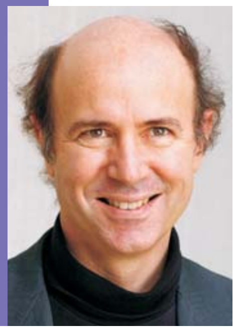

Reasonably effective: I. Deconstructing a miracle

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In 1960 Eugene Wigner wrote a famous essay entitled "The Unreasonable Effectiveness of Mathematics in the Natural Sciences."[1] After recounting several remarkable mathematical success stories, he concluded:

> 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 nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or for worse, to our pleasure, even though perhaps also to our bafflement, to wide branches of learning.

And of course that miracle did remain valid in future research. Indeed, perhaps the most startling success of the language of mathematics in the formulation of the laws of physics occurred about a decade after Wigner wrote his essay, with the emergence of non-abelian gauge theories of fundamental interactions. In those theories, the building blocks of matter emerge as nearly ideal embodiments of intricate, abstract symmetry principles.

Yet, despite its tension with Wigner's thesis, I hold this truth to be self-evident: that all correct general principles must be reasonable. For the burden of reason is to clarify reality through the application of correct general principles. If a correct general principle appears unreasonable, that appearance is a fault we must repair—either by reasoning more deeply or, failing that, by forcing our reason into line.

Given that truth, Wigner's case against the reasonableness of mathematics' effectiveness presents us with three choices, as follows: Either the effectiveness of mathematics in the natural sciences is not a correct general principle, or it has a reasonable explanation, or we must accept it as a postulate of reasoning itself. I will argue that each of those options supplies part of an adequate response.

Success through selection

One way to succeed at archery is to draw a bull's-eye around the place your arrow lands. Part of the explanation for the success of mathematics in natural science is that we select what we regard as the scientifically interesting aspects of nature largely for their ability to allow mathematical treatment.

For example, the amount of work devoted to the behavior of ultrapure semiconductor heterojunctions subject to ultrastrong magnetic fields at ultralow temperatures is grossly out of proportion to the technological importance of that physical domain or to its significance in the natural world. What drives the work is the fact that a rich and beautiful mathematical theory of the quantum Hall effect comes into play. And the study of critical phenomena reached a new level of popularity when the mathematical concepts of universality and the renormalization group entered the scene.

To avoid misunderstanding, let me emphasize that I don't mean to condemn our attraction to phenomena that support rich mathematical theories—though it can be overdone. In fact, my two examples come from two fields I've cultivated myself.

In his essay Wigner approvingly quotes Michael Polanyi to the effect that mathematics is designed to be interesting:[2] "Mathematics cannot be defined without acknowledging its most obvious feature: namely, that it is interesting."

Accepting Polanyi's thesis, we understand that a sort of natural selection is at work. Scientists choose to work on problems that are interesting, and mathematics is designed to be interesting, so science evolves toward areas where mathematics can be successfully applied.

To cement the case that selection of topics plays a major role in the perceived effectiveness of mathematics, let's examine its record of effectiveness at the more conventional kind of archery, where the bull's-eye gets drawn beforehand. There the score is much less impressive. Turbulence, friction, and protein folding, for example, are technologically important and ubiquitous phenomena in the natural world, but despite much effort, they remain largely mathematical wilderness.

Richard Feynman expressed his yearning for a more effective mathematics:

> The next great awakening of human intellect may well produce a method of understanding the qualitative content of equations. Today we cannot. . . . Today we cannot see whether Schrödinger's equation contains frogs, musical composers, or morality—or whether it does not. We cannot say whether something beyond it like God is needed, or not. And so we can all hold strong opinions either way.[3]

Centuries earlier, in a similar vein, Gottfried Wilhelm Leibniz expressed the vision of a "universal characteristic." He wrote,

> If controversies were to arise, there would be no more need of disputation between two philosophers than between two accountants. For it would suffice to take their pencils in their hands, to sit down to the slates, and to say to each other (with a friend as witness, if they liked): Let us calculate.[4]

Can mathematics be used to extract qualitative predictions from physical laws—or, for that matter, useful laws from data—automatically? Perhaps, but the omens aren't auspicious. With Gödel's theorem (the existence of true statements that can't be proved formally) and the concepts of computational complexity (the existence of many natural problems that can't be solved by practical algorithms) and chaos (the existence of natural equations that can't be solved systematically), mathematics has identified limits to its own power.

Paul Dirac once said that he considered he understood an equation when he could anticipate the properties of its solution without actually solving it. For better or worse, thanks to computational complexity and chaos, we now know many examples in which the solutions of innocuous-looking equations are either inaccessible or incapable of simple description.

Sitting on my porch in New Hampshire, I look out over a wind-rippled lake, a piney horizon, a cumulus-patched sky, and a pair of loons with their baby. Just what aspect of that scene can be derived from beautiful equations? To expose beautiful equations at work, we can't just look out from our porches at natural scenes. We must do extraordinary things such as building and operating engineering marvels like the Large Hadron Collider and the *Wilkinson Microwave Anisotropy Probe*, and then working hard to cleanse their raw output of irrelevant complications, artifacts, and noise before interpreting the highly refined product as proper data.

Gifts from nature

As explorers seeking the Northwest Passage, El Dorado, and the Fountain of Youth discovered, searching for your heart's desire by no means guarantees that you'll find it. Selection by itself is not enough to explain the power of beautiful mathematics in our theories of nature. Looking for beautiful, powerful mathematical laws of nature wouldn't succeed were there not beautiful, powerful mathematical laws to be found. Why are there?

We can illuminate "Why?" questions by asking whether things could possibly have been otherwise. Can we imagine worlds where behavior is governed by something other than elegant mathematical laws? It's all too easy. Children, Aristotle, the authors of the Bible, and the designers of Super Mario Brothers, among many others, imagine such worlds for us (in some cases, while mistakenly thinking they are describing our world).

Because of some special features of the way the real world works, it is much more receptive to elegant mathematics. The two most crucial of those, I think, are symmetry and locality. It is symmetry under changes in the moment you start your clocks that makes the laws unchangeable and eternal; it is symmetry under changes in the place you put your lab that makes them universal. Locality allows us to build up the description of nature by mathematical deduction from a description of simple interactions among elementary building blocks. Working together, locality and symmetry give the laws the character of differential equations with restricted forms.

Moreover, it is because coarse-grained versions of local and symmetric equations remain local and symmetric that even approximate forms of the laws retain much of their elegance. Physics had beautiful equations long before the emergence of the standard model; and the standard model has beautiful equations even though it is surely not the ultimate truth.

Alert Reader: "You've argued in a perfect circle! Mathematics, you say, is effective in describing nature because nature obeys mathematical concepts."

Allow me to remind you, my critical friend, that the world line of a circular argument can be an ascending helix. When people like Archimedes, Johannes Kepler, and Galileo discovered the earliest "unreasonably effective" mathematical laws of nature, each such law seemed like a newly revealed miracle, unanticipated and logically disconnected from the others. After traversing a long history, we can now look back to see the same laws in quite a different way, as particular consequences of a more profound and encompassing theoretical framework in which symmetry and locality emerge as dominant features of the world's deep structure. Having found those dominant features, we've uncovered the underlying reasons why mathematics is so effective in describing nature (that is, when it is!).

Acts of faith

Since any answer to a "why" question can be challenged with a further "why," any reasoned argument must terminate in premises for which no further reason can be offered. At that point we pass, necessarily, from reason to faith. Our present faith in symmetry and locality is grounded in the good experience we've had with them so far. At present, I think, we can carry our explanations no deeper.

As good believing scientists we must take our faith seriously—so seriously that we feel compelled to act on it, and thereby to test it. Symmetry as a guide to physical law will, I hope and believe, soon achieve spectacular new triumphs.

There are many promising avenues. We can enhance the symmetry of the equations describing the strong, weak, and electromagnetic interactions by postulating a more extensive gauge symmetry that includes them all. When we do that, many peculiar loose ends of the standard model get tied up neatly. But the bigger symmetry requires additional interactions, which destabilize protons. The predicted rate of decay is very small, but perhaps accessible to observation.

Unification of the different interactions requires new particles. Particles in their virtual form, as quantum fluctuations, contribute to vacuum polarization. The obscuring effect of those ever-present fluctuations can explain the difference between the observed, differing values of the couplings and the single, unified, "bare" value we'd like to think is fundamental. For accurate unification, the new particles can't be too heavy. Specifically, they should be light enough to be produced in their real (that is, not virtual) form at the upcoming great accelerator, the Large Hadron Collider.

Unifying symmetry can be extended in a second direction. Extended gauge symmetry connects the spin-1 (bosonic) color gluons of quantum chromodynamics, the W and Z bosons, and the photons to one another, and also the different spin-$1/2$ (fermionic) quarks and leptons to one another. But it does not connect those bosons and fermions to one another. Supersymmetry accomplishes that feat. Implementing supersymmetry also requires new particles.

Remarkably, the particles required for supersymmetry supply just what we need to get accurate unification of couplings. (See the article by Savas Dimopoulos, Stuart Raby, and me, PHYSICS TODAY, October 1991, page 25.) Is that coincidence a cruel tease, or a harbinger of spectacular synthesis? We'll soon find out.

Symmetry also leads us to expect Higgs particles, which can explain why electroweak gauge symmetry is broken, and axions, which explain why strong-interaction time-reversal symmetry is not.

Eventual discovery of any or—as I expect—all of these new phenomena will be wonderful new confirmation of the effectiveness of mathematics in natural science. But those discoveries will constitute the pinnacle of reason, not an "unreasonable" anomaly.

References

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2. M. Polanyi, *Personal Knowledge: Towards a Post-Critical Philosophy*, U. Chicago Press, Chicago (1958), p. 188.
3. R. Feynman, R. Leighton, M. Sands, *The Feynman Lectures on Physics*, vol. 2, Addison-Wesley, Reading, MA (1964), p. 41-12.
4. G. W. Leibniz, *The Art of Discovery* (1685). ■