

Mathematics and Science

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Roughly one-third of these one hundred Princeton University Press books (PUP100) fall in the domain of science or mathematics, approximately evenly divided between the two.

Some readers, particularly those who disliked both science and mathematics in school, may ask why distinguish the two. Surely science and mathematics are inseparable, two sides of the same coin. Given that most of this essay will belabor the essential truth of this belief, it seems a good idea to begin by explaining the ways in which mathematics, qua mathematics, does differ from science (as conventionally understood, embracing medicine and engineering).

Mathematics, in its purest forms, deals with logical systems: assume this, prove that. If a triangle has two angles equal, then it will have two sides of equal length. No ifs, buts, or maybes. As amplified somewhat in the brief essay on Gödel's *Consistency of the Continuum Hypothesis*, there can be circumstances when the answer to a well-posed mathematical problem is undecidable, but this theorem itself can be proved.

In contrast, science asks questions about how the world actually works. All answers must be anchored in observations about how things really are. Although it may seem utterly certain that the sun will rise tomorrow, we cannot prove it in the same way as we can prove mathematical theorems within the confines of their closed logical structures.

This being acknowledged, it of course remains true that mathematics and science have been closely entwined since humans first began to inquire about the world around them and the heavens above. We still experience this every day, with 60 seconds in a minute, 60 minutes in an hour, and 360 degrees in a circle preserving the memory of the Babylonians' 60-based number system in practical things, despite its being supplanted by decimal systems in all other contexts.

If we fast-forward to the seventeenth and early eighteenth centuries, we find Newton and Leibniz quarreling about who "invented" calculus.¹ More interesting than this question, I think, is that one of Newton's first applications of this new mathematics was to show that, under an inverse square law of gravitational

attraction, spherical planets could be treated as if they were point masses. Increasingly, as the scientific-industrial revolution gathered momentum in the eighteenth and nineteenth centuries, experiment and observation combined with mathematical advances, each stimulating the other, to give insight into essential simplicities underlying much of the physical world. This interplay between mathematical tools, some of them astonishingly beautiful,² and scientific understanding is seen clearly in several of the PUP100. Einstein's *The Meaning of Relativity* is an iconic example, but other books by Peebles, Binney and Tremaine, Feynman, Hawking and Penrose, and Anderson speak equally eloquently of the happiness and fecundity of this marriage.

Other books testify to the continuing dialogue between pure mathematics—questions pursued for their own sake, with unabashed motives of curiosity and the pleasure of the chase—and applications to problems of scientific understanding. A notable example is Milnor's book on "Morse theory," with its possible applications, among other things, to quantum field theory.

In short, in the physical sciences mathematical theory and experimental investigation have always marched together. Mathematics has been less intrusive in the life sciences, possibly because they have until recently been largely descriptive, lacking the invariance principles and fundamental natural constants of much of physics. Indeed, it is startling to reflect how recent are the beginnings of the basic task of codifying the diversity of other species which share the planet with us. The canonical date for Linneaus' *De Rerum Naturae* is 1758, a full century after Newton and the founding of the Royal Society in 1660. In many important ways, the legacy of this lag remains with us today.

The longest-serving president of the Royal Society was Joseph Banks, from 1778 to 1820. And what a turbulent forty-two years these were: French Revolution; Napoleonic Wars; much else. Banks, who had briefly studied with Linneaus, sailed with Cook on his first, Royal Society-sponsored, voyage to observe the transit of Venus and onward to the first European mapping of New Zealand and the east coast of Australia. Banks brought back extraordinary collections of botanical and other specimens, eventually adding 13,000 new plant species to the total then known (this, remember, at a time not that long after Linneaus' catalog which in 1758 recorded some 9,000 species of plants and animals in total). However, although Banks, the "flora explorer," may be seen today as one of the pioneers of plant biology, in his day there were bitter quarrels within the Royal Society, where many physical scien-

tists simply did not recognize such atheoretical, descriptive work as lying within the domain of proper science.

Roughly fifty years later we find Darwin, still for me the greatest life scientist ever, writing: “I have deeply regretted that I did not proceed far enough at least to understand something of the great leading principles of mathematics; for men thus endowed seem to have an extra sense.” With the benefit of hindsight, we can see how much such an “extra sense” could indeed have contributed to the solution of one of Darwin’s major problems. In his day, it was thought that inheritance “blended” the characteristics of mother and father. However, as forcefully pointed out to Darwin by the engineer Fleeming Jenkin and others, with blending inheritance it is virtually impossible to preserve the natural variation within populations that is, on the one hand, observed and, on the other hand, essential to Darwin’s theory of how evolution works. Mendel’s observations as to the particulate nature of inheritance were contemporary with Darwin, and his published work accessible to Darwin. Fisher and others have suggested that Fleeming Jenkin’s fundamental and intractable objections to the *Origin of Species* could have been resolved by Darwin or one of his colleagues, if only they had grasped the mathematical significance of Mendel’s results.³ But half a century elapsed before Hardy and Weinberg (H-W) resolved the difficulties by proving that particulate inheritance preserved variation within populations.⁴

Today, the H-W Law stands as a kind of Newton’s First Law (bodies remain in their state of rest or uniform motion in a straight line, except insofar as acted upon by external forces) for evolution: gene frequencies in a population do not alter from generation to generation in the absence of migration, selection, statistical fluctuation, mutation, and the like. Subsequent advances in population genetics, led by Fisher, Haldane, and Wright, helped make the neo-Darwinian revolution in the early twentieth century. In the PUP100, George Williams’ *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought*, along with work by Bill Hamilton and others, has been influential in further advances and clarifications in evolutionary biology. And, as explained in *The History and Geography of Human Genes* by Cavalli-Sforza, Menozzi, and Piazza, an increasing abundance of data at the molecular level about the genetic composition of, and variability within, populations is combining with computational power to give new insights into how human groups and societies have moved around the world’s land masses and over its oceans. Perhaps surprisingly, yet another influential PUP100 book in this

context of evolutionary biology is von Neumann and Morgenstern's *Theory of Games and Economic Behavior*. The seminal ideas set out in this work find applications today in many areas of behavioral ecology, particularly in attempts to understand how cooperative behavior evolved, and has been maintained, in groups of humans and other animals.⁵

A paradigmatic account of the uses of mathematics in the natural sciences comes, in deliberately oversimplified fashion, from the classic sequence of Brahe, Kepler, Newton: observed facts, patterns that give coherence to the observations, fundamental laws that explain the patterns. Mathematics enters at every stage in this process of scientific understanding: in designing the experiment; in seeking the patterns; in reaching to understand underlying mechanisms. In biology, of course, every stage in this caricature is usually vastly more complex than in the early days of physics. But the advent of computers, and the extraordinary doubling of their capability roughly every eighteen months for the past several decades, permits exploration—and sometimes understanding—we could not have dreamed of fifty years ago.

Consider the role played by applications of mathematics in sequencing the human and other genomes. This adventure began with the recognition of the doubly helical structure of DNA and its implications, an oft-told tale in which classical mathematical physics played a central role. Brilliant biochemical advances, allowing the three-billion-letter-long human sequence to be cut up into manageable fragments, were a crucial next step. The actual reassembling of the sequence fragments, to obtain a final human genome sequence, drew on both huge computational power and complex software, itself involving new mathematics. The sequence information, however, represents only the Tycho Brahe stage. Current work on various genomes uses pattern-seeking programs to sort out coding sequences corresponding to individual genes, from among the background which is thought to be noncoding. Again, elegant and sometimes novel mathematics is involved in this Keplerian stage of the “work in progress.” We are only just beginning, if that, the Newtonian stage of addressing the deeper evolutionary questions posed by these patterns (not least, the surprising finding of the large number of genes we share with other species, and how numbers of genes appear to be uncorrelated with what we regard as the complexity of the organism; rice, for example, appears to have more genes than we do).

In this Newtonian quest, mathematical models will offer a different sort of help from what they provided in the earlier stages,

although more akin to the help they give in the physical sciences. Various conjectures about underlying mechanisms can be made explicit in mathematical terms, and the consequences can be explored and tested against the observed patterns. In this general way, we can, in effect, explore possible worlds. Some hard-nosed experimental biologists may deride such exploration of imaginary worlds. And such derision may have some justification when the exploration is in vaguely verbal terms (as it too often still is in some areas of the life sciences). As physicists long ago discovered, the virtue of mathematics in such a context is that it forces clarity and precision upon the conjecture, thus enabling meaningful comparison between the consequences of basic assumptions and the empirical facts. Here mathematics is seen in its quintessence: no more, but no less, than a way of thinking clearly.

The history of the use of mathematical models, and of mathematics more generally, varies among different areas in the life sciences. I referred above to population genetics; ecology and immunology provide two further, and interestingly different, examples.

Ecology is a relatively young subject (the word was coined only a little over a century ago), and much early work was largely descriptive. Seminal studies by Lotka and Volterra explored mathematical metaphors for competition and other interactions among species, but things did not really take off until the 1960s and 1970s, when Hutchinson, MacArthur, Wilson, and others began to ask focused and testable questions in the idiom of theoretical physics. What explains the observed power-law relationship between numbers of species (of birds, plants, butterflies, whatever) on different islands in an archipelago (real islands or virtual islands, as in lakes or mountaintops) and the islands' area? How similar can species be yet persist together? How do the patterns of species' interactions within a food web affect its ability to withstand disturbance? Why are some natural populations relatively steady from year to year, others cyclic, and others widely fluctuating? MacArthur and Wilson's *Island Biogeography*, the initial volume in the influential PUP series of Monographs in Population Biology, focuses mainly on the first of these questions, and did so in a way which marked a seismic shift in the discipline.

At first, some ecological empiricists resented arrivistes, who had paid no dues of years of toil in the field, presuming to mathematicize their problems (sometimes sweeping aside arguably irrelevant, but certainly much loved, details in the process). Others welcomed the newcomers too uncritically. Look, however, at the

ecology texts of fifty years ago, and you will find very few equations; today's, by contrast, contain a blend of observation, field and laboratory experiment, and theory expressed in mathematical terms. I think this reflects the maturing state of this vital subject, although it still has more questions than answers. The mathematical traffic, moreover, has not been all one-way: some of the seminal developments in chaos theory were prompted by ecological problems.⁶

Immunology offers a somewhat different picture. Here there are truly remarkable advances in describing and understanding, at the molecular level, how individual viruses and other infectious agents interact with individual immune system cells. And on the basis of such knowledge, so brilliantly detailed on the molecular scale as almost to defy intuitive comprehension, we can, for example, design drugs that suppress viral replication. Chemotherapy against HIV is one notable example. At the same time, however, there is as yet no agreed explanation for why there is so long, and so variable, an interval between infection with HIV and the onset of AIDS. Indeed, I guess that many researchers in this field do not even think about this question, much less recognize that it has not yet been answered. But I suspect the answer may necessarily involve understanding how whole populations of different strains of HIV interact with whole populations of different kinds of immune system cells, within infected individuals. And understanding the nonlinear dynamics of such a system will require mathematical models, with similarities to, and differences from, those that have helped us understand population-level problems in ecology and the epidemiology of infectious diseases.⁷ It may even be that the design of effective vaccines against protean agents like HIV or malaria will require such population-level understanding. As yet, this maturation from technically superb description at the level of individual cells and invasive agents (viruses, etc.), to fundamental dynamical understanding at the level of populations of cells and agents, is in its early stages. Today, understanding in the idiom of theoretical physics is even less to be found in immunology textbooks than were mathematical models in ecology texts a generation ago. I venture to predict that the corresponding immunology texts will indeed look different in, say, twenty years' time. In the meanwhile, however, immunology awaits its MacArthur and Wilson.

I must emphasize that none of this is intended to belittle purely descriptive work. It is the foundation on which all subsequent structures rest. For this reason, I was pleased to see two "bird

guides”—Ridgely and Gwynne’s *Guide to the Birds of Panama: With Costa Rica, Nicaragua, and Honduras* and Hilty and Brown’s *Guide to the Birds of Colombia*—among the PUP100. These and their kin are multidimensional: scholarly tools for ever larger cadres of tropical ecologists; catalogs for obsessive bird-watchers; useful companions for the casual tourist. They can also be highly profitable, as the following tale illustrates. On one occasion, about twenty-five years ago, PUP accidentally included a flyer for some of these bird guides with its customary mathematical books flyer sent to the academic mathematical community. The result: one of the bird guides recorded the highest sales ever for such a mathematics-community mailing, beating any mathematics book.

I, like many of my colleagues, was attracted to a life in science for essentially hedonistic reasons. How endlessly fascinating to be able to spend much of one’s life engaged in a game with nature, with the rather peculiar rules of the game being to work out what the rules are. So what about the manuals on how to play this game well? In this regard, the year 1945 was an exceptional one for Princeton University Press, with three remarkable and influential books, all included here in the PUP100. I read Hadamard’s *The Psychology of Invention in the Mathematical Field* as saying that there simply are no recipes for invention, but that a lot of odd stuff happens in what might best be called “the subconscious.” Pólya’s *How to Solve It* is simply stunning but, like Hadamard, should never be mistaken for suggesting there is a recipe for, or routine route to, success. If every young teen were to read Pólya’s book, and be exposed to the intellectual pleasures it reveals, a lot more people would major in mathematics! And, to complete the trio, Popper’s *The Open Society and Its Enemies* is a magnificent and humane achievement in political and social philosophy. It is, incidentally but not irrelevantly, a book that can be read as a caution against dogma, doctrine, and stultifying bureaucracy; it is a sad irony that some of his earlier work on the philosophy of science has, on occasion, been misread by some academic apparatchiks as a rigid blueprint on “how science is to be conducted” (as if it were some form of exercise in painting by numbers). Forty years on saw another PUP100 trio (two in 1985, one in 1986) of fascinating insights into the existential character of the creative process, by Richard Feynman, Steven Shapin and Simon Schaffer, and Amos Funkenstein.

In most OECD countries today there is much talk of the knowledge economy and a pleasing recognition that the fruits of new knowledge play at least as large a part in productivity growth as

do the more time-honored labor and capital. Furthermore, there are many recent studies⁸ showing that most new knowledge derives from basic research in university settings (particularly in the USA and the UK), infested as they are by the irreverent young. As I mentioned above, these researchers are, by and large, driven by curiosity. On the other hand, their patrons, these days primarily governments on behalf of taxpayers, are understandably driven by economic practicalities. Such a dissonance of motives has inherent tensions, although it has served us well up to now. However, as the world becomes effectively ever smaller and the knowledge economy a more competitive marketplace, there are signs that research funders and administrators are seeking to do “a better job of managing creativity.” I think this is worrying. There are good questions to be asked about how best to help originality and creativity flourish. We do not really understand what made Pericles’ Athens or Leonardo’s Florence or Shakespeare’s London the places that they were. The question is indeed rarely asked, much less answered. In short, the notion of managing creativity is not necessarily oxymoronic, but if any such aim is pursued carelessly, the outcome can be truly moronic. Many hapless readers will be able to provide examples from their own institutions.

These are important questions and worries, which I suspect loom larger today than they did a generation ago. Some of the books in the PUP100 on economic ideas, policies, and management are of some relevance, but I think the definitive work on managing creativity and originality in mathematics and science has yet to be produced. Maybe we should all hope it never will be.

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Notes

1. See Carl Djerassi’s 2004 play *Calculus* or James Gleick’s biography *Isaac Newton* (New York: Pantheon, 2003).

2. A simple, and quite magical, example is the relationship $e^{i\pi} = -1$ which involves the fundamental constants e and π , and the ethereal “square root of minus one,” i .

3. See, for example, R. A. Fisher, *The Genetical Theory of Natural Selection* (reprint, New York: Dover, 1958).
4. G. H. Hardy, *Science*, 28, 49 (1908).
5. See, for example, M. A. Nowak et al., “The arithmetics of mutual help,” *Scientific American*, 272, 76–81 (1995).
6. For further discussion, see G. Farmelo, ed., *It Must Be Beautiful: Great Equations of Modern Science* (London: Granta Books, 2002), 212–229.
7. M. A. Nowak and R. M. May, *Virus Dynamics: Mathematical Principles of Immunology and Virology* (Oxford: Oxford University Press, 2000).
8. Publicly funded research, predominantly in universities, accounted for 73 percent of all papers (46 percent U.S., 29 percent “foreign”) cited in U.S. industrial patents in 1993–1994. Corresponding sectoral figures are 79 percent for patents for drugs and medicines and 76 percent for chemicals, to a low of 49 percent for electrical components. See F. Narin et al., *Research Policy*, 26, 317–327 (1997).

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