THE FACES OF EVIDENCE *(IN MATHEMATICS)*

BARRY MAZUR

Rough notes for my before-dinner talk[1] *at the Cambridge Scientific Club, Dec. 5, 2013*

In contrast to the complex evolution and unfolding of the meanings—through centuries of usage—of many of the words we currently use, the word *evidence* has a seemingly uncomplicated history. It comes to English around the 1300s, meaning then, as now, "appearance from which inferences may be drawn." It was a loan-word from Late Latin *evidentia*, i.e., proof. Originally it meant *distinction, clearness,* from Latin ex+videre.

But within each of the disciplines represented in this room, the term has, I'm sure, an interesting and distinct complexion, a distinct profile; each has, perhaps, a different kind of goal—let alone a different specific goal.

In the Fall semester 2012 I had the pleasure of co-running a seminar-course—*The Nature of Evidence*— in the Harvard Law School with Noah Feldman (who, two weeks ago, gave a public lecture at the Science Center on this subject, and with that title). The seminar-course was structured as an extended conversation between different practitioners and our students, and us. We found it very useful to learn, in some specificity, from people in different fields–via concrete examples graspable by people outside the field–what evidence consists of in *Physics, Economics, Biology, Art History, History of Science, Mathematics, and Law.* We heard about the very articulated internal debate in Economics on the relative merits of randomized control experiments, natural experiments, and more discursive modes of argument. We heard about issues in Biology regarding the use of genetic data. In Physics, about the structure of evidence that determined the existence of the Higgs boson, in the History of Physics, about evolution of the cloud chamber. And Law, which has its own precise rules explicitly formulated.

I'm hoping that this evening is an occasion to continue this dialogue to compare a bit of the structure of evidence in each of our own fields: what is the nature of the evidence that must be presented in your field to reach a consensus conclusion about a 'new result"? Let's also aim for a comprehensive view and not merely a fragmented "evidence-in-X, evidence-in-Y, etc." with no matrix that ties these bits together. For there are indeed general formats and attitudes towards evidence that can bear some discussion. For example, compare these (standard, but cartoon) formats for the treatment of evidence:

[1]A close version of what I am about to present has already been published as *Shadows of Evidence* in the Science Newsletter of the Simons Foundation: `https://www.simonsfoundation.org/mathematics-and-physical-science/shadows-of-evidence/`.

- the classical Baconian view, where data is (supposedly) gathered as evidence to *test* explicit hypotheses;
- the Bayesian view, where evidence is fed back to modify the (meta-)hypotheses—i.e., to 'educate' the *priors*;
- a recent strident claim: given that it is dead easy to amass as much 'Big' data as one wants—the data itself is meant to stand as some pure source of 'evidence-without-a-specific-intent,' independent of any hypothesis, any model, and simply mined whenever needed.

It is striking, though, how different branches of knowledge—the Humanities, the Sciences, Mathematics—justify their findings so very differently; they have, one might say, quite incommensurate rules of evidence. Often a shift of emphasis, or framing, of one of these disciplines goes along with, or derives from, a change of these rules, or of the repertoire of sources of evidence, for justifying claims and findings in that field.

The way the word *evidence* is used in a field, and how its meaning evolves, can already tell us much about the profile, and the development, of an intellectual discipline.

Consider, for example, Charles Darwin's language in *The Origin of Species*—specifically, his use of the words *fact* and *evidence*—as offering us clues about the types of argumentation that Darwin counts in support, or in critique, of his emerging theory of evolution[2]. Sometimes Darwin provides us with a *sotto voce* commentary on what *shouldn't* count—or should only marginally count—as evidence, such as when he writes:

> But we have better evidence on this subject than mere theoretical calculations.

He spends much time offering his assessment of what one can expect—or not expect—to glean from the fossil record. He gives quick characterizations of types of evidence—'historical evidence' he calls 'indirect' (as, indeed, it is in comparison with the evidence one gets by having an actual bone in one's actual hands). These types of judgments frame the project of evolution.

The subsequent changes in Darwin's initial repertoire, such as evidence obtained by formulating various mathematical models, or the formidable technology of gene sequencing, etc. mark changes in the types of argument evolutionary biologists regard as the constituting a genuine result in the field—in effect, changes of what they regard *evolutionary biology* to be.

To start a conversation, I'll show that this notion, *evidence,* is not cut and dried, even in mathematics, where people often construe it to mean proof, and nothing more. Although

[2]As is perfectly reasonable, Darwin reserves the word *fact* for those pieces of data or opinion that have been, in some sense, vetted, and are not currently in dispute; The word *evidence* in *The Origin of Species* can refer to something more preliminary, yet to be tested and deemed admissible or not. Sometimes, if evidence is firmer than that, Darwin will supply it with an adjective such as *clear evidence* or *plainest evidence*; it may come as a negative, such as "there isn't a shadow of evidence."

clarity is the signature of mathematics, the shape of evidence relevant to understanding mathematics, or working with mathematics, is not at all straightforward.

There are, it seems, two distinct discussions about *evidence in math* to be had:

- The issue of proof, the vaguaries of rigor, formal structures, logic and the limits of logic, and the well-known crises in the foundations of mathematics.

This is a conversation that has been steady, is on-going, and is a mainstay in the philosophy of mathematics. It is astonishing how intense, how rock solid, the consensus of agreement among mathematicians is, regarding whether a result is established or not. This is not to say that a disagreement can't occur, but when it does and persists it often has a *name* and star-billing, such as the *Hilbert-Brouwer Controversy.* Nevertheless, when you are working within mathematics you often feel how vastly more there is left to be done: most of this edifice is yet to be conceived, designed, built. Languages wait to be fashioned, that are needed even to set down the architectural plans of these future structures.

So there is also:

- the more personal, more individual, modes of evidence that persuade the mathematician to think that a certain direction is promising, another not—that this formulation may be wrong-headed (even if not wrong)—that this definition is important—that this statement is likely true.

Usually under limited knowledge and much ignorance—often plagued by mistakes and misconceptions—we wrestle with analogies, inferences, expectations, rough estimates, partial patterns, heuristics. The Emily Dickinson line "Tell it true but tell it slant" is second nature to us since *slant* is often the way we find the truth in the first place. We depend on "evidence" in order to proceed—to assess the promise of an approach to a problem—to guess where to look and what to look for—and the evidence often comes from unexpected places, and sometimes in unexpected vocabulary.

I suspect that every discipline has such two-tiered levels of possible discussion. It is the shadows of evidence that mathematicians use to negotiate through conjectures, guesses, aspirations—i.e., the second of the possible 'discussions'—that I will focus on.

For reasons of time I can't give examples illustrating all the species of evidence playing such a role in mathematics. I will only talk about the following three categories.

- Evidence stemming from 'visual' intuition
- Numerical evidence
- Analogy.

1. Evidence stemming from 'visual' intuition

This is so ubiquitous, that it almost doesn't need any concrete example to illustrate it, but here's a dramatic emergence of an energized direction of research stemming from new pictures.

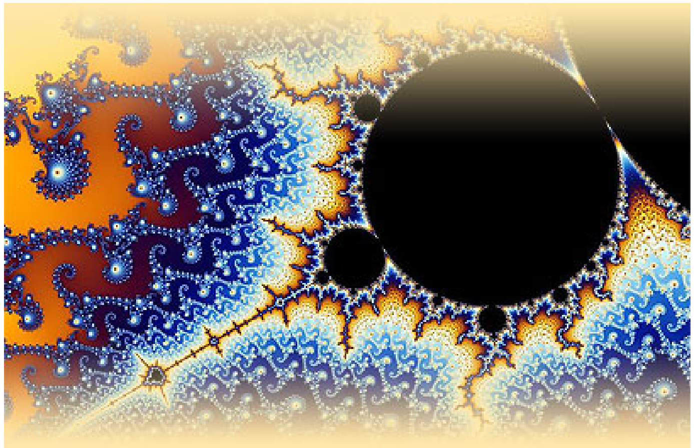
I'm referring to the rise of the *fractals* of Benoit Mandelbrot.

Up to the end of the First World War, the theory that was the progenitor of 'fractals' was called Fatou-Julia theory. That theory studied the structure of certain regions in the plane (very) important for issues related to dynamics. By 'dynamics' here I mean a subject that might be called 'mathematical dynamics,' which is the study of the qualitative, or quantitative, teleological phenomena that occur when you iteratively apply the same transformation (or a systematically modified transformation) to a space, a 'phase space' for example. If I use this description, mathematical dynamics is everywhere: any differential equation is such, or any algorithm that can be iteratively applied. Take–for example–Newton's method for finding the zero of a function.

Surely, Fatou or Julia would not have been able to make too exact a numerical plot of regions arising related to the teleological nature of orbits arising from such iterations, if those regions are not utterly simple. And unless you plotted such a region very accurately, it would show up as blob in the plane with nothing particularly interesting about its perimeter–something like this:

Partly due to the ravages of the first world war, and partly from the general consensus that the problems in this field were essentially *understood*, there was a lull, of half a century, in the study of such planar regions—a particular class of them now called *Julia sets*.

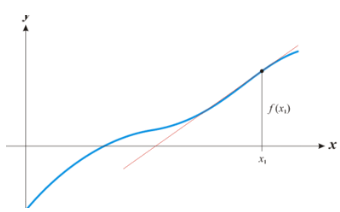
But in the early 1980s Mandelbrot made (as he described it) "a respectful examination of mounds of computer-generated graphics." His pictures of such Julia sets and related planar regions were significantly more accurate, and tended to look like the figures below (which is of a slightly different dynamical problem than the one considered by Julia, and is a slightly more modern version of the one Mandelbrot produced).



Iterative methods tend to provide a richness of structure when accurately visualized, and they tend to lead to new applications, new theoretical considerations.

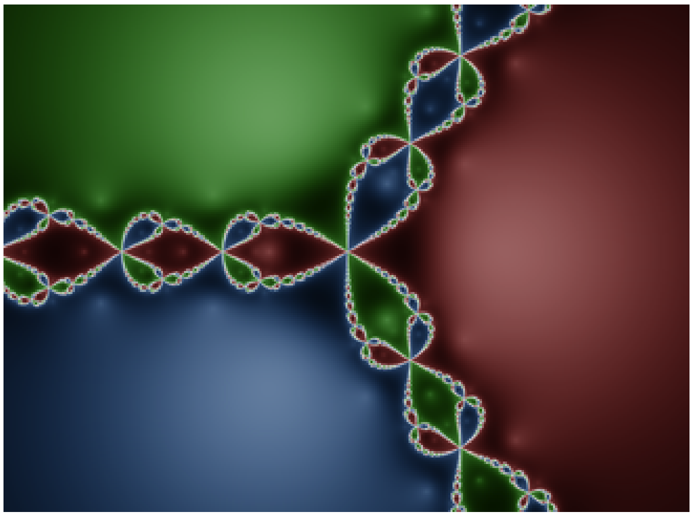
For example, consider Newton's method, which allows you to find (successive approximations to) a *zero* of a real valued (smooth) function $f(x)$. If you're sensible, and are interested in it as a handy method for the job of locating a zero of a given function, you begin by looking for a starting point x_1 at which the function f has a value close to zero. So, x_1 is *your prior*. You then correct your starting guess x_1 by the (pre-designated) procedure:

$$x_2 := x_1 - f(x_1)/f'(x_1)$$



making x_2 as your better guess, and then do this again (and again).

This is enormously useful but when simply visualized in the complex plane, it unfolds in a way that suggests much more in a theoretical framework than its germane applications might hint at[3].



From such pictures alone it became evident that there is an immense amount of structure to the regions drawn and to their perimeters. This almost immediately re-energized and broadened the field of research, making it clear that very little of the basic structure inherent in these Julia sets had been perceived, let alone understood. It also suggested new applications. Mandelbrot proclaimed—with some justification—that "Fatou-Julia theory 'officially' came back to life" on the day when, in a seminar in Paris he displayed his illustrations.

Computers nowadays (as we all know) can accumulate and manipulate massive data sets. But they also play the role of *microscope* for pure mathematics, allowing for a type of extreme visual acuity that is, itself, a powerful kind of evidence.

2. Numerical evidence

Lacking a general theoretical result about some question about which one hopes one has correctly guessed the answer, one naturally resorts to checking particular instances to bolster confidence in the guess. But how many instances is enough? How many instances do you need to strengthen your confidence? I'll allude here to two utterly different types of contemporary examples of numerical issues, perhaps somewhat contrary to each other. I say 'allude to" because I'll give no real details for either, but still hope that the contrariness will come through.

[3]Here, Newton's method is applied to arbitrary points in the complex plane in order to "find" the zeroes of the function $f(z) = z^3 - 1$.

The first question has to do with a certain class of algebraic curves[4] (given with rational number coefficients). It asks for the percentage of these curves that have *infinitely many* rather than *finitely many* solutions[5]. People have computed something on the order of 100 million such curves to produce evidence for the answer. When computed 'only' for these 100 million curves, however, you get that around 60% of them have infinitely many solution. What is curious here is

- People (who know about the resources available for computation) are not hopeful that one can expect, in our lifetime, to make vastly more than those 100 million computations. That is, as far as numerics go, we won't see any change in the data. Yet:
- There is a firm consensus (of belief in certain conjectures that imply) that the ultimate percentage is not "about 60%" but rather it is *exactly* 50%.

So much for the power of mere numerical computation—however massive—to revise an opinion based solely on conjecture and shadows of evidence!

The second issue, with a spirit quite different from the first, involves a *single* computation, one that any of us can do:

$$196884 \quad = \quad 1 \quad + \quad 196883.$$

In the early seventies, the mathematician John McKay made that simple computation, but coupled it with a very important observation. What is peculiar about this formula is that the left-hand-side of the equation, i.e., the number 196884, is well-known to most practitioners of a certain branch of mathematics (*complex analysis*, and the *theory of modular forms*)[6] while 196883 which appears on the right is well-known to most practitioners of (what was in the 1970s) quite a different branch of mathematics (*The theory of finite simple groups*)[7]. Mckay took this 'coincidence"—the closeness of those two numbers[8]—as evidence

[4]these are elliptic curves ordered systematically in terms of the size of their conductor

[5]I.e., *rational points*, but experts will realize that I'm using a very firm classical conjecture to interpret the data this way.

[6]196884 is the first interesting coefficient of a basic function in that branch of mathematics: the elliptic modular function.

[7]196883 is the smallest dimension of a Euclidean space that has the largest sporadic simple group (*the monster group*) as a subgroup of its symmetries.

[8]McKay gave a convincing interpretation of the "1" in the formula as well

that there had to be a very close relationship between these two disparate branches of pure mathematics; and he was right!

3. Analogy

I'm annoyed by André Weil's famous paragraph on analogy in mathematics, but feel that it tends to start discussions with a bang, so I'll quote it:

> Nothing is more fruitful—all mathematicians know it—than those obscure analogies, those disturbing reflections of one theory on another; those furtive caresses, those inexplicable discords; nothing also gives more pleasure to the researcher. The day comes when this illusion dissolves: the presentiment turns into certainty; the yoked theories reveal their common source before disappearing. As the Gita teaches, one achieves knowledge and indifference at the same time.

Now mathematics is built on analogy. Some of these analogies are so successful, and in fact so universally acknowledged as to become almost transparent, 'evident.' We all 'have' these analogies in our central nervous system. *Time* as *Distance*, for example. (We say "far in the future," or "a long time ago" without acknowledging the depth of insight that some proto-mathematician must have had in order to utter this quintessentially mathematical leap.)

The history of mathematics is studded with analogies between theories A and B, where the analogy has the force of some level of evidence, in that things true about A, appropriately translated to B may not be deemed true yet for B, but perhaps are thought to be sufficiently plausible—based merely on the 'evidence' of the analogy—that it may be worth your time to look for a proof of it in the theory B.

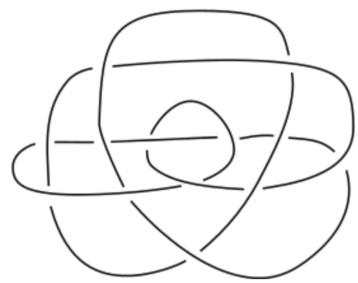
One example is the Euclidean study of *ellipses, parabolas, hyperbolas.* These were viewed fruitfully as analogous—until—at the time of Appolonius of Perga—that "presentiment turned into certainty," when they were all three of these types of mathematical objects were viewed as 'the same thing,' i.e., conic sections.

Another example is Archimedes 'Method' where volumes were taken to be like objects that have weight and were laminated into a continuous stream of cross-sections, each of which was weighed on a lever (using 'Archimedes law of the lever') to heuristically show his famous quadrature theorem.

But to give another example of an analogy, a contemporary analogy that might be surprising to some:

$$\textbf{Knots} \quad \Longleftrightarrow \quad \textbf{Prime numbers}$$

Now, a **Knot** is something like this:



and a **Prime number** is something like this:

$$37.$$

So, how in the world can these distinct kinds of mathematical objects be analogous. Nevertheless, the analogy between them is staggeringly strong, so that if we know some particular thing about one of these kinds of objects, we have motivation, at least, to take this knowledge as analogical *evidence* that the corresponding 'thing' might possibly hold for the other kind of object.

For example, certain ways of measuring the 'interaction' between two different prime numbers carries over directly—via this analogy—to ways of measuring the 'interaction' or the linking of two different knots. To give you a sense of what it might mean for primes to interact with each other, let P and Q be two primes—for example:

$$P = 2^{57,885,161-1} \text{ and } Q = 2^{43,112,609-1}.$$

Now ask yourself the following two (very different, but linked) questions:

1: Is there a solution (in whole numbers X and Y) to the equation:

$$P = X^2 + QY?$$

2: Is there a solution (in whole numbers X and Y) to the equation:

$$Q = X^2 + PY?$$

A famous theorem of Gauss tells us that for any pair of prime numbers P and Q, no matter how difficult, or easy, it is to answer either one of those YES or NO questions, the answers to them are linked. By 'linked,' I mean that it is *dead easy* to tell, for a given P and Q, whether the answers agree (i.e., YES to both or NO to both) or disagree (i.e., one of them gets a YES and the other gets a NO)[9] even though you may have a slightly more difficult time pinning a definitive YES or NO to either of these questions.

[9]That's the case for $P = 2^{57,885,161-1}$ and $Q = 2^{43,112,609-1}$.

The analogous result in the world of knots has to do with what is called the *linking number of two knots* K and L which says how intertwined the two are with each other[10].

Mathematics is simply laced with analogies that are bearers of evidence from one sub-discipline to another!

4. Is there a format for having a general conversation about evidence across fields?

. . .

[10] The corresponding "Question 1" for knots would compute this linking number by–in effect–measuring in an appropriate way how many times K winds around L (the definition needed here is elegant) and the "Question 2" for knots would reverse the roles of the two knots in that computation. The theorem, again says that a response to "Question 1" gives us a response to "Question 2" and, of course, vice versa.