CHAPTER 1

The structure and eventual end of math

1. Apologia

Skip this if you just want to get to my analysis; I include this because eschatology is deeply disturbing and offensive to many people (see further discussion in my essays on eschatology).

To get it out of the way:
I do *not* believe that math is over or anywhere close to over. That's why there's an "eventual" in the title: I believe that math will *eventually* end (possibly *millenia* from now), in some sense, and wish to contemplate in what sense and what an end might look like, and what the overall structure of math is, both as it's visible now, and how I imagine it'll look in retrospect.

There are huge worlds to explore – the journey is very far from over; I just want to speculate on how the view from the top will look.

2. Intro

How might math end – with a bang or a whimper?

I argue that math on the whole can be seen as a digraph of theories, with a highly interconnected core. Math is the study of abstract patterns, and there are potentially very many of these, not necessarily related: new fields may arise forever, and math as a whole may never end. However, particular *fields* may end, and I expect the field itself to likewise end. Particular subfields generally end with a whimper, exhausting interesting results, and I expect the field as a whole to gradually end in this way: not only all interesting results, but all interesting fields, all interesting perspectives will eventually be exhausted – this is my eschatology of science generally.

I expect the core of math to end in a more dramatic and satisfying way, via developing a meta-mathematically closed theory.

I elaborated on the first end in "ω: with a whimper", and on the second in "Ω: with a bang".

Below, I sketch an overview of the structure of mathematics, and how I'm lead to these posited ends.

3. Outline

Math, as a science (an abstract science), has a finite domain, and will eventually be exhausted. More subtly, it's infinite but it degenerates into randomness (just listing facts): there aren't *that many* big patterns, and you usually find them first; eventually you exhaust the big and not-so-big patterns and just have myriad details.

See more in my essay "Eschatology of science" (on why I think sciences end), and in "ω: with a whimper" (on this exhaustion).

Within math, there are subfields ("theories"), and theories spawn theories; a classic example is how solving polynomials lead to Galois theory, which lead to group theory. To wax biblical, X begat Y, Y begat Z, and so forth. You can draw a digraph of theories, drawing an arrow if one applies to the other (historically, was spawned by the other).

This much is common to all sciences: one expects phenomena to have a theory describing/explaining their structure, and for particular aspects to have their own specialized subfield. Math has two special properties:

- Meta-math is math;
- This graph is very interconnected.

These facts are related. Regarding meta-math, because the theory of math is more math (*unlike* physical science, where the theory of a science is math), it means that math is self-referential: thus instead of the graph being acyclic, you can have fields refer to each other, or even to themselves. This is unlike in physical sciences, where there is more direction, largely in terms of scale: to do biology it helps to know chemistry, but knowing biology helps hardly at all in chemistry (beyond providing some examples). There are phenomena at the same scale which interact (for example, the oceans and the atmosphere), and these are fascinating, but these are less pronounced than in math.

Regarding the interconnectedness, I'd dub this "the unreasonable effectiveness of mathematics in mathematics" (echoing Wigner's "The Unreasonable Effectiveness of Mathematics in the Natural Sciences"). That is, math is more relevant to *other* math than you'd naively expect: mathematical theories have more (meta-)mathematical structure than you may expect, and *the same* mathematics recurs. For instance, isn't it surprising that eigenvalues of matrices tell you something (in fact, a great deal) in graph theory? Of course it's possible to understand this connection[1] (and any other specific connection), but the very frequency is striking.

Now, all knowledge is related in some ways (often very long or tenuous chains), and it is possible to do math that is not very relevant to the rest (work out on the branches of the graph), but much math is rather more closely related: there is a sense of an ordered whole.

One might imagine mathematicians as like blind folk, studying different parts of at elephant (to use an old image), but the picture is more beautiful than that: rather than studying a disconnected part (like the trunk), fields of math instead provide different perspectives *on the whole enterprise*.

I suggest that math is not only richly interconnected, but has a "core" of particularly interconnected areas, and that this core, in addition to being particularly beautiful, may have a satisfying conclusion, in a unified theory.

As examples of "central" areas, I'd suggest algebraic geometry and Lie theory; they are connected with linear algebra and number theory and complex geometry and differential equations and geometries, for instance. I don't have a good feel for how central algebraic topology and geometric analysis are: they feel deep and central, as in topological modular forms and Atiyah-Singer, but I don't know well enough.

If one actually wrote down this proposed digraph (say, by papers and their references), I suspect you'd see this sort of core, and could indeed quantify it (you'd also obviously note clumping in particular subfields).

FIXME: draw a picture

Outside of this core, other areas are more tangential and have less elegant theories, degenerating more quickly into tedium. These facts are not independent: interconnections yield more elegant theories.

4. Others' overviews of math

Two exceptional essays are Timothy Gowers's "The Two Cultures of Mathematics" and Terence Tao's Simons Lectures (and ICM address) on "the dichotomy between structure and randomness".

I summarize these and relate them to each other and to my picture.

[1] To wit: geometric analysis says that you can understand geometric spaces by studying analytical structures on them, and here I'm referring to eigenvalues of the Lagrangian; think of a graph as a geometric object (in fact, a negatively curved space).

Gowers classifies mathematicians into “theory-builders and problem-solvers”, not suggesting that there is a sharp distinction, but different relative priorities. I prefer to refer to “theory-building and problem-solving”, to make it clearer that these are different *activities*, but that individuals engage in both. (I’m told this point of view originated earlier, in Pal Turán’s 1970 speech on Alan Baker’s Fields Medal.)

Tao distinguishes structured and unstructured objects, and the corresponding mathematical tools. He is particularly interested in hybrid objects, and the question of disentangling the structured and unstructured parts of such objects.

Particular fields are more or less structured, and feature more or less theory-building versus problem-solving; I identify the core as the most amenable to theory-building area, which generally coincides with the most structured.

These are not sharp distinctions: even structured fields have unstructured objects: algebraic geometry is a very structured field, but the moduli space of curves of high genus ($\geq$ 22 at least) is of general type, hence has no nice, structured parametrization. Conversely, even unstructured objects have some structure (Ramsey theory).

As an example of the value of problem-solving, even in a very heavy theory area, consider Deligne’s proof of the Weil conjectures (notably the analogue Riemann hypothesis) by a clever argument, rather than by proving Grothendieck’s “standard conjectures”. It is said that Grothendieck was uninterested in the proof, since it didn’t contribute to his theory by proving the standard conjectures. On the other hand, the standard conjectures remain unproven, over 30 years later, validating Deligne’s approach.

Conversely, Andre Joyal’s categorification of generating functions (as in Baez’s “This Week’s Finds” Week 202) is an example of the value of theory-building in a problem-solving area.

Now, there is an obvious rough analogy between “structured objects = theory-building” and “unstructured objects = problem-solving”, but this shouldn’t be taken too far. Obviously you can build a more elaborate theory about structured objects (generic unstructured objects have no global symmetries, for instance), and thus studying unstructured objects is more often problem-solving, but there are general theories of unstructured objects (such as statistics, computation / algorithmic information theory, probabilistic results in combinatorics, and genericity results in analysis), and problem-solving is very common and applicable about structured objects.

Note also that empiricism in math is most useful for *unstructured* areas. In structured areas, they may reveal or suggest patterns, but you then expect there to be an elegant proof of that precise pattern. These are *really different kinds of math*.

5. Centrality

Returning to centrality, being very structured is no guarantee of centrality: the octonions and E_8 and the Leech lattice and exceptional Lie and Jordan algebras and sporadic simple groups and Golay codes and the corresponding Steiner systems are an extraordinarily beautiful chapter in math – probably *the* most beautiful collection of objects, but they feel far less central than the more mundane complex numbers. One might say that they are *too* structured: they are beautiful but inapplicable. Notably, the unitary groups are far more relevant to physics than the exceptional groups.

The key point of centrality is not structure, but instead *reference*, especially *self-reference*, and more generally *mutual reference*: an object is central if its theory (the study of that object) both:

- is applicable to objects other than itself
- has structure of its own

...and if the object itself occurs in the structure of one of these other theories, so it connects back to itself, or at least these fields have rich connections, instead of being separate theories, each a deeper strata, unrelated to the others.

A dramatic example of self-reference is representable functors, which I elaborate in "Ω: with a bang". For instance, moduli spaces of curves are themselves algebro-geometric objects, and generalized cohomology theories are represented by spectra.

By this measure, combinatorics looks not core because of a *lack* of meta-mathematics: it has general principles (as Gowers describes), but these are *rough*, and lack structure, so you can't build a *mathematical* theory of combinatorial principles.

A similar problem occurs in logic: it does have meta-mathematics (in spades), and has applications to other areas (via model theory, AIT, undecidability), but other areas are not applicable *to it*: it's the *mutual* connections that are most valuable.

Also, the applications of combinatorics and logic to other fields are to "non-core aspects" of these fields.

These may simply be current limitations – deep new connections arise with some frequency – or these fields may simply be relatively peripheral.

5.1. Ethics: value in mathematics. An obvious measure of value in math is *power* a result that proves many other results is powerful.

Turning next to field, all else being equal, a result in a central field is more valuable than an equally powerful result in a more peripheral field.

That said, deep results in relatively peripheral fields are still very valuable and respected: very few (if any) mathematicians outside of logic use Cohen's set forcing, and the main external application (you can safely reject the axiom of choice) is interesting but not particularly useful, but it is still considered an important and valuable result, and Cohen got a Fields medal for his work.

6. Historical notes

6.1. Classical thread. I haven't mentioned the roots of this digraph of math. Math is not hermetic and self-justifying (though it can do: the theory is beautiful enough to justify studying on those grounds alone): at the bottom are natural questions, either arising mathematically (classically arithmetic, geometry and logic) or from scientific applications (notably physics; more recently, gambling and computers).

The interconnected core roughly corresponds to the thread coming from the Greeks; logic has become very specialized and thus less central, but geometry continues to be central (notably Klein's Erlangen program and philosophy, and also in the forms of analysis, topology, and group theory), and arithmetic (in the form of number theory) certainly is!

This is not a failure of imagination: math has come up with myriad exotic concepts, but the most interconnected areas are mostly connected to intuitive ideas.

6.2. New simple structures. Often the study of a complex area reveals a simple basic structure, one that can be studied (say, by undergraduates) without need the elaborate framework in which they were first studied.

Topology is a dramatic example of this: it is a very elementary concept that was not identified as worthy of study in its own right until the 20th century.

Similarly, you can do algebraic geometry without studying elliptic integrals, and Fourier theory without studying Bessel functions.

This is an extremely valuable aspect of math, making much of its breadth accessible without first requiring great technicality. On the other hand, it means that many basic math classes are dry basic technicalities, (as the worst aspects of Bourbaki) rather than rich studies of complex examples: PDEs are more engaging than measure theory, though less elementary.

Again, I suspect that these new simple fields will eventually peter out: there are only so many simple structures.

7. Structuralism in math

This notion of "increasingly complex patterns" and "more central areas" underlies my structuralist point of view on math: math is primarily about finding patterns, not solving problems – and as brilliant as the techniques people develop are, it has more the character of discovery than invention.

Why? Because *to the extent* that math (or science) is about solving problems, the answer will "often" be "have a computer try everything" (as demonstrated in "ω: with a whimper") – and this answer is both unsatisfying and "not mathematical". You can answer the question, but you haven't learned anything (beyond the answer), and answering the next question is no easier – *but that is what we should expect in general*. You're not just looking for answers to questions, you're looking for *nice* answers.

Proofs by routine applications of existing results are similarly not respected, but that's also because these results are easy, not just because they don't reveal anything new.

The fact that "bad answers" aren't as respected as "good answers" indicates that this is a generally held aesthetic, even if not articulated: revealing structure is "good math"; just answering isn't especially good in and of itself.

Pragmatically, there's nothing wrong with computer-assisted proofs, as of the 4-color theorem or Kepler's conjecture (other than the practical difficulty in verifying such a proof): after all, you've solved the problem – however, this suggests that the result and the field "aren't that interesting". The proofs of the 4-color conjecture and Kepler's conjecture are great achievements, but they don't suggest interesting structure to study further.

8. To conclude

So the upshot is:

- Math contains a large number of interconnected subfields
- I expect both these subfields individually, and the entire enterprise, to eventually end (or rather, degenerate into tedium).

- To some extent math is irreducibly diverse: the subfields are simply different phenomena.
- There is a certain core of highly interconnected fields, which may have a elegant end.

From what I know, math has come a very long way, and has a long way yet to go. One can take this as intimidating or exciting (so much to learn; so much yet to do!), and shouldn't expect ultimate closure in this lifetime.

Indeed, even in a particular field, closure may be centuries away, and is futile to expect: of Grothendieck's work on foundations of algebraic geometry, David Ruelle says: "He had achieved level -1 and was working on level 0 of something that must be 10 levels high.... At a certain age it becomes clear you will never be able to finish the building".

Practically, we've many objects to study, theories to build, problems to solve, and should take joy and get satisfaction from the partial answers, the steps on the way.

Math is building (or revealing) a cathedral, and while we will not live to see it done, we can delight in the individual contributions and slow but perceptible progress.