How do Mathematical Examples contribute to Intelligibility?

Exploratory Practice

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The basic point of this talk is that roles of mathematical examples in mathematical understanding, or as I prefer to cast the matter, how mathematical examples contribute to mathematical intelligibility, is philosophicaly unexplored territory —terra incognita. I hope to encourage conquistadores (y conquistadoras.)

I hasten to admit this is not my point originally: patiently listening to me philosophize, Tim Gowers suggested this was the (or my?) salient unaddressed issue. Of course, Tim would be marvellously better equipped to address it than I; had the organizers thought of it, they might well have invited him instead... I thank them for leaving the opportunity to me, outright of course, but also because Tim might be a hard act to follow.

A professional philosopher might have tried a sharper challenge: that *my* way of thinking about math might be unable to accommodate major mathematical roles of examples. (I trust Tim's point would have been just that I had not yet done the work required.) A particularly unprejudiced philosopher could broaden the challenge: that no known philosophical approach could usefully address this (beyond Popper's old but altogether valid logical point about counterexamples).

Beyond the Popper Point, the relevant roles of mathematical examples seem to lie, broadly speaking, in landscape-structuring and exploratory practice.

Lakatos' *Proofs and Refutations* comes to mind here, one prominent counterexample to my basic point. Just such contributions would indeed seem particularly salient to creative mathematicians, and I follow Lakatos in taking mathematical intelligibility to creative mathematicians as my philosophical target.

In Anglo-American analytic (AAA) mainstream, there are all-to-familiar objections to such a project; I will take some on of those first. To the extent mathematical examples have such landscape-structuring and exploratory roles, it seems pretty obvious that the sharper challenge (no known philosophical approach...) hits the very core of the logico-analytical tradition in philosophy of mathematics ('FR' hereafter).

The nature of intelligibility falls squarely within the broader epistemological concern in philosophy; patently from the moment in Plato at which *episteme* is thematized in Western philosophy, with mathematics as paradigm. The 20th-century analytic tradition insists, however, that for somewhat decent Reichenbachian reason, the only genuinely epistemological issues in science and mathematics are after-the-fact-justificational ones. Historically, this derives from FR which came to insist in the mathematical case that only logically rigorous proof can address epistemological issues. My claim follows: from the outset AAA altogether excludes the relevant issues from philosophy, and a fortiori is incapable of addressing them.

Back to Reichenbach then. His argument, as it plays in FR and philosophy of science, is well summarized in two claims.
(1) Rigor in philosophy, where possible, is preferable; as Frege showed, it is possible in regards justification treated after-the-fact, ie., reconstructively.
So far, so good; but that is a charitable reading. That is, it might be taken as methodological suggestion: "Be as rigorous as you can, and here is a really promising really rigorous direction." As Kreisel once pointed out (in a talk at a European Summer Logic meeting), however, methodologies succeed by way of what they exclude; that coin has an opposite side: they also fail on what they exclude.

AAA/FR has exclusions as core principles, to great effect in good and (for reasons just noted above) bad. Why should we take those particular exclusions as mandatory? I don't recall how strongly this is put by Reichenbach, where the attraction of an in philosophy of science unexplored and promising avenue with then unenvisiged limitations requires little weight be upon it. Long since, though, the exclusions are taught as mandatory based on something like:
(2) Only justification may be treated by a sufficiently rigorous theory to count in the serious subject of epistemology. Hempel famously backs this up with (what I call a "scarecrow"; the move is standard): Kekulé got the structure of the benzene ring while looking at flames in a fireplace. Surely, that does not belong in serious philosophy? Surely, there are no end of such examples?

Surely indeed. But all that follows from such arguments is
(a) there are aspects of epistemically serious practice that should not be taken serious epistemologically (but how about giving Kekulé some credit for a lifetime of pioneering benzene research, rather than giving it all to that one flame); indeed, accounts of rationality in creative practice are bound to be incomplete accounts of creative thought.
(b) we (all those taken in by the scarecrow) shall not envisage any epistemological theory not addressed exclusively to complete and rigorous justification.

Granting (a), I take it from (b) that its adherents are either unmotivated to try to create sufficiently rigorous theory (whatever the cost in inability to account philosophically for epistemically major matters, and whatever might turn out to count as sufficient); or are asserting they are incapable of doing so. Teaching practice shows (2) backed up by the scarecrow works well as an initiation ritual to such a stance, given the notorious power imbalance between Tradition and novice.

That it should stand unchallenged in a tradition that more than any other since mediaeval times, emphasizes precision and care in philosophical argument, surely towers as the single greatest incongruity and lapse in our discipline (if it still deserves that qualification) in the past century. When these same philosophers go on to claim that their logical/ontological reconstruction of mathematics is what (philosophically) “mathematics really is”, they speak for their epistemologically blinkered (if academically dominant) community alone.

All this is not to deny, first, that (in keeping with Kreisel’s point) on those aspects of mathematical thought on which FR methodology sheds light, it is laser-like in intellectual power and value; and second, that the burden of creating theoretical accounts of further epistemic features of mathematics falls fairly and squarely on those of us who would try; we are a century behind. So far for my second point that, like my first one, I am not first to make but find still necessary.

It has, moreover, positioned me to set up core theoretical terms. To articulate a bit: Scientific thought has struck observers in many cultures as having particular virtues overall, lapses admitted. For Plato, mathematics instead happened to be the paradigm, giving insight though unusual ways of looking at familiar things, insight co-enabled by and able to stand up to a broad variety of criticism. His great project is to provide services with similar virtues to human affairs generally. In the course of such attempts, he sometimes tries to say something about those particular virtues, thematising their embodiments as *episteme*.

Thus, all (should) designate as *epistemic* those aspects of intellectual proceeding that we thereby honor as so virtuous. Philosophers try to refine insight into those virtues; that work, if serious, so recognizable after the fact, and itself so virtuous, is epistemology: so I insist.

Moreover, we will need to construct a usage in order to describe and analyse epistemic features of mathematical activity; notably, such features as may seem to go beyond the vocabulary of and about logical reconstructions. For this, I recommend we speak of mathematical practice, understood as self-critically governed by standards. Here we are forced, by the needs of what must be articulated as bearing epistemic virtues, to say that such standards reside in communities that we must admit to be diverse. That is, we find ourselves needing to negotiate tensions between expressing individuality and unity; for we cannot, without losing the ability to grasp much that is epistemic, impose a unique universal Unity (Mathematics). This, notwithstanding that contemporary FR does so to considerable profit for us all, where it succeeds. We are to address its failures, not deny its goods.

Notions of community, community standards, and other epistemic features we may need to bring into our discourse (Wittgenstinians say practice) is key to negotiating those tensions in a way that promotes appreciation of the epistemic. It allows us to contrast human individuals complete with their flame-watching-or-not individualities that we need to recognize in other intellectual projects and in human affairs with agency that counts epistemically. (Some disciplines speak of actors, I use 'agents'.) Doing so is, thus, exclusionary, a methodology towards an intellectual end in Kreisel's sense. One might elaborate on what is to be excluded: the criterion is epistemic interest, pertinence to the virtues we are sensitive to under that banner. Debating just what counts is inherent to the project to be undertaken, to be argued in each case until a pattern emerges, not to be settled in advance by a sharp definition.

That we then sometimes need to speak of epistemically distinct communities (say, Euclidean vs Cartesian geometers) is a necessary embarassment; in philosophical tradition it tends to raise demarcation questions. I recommend not getting hung up on them at the current stage, as unproductive. Newton's early notebooks show him a Cartesian geometer to the point he is said to nearly have failed his geometry qualifier because Euclid was expected. Newton's physics show him a diagram-based geometer. One flame-watching guy, two practices that need to be epistemically distinguished. Here's why. Youthful Newton was ahead of his time (for Britain) in appreciating the breakthrough empowerment of Descartes' way, taking geometry way beyond Euclid; physicist Newton, as Huyghens, recognized that if dynamics required determining genuinely unknown curves from their tangency properties, you had to express yourself diagrammatically again: until a sufficient and manipulatively richly engaged repertoire of curve forms was available, no one could proceed formally. No practice-demarcation theory could shed better light on this, or help telling the two apart.

A broader lesson from this case: if you want to make an epistemic point of potential broad philosophical interest that might require notions not yet worked up in philosophy, you better be working on a clear case! Cut to the chase. *Philosophical interest is acquired in the making of the point*, not by theorizing abstractly about practices. Community-standard-agency talk serves at the current stage because it is flexible and richly extendable by promoting everyday experience talk to theoretical term as needed. If we find something interesting, someone may later come along with a new, restricted discourse (like logic) in which it may be more rigorously considered. My recommendation is methodological, not dogma for ultimate philosophy.

This third point is little newer than the ones before, if perhaps not yet so energetically put. Unfortunately, such preliminaries seem needed to help break debilitating philosophical habits. Now let's explore, however tentatively, some epistemics of mathematical examples!

Examples “sit tight” in mathematical practice, like $2+2=4$ or $2+3=3+2$. In this, they do not profit from logical foundation, though that provides other interesting perspective. Nor do they require any particular concepts under which they may be brought (whatever that may mean, we must say more), there are many and that this can be done (a false unity if ever there was one) is required for their epistemic import as examples. It is not that they are “concrete” in some ontological or non-abstractness sense. Try kicking the standard example of a topological space that is T_1 but not Hausdorf: you have to digest loads of abstractions before you get there. But it is determinately (eg., vs. vaguely or controversially) T_1 and not Hausdorf, and can play the Popperian role there.

Moreover, and this might be the more central point about tight-sitting: Just as one meteor stone can definitely answer centuries of scientists’ questions unimagined at the time of its recovery, indefinitly many further topological issues can get definite clear-cut answers on this example, should one bring them up. But an example’s ability to play those roles is not required in an unlimited sense: it involves the Real Line but can’t speak to set-theoretically indeterminate issues about the Reals.

To be a bit more rigourous: what I here call tight-sitting (to problematize it) is a *role* in exemplification, in being-brought-under-a-concept. The name, however, suggests one typical requirement for playing that role in a given case, or a range of cases: the ability to give a determinate answer in those cases. Beyond that, tight-sitters are not as such any particular kind of thing. Indeed, one mathematician’s concept may be the next mathematician’s example.

Examples, the topological one included, tend to contribute in further ways less obvious. When an example does not serve to distinguish two given properties, its story with them need not end there. The process of confronting the two topological properties with it, seeing why both are satisfied/failed, tends (in favorable cases) to structure the search for an example that does distinguish them or a proof that they are coextensive. If (say) both properties are satisfied, this draws attention to features of each that are responsable for that, and to what specific modifications of the example might change that. This need not be merely a matter of drawing out as relevant some considerations from an abstractly considered prior assemblage the agent brings to the example: it may genuinely be a product of the interaction.

Thus, examples can help structure search spaces and suggest generalities. Notably, multiple examples *jointly* (that is, viewed jointly in some regard) may set in motion recognition of a pattern. A famous historical case (Fermat, Euler, Legendre) that continues to motivate generalizations in contemporary number theory is *quadratic reciprocity*: a periodicity property of the primes (in spite of their innate aperiodicity). Take a bunch of arithmetic tight-sitter candidates like

$$\mathbf{7} = 1^2 + 2 \times \mathbf{3} \qquad (\mathbf{7} \text{ “is a square}'' \text{modulo}\,\mathbf{3}), \quad vs.$$

$$\mathbf{7} \neq 0^2 + k \times \mathbf{5}, \quad k = 0,1,2,3,4 \qquad\qquad \mathbf{7} \neq 1^2 + k \times \mathbf{5}, \quad k = 0,1,2,3,4$$

$$\mathbf{7} \neq 1^2 + k \times \mathbf{5}, \quad k = 0,1,2,3,4 \qquad\qquad \mathbf{7} \neq 3^2 + k \times \mathbf{5}, \quad k = 0,1,2,3,4$$

$$\mathbf{7} \neq 4^2 + k \times \mathbf{5}, \quad k = 0,1,2,3,4;$$

where p (here $\mathbf{7}$) is a fixed prime and q (here **3, 5**) runs through the other odd primes.

Arrange the primes q where they fall in the natural numbers in $4p$ (here 28) columns and the two types of behaviors segregate exactly in the columns!

Remarkably but mysteriously (still, after many proofs) the same happens for any odd primes p and q (and something similar for $p = 2$). This is quadratic reciprocity, at least its central part.

No one (or few) of the arithmetic statements such as $7 = 1^2 + 2 \times 3$ by itself could nudge us to recognize quadratic reciprocity. Multiple tight-sitters jointly play a role in being-brought-under-a-concept that they could not play individually.

The quadratic reciprocity case also illustrates another key point: being-brought-under-a-concept could be many, richly-structured things, not a simple yes/no matter; as it may appear (to FR) after the concept has a name or all concepts are taken as pre-given (by set theory?). Let's see more of this.

In my own recent work, fortuitously noticing that five of the then 20-some known reasonable-coefficient degree-9 polynomials with a Galois property P agree on all but 3 coefficients, as

$$p(x) = x^9 + 9\,x^8 + 36\,x^7 + 84\,x^6 + a\,x^5 + a\,x^4 + b\,x^3 + 36\,x^2 + 9\,x + 1$$

two of which were equal, narrowed the search to varying 2 rather than 8 quantities.

This allowed 26 such examples to be found. Arranging these coefficient pairs as points in the plane made plain that they were on a curve, for which a parametrization formula could be obtained, now giving a family of infinitely many polynomials with P. Varying one more coefficient (84) led to enough examples to find several similar curves/families/formulas. Next, those formulas play the tight-sitter role: Using these formulas as examples, a two-parameter family could be found.

Again none of the original five examples by itself would have suggested that this pattern should be investigated; fewer than 22 total then found would have been inadequate to obtain a formula. In both situations, multiple tight-sitters jointly play a role in being-brought-under-a-concept that they could not play individually.

In other such situations, more than just a few tight-sitters jointly form a starting point for discovery (and hopefully theory). Varying three coefficients in a degree-6 polynomial gave some 10000 polynomials with a Galois property Q. Representing each as a point in 3-space and rotating the totality brings out unanticipated structures. [After the talk, the display was shown.] One notes parabolas, and even collections of parabolas forming a surface. These families may subsequently be parametrized; hopefully, as examples in further theory.

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All these cases illustrate some important general points. First, tight-sitters include what is commonly called ”data” as well as examples. The tight-sitting *role* is the key point.

Finally, only with imposition of a particular “conceptual” organization can tight-sitters reveal a pattern (and then, typically without indicating why it should hold, or hinting at a proof approach). The kinds of things that need to be “added” seem richly structured, and in diverse cases vastly diverse.

In this connection, consider another prominent role of examples: they “illustrate” theoretical concepts and arguments. ’Illustrate’ here is a term of praise for a contribution to intelligibility. Students need examples (and exercises) in this sense. Some philosophers might dismiss this as “mere” paedogogy and look no further. But mathematicians at all levels need examples in this sense in order to understand; it does pertain to our epistemic topic. Perhaps, I suggest, need for illustration arises because mathematical definitions and statements under-specify, or fail to make sufficiently explicit, what the bringing-under-a-concept involves in the case at hand.

Much further philosophical investigation of such matters seems appropriate. This should involve both a continued census of such example-like roles in creative mathematics, and the philosophical articulation of specific types (counterexample, paradigm,...)

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