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ORCiD
SS: 0000-0002-4895-0772

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Semantic indeterminacy, concept sharpening, and set theories

STEWART SHAPIRO

Ohio State University & University of Connecticut, USA.
Email: shapiro.4@osu.edu

Abstract: Friedrich Waismann once suggested that mathematical concepts are not subject to open-texture; they are "closed". This is not quite right, as there are some traditional mathematical notions that were, at least at one time, open-textured. One of them is the notion of "polyhedron" following the history sketched in Imre Lakatos's *Proofs and refutations*. Another is "computability", which has now been sharpened into an arguably closed notion, via the Church-Turing thesis.

There are also some mathematical notions that have longstanding, intuitive principles underlying them, principles that later proved to be inconsistent with each other, sometimes when the notion is applied to cases not considered previously (in which case it is perhaps an instance of open-texture). One example is "same size", which is or was governed by the part-whole principle (one of Euclid's Common Notions) and the one-one principle, now called "Hume's Principle". Another is the notion of continuity.

The purpose of this paper is to explore the notion of "set" and other related notions like "class", "totality", and the like. I tentatively put forward a thesis that this notion, too, is or at least was subject to open-texture (or something like it) and has been sharpened in various ways.

This raises some questions concerning what the purposes of a (sharpened) theory of sets are to be. And questions about how one goes about trying to give non-ad-hoc explanations or answers to various questions.

Keywords: Open texture, Waismann, Semantic indeterminacy.

1. Introduction: open texture and mathematics

Friedrich Waismann (1945) introduced the term "open texture" into philosophy. As a first approximation, we might say that a predicate P, from a natural language, exhibits *open texture* if it is possible for there to be an object a such that nothing concerning the established use of P, and nothing concerning the non-linguistic facts, determines that P holds of a, nor does anything determine that P fails to hold of a.[1]

[1] Here I primarily take open texture to hold (or not) among words and phrases, but one can also think of concepts as being open textured (or not). I often use a term like "notion" meant to be ambiguous between phrases and concepts.

This characterization may not be quite right, as it does not seem to distinguish instances of open texture from borderline cases of a vague predicate, say in a Sorites series—at least according to some views of vagueness (e.g., Shapiro, 2006b). With open texture, the cases typically come out of the blue, sometimes as the result of philosophical thought experiments and sometimes in the normal use of the expressions, but in new contexts. The examples speak for themselves.[2]

In effect, if a predicate P is open-textured, then the truth of some sentences in the form Pa is left open by the use of the language and the non-linguistic facts: nothing languages users have said or done to date—whether by way of the ordinary use of the term in communication or in an attempt to stipulate its meaning—fixes how the term should be applied to the new cases. Open texture is a kind of semantic indeterminacy.

Simon Blackburn's *Oxford Dictionary of Philosophy* (1994) contains the following entry:

> **open texture**: The term, due to Waismann, for the fact that however tightly we think we define an expression, there always remains a set of (possibly remote) possibilities under which there would be no right answer to the question of whether it applies . . .
> For example, [the open texture] of the term "mother" . . . is revealed if through technological advance differences open up between the mother that produces the ovum, the mother that carries the foetus to term, and the mother that rears the baby. It will then be fruitless to pursue the question of which is the 'real' mother, since the term is not adapted to giving a decision in the new circumstances.

Waismann himself says that mathematical terms—presumably all mathematical terms—are *not* subject to open texture; they are "closed":

> If, in geometry, I describe a triangle, e.g., by giving its three sides, the description is *complete*: nothing can be added to it that is not included in, or at variance with, the data. (Waismann, 1945, 125)

To echo Waismann's somewhat Wittgensteinian rhetoric, however, what should we say of various three-sided figures in non-Euclidean spaces, say those with variable curvature, where it is not clear what counts as a "straight line"?

Perhaps "Euclidean triangle" is a better candidate for a term that has no open texture. When the restriction to Euclidean spaces is made explicit, the case for the term being "closed" is better. At least it is not as easy to think of cases that would or even might indicate the open texture of this notion.

Shapiro (2006a) argues that "polyhedron" and "sameness of area" were, at least once upon a time, subject to something like open texture, and yet these notions are mathematical, if any are. Theorems about polyhedra and area are found in mathematics texts from antiquity, long before any rigorous formal definitions for these notions were provided.

[2] It is fair to say that "open texture" is homological—it is itself subject to open texture. It turns out that just about all empirical terms from ordinary language are subject to open texture, as are at least some technical terms that arise in the development of the sciences. There is no reason to think that technical terms in philosophy are any different.

To be sure, each of these terms, and many others, were eventually sharpened via rigorous definitions, sharpened to the point where it at least appears that there is no remaining open texture.

2. Proofs and refutations

Let us briefly revisit one such case here, highlighted by Imre Lakatos *Proofs and refutations*" (1976). The bulk of that little book is a lively dialogue involving a class of rather exceptional mathematics students. The dialogue is a rational reconstruction of the history of what may be called "Euler's theorem or, perhaps "Euler's conjecture".[3]

> Consider any polyhedron. Let V be the number its vertices, E the number of its edges, and F the number of its faces. Then $V - E + F = 2$.

The reader is first invited to check that the equation holds for standard polyhedra, such as rectangular solids, pyramids, tetrahedra, icosahedra, and dodecahedra. One would naturally like this quasi-inductive "evidence" confirmed with a proof, or else refuted by a counterexample. As indicated by the title of the book, Lakatos provides examples of both.

The dialogue opens with the teacher presenting a proof of Euler's conjecture. Think of a given polyhedron as hollow, with its surface made of thin rubber. Remove one face and stretch the remaining figure onto a flat wall. Then add lines to triangulate all of the polygonal faces, noting that in doing so we do not change $V - E + F$. When the figure is fully triangulated, start removing the lines one or two at a time, doing so in a way that does not alter $V - E + F$. At the end, we are left with a single triangle, which, of course, has 3 vertices, 3 edges, and 1 face. So for that figure, $V - E + F = 1$. If we add back the face we removed at the start, we find that $V - E + F = 2$ for the original polyhedron. QED.

The class then considers a series of counterexamples to Euler's conjecture. These include a picture frame, a cube with a cube-shaped hole in one of its faces, a cube with cube-shaped hole in its interior, and a "star polyhedron", a figure with pyramids sticking out from some of its sides.

A careful examination shows that each counterexample violates (or falsifies) at least one of what Lakatos calls the "hidden lemmas" of the teacher's proof. In some cases, the three-dimensional figure in question cannot be stretched flat onto a surface after the removal of a face (at least not without some of the faces overlapping others). In other cases, the stretched plane figure cannot be triangulated without changing the value of $V - E + F$ (or cannot be triangulated at all), and in still other cases, the triangulated figure cannot be decomposed without altering the value of $V - E + F$.

The dialogue then takes an interesting turn, especially given that it more or less follows some threads in the history of mathematics, or at least the history of this mathematics. Some students declare that the counterexamples are what Lakatos calls "monsters" and do not refute Euler's conjecture. The idea here is to insist that the weird figures in question are not really polyhedra, or are not really proper polyhedra. One can imagine some philosophers arguing that an analysis of the concept of "polyhedron" reveals this.

[3] Or at least it could be called that if there were not hundreds of other results that equally deserve the title. Parts of the actual history of this particular theorem or conjecture are sketched in Lakatos's footnotes.

The class does consider a desperate attempt along those lines: one *defines* a polyhedron to be a figure that can be stretched onto a surface once a face is removed, and then triangulated and decomposed in a certain way. That would make the teacher's "proof" into a stipulative definition of the word "polyhedron". This move is quickly dismissed.

A second attempt to resolve the matter is to restrict the theorem so that the proof holds: the proper theorem is that for any convex, "simple" polyhedron, $V - E + F = 2$. Apparently, an advocate of this maneuver is content to ignore the interesting fact that $V - E + F = 2$ does hold for some concave, and some non-simple polyhedra.

A third line is to take the counterexamples to refute Euler's conjecture, and to declare that the notion of "polyhedron" is too complex and unorderly for decent mathematical treatment. Apparently, those inclined this way just lose interest in the notion, at least in their capacity as mathematicians.

A fourth maneuver is to accept the counterexamples as refuting Euler's conjecture, and then look for a generalization that covers the Eulerian and non-Eulerian polyhedra in a single theorem.[4]

It is straightforward to interpret the situation in Lakatos's dialogue—or, better, the history it reconstructs—in terms of Waismann's account of language. The start of the dialogue refers to a period in which the notion of polyhedron had an established use in the mathematical community (or communities). As noted, theorems about polyhedra go back a long way in mathematics. Nevertheless, the word, or notion, or concept, was not defined in a way that either explicitly included or explicitly ruled out the alleged counter-examples. Euclid, for example, defines a "solid" to be "that which has length, breadth, and depth" (Book XI, Definition 1), but he makes no mathematical use of that definition. In that respect, it is similar to some of the definitions in Book I, like "a point is that which has no part". Surely, a necessary condition for a solid to be a polyhedron is that it be bounded by plane polygons (i.e. networks of edges all of which lie in the same plane and enclose a single area). But what are the sufficient conditions for being a polyhedron?

The mathematicians of antiquity were working with a notion governed more by a Wittgensteinian family resemblance than by a rigorous definition that determines every case one way or the other. In other words, the notion of polyhedron exhibited open texture.

Note that this open texture did not prevent mathematicians from working with the notion, and proving things about polyhedra—witness, again, Euclid's *Elements*. Still, at the time, it simply was not determinate whether a picture frame counts as a polyhedron. The same goes for a cube with a cube-shaped hole in one of the faces, etc.

When the troubling cases did come up, and threatened to undermine a lovely generalization discovered by the great Euler, a decision had to be made. As Lakatos shows, different decisions were made, or at least proposed. Those who found the teacher's proof compelling (at least initially) could look to its details—to what Lakatos calls its "hidden lemmas"—to shed light on just *what a polyhedron is*. And those who found the counterexamples compelling (but do not lose interest in the notion) can look to the details of the proof, and to the counterexamples, to formulate a more general definition of "polyhedron", in order to find the characteristics that make some polyhedra, and not others, Eulerian (i.e., such that $V + E - F = 2$).

[4] Toward the end of the book (in a late section possibly added by Lakatos's students and editors), one character proposes what we may call an algebraic definition: a polyhedron just is a collection of things called "vertices", "edges", and "faces" that have certain relations to each other.

In this case, at least, both approaches proved fruitful. We can look back on the history and see how much was learned about the geometry of (Euclidean) space.

3. Computability

The history of mathematics provides many similar examples. Consider, for example, the notion of a real-valued function. Mathematicians were confronted with "monsters", such as functions that are discontinuous everywhere, and functions that are continuous everywhere, but differentiable nowhere.[5] Or the notion of area (or integral), continuous function, convergent series, derivative (see Smith, 2015), etc. Eventually, all of these notions received rigorous definitions, to the point where it at least appears that no open texture remains.

Shapiro (2006a) argues that, at least in the early 1930's, the notion of computability was subject to open texture. At some point, and in many contexts today (mathematical and otherwise), matters of feasibility are relevant to what counts as computable. For example, an Ackermann function is not computable in any reasonable or at least any practical sense, since it would take more particles in the universe to compute its value for even small inputs (like the pair $\langle 5, 5 \rangle$). Some of the relatively early debates over the Church-Turing thesis saw key mathematicians and mathematical logicians claiming that certain recursive functions are not computable due to matters of feasibility (Mendelson, 1963). That at least suggests that it was not completely clear at the time whether matters of feasibility should matter for computability.

The mathematical (and philosophical) field of computability gained considerable insight and depth when partial functions—functions that are not defined on every input—were considered. Of course, one always knew that division by the natural numbers is not defined on 0, and that many natural numbers do not have square roots, but the consideration of partial functions in computability goes far beyond these easy examples. It is not much of an exaggeration to say that the entire enterprise of recursive function theory is built around them.

For example, one can enumerate all of the Turing machines, and thus one can enumerate (with repetitions) all partial recursive functions. However, one cannot "diagonalize" out of this enumeration, just because (some of) the functions are partial.

On the contemporary definitions, a partial function f on the natural numbers is said to be computable if and only if there is an algorithm A such that, for each natural number n, if A is given n as input:

- If f is defined on n, then A produces $f(n)$, and
- if f is not defined on n, then A produces no output at all; typically, A runs forever in these cases.

Oliver and Smiley (2013, pp. 24-326), argue that this definition is incorrect. Their conclusion seems to be based on intuitions about what it is to be computable; they seem to attempt a traditional conceptual analysis of "partial computability" (i.e., computability of partial functions):

> So what are the computable functions? They are those for which there is an algorithm that delivers the value whenever there is one, and registers that there is no value when there is none, whether implicitly by halting without producing

[5] See Youschkevitch (1976), or any text in the history of mathematics.

> an approved numerical output, or explicitly by adding O to approved outputs and halting with O as output. (p. 325)

I submit that the intuitive, pre-theoretic notion of computability, available in the 1930's and for a bit after, did not adjudicate between the contemporary conception of what may be called "partial computability" and the one insisted on by Oliver and Smiley. This is, or was, a case of open texture. Nothing that mathematicians said or did, either by way of how the notion of computability was were used, nor in attempts at a definition, determined what to say about the computability of partial functions. In retrospect, the contemporary account—the one criticized by Oliver and Smiley—proved to be fruitful.

One can ask whether the word "computable", or the corresponding concept, has the same meaning today as it did in the 1930s when computability became a focus of the mathematical and logical worlds. In the third item in the Analytic-Synthetic series, Waismann asked if the meaning of the word "time" was changed when it was discovered how to measure time or, perhaps better, when it was discovered that there are reliable ways to measure time. He wrote:

> whether the meaning of "time" … changes when a method of measuring is introduced, we were thinking of the meaning of a word as clear-cut. What we were not aware of was that there are no precise rules governing the use of words like "time" … and that consequently to speak of the "meaning" of a word and to ask whether it has, or has not changed in meaning, is to operate with too blurred an expression. (Waismann, 1951, p. 53)

The "too blurred expression" here is "means the same as", when applied to words and concepts separated by sufficient periods of time (or place).

In the case at hand, there is no sharp fact of the matter whether the notion of computability in play before the 1930's is the same or different from the one invoked today, especially as to how that term is supposed to apply to partial functions.

Something similar can be said about other notions mentioned above: polyhedron, sameness of area, and the like. As noted, all of these notions eventually received rigorous definitions which at least appear to be free of open texture. But they did not start out with such definitions, even though there was a rich mathematical practice around them. Following Waismann, to ask whether the rigorous definitions got things right with respect to previous practice is to operate with too blurred an expression. Mathematics does resist open texture, but is not immune to it (cf. Shapiro and Roberts, 2021). Indeed, one might say that, in mathematics, open texture is more the rule than the exception, at least historically.

4. Contradictory principles

Occasionally, there are some principles associated with a given notion. Some of these are thought to be constitutive of the notion, telling us just what the notion is. And sometimes some of these principles are found to be inconsistent with each other. This may happen when the notion is applied to heretofore unconsidered cases, thus invoking something at least in the neighborhood of open texture. The difference is that with open texture, the application of the notion to the new cases is indeterminate—nothing speakers have said or done determines whether the notion applies or not. In the present cases, the principles conflict with each other (dialethism aside).

Although matters are controversial (aren't they always?), perhaps truth is one such notion. The principles in question are the two directions of the so-called "T-scheme":

- If 'φ' is true, then φ.
- If φ then 'φ' is true.

A Liar sentence or proposition would be a heretofore unconsidered case.

Here we will stick to mathematical notions. Perhaps the clearest case is the relation of two collections being the same size. One arguably constitutive principle of "same size" is the *Part-Whole Principle*, enshrined in one of Euclid's Common Notions:

> The part is smaller than the whole.

The other is now called *Hume's Principle*:

> Two collections are the same size if and only if there is a one-to-one correspondence from one onto the other.

Of course, these come into conflict if the collections are infinite.

Beginning with Aristotle, philosophers argued that there are no completed infinite totalities. Sometimes this was defended on the grounds of the Part-Whole principle. What we today call "Hilbert's Hotel", a feature of infinite collections, was called "Galileo's Paradox", a refutation of the very idea of a completed infinite collection. When the calculus was being developed, mathematicians like Galileo and Leibniz came to accept completed infinite totalities, but insisted that they don't have sizes, or that they cannot be compared for size.[6]

Of course, the contemporary Cantorian resolution is to simply abandon the Part-Whole Principle, in favor of what is now called Hume's Principle. Someone who favors contemporary practice may suggest that the Part-Whole Principle is not, *and perhaps never was*, constitutive of the notion of "same size". Adopting that principle is a (perhaps understandable) mistake that our ancestors made, due to not considering enough cases.

In "Measuring the size of infinite collections of natural numbers: was Cantor's theory of infinite number inevitable?", Paolo Mancosu (2009) shows how it is indeed possible to develop a consistent theory of "same size" based on the Part-Whole Principle, rejecting Hume's Principle. The account is not nearly as elegant as the Cantorian one, and requires some rather ad hoc parameters, but it is consistent.

Another example (perhaps) is the notion of a continuous substance. One longstanding principle is that a continuous substance has a kind of unity. There is something that binds it together, making it a coherent whole. This goes back to Aristotle's *Physics* (227a6):

> The continuous is just what is contiguous, but I say that a thing is continuous when the extremities of each at which they are in contact become one and the same and are (as the name implies) contained in each other. Continuity is impossible if these

[6] See the papers in Hellman and Shapiro (2021).

> extremities are two. This definition makes it plain that continuity belongs to things that naturally, in virtue of their mutual contact form a unity.

Let us call this (alleged) feature of continuous substances *viscosity*.

When it comes to mathematical things like lines, one fallout of viscosity is sometimes called *indecomposibility*: it is impossible to cleanly break a continuous substance into two parts. For Aristotle, if one attempts to break a line, for example, one will produce something new, or, perhaps better, one will make actual something that was only potential before, namely the boundaries of the new lines (the endpoints).

In contemporary intuitionistic analysis and in smooth infinitesimal analysis, also based on intuitionistic logic, viscosity plays out differently: for any given line a is not the case that there are two lines b, c that are both part of a, discrete from each other, and such that b and c together are a. It is sometimes put as a metaphor: if you try to cut a line, something will stick to the knife.

A second intuitive principle about continuous lines is expressed by the intermediate value theorem:

> If two lines cross each other, then they intersect: there is at a point common to the two lines where they meet.

This principle is also displayed, or perhaps presupposed, in one of the so-called "gaps" in Euclid's *Elements*. It occurs in the very first proposition (I:1): Euclid constructs two circles that cross each other, and instructs the reader to let A be the point where the circles meet. One might ask how one is supposed to know that there is a point where the two circles meet. Perhaps this is implicit in the continuity of the lines.

In contemporary mathematics, viscosity is completely lost in the prevailing Dedekind-Cantor theories of real analysis, and continuity generally. A line just is a set of points, and with the background classical logic (and set theory), every non-empty subset of a given line has a non-empty complement. One might say that the Dedekind-Cantor line is fragile, easily broken at every point, with nothing lost. Of course, the intermediate value theorem holds.

On the other hand, viscosity, or at least indecomposibility, is maintained in intuitionistic analysis and smooth infinitesimal analysis, but the intermediate value theorem fails there. The main theme of smooth infinitesimal analysis is that all functions are smooth: they are differentiable, their derivatives are differentiable, etc. Arguably, this is a natural extension (or consequence) of viscosity.

John Bell (2008) argues that this mantra—that all functions are smooth—is incompatible with the intermediate value theorem. He gives an example of a cubic polynomial, with two parameters ($x^3 + bx + c$). For each instance of the parameters b and c, there will be some positive and some negative values of the polynomial. So, if the intermediate value theorem held, there would be a projection function that takes the parameters as arguments and returns a zero of the polynomial with those parameters. Then Bell points out that no projection function of this polynomial is smooth.

So, we have to choose between two rather intuitive and longstanding principles once thought to underlie continuity. The natural extension of viscosity vs. the intermediate value theorem—just as we must choose between the Part-Whole Principle and Hume's Principle. Those who adopt the prevailing Dedekind-Cantor account of continuity make one choice, those who favor intuitionistic analysis and smooth infinitesimal analysis make the other.

5. Collections, sets, classes, totalities, . . .

Arguably, the word "set" is now a technical term in mathematics and philosophy, referring to the iterative hierarchy, as encapsulated by Zermelo Fraenkel set theory with choice (ZFC). Perhaps "class" is also a technical term, as in "proper class". So let us settle on the more common term *collection*. Is that notion, or was that notion, subject to open texture, or was it governed by intuitive but mutually inconsistent principles? Or is the notion of collection monolithic, completely determined? Does it correspond, exactly, to the notion of "set" codified in ZFC? George Boolos (1988) seemed to think so. In a witty attack on a proposal to admit so-called "proper classes" to the theory, he wrote:

> Wait a minute! I thought that set theory was supposed to be a theory about all, "absolutely" all, the collections that there were and that "set" was synonymous with "collection". If one admits that there are proper classes at all, oughtn't one to take seriously the possibility of an iteratively generated hierarchy of collection-theoretic universes in which the sets which ZF recognizes play the role of ground-floor objects? I can't believe that any such view of the nature of [membership] can possibly be correct. Are the reasons for which one believes in classes really strong enough to make one believe in the possibility of such a hierarchy?

Sets, as codified in ZFC, are, of course, well-founded. Are there non-well-founded collections, presumably collections that are not sets? In particular, can a collection be a member of itself? Perhaps. Suppose that I am inspired by Julie Andrews's performance in *The sound of music* and elaborate a collection of my favorite things. Like Julie Andrews's character, the items on the list comfort me when I'm feeling sad.

The only item on my list that is also on Julie Andrews's list is warm woolen mittens, a must for anyone who lives in certain climates (such as Ohio and Connecticut). My list includes my iPad, my house, my atomic watch, my wife's record collection, my wife herself, my children and grandchildren, some of my colleagues, and my old running shoes—they are still very comfortable. There are also some abstract objects among my favorite things. They include the number 17 (because it is prime), the number 34 (because it is composite), the number $33,550,336$ (because, apparently, it is the smallest perfect number not known by ancient mathematicians), the pair $\langle 101, 103\rangle$ (a pair of twin primes), democracy, and free speech.

As it happened, I grew fond of the collection of my favorite things. I found that thinking about this collection (as opposed to its members) also sometimes comforts me when I am feeling sad. So I add the collection of my favorite things to the collection of my favorite things.

I presume that the gentle reader saw that coming. There does not appear to be a contradiction in my thinking here, at least not yet. Unless someone insists that comprehension or separation is somehow a constitutive principle of the very notion of "collection", but why think that? To channel Waismann, most notions are not articulated in every conceivable direction.

In one of his celebrated philosophical papers, "What is Cantor's continuum problem?", Kurt Gödel (1983) writes that there are two distinct intuitive or pre-theoretic notions of "set" or, in present terms "collection". One is when a collection is somehow tied to a property. A set, in this sense, is something like a property in extension, perhaps what Frege called a "concept". This seems to be the notion behind Frege's "course of values" or "extension" operator, which leads naturally (but tragically) to contradiction via Basic Law V.

According to Gödel, the other notion of "collection" occurs when we start with a determinate totality of objects, such as the natural numbers, and consider collections of those—sets of numbers. And this notion iterates, we can talk about sets of sets of numbers, sets of sets of sets of numbers, etc. As Gödel put it in a footnote, the iteration continues into the transfinite. He concludes:

> As far as sets occur in mathematics ... they are sets of integers, or of rational numbers ... or of real numbers ... or of functions of real numbers ... This concept of set, however, according to which a set is something obtainable from the integers (or some other well-defined objects) by iterated application of the operation "set of", not something obtained by dividing the totality of all existing things into two categories, has never led to any antinomy whatsoever; that is, the perfectly "naïve" and uncritical working with this concept of set has so far proved completely self-consistent. (Gödel, 1983, §2, 474-5)

This is sometimes called the *iterative* notion of set, explicitly articulated in Zermelo (1930) and, of course, others. It is indeed natural to conclude that iterative sets are well-founded, and thus cannot be members of themselves. As Gödel puts it, an iterative set is "formed" from some pre-existing or otherwise available things. So it can't contain itself as a member. It follows that the collection of my favorite things is not an iterative set, at least not when I put that very collection into that very collection. But does that disqualify it from being a collection of a different sort?

In some moods, I am tempted to include the entire iterative hierarchy, V, in the collection of my favorite things, although I must admit that thinking about V does not always comfort me when I am feeling sad. Sometimes it just confuses me. Besides, it is also not so clear that V is a thing, and so it may not be eligible to be a favorite thing. If V is a thing, surely it is a collection, and not an iterative one.

Our question now is whether Gödel and Boolos are right that a monolithic, fully consistent, and completely determinate conception of collection was in place in mathematics all along, or whether the notion of collection is more like polyhedron, area, computability, and same-size, at least as characterized above.

In other words, is the iterative notion of set enshrined in ZFC a sharpening of a prior notion that had some open texture or perhaps was governed by inconsistent principles? What are we to make of the vigorous mathematical work on other kinds of set theories? There is work on non-well-founded sets (Aczel, 1988), and set theories that have a universal set, such as Quine's (1937) "New foundations" (see Forster, 1995), not to mention work on inconsistent set theory (Weber, 2009).

Is this apparently mathematical work simply a mistake, putting forward principles that are false of the given notion? Or are these other, perhaps less successful, accounts different ways to sharpen a pre-theoretic notion that was subject to open texture?

6. Upshot: in lieu of a conclusion

Recall the passage from Waismann (1951, 53):

> [T]here are no precise rules governing the use of words like 'time', 'pain', etc., and . . . consequently to speak of the 'meaning' of a word, and to ask whether it has, or has not changed in meaning, is to operate with too blurred an expression.

The "too blurred expression" here is something like "has the same meaning as", or, in our cases, "is the same notion as" applied across contexts separated by significant temporal intervals. The thesis here is that the question as to whether the notions from sufficiently different times are the same or different is not always a good question. Without more context filled in, there might not be a determinate answer.

I submit that open texture, once it is discovered, and contradictory principles, once those are discovered, are resolved through semantic *choice*. The relevant linguistic/scientific/mathematical community collectively *decides* how to go on. The Waismann-inspired thesis is that after such choices are made, it is not always determinate whether the result is a change in the meaning of a given term or concept (*pace* one of the main arguments of LaPorte (2004)). To put the point in more Wittgensteinian terms, it is not always determinate whether the relevant intellectual community goes on as before after open texture is at least partially resolved.

Our question here is how do we make these kinds of choices? And who are "we"? We must get speculative. The proposal to be explored here is that it is a matter of what is now called conceptual engineering, or something similar like providing a Quine/Carnap explication (if either of those is different from conceptual engineering). Not to undermine or simply ignore a growing literature on this, what are the rules of that enterprise, in the case of mathematical notions that are up for sharpening or explication or . . . ? Our primary example, of course, is the notion of collection, as presented in the previous section.

Consider a remark that Michael Dummett (1991, 316) once made about the notion of "cardinal number", once we ponder the transfinite:

> We can gain some grasp on the idea of a totality too big to be counted . . . but once we have accepted that totalities too big to be counted may yet have numbers, the idea of one too big even to have a number conveys nothing at all. And merely to say, "If you persist in talking about the number of all cardinal numbers, you will run into contradiction", is to wield the big stick, but not to offer an explanation.

Presumably, the explanation here would somehow invoke the pre-theoretic notion of "cardinal number", as it applies to infinite collections. However, a main theme here is that in cases like this, it is sometimes indeterminate just how the pre-theoretic notion applies to the new cases. Again, can we appeal to the Euclidean Part-whole principle, or do we favor Hume's Principle, or perhaps something else entirely instead?

Our current question is this: when trying to develop rigorous theories of notions subject to open texture and/or other sorts of indeterminacy—when we are trying to sharpen words or concepts—when is it acceptable to simply wield the Big Stick (even if one speaks softly) and when is a more satisfying explanation required (or at least strongly desired)? If an explanation is required, it is presumably to come from the pre-theoretic concept.

Let P be a target notion. There a number of possibilities. Here is one:

> **Scenario 1**: P is monolithic, in the sense that it has no open texture, is completely coherent, and is not up for sharpening, at least not in different ways.

This seems to be Gödel's and Boolos's view of the present case. They claim (or presuppose) that there always was a single, coherent notion of collection, or at least of (iterative) set, and we are invoking that very notion today.

In a case like Scenario 1, we are up for traditional conceptual analysis, not conceptual engineering, explication, or the like. It is a matter of discovering just what the monolithic phrase or concept *is*. Wielding the Big Stick is clearly inappropriate. We are looking for an explanation, not an explication and thus not a replacement or improvement or sharpening. In the present case, for example, we might be looking to explain why all sets are well-founded and thus why there is no universal set. All proposed axioms and principles should be justified by the nature of P, whatever that nature should turn out to be.[7]

On to a second possibility:

> **Scenario 2:** P is not monolithic—it has some open texture or is governed by mutually inconsistent principles, or something similar. But it turns out that there are two (or three or four) notions that somehow underlie it, and each of those underlying notions is itself monolithic: each has no open texture and each is completely coherent.

As we have seen, when it comes to "collection" Gödel did not say that we are (or were) in this Scenario with respect to the notion of "collection". He did say that there are two notions, but he insisted that one of them—the one based on properties—is incoherent, and so of no mathematical interest. He directed our attention to the other notion of collection, the iterative one, which he did regard as monolithic. Gödel did famously say that once we realize the our target notion is that of iterative set, the axioms of ZFC "force themselves on us" as true. Moreover, he claimed that the iterative notion is the the *only* notion of collection in use in mathematics.

Luca Incurvati (2021) articulates an interesting and insightful program much like Scenario 2 concerning our target notion of set (or collection). He begins (pp. 16-17) by laying out a (or the) pre-theoretic, or intuitive, "concept" of set which, he explicitly notes, is subject to open texture, citing Waismann (and Shapiro, 2006a). A "conception" of set is a further articulation of the underlying concept, filling in resolutions to open texture (and other indeterminacies) in various ways. A "conception" of set is at least of-a-piece with what we here call a "sharpening" of the notion (or concept).

Incurvati develops several "conceptions" of set in delightful metaphysical, epistemic, and mathematical detail, including an iterative conception, a dialetheic naïve conception invoking unrestricted comprehension and a paraconsistent logic, a "limitation of size" conception, a stratified conception based on W. V. O. Quine's (1937) "New foundations for mathematical logic", and a "graph" conception based on Aczel's (1988) non-well-founded set theory. After laying out criteria for choosing among conceptions, along with the strengths and weaknesses of each of the conceptions, Incurvati settles on the iterative conception via an "inference to the best conception". Of course, one might ask, best for what?

In linguistic terms, one way to describe a case like that of Scenario 2 is to say that that the word or phrase for P is polysemous: it has more than one meaning, although the meanings are

[7] We are not concerned here with specific statements about sets, such as the continuum hypothesis, whose truth value is not determined by what we have assumed about the (alleged) monolithic notion. Clearly these matters are related.

related to each other in systematic ways. If each of the sharpenings (or meanings) is itself free of open-texture and the like, then we have two (or three, four, or, in Incurvati's case, five) instances of Scenario 1.

Like Scenario 1, there is no conceptual engineering or explication in Scenario 2. *Each* of the underlying notions is up for traditional conceptual analysis, and so there should be no wielding of the Big Stick. Any proposed axioms or principles for any of the underlying notions (or, in Incurvati's terms, conceptions) should be explained by that underlying notion, assumed to be coherent and sufficiently determinate.

And now for our third possibility:

> **Scenario 3:** P is not monolithic—it has some open texture or is governed by mutually inconsistent principles, or something similar. But in this case, there is no clear candidate for an intuitive, pre-theoretic and coherent notion or notions, that somehow underlie P. The notion P can be sharpened in various ways that are incompatible with each other. And these underlying notions are still subject to open texture (perhaps in different ways).

Here P *is* up for conceptual engineering, explication and/or the like, possibly in more than one way. In this kind of case, we have to look at the *purposes* for which the conceptual engineering and/or explication is done. After all, with any engineering project—conceptual or otherwise—one proposes to make (or articulate) something that is to serve a particular purpose. We make this in order to accomplish that. Well, just what is the purpose (or purposes) of set theory, our mathematical theory of collection?

Similarly (or identically), a Carnap/Quine explication is a proposal that a certain articulated notion is to replace an intuitive or pre-theoretic one *for certain purposes*. So the explicator should say what the purposes are, and then show how those purposes can be used to guide what the replacement, or replacements, might be. They should show just how the replacement accomplishes the stated purpose.

Let us assume that something resembling Scenario 3 makes sense (or did make sense in the past) with respect to our candidate notion of collection. So we are (or were) up for conceptual engineering or explication. For what purpose(s)? What is the collection-theory to do? What is it for?

These, of course, are interesting and perhaps complicated questions, calling for speculation on what the purposes of various mathematical theories are, presumably assuming that each theory has a single purpose. As a first answer, one *might* just think of set theory as like any other mathematical theory. Then we should perhaps adopt the now common Hilbertian theme that consistency is the only legitimate criterion that a mathematical theory must meet.[8]

The usual model here is geometry. As Alberto Coffa (1986, 8, 17) once put it:

> During the second half of the nineteenth century, through a process still awaiting explanation, the community of geometers reached the conclusion that all geometries were here to stay ... [T]his had all the appearance of being the first time that a community of scientists had agreed to accept in a not-merely-provisory way all the members of a set of mutually inconsistent theories about a certain domain.

[8] If dialetheism is not to be set aside, then perhaps non-triviality is the relevant criterion.

The idea is that, as mathematics, all of the geometries are legitimate theories. There is no more conflict between Euclidean, spherical and hyperbolic geometries than there is between arithmetic and real analysis. They are just different mathematical theories—about different kinds of things.

Of course, there is still a question of whether a given theory is interesting, or theoretically fruitful. But perhaps we can just continue this quasi neo-pragmatist query: fruitful for what purpose? There is also an interesting and important question of which mathematical theory is best employed in a given scientific theory. But the question of which geometry to employ in physics is not a mathematical question, or at least it is not a question for mathematicians, qua mathematicians, to settle. It is a matter of applied mathematics. When it comes to so-called pure mathematics, Coffa is correct: all of the geometries are here to stay.

So *if* a given collection theory is to be understood as like any mathematical theory, then mathematicians can simply choose whatever axioms they want to explore. There can be, and indeed, there are, non-well-founded theories, iterative theories, theories with and without various axioms of choice or various determinacy principles, theories that accept the continuum hypothesis, or V=L, or various large cardinals, not to mention theories using the Part-Whole principle (for the associated notion of cardinality), etc.

If, following Hilbert, consistency is the criterion—indeed the only criterion— for a theory of collection, then it *is* appropriate to simply wield the Big Stick. From this perspective, if one is asked to explain why it is that there is no universal set in ZFC, the only answer is that this follows from the axiom of foundation or, if you like, the axiom of separation. One simply declares that this is the theory I am interested in developing (putting applications aside, as in much of mathematics). If seemingly contrary decisions could have been made, one is free to make them instead, just as in geometry (or at least the emerging view of geometry toward the end of the nineteenth century). As we have seen in the explosion of work in set theory (or set theories), different decisions were made and have been (and continue to be) explored in detail.

Hilbert did declare that intuition is the source of the axioms of (Euclidean) geometry, and it is undeniable that at least some of the axioms of ZFC (or the other set theories) do conform to intuitive or pre-theoretic concepts. The key observation in both cases is that once the axioms are chosen, they alone guide the mathematical development of the theory.

It might be added that in some cases, applications for a given theory were found only after the theory had been developed and explored by mathematicians. In general, one cannot always tell, in advance, whether a given theory will prove fruitful for this or that extra-mathematical purpose (see Steiner, 1997), or whether it will continue to hold of some intuitive or pre-theoretic concept or idea, especially if the pre-theoretic concept or idea is fraught with open texture. So perhaps the best policy is to let a thousand flowers bloom, or at least to let a thousand flowers try to bloom, even if not many do.

On the other hand (and finally), it is sometimes thought that set theory is *not* just another mathematical theory like any other. It has a certain *foundational* role in mathematics. It is, of course, controversial just what this foundational role is (and also whether mathematics needs a foundation). This is not the place to engage that matter in any detail, but we can sketch it (see Shapiro, 2004).

For some, a foundation is metaphysical. One might think that set theory provides the ultimate ontology of all of mathematics: natural numbers, real numbers, geometric spaces, functions, graphs, and the like really are sets (ultimately). In another sense, perhaps, a foundation is epistemological—it tells what mathematical knowledge (in, say, analytic function theory) is—it is really or ultimately knowledge about the universe (or universes) of sets.

Neither of these senses is all that relevant here. The foundation we have in mind is more internal to mathematics. Here is Penelope Maddy's (2007, 354) summary of some of the relevant foundational features of set theory:

> ... set theory hopes to provide a dependable and perspicuous mathematical theory that is ample enough to include (surrogates for) all the objects of classical mathematics and strong enough to imply all the classical theorems about them. In this way, set theory aims to provide a court of final appeal for claims of existence and proof in classical mathematics ... Thus set theory aims to provide a single arena in which the objects of classical mathematics are all included, where they can be compared side-by-side. Given this foundational goal, ... set-theoretic practice must strive to settle on one official theory of sets, a single fundamental theory. This is not to say that alternative set theories could not or should not be studied, but their models would be viewed as residing in one true universe of sets, V.

In a case like this, where the conceptual engineering or explication is to further a specific goal—in this case a foundational one in the relevant sense—it seems that it is not sufficient to simply wield the Big Stick when choosing axioms or constitutive principles. Clearly, however, we are not looking for a traditional conceptual analysis either, not in terms of an intuitive or pre-theoretic concept (or a conception in Incurvati's sense). The details of the theory need not answer to a pre-existing monolithic notion of collection (or anything else). Instead, we are looking for an explanation of the proposed axioms in terms of the foundational goals of the theory.

Indeed, following Maddy, any decisions to be made in generating the theory should be addressed by how best to further the stated foundational goal of the enterprise. This, of course, is just what Maddy and others do concerning matters like V=L and some large cardinal hypotheses.[9] I have no insights to offer on what the rules of this enterprise are.

I conclude with an observation that, in this context, we have some reason to prefer a potentialist version of set theory, along the lines of Linnebo (2013), Hellman (1989), or others.

Of course, ZFC has no universal set—V is not a set. Again, if we think of ZFC as a mathematical theory like any other, then, as above, there is no need to explain this. The lack of universal set is a consequence of the axioms that characterize the theory.

But if set theory is to serve its foundational role, then perhaps we do need an explanation. It is not enough to wield the Big Stick. As noted, Gödel and others explain the lack of a universal set in terms of the iterative notion of set. Each set is "formed" from previously available objects (typically other sets). There is no stage when all sets are "available". Each stage can be surpassed.

These proposed explanations are thus tied to the presumably pre-theoretic notion of iterative set, and do not address the foundational role of the theory, at least not directly.

As Maddy notes, the foundational theory should have isomorphic surrogates for *every* legitimate mathematical structure. And if set theory is to be a legitimate mathematical theory, it

[9] It is thus quite relevant that Maddy explicitly prefers "extrinsic" justifications for axioms, based on the foundational goals, to "intrinsic" justifications which relate to the underlying concept or, in Incurvati's terms, conception of set.

should have a surrogate for its own domain, V. Surrogates are always sets, so V should be a set (or at least be represented by one).

Perhaps Maddy's talk of models of set theory (which also presumably reside in V) can play this role, but, as is well-known, set theory cannot prove that it has a model (in light of the second incompleteness theorem). And it can be proved that no set has, as members, all of V. Thus, there is no surrogate for V itself, at least if we assume that surrogates are supposed to isomorphic copies.

One option, perhaps, would be to back off slightly from the foundational claim, and insist that set theory serves as a foundation for all of mathematics except set theory itself (and perhaps other proposed foundations, like the category of all categories). If we accept the prevailing foundational role, then set theory remains different from every other mathematical theory. If nothing else, there is no surrogate—no isomorphic copy—of its own domain in the iterative hierarchy (thanks to the axiom of foundation and/or the axiom of separation).

A better option, I propose, it to adopt a potentialist set theory for this foundational purpose, perhaps in a background modal language (following Hellman, 1989; Linnebo, 2013, or others). So what is the explanation of why there is no universal set according to this theory? We do not invoke a prior, intuitive or pre-theoretic concept or conception of collection. Rather, we argue that there is no universal set because mathematics does not and cannot have a single domain for all of its present and future endeavors.

In other words, there is a kind of indefinite extensibility to mathematics itself—there simply is no fixed domain for all of it. Clearly, one might postulate a single domain—a single set—that has surrogates for all of the mathematics *at a given time*. If we focus on the period just before ZFC came along, for example, surely $V_{\omega+4}$, or so, would do (depending on how much coding one wants to include). But as soon as this particular domain is countenanced, mathematics will go beyond it, and encounter richer domains. If nothing else, we have its powerset: $V_{\omega+5}$.

It is clear that mathematics has gone well beyond Aristotle's rejection of actual infinity. Apparently, mathematics has also gone beyond Aristotle's teacher and main opponent. Plato was critical of the geometers of his day, arguing that their dynamic, or constructive language is inconsistent with the nature of the true subject matter of geometry:

> [The] science [of geometry] is in direct contradiction with the language employed by its adepts ... Their language is most ludicrous, ... for they speak as if they were doing something and as if all their words were directed toward action ... [They talk] of squaring and applying and adding and the like ... whereas in fact the real object of the entire subject is ... knowledge ... of what eternally exists, not of anything that comes to be this or that at some time and ceases to be. (*Republic*, VII)

We can leave it to others to debate whether this Platonic thought accommodates the mathematics of Plato's day, or of any given time slice of mathematics. But it is getting clear that there can be no timeless, static, and eternal World of Being that somehow contains all that there is or ever will be in mathematics.

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