Learning from Questions on Categorical Foundations

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We can learn from questions as well as from their answers. This paper urges some things to learn from questions about categorical foundations for mathematics raised by Geoffrey Hellman and from ones he invokes from Solomon Feferman.

There are two ways to take the question 'Does category theory provide a framework for mathematical structuralism?' (Hellman [2003]).¹ In terms of a working framework, Awodey had to say: 'obviously, yes' (Awodey [2004], p. 54). Category theory has been the standard research framework for topology, most algebra, and much functional analysis since the 1950s.² It has been so in algebraic geometry and number theory since the 1960s and increasingly in all mathematics. It has been the textbook method of structuralist mathematics since the *Algebras* of Serge Lang [1965] and then Mac Lane and Birkhoff [1967]. So Hellman went on to the theoretical question whether 'category theory provides an autonomous foundation for mathematics as an alternative to set theory' (Hellman [2003], p. 129).

I have said yes. Awodey [2004] is disinclined to any 'foundation'— though I think he gives the word an unduly strict meaning.³ But we do not

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¹ This article is closely based on my part in a panel discussion organized by Steve Awodey at the Chicago 2004 joint meeting of the ASL and the Midwest APA, with papers by Geoffrey Hellman and myself, and comments by Dana Scott and Stewart Shapiro. I refer often to Hellman's part which was a work in progress and so not suitable for quotation. A version of that talk is to appear as Hellman [to appear]. I cite ideas raised by Hellman's talk—to say they are important, and I thank Hellman for them—but not to guarantee they represent his precise thinking then or now.

² For historical accounts see, for example, discussion of the 1950s in Dieudonné [1981], [1989], and all articles dealing with the time since 1945 in James [1999].

³ I take it that Russell showed no logical formulation can give what mathematics was 'really all about in the first place' as mathematics itself has changed from Babylonia to today, see especially Russell [1919]. He and Zermelo found that no naïvely self-evident axioms suffice for current mathematics. Independence proofs since then have shown they were right. Aristotle asked for too much when he said axioms must not admit of any proof. Any statement can be 'proved' from some other statement roughly as plausible. I follow the line Saunders Mac Lane often takes ([1986], p. 406), whereby foundations are 'proposals for the organization of Mathematics', which I believe is much like what Shapiro [1991] means by 'foundations without foundationalism'. To count as a foundation the axioms must only learn from answers and their reasons. We also learn from questions, and I want to urge some things to learn from Hellman's questions, and from the ones he invokes from Solomon Feferman's 'Categorical foundations and foundations of category theory' [1977].

Hellman emphasizes the question of how to tell the presupposition of an axiom system, and the need to distinguish two senses of 'structural axioms'. In one sense 'structural' axioms posit entities with only structural properties. In the other sense structural 'axioms' state structural properties and posit no entities at all. Hellman well argues that any foundation for mathematics must say what entities it posits and in what sense, so that structural axioms in this second sense cannot be foundations. His argument builds on Feferman's. The deepest point of Feferman's paper as it seems to me is to show that we want much more from a foundation than formal adequacy and practical efficacy. In his metaphor, to accept a given foundation merely because it is formally adequate and practically productive is like 'not needing to hear, once one has learned to compose music' (Feferman [1977], p. 153). We want to hear the music.

1. Feferman

Feferman's [1977] is the most sustained critique of categorical foundations to date. Categorists consider his arguments well refuted. For example Bell [1981] endorsed Feferman's ideas provisionally and then, after pursuing the subject for a time, he decided for categorical foundations in [1986], [1988], and [1998]. Yet there has been no explicit reply to Feferman until now, and it is worth giving because his position is more subtle than many people realize. His critique occupies only five pages and can be summed up in three points: Category theory cannot be a logical foundation; it is also psychologically derivative; and it is unmusical. What are logically and psychologically prior, he says, are notions of operation and collection.

He says categorical notions of arrows cannot be logically prior to settheoretic accounts of objects:

My use of 'logical priority' refers not to relative strength of formal theories but to order of definition of concepts, in the

be independently plausible to a reasonably sophisticated mathematician. One requirement of independence, for me, is that formal first-order versions of the axioms must suffice for first-order proofs of the theorems. Categorical set theory is offered as a foundation for 'all' mathematics modulo the kind of constraints Gödel proved, as are some versions of axioms for a category of categories. Elsewhere I discuss categorical foundations for specific parts of mathematics, an idea I especially associate with William Lawvere, and which I have called 'radically categorical foundations' as in McLarty [1998]. So far as I know all categorical foundationalists have enough faith in the advance of mathematics to say category theory will not be the last word. It is the latest and currently best word in structuralist organization of mathematics.

cases where certain of these must be defined before others. For example, the concept of vector space is logically prior to that of linear transformation. (Feferman [1977], p. 152)

This brings us face to face with mathematical practice. The first mathematicians to work with linear transformations defined them as functions on lists of numbers. They did not define 'vector space' at all, and at most defined a 'vector' as either a 'directed line segment' or an 'ordered triple of numbers'. Those definitions of 'vector' are still used today by some engineers and even a few physicists.

By the 1920s leading mathematicians, notably Hermann Weyl and John von Neumann, knew this was not the best definition for their purposes. For them and for many today the best order of definition was to define a *vector space* as any commutative group acted on by a field of scalars. Then a vector is any element of a vector space, which is to say anything can be a vector by placing it in a suitable context, and nothing is in itself a vector.⁴ A linear transformation is a group homomorphism that preserves scalar multiplication. Feferman uses this definition of linearity.

In turn, though, this is not the best definition for many purposes today. The most widely used of Alexander Grothendieck's ideas in practice is his axiomatization of *Abelian categories*. This appeared in Grothendieck [1957] twenty years before Feferman's article. The most influential textbook treatments today are Lang's *Algebra* [1993], first printed in 1965, and Hartshorne's *Algebraic Geometry* [1977], published the same year as Feferman's article. On this definition a linear space is any object in an Abelian category, although it is more normal to call the arrows of an Abelian category with a suitable relation to a field k is a category of k-vector spaces. On this view anything can be a vector space when placed in a suitable categorical context, and nothing is in itself a vector space.

An Abelian category is a category of linear transformations between linear objects. The axioms say nothing about those objects except that they are domains and codomains of the transformations. In fact the arrowsonly formulation of the category axioms can axiomatize Abelian categories without ever mentioning objects. Those axioms speak only of arrows, that is, of transformations.

Linearity of the transformations is expressed by saying they add and subtract, and composition preserves addition. So for any transformations

⁴ Admittedly, in Zermelo-Fraenkel set theory, vectors are not elements of vector spaces. A vector space is a 6-tuple $\langle V, +_V, k, +_k, \cdot_k, (.) \rangle$ of a set *V* with addition $+_V$ making it a commutative group, and a set *k* with addition $+_k$ and multiplication \cdot_k making it a field, and a scalar multiplication (.). Vectors are elements of an element of a vector space or something like that depending on exactly how you define 6-tuples. But the point is clear.

 $f, g, h: A \rightarrow B$ and $j, k: B \rightarrow C$:

$$f + g: A \to B \qquad 0_{A,B}: A \to B \qquad -f: A \to B$$

$$f + g = g + f \qquad (f + g) + h = f + (g + h)$$

$$f + 0_{A,B} = f \qquad f + (-f) = 0_{A,B}$$

$$j(f + g) = (jf) + (jg) \qquad (j + k)f = (jf) + (kf)$$

An Abelian category satisfies these plus some axioms on products, kernels, and quotients. See Grothendieck [1957], Lang [1993], Mac Lane [1998], and for issues of axiomatics see especially Freyd [1964].

We can take these axioms alone as a categorical foundation for this notion of linearity. Or we can interpret them in set theory to get a (much stronger) set-theoretic foundation for the same notion. Either way the axioms describe linear transformations without saying anything about vector spaces or other linear spaces—except, again, that those spaces are domains and codomains of the transformations. And even that can be eliminated by using the arrows-only form of category theory. All the usual theorems of linear algebra follow using transformations rather than elements. The resulting theory applies to structures much more general, and more complicated from a set-theoretic point of view, than just the classical vector spaces and modules.

In short, there is not just one notion of 'vector' or 'linear transformation', and different ones have different formalizations. Others are described in the appendices below. In formalizing any concept, if you want to achieve what set-theoretic foundations achieve in the way they achieve it, then you need set-theoretic foundations. Of course that could be categorical set theory. If you want specifically what membership-based set-theoretic foundations like ZF do, in the way they do it, although these specifics find no significant echo in practice, then you need specifically membership-based set-theoretic foundations like ZF. But those are not the only things you *can* do.

The point is that Feferman knows all this. He knows the Abelian category axioms. He knows that these and related axioms and practices show it is formally possible to make arrows prior, and it is productive in fact.⁵ It remains a logical error in his sense despite people doing it successfully. It is also a psychological error:

My claim above is that the general concepts of operation and collection have logical priority with respect to structural notions (such as 'group', 'category', *etc.*) because the latter are defined

⁵ Recall that at this stage in Feferman's argument it is not a question of ultimate foundations but of 'order of definition of concepts.... [*e.g.*,] the concept of vector space is logically prior to that of linear transformation' (Feferman [1977], p. 152)

in terms of the former but not conversely. At the same time, I believe our experience demonstrates their psychological priority. I realize that workers in category theory are so at home in their subject that they find it more natural to think in categorical rather than set-theoretical terms, but I would liken this to not needing to hear, once one has learned to compose music. (Feferman [1977], p. 153)

Even in 1977 this method was hardly confined to category theorists. The most famous exercise in Lang's *Algebra* was on the Abelian category axioms. After a ten-page introduction to homology built around these axioms, the sole exercise read: 'Take any book on homological algebra, and prove all the theorems without looking at the proofs given in that book' (Lang [1965], p. 105).⁶ Lang is not a category theorist and his *Algebra* has been a standard graduate textbook for decades.

So a great many mathematicians miss the music Feferman cares for. For him the music lies, *almost*, in the structure of the iterative hierarchy of ZF sets, more in proof theory, and most of all in philosophic questions of realism *versus* constructivism, which he wants to build into the foundations. He poses an ambitious philosophical goal:

Since neither the realist nor constructivist point of view encompasses the other, there cannot be any present claim to a *universal foundation* for mathematics, unless one takes the line of rejecting all that lies outside the favored scheme. Indeed, *multiple foundations* in this sense may be necessary, in analogy to the use of both wave and particle conceptions in physics. Moreover it is conceivable that still other kinds of of theories of operations and collections will be developed as a result of further experience and reflection. I believe that none of these considerations affects the counter-thesis of this part, namely that foundations for structural mathematics are to be sought in theories of operations and collections (if they are to be sought at all). (Feferman [1977], p. 151)

He does not know what the new theories may look like but he knows what they should not look like:

To avoid misunderstanding, let me repeat that I am not arguing for accepting current set-theoretical foundations of mathematics. Rather, it is that on the platonist view of

⁶ This disappeared from later editions since by the mid 1970s essentially all homological algebra books were themselves organized around those axioms and proofs, even if the theorems were not explicitly stated in that generality. *I.e.*, they had all taken Lang's advice.

mathematics something like present systems of set theory must be prior to any categorical foundations. (Feferman, p. 15)

Feferman offers a non-extensional theory of his own as progress towards a correct non-platonist foundation for category theory. It starts with a fragment of first-order Peano arithmetic; so it has a term 0, and an operator ' satisfying axioms:

$$x' \neq 0;$$

$$x' = y' \rightarrow x = y.$$

Reading the obvious way, these say 0 is not a successor, and if the successor of x is the successor of y then x = y. But there is no induction axiom at this level, terms are not taken to be natural numbers, and ' is not taken to be successor. Feferman adds a *partial application* symbol so each term becomes a partially defined operator. The formula $xy \simeq z$ says, intuitively, 'applying operator x to argument y gives value z'. The idea is familiar from recursive-function theory when natural numbers are taken as codes for partial recursive functions and then $xy \simeq z$ says the xth partial recursive function is defined at argument y, and takes value z.

The theory handles collections by way of *partial classifications* defined as partially defined operators whose values are all 0 or 1, where 1 is officially written 0'. The idea is also familiar from recursive function theory, where a number n is taken as coding a set S of numbers if n codes a classifying operator for S in this sense:

(1) whenever nx is defined, then nx = 0 or nx = 1;

(2) nx = 1 if and only if $x \in S$.

Curiously, then, operators are prior to collections in the foundations of Feferman's theory. Apparently this order of definition will be reversed by the time the system is applied to concepts like linear transformation and vector space but Feferman's article does not go that far.

Obviously I agree with Feferman that foundations of mathematics should lie in a general theory of operations and collections, only I say the currently best general theory of those calls them *arrows* and *objects*. It is category theory. And I think there is no question of whether to seek foundations for structural mathematics. Of course we should. The theoretical unity and practical power of modern structural methods make them, to my ear, actually finer music than proof theory or realism *versus* constructivism. The efficacy of structuralism in practice makes it a *more* compelling topic for foundations, to me. But Feferman asks the right question—the question of whether we 'hear the music'. It is not a matter of merely technical logic.

2. Hellman

Hellman's paper takes up a number of themes from Feferman's and some I think are just wrong. It is a mistake when Geoffrey follows Feferman [1977] to say: 'There is frank acknowledgement that the notion of *function* is pre-supposed, at least informally, in formulating category theory' (Helmann [2003], p. 133). A very general notion of function, older than set theory, certainly does motivate category theory.⁷ But motivation is not presupposition. Feferman's system is motivated by the idea of natural numbers coding partial recursive functions. That does not mean his system presupposes codings or recursive function theory.

My [2004] dealt with other concerns Hellman raised, notably whether categorical set theory uses something so arcane as sheaves on topological spaces in order to define the real numbers (it does not), and whether it can express the replacement axiom scheme (it can, in terms closer to Cantor's than to Zermelo-Fraenkel) (McLarty [2004], pp. 38–41, 47–50).

Now Hellman has expanded on the really central issues though. They concern recognizing the presuppositions of a theory and especially the presuppositions of structural theories. Of course we cannot give *the* presuppositions of 'category theory' *per se* because there are too many things 'category theory' can mean. It would be wrong to assume that in practice 'category' and 'functor' are usually algebraic or structural in the sense of being axiomatic primitives with no intended interpretation.

When checked recently, forty-one of the latest fifty references to 'category' in *Mathematical Reviews* were to specific categories, *i.e.*, they did have intended interpretations—insofar as anything in mathematics ever does. These uses of 'category' and 'functor' are as specific, as un-'algebraic', as upper-level mathematics ever gets. They are meant as already situated within a foundation and not themselves open to foundational re-interpretation. Even if we go with Awodey's idea of mathematics as *schematic*, these papers take the ambient structure as a *datum* and not an explicit variable of interest to their work.

In general category theory, the terms 'category' and 'functor' are taken algebraically, in Hellman's sense. They are general and do not have an intended interpretation. This accounts for seven of the fifty latest *Mathematical Reviews* references. The two remaining references were to *Philosophia Mathematica* papers we are discussing, Awodey's [2004] and my [2004].

⁷ The motivation was more general than set-theoretic functions in that already in 1945 prominent examples included 'measurable functions'—each of which is an equivalence class of set-theoretic functions, where two functions are equivalent if they agree on a set of measure 0. See Stone [1932], p. 23 and *passim*. Another example was 'rational functions' in algebraic geometry—each one of which is an equivalence class of finite lists of set-theoretic partial functions. Others were more complex than those. None of these are themselves set-theoretic functions although they involve such functions.

As I said in my paper [2004], there is no similar variety of uses of axioms for set theory, but 'the "category axioms", as given by Eilenberg and Mac Lane in 1945, are used in myriad ways to myriad ends in the daily practice of mathematics' (p. 42). Most often, so far as notices in *Mathematical Reviews* show, they describe specific categories given within a larger foundation. Rather often they are used 'algebraically' in general category theory or its branches. There are other technical uses which would normally not even be remarked in *Mathematical Reviews* such as when a poset *P* is treated as a category, so that ordered systems indexed by *P* can be treated as functors. And just sometimes they describe one or another categorical foundation. These different uses have wholly different kinds of presuppositions.

We are concerned with the presuppositions of various categorical foundations. I discussed several in my paper, but here I shall focus on one that Hellman discusses, my paper 'Axiomatizing a category of categories' [1991].⁸ It is based on the 'Category of categories as a foundation' axioms (CCAF) from Lawvere [1966]. Hellman's discussion suggested three questions to answer when such axioms are offered as a categorical foundation:

- (1) What concepts are presupposed in such an axiomatization?
- (2) Do these sustain the autonomy of category theory *vis-à-vis* set theory, or do they reveal a (possibly hidden) dependence thereon?
- (3) What is the scope of such a (meta) theory, in particular, what are the prospects for self-applicability and the idea of '*the* category of (absolutely) *all* categories'?

'Presupposition' in the broad Feferman-Hellman sense includes motivations. The motivation of my article was to axiomatize 'the major theorems of category theory' (McLarty [1991], p. 1243). As to presuppositions in the ordinary sense of unstated assumptions used in the axiomatization, there are none. It is stated to be a 'two-sorted first-order theory . . . [with] Boolean logic' and all the theorems are derived from the axioms in that logic (McLarty [1999], p. 1244). There is no hidden or overt dependence on set theory in the formal theory.

But what about the motivation? If the major theorems of category theory are proved in set theory, and then I want to axiomatize them, is that not a kind of dependence on set theory? Well in the first place these theorems are not exactly proved in set theory. Their usual naïve versions are incorrect in set theory. They quantify over collections too large to be ZF sets, and manipulate them too freely for Gödel-Bernays classes, and treat them too uniformly for Grothendieck universes. There are many well-known

⁸ That is, Hellman discussed this in Chicago. Again, what he presented there was a work in progress. So I do not quote it directly but acknowledge its role in raising these topics.

and sufficiently workable set-theoretic fixes for handling these theorems but they are all just that—fixes. The motives exceed set theory. And in the second place even if those theorems were set-theoretically proved in just the form I axiomatize, that would no more make my axioms 'presuppose' set theory than Hilbert's axioms for geometry 'presuppose' Thales and Pythagoras (Hilbert [1971]). In fact it would say less than that since these theorems certainly did not come from set theory the way many of Hilbert's did come from Greek geometry. It would only indicate a vast intellectual debt to earlier mathematics, including set theory, which I am sure all category theorists freely acknowledge.

The key point to grasp here is precisely that categorical foundations for category theory are not set-theoretic foundations for category theory. When we axiomatize a metacategory of categories by the axioms CCAF, the categories are *not* 'anything satisfying the algebraic axioms of category theory'—*i.e.*, the Eilenberg-Mac Lane axioms. They are *anything whose existence follows from the* CCAF *axioms*. They are precisely not *sets* satisfying the Eilenberg-Mac Lane axioms. They are categories as described by Lawvere's CCAF axioms.

The third question raises two separate issues. Self-applicability is the question of whether these axioms can be extended to posit a 'category of all categories' as for example New Foundations set theory posits a set of all sets. Not much is yet known about that and I raised the issue only briefly in the article ([1991], p. 1243).⁹ But even if there is such a category it will not be *the* category of *absolutely* all categories any more than some extension of New Foundations posits *the* set of *absolutely* all sets.

Hellman's first two questions apply to any new candidate foundation. In particular they suggest a contrast which has not been emphasized up to now between *Synthetic Differential Geometry* (SDG) on one hand and *Smooth Infinitesimal Analysis* (SIA) on the other. Both describe categories of spaces, including a line *R* and an infinitesimal subspace of it $D \rightarrow R$, with properties such that every function in this category has a derivative. The axiom that guarantees this is called the Kock-Lawvere axiom. SDG consists of the topos axioms, the Kock-Lawvere axiom, and possibly further axioms to strengthen the theory. SIA is John Bell's term for a theory omitting some of the topos axioms, though also open to further stronger axioms (Bell [1998]).

Bell uses SIA exactly to leave the logical presuppositions somewhat open, unfixed, to suit his genetic/pedagogical account of the subject. Bell relies on the fact that the essential analytic or geometric ideas of the subject do not depend on the topos machinery. But the topos axioms do something else: They allow us to conceive of SDG as a foundation because they answer Hellman's question 1, and so permit an answer to his question 2.

⁹ New Foundations indeed proves there is a 'category of all categories' but with hopelessly bad properties. See McLarty [1992].

What concepts are presupposed by the Kock-Lawvere axiom? In SDG we presuppose standard topos logic-or, in foundational accounts, we state that the context is the topos logic axioms and presuppose nothing. In SIA the presuppositions are meant to be less clear. Taken this way, SIA works rather as Awodey ([2004], pp. 58 ff.) says most mathematics does, not specifying the background assumptions or 'ambient theory'. That is a fine way to work for some purposes but Hellman is right that we also have foundational concerns. When we pursue those we cannot be satisfied with Awodey's equation, where he says 'the question of whether the conditions [for a given theorem] are ever satisfied' is just the question of 'whether they are consistent' ([2004], p. 60). Different logical presuppositions make different theories consistent.¹⁰ On Awodey's view (p. 62) when stronger existence assumptions are used this merely means 'specifying more of the ambient structure to be taken into account'. I am very sympathetic to that. But it does posit an ambient structure and another indispensable part of mathematics (and not only the philosophy of mathematics) is the effort to articulate the ambient structure for any body of work.

This brings us to the other key question in Hellman's paper: How can a structuralist theory pick out any intended interpretation? If we identify 'structuralist' theories with 'algebraic' theories in the sense of theories that describe only a general structure with many different instantiations, then they cannot pick out any specific model. On my view categorical foundations are not structuralist in that sense. Each one posits a specific category, and I quoted Mac Lane and Lawvere expressing this view of the category of sets (McLarty [2004], pp. 43–44).¹¹ They are structuralist in this precise sense: They attribute only structural properties to their objects, that is only isomorphism-invariant properties.

This goes to the impetus behind most of the recent interest in 'structuralist' conceptions of mathematics. Benacerraf argued that numbers cannot be sets because, for example, the set Zn of Zermelo natural numbers has

$$1 = \{\emptyset\}, 2 = \{\{\emptyset\}\}, 3 = \{\{\{\emptyset\}\}\}, \dots$$

while the set Vn of von Neumann natural numbers has

$$1 = \{\emptyset\}, 2 = \{\emptyset, \{\emptyset\}\}, 3 = \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}, \dots$$

¹⁰ As a trivial example, $x \in x$ is consistent with some membership-based set theories and inconsistent with others. The SIA axioms are consistent in topos logic but inconsistent with the law of excluded middle.

¹¹ Hellman warns against pretending to all embracing completeness in foundations. I agree. I only find it a less pressing issue because I would not know how to pretend to it if I tried. Clearly there are more sets, categories, smooth spaces, or whatever, than any given axiomatization can prove, and no axiomatization I have ever seen denies it.

The two sets Vn and Zn both model the Peano axioms for arithmetic and so both have equally good claim to be the natural numbers. They are isomorphic and indeed any set isomorphic to them can represent the natural numbers. Yet they have different properties, and so Benacerraf says they cannot both be the natural numbers, thus neither one can, nor can any other set be the natural numbers (Benacerraf [1965], p. 57). For one explicit example, let $\mathcal{P}(X)$ be the ZF formula

$$\mathcal{P}(X) = (\forall Y \in X)(\emptyset = Y \lor \emptyset \in Y).$$

In words that says every nonempty member of X has the empty set as a member. Then ZF proves:

$$\mathcal{P}(Vn)$$
 and $\neg \mathcal{P}(Zn)$.

For technical purposes notice $\mathcal{P}(X)$ is equivalent to a formula with no constants and no free variables but *X*:

$$\mathcal{P}(X) = (\forall Y \in X)((\forall Z)(\neg Z \in Y) \lor (\exists W \in Y)(\forall Z)(\neg Z \in W)).$$

So Benacerraf asks for a theory of *structures* in which:

numbers are not objects at all, because in giving the properties (that is, necessary and sufficient) of numbers you merely characterize an *abstract structure*—and the distinction lies in the fact that the 'elements' of the structure have no properties other than those relating them to other 'elements' of the same structure. (Benacerraf [1965], p. 70)

The sets of categorical set theory are themselves *abstract structures* in exactly this sense. An element $x \in S$ in categorical set theory has no properties except that it is an element of *S* and is distinct from any other elements of *S*. This is discussed historically and philosophically in Lawvere [1994]. Benacerraf's goal was met in the *Proceedings of the National Academy of Science* one year before Benacerraf posed it (Lawvere [1964]).

In categorical set theory isomorphic sets $S \cong S'$ provably have all the same properties. To be quite explicit: Let $\mathcal{P}(X)$ be any formula in categorical set theory, with no constants and no occurrence of variables *S* or *S'*. Let Isom(*S*, *S'*) be the formula saying *S* and *S'* are isomorphic. Then the following statement is provable. It is a theorem of categorical set theory:

 $\operatorname{Isom}(S, S') \implies [\mathcal{P}(S) \Leftrightarrow \mathcal{P}(S')].$

For details see McLarty [1993]. Categorical set theory can express no properties that can distinguish between isomorphic sets. The usual versions

cannot even prove there are distinct isomorphic sets. They are consistent with a *skeletal axiom* saying:¹²

$$\operatorname{Isom}(S, S') \implies S = S'.$$

In categorical foundations for category theory, such as the CCAF axioms, isomorphic categories $C \cong C'$ provably have all the same properties. This is normal for categorical foundations as it is for the categorical methods mathematicians use every day in practice.

Altogether, I think Hellman has asked the right questions about foundations, and categorical foundations answer them.

3. Historical Appendix on Linear Transformations Prior to Vector Spaces

Steve Awodey and Dana Scott in Chicago mentioned two senses in which the *derivative* was known as a linear transformation before any vector spaces were defined for it to transform. Long before anyone conceived of infinite-dimensional vector spaces, or vector spaces of functions, it was known that taking derivatives preserves addition and real multiplication. That is, for any functions $f, g: \mathbb{R} \to \mathbb{R}$ and any real number $a \in \mathbb{R}$, using a prime to indicate the derivative:

$$(f+g)' = f'+g'$$
 and $(a \cdot f)' = a \cdot f'$.

This property of the derivative was used from the beginning to evaluate derivatives and integrals, and especially to solve what were called 'linear' differential equations at least as early as 1853 (Petzval [1853]).¹³ The derivative was soon seen as a linear transformation on functions and 'by the late nineteenth century it was apparent that many domains of mathematics dealt with transformations or operators acting on functions' (Kline [1972], p. 1076). But these transformations were understood just that

¹² I never use skeletal axioms because I believe they achieve nothing of interest. Take the example of the natural numbers. Categorical set theory can assume there is exactly one set \mathbb{N} modelling the Peano axioms, but there are still provably infinitely many different models. They differ in the choice of a zero element $0 \in \mathbb{N}$ and successor function $\mathbb{N} \to \mathbb{N}$. Given any model \mathbb{N} , $0, s: \mathbb{N} \to \mathbb{N}$, there are provably infinitely many (recursive) non-identity isomorphisms $u: \mathbb{N} \to \mathbb{N}$, and for each of them there is another model of the Peano axioms \mathbb{N} , u0, $u^{-1}su: \mathbb{N} \to \mathbb{N}$. The skeletal axiom does not eliminate multiple models. Even without a skeletal axiom, categorical set theory already eliminates any distinction among the multiple models.

¹³ For one important historical example, see how George Hill studied the moon's motion by treating certain differential equations as infinite systems of infinite linear equations, ridiculed by his contemporaries until Poincaré took it up (Kline [1972], pp. 731 f.).

way: as operating on functions and not as transforming vector spaces *of* functions. The theory was well advanced decades before anyone formulated the idea of any space of functions, or of any infinite-dimensional vector space.

Today there are many different ways to formalize these ideas. For example the set $C^{\infty}(\mathbb{R}, \mathbb{R})$ of smooth real-valued functions on the reals forms an infinite-dimensional real vector space, and there is a linear transformation

$$C^{\infty}(\mathbb{R},\mathbb{R}) \rightarrow C^{\infty}(\mathbb{R},\mathbb{R})$$

taking each function f to its derivative f'. Those ideas seem to begin with Fréchet around 1906 (Kline [1972], p. 1078).

It has also been known essentially from the beginnings of calculus that the derivative f'_x of a function f at a point x is a kind of linear transformation around x. For the case of a real-valued function $f : \mathbb{R} \to \mathbb{R}$ on the reals the derivative f'_x was early seen as the slope of the tangent line to the graph of f, at the point (x, f(x)). The tangent is a linear approximation to the graph. This was soon construed as a *Taylor series* expansion of f around x. Taking x as fixed, for any real number $y \in \mathbb{R}$:

$$f(x + y) = f(x) + y \cdot f'_{x} + O^{2}(y),$$

where $O^2(y)$ is some function of y that vanishes to second order. Then, either the second term $y \cdot f'_x$ or the first two terms $f(x) + y \cdot f'_x$ can be seen as a linear function of y. The terminology was unsettled in the nineteenth century.

Deeper ideas applied to functions $f: M \to N$ between differentiable manifolds. It was known, and crucial, that such a function has a derivative f'_x at each point $x \in M$, and f'_x is a linearization of f, but in what sense? Intuitively, f'_x is linear from the infinitesimals around $x \in M$ to those around $f(x) \in N$. But infinitesimals were not too precisely defined, and few if any nineteenth-century mathematicians understood infinitesimals as forming anything like vector spaces around points. For calculation, one could put co-ordinate systems around x and f(x) and represent f'_x as a linear function of those co-ordinates pretty much the way $f(x)+y \cdot f'_x$ was a linear function of y in the case of a real-valued function on the reals. But f'_x existed independently of any co-ordinate systems. It was decades before differential geometers articulated the idea of the *tangent space* to a manifold at a point.¹⁴ Then the derivative $f_x: M_x \rightarrow N_{f(x)}$ became a linear

¹⁴ Steve Awodey specifically mentioned the *tangent bundle* TM to a manifold M. This combines all the tangent spaces to M in a coherent way so that the derivatives at all points form a single function $f': TM \rightarrow TN$, which is linear in a more sophisticated sense.

transformation from the tangent space M_x of M at x, to the tangent space $N_{f(x)}$ of N at f(x).

In both cases mathematicians used general theories of certain kinds of specific linear transformations, which they explicitly regarded as linear transformations, before they had any account of the linear spaces those transformations would transform. In both cases it was real progress to articulate spaces for the transformations: in the first case to articulate infinite-dimensional function spaces, and in the second to articulate tangent spaces and tangent bundles. The axiomatization of Abelian categories similarly led to substantial progress in topology, algebraic geometry, and number theory.

4. Logical Appendix on Linear Transformations Prior to Vector Spaces

Here is a short account of how to pass directly from the historical starting point of linear algebra, using vectors and matrices as calculational tools, to the current theory of vector spaces and linear transformations, without saying anything about vector spaces except that they are the termini of linear transformations. This is not at all deep, does not use the Abelian category axioms, and need not rest on categorical foundations.

Take any set-theoretic foundation whether it be naïve Cantorian set theory, Zermelo-Fraenkel, or categorical set theory. In any case define a category of matrices this way:

Objects are natural numbers n.

An arrow $f: n \rightarrow m$ is an $m \times n$ matrix of real numbers.

Arrows compose by the familiar matrix multiplication.

Clearly this is equivalent to the conventional category of finitedimensional real vector spaces and linear functions. The arrows to and from any natural number n are exactly the conventional real-linear functions to and from the conventional vector space \mathbb{R}^n .

Define a *vector* v in an object n to be an arrow $v:1 \rightarrow n$. That means it is an n-tuple of real numbers $\langle r_1, r_2, \ldots r_n \rangle$, treated as the column of a $n \times 1$ matrix. Any arrow $f:n \rightarrow m$ takes each vector v in n to a vector f(v) in m, namely the composite

 $f(v) = 1 \xrightarrow{v} n \xrightarrow{f} m$

which by definition is the usual matrix operation on a column vector. This notation makes our category look just like the conventional category of finite-dimensional real vector spaces. The conceptual difference remains that, instead of being elements of vector spaces, vectors are arrows to vector spaces. The coordinate-free methods of linear algebra appear in this category as the usual 'up to isomorphism' categorical methods. Infinite-dimensional vector spaces use infinite matrices: For any sets *S* and *T*, define a $T \times S$ real matrix to be a set of real numbers $M_{x,y}$ for each $x \in T$, $y \in S$ such that, for each $y \in S$ there are only finitely many $x \in T$ with $M_{y,x} \neq 0$. These are the usual matrices when *S* and *T* are finite. Define the product of a $U \times T$ matrix *N* with a $T \times S$ one *M* by the obvious formula:

$$(N \cdot M)_{x,z} = \sum_{y \in T} M_{x,y} N_{y,z}.$$

The sum is well defined since all but finitely many terms are 0.

Then define a category whose objects are sets *S*, and arrows $M: S \rightarrow T$ are $T \times S$ real matrices composing by matrix multiplication. This is equivalent to the usual category of all real vector spaces. For vector spaces over any field *k* simply use matrices of elements of *k*.

We do not only want linear algebra. We want to apply calculus, say, to smooth paths in finite-dimensional real vector spaces. We cannot define smooth functions $c: \mathbb{R} \to n$ for a 'vector space' *n*. We can define smooth functions $c: \mathbb{R} \to \text{Hom}(1, n)$, in the usual way since Hom(1, n) is the set of *n*-tuples of real numbers, and then everything goes just as in undergraduate textbooks.

Such smooth functions to linear hom sets are no kind of makeshift or detour. Whatever definitions we use they are indispensible in dynamics, Lie group theory, and elsewhere. We can use either our matrix hom sets $\operatorname{Hom}(n, m)$ or the conventional vector space ones $\operatorname{Hom}_{\mathbb{R}}(\mathbb{R}^n, \mathbb{R}^m)$. Either way an *n*-dimensional linear family of smooth trajectories in *m*-dimensional space is a smooth function

 $\mathbb{R} \xrightarrow{t} \operatorname{Hom}(n, m), \quad \mathbb{R} \xrightarrow{t} \operatorname{Hom}_{\mathbb{R}}(\mathbb{R}^{n}, \mathbb{R}^{m}).$

Looked at another way, *t* represents a smooth one-parameter family of linear transformations from *n*-dimensional to *m*-dimensional space.

This approach to linear transformations hardly changes anything from the current textbook approach. It is just one trivial way to define linear transformations without defining or describing linear spaces except to say they are domains and codomains of the transformations.

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