WHAT IS CATEGORICAL STRUCTURALISM?*

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In a recent paper [Hellman, 2003], we examined to what extent category theory ("CT") provides an autonomous framework for mathematical structuralism. The upshot of that investigation was that, as it stands, while CT provides many valuable insights into mathematical structure---specific structures and structure in general---, it does not sufficiently address certain key questions of logic and ontology that, in our view, any structuralist framework needs to address. On the positive side, however, a *theory of large domains* was sketched as a way of supplying answers to those key questions, answers intended to be friendly to CT both in demonstrating its autonomy *vis-à-vis* set theory and in preserving its "arrows only" methods of describing and interrelating structures and the insights that those methods provide. The "large domains", hypothesized as logico-mathematical possibilities, are intended as suitably rich background universes of discourse relative to which both category-and-topos theory and set theory can be developed side by side, without either emerging as "prior to" the other. Although those domains, as described, resemble natural models of set theory (on an iterative conception) or toposes suitably enriched with an equivalent of the Replacement Axiom, they are defined without set-membership as a primitive, and also without 'function' or 'category' or 'functor' as primitives; all that is required is a combination of 'part/whole' and plural quantification (in effect, the resources of monadic second-order logic). This background

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logic (including suitable comprehension axioms for wholes and "pluralities") suffices; and ontological commitments are limited to claims of the possibility of indefinitely large domains, any one extendable to a more encompassing one, without end.

Two interesting responses to this have already emerged on behalf of CT proponents, one by Colin McLarty [2004] and the other by Steve Awodey [2004]. Here we take the opportunity to come to terms with these and to assess their bearing on our original assessment and proposal. We will begin with a brief review of the main critical points of [Hellman, 2003]; then we will take up the responses of McLarty and Awodey in turn; and finally, we'll try to draw appropriate conclusions.

"Category Theory" and Structuralist Frameworks

The first point to stress is that the very term "category theory" is ambiguous, and the ambiguity follows closely on the heels of another, more basic one, that of "axiom" itself. On the one hand, axioms traditionally are conceived as basic truths *simpliciter*, as in the traditional conception of Euclidean geometry, or the axioms of arithmetic, or the axioms of, say, Zermelo-Fraenkel set theory. Call this the "Fregean conception" of axioms. In the geometric case, primitive terms such as 'point', 'line', 'plane', 'coincident', 'between', 'congruent' are taken as determinate in meaning, so that axioms employing them have a determinate truth-value. For number theory, 'successor', 'plus', 'times', 'zero', etc. have definite meanings leading to true axioms (say the Dedekind-Peano axioms); and for set theory, of course, 'membership' is taken as understood, and the axioms framed in its terms true (or true of the real world of sets). In contrast, there are algebraic-structural axioms for groups, modules, rings, fields, etc., where now they are not even assertions, but rather *defining conditions* on types of structures of interest. The primitive terms are not thought of as already determinate in meaning but only as schematically playing certain roles as required by the "axioms". Call this the "Hilbertian conception". Any objects whatever interrelated in the ways required by the defining axioms constitute a structure of the relevant type, say, a *group*, or a *module*, … , or *a category*. In the latter case, primitive terms such as 'object', 'morphism', 'domain', 'codomain', and 'composition' are not definite in meaning, but acquire meaning only in the context of a particular interpretation which *satisfies* the axioms. Thus, "morphisms" need not be functions, and (so) "composition" need not be the usual composition of functions, etc.[1] A large part of the debate between Frege and Hilbert on foundations turned on their respective, very different understandings of "axioms" along precisely these lines.

It is worth noting, incidentally, that Dedekind [1888] presented his "axioms" for arithmetic explicitly as defining conditions, i.e. axioms in the Hilbertian sense, as part of his definition of a "*simply infinite system*", and not as assertions. One may, oversimplifying a bit, say that the tendency of modern mathematics toward a structuralist conception has been marked by the rise and proliferation of Hilbertian axiom systems (practically necessitated by the rise of non-Euclidean geometries), with relegation of Fregean axioms to a set-theoretic background usually only mentioned in passing in introductory remarks. Category theory surely has contributed to this trend; we now even have explorations of "Zermelo-Fraenkel algebras" (Joyal and Moerdijk [1995]).

[1] Thus, "morphisms" may be realized as homotopy classes of maps (between topological spaces), as formal deductions of formulas in a logical system, as directed line segments in a diagram, etc.

This ambiguity over “axioms” is, of course, passed on to “theories” of algebraic structures, as in “group theory”, “field theory”, … , and, indeed, “category theory” and (with some qualifications to be discussed below) “topos theory” as well. On the one hand, there is the first-order theory (definition) of groups, or of categories or toposes; but, on the other, there is a body of substantive theorizing *about* such structures, which, while constantly appealing to the first-order definitional axioms, is intended as *assertory*, and takes place in an informal background whose primitive notions and assumptions usually require logical analysis and reconstruction to be identified. Standard practice refers (in passing) to a background set theory, as it is well known that that suffices for most purposes. But of course that cannot serve in the context of “categorical foundations” where autonomy from set theory is the name of the game.

So what *is* the background theory? It is not clear. And so we find ourselves uncertain when it comes to comparing categorical structuralism with other frameworks that have been proposed. Here are five fundamental questions that we would submit *any* such framework should address:

(1) What is the background logic? Is it classical? Is it modal? Is it higher order? If so, what is the status of relations as objects?

(2) What are the extra-logical primitives and what axioms--presumably assertory--govern them? Are ‘collection’, ‘operation’, ‘category’, ‘functor’, for example, on the list? Especially, what axioms of mathematical existence are assumed?

(3) Is indefinite extendability of mathematical structures recognized or is there commitment to absolutely maximal structures, e.g. of absolutely all sets, all groups, etc.?

(4) Are structures eliminated as objects, and, if not, what is their nature?

(5) What account, if any, is given of our reference and epistemic access to structures?

In the case of set-theoretic structuralism, it is fairly clear how to answer at least (1) – (4); similarly, in the case of Shapiro's [1997] *ante rem* structuralism, and he takes a stab at (5) as well. In the case of modal-structuralism, answers to (1) – (4) are also forthcoming. (As an eliminativist structuralism, questions of reference are replaced with questions of knowledge of possibilities, related to, but even more difficult than, questions of knowledge of consistency, for in central cases we are interested in *standard* structures.)[2] But when it comes to categorical structuralism, it isn't clear what to say even with regard to (1) – (4). At most, bearing on (3), one finds widespread opposition to the view that a fixed background of sets is the privileged arena of mathematics.[3]

McLarty's "Fregean" Response

In a nutshell, McLarty claims that, while the algebraic-structuralist reading of CT axioms and general topos axioms is correct, nevertheless specific axioms for certain particular categories and toposes are intended as assertory. In particular, he singles out ETCS, the elementary theory of the category of sets, CCAF, the category of categories as a

[2] For detailed comparisons of these varieties of structuralism, see Hellman [2001].
[3] For further details, see Hellman [2003].

foundation, and SDG, synthetic differential geometry as a theory of the category of smooth spaces. The axioms of these systems are not to be read merely as defining types of structures but rather as assertions, true of existing parts of mathematical reality, much as the axioms of ZFC are normally understood. Indeed, in the case of ETCS, this could be understood as describing the very same subject matter as ZFC, although with the characteristic arrows machinery rather than a primitive set-membership relation.

It appears to me that CCAF, or, better, McLarty's own approach [1991] to axiomatizing a category of categories, is actually the most promising in relation to the above questions. Let us return to consider this below. First, let us take up the other two examples, ETCS and SDG.

Now, I would not wish to deny that ETCS provides an important part of a structuralist analysis of sets. Through its "arrows only" formulations and generalizations, it abstracts from a fixed set-membership relation and analyzes sets in their functional roles, "up to isomorphism", which is all that really matters for mathematics. What remains problematic, however, regarding McLarty's reading of ETCS (which he attributes to Mac Lane), is its apparent commitment to a fixed, presumably maximal, real-world universe of sets, "*the* category of sets". This just strikes me as a convenient fiction. First, there is the question of multiplicity of conceptions of sets, e.g. non-well-founded as well as well-founded, possibly choice-less as well as with choice, with or without Replacement, the various large cardinal extensions, and so forth. Presumably, all of these conceptions are mathematically legitimate, and it would be arbitrary to treat just one as ontologically privileged. But even if suitable qualifications of the "intended universe" are added to the (meta) description, the problem of indefinite extendability still looms. Whatever domain of sets we recognize can be transcended by the very operations that set theory seeks to codify, collecting, collecting everything "already collected", passing to collections of subcollections, iterating along available ordinals, etc. (This, incidentally, is entirely in accord with Mac Lane's expressed views [1986] on the open-endedness of Mathematics.) Set theoretic structuralism can be faulted precisely for failing to apply to set theory itself, especially in regard to the very multiplicity of universes of sets that it naturally engenders. Categorical structuralism promises to do better, but it is hard put to keep that promise if it falls back on a maximal universe of sets or, more generally, on an absolute notion of "large category".

When it comes to a realist interpretation of SDG, the problems are quite different but equally challenging. This is a non-classical theory of continua which can be developed independently of category theory, known as "smooth infinitesimal analysis" ("SIA"). (Topos theory has proved useful in providing models of SIA, but the essential analytic ideas do not depend on the topos machinery.) This is a theory intended as an alternative to classical, "punctiform" analysis; it introduces nilsquare (and nilpotent) infinitesimals, while at the same time limiting the class of functions of reals to continuous ones. A central axiom, the Kock-Lawvere axiom, stipulates that any function on the infinitesimals about 0 obeys the equation of a straight line. (The axiom is also called the "Principle of Microaffineness". This actually implies the restriction to continuous functions. And the constant slope of the "linelet" given by the axiom serves to define the derivative of a function. (The "linelet" can be translated and rotated, but not "bent".)) To accommodate nil-squares, certain restrictions apply to the classical ordered field axioms for the reals: nil-squares do not have multiplicative inverses, nor are they ordered with respect to one another or with respect to 0. Indeed, one proves that "not every *x* is either = 0 or not = 0". Not only does SIA refrain from using the Law of Excluded Middle (LEM), it derives results that are formally inconsistent with it (similar in this respect to Brouwerian intuitionism but contrasting with Bishop constructivism, which, with LEM added, gives back classical analysis.) But SIA, consisting of the (restricted) ordered field axioms, the KL axiom, and a certain "constancy principle", suffices for a remarkable development of calculus in which limit computations are replaced with fairly straightforward algebra, placing on an alternative, consistent and rigorous footing early pre-limit geometric methods in analysis and mechanics. (Cf. Bell [1998] for a nice survey of such results.)

Why is there a problem with thinking of this theory as an objective description of continuous functions or phenomena? After all, the charge by Russell and fellow classicists that infinitesimals lead to inconsistencies, while true of some naïve, informal practice, is demonstrably not true here, at least relative to the consistency of classical analysis. One simply must renounce LEM and, as already said, tolerate negative instances of it. The difficulty comes when we attempt to explain *why* LEM fails, even though (under the realistic hypothesis we are entertaining) there really are nilsquares making up the "glue" of actual continua, the points of classical analysis now regarded as a useful but fictitious formal artifact of analytic methods. If there really are such "things", doesn't logical identity apply to them just as to everything else, regardless of our abilities to discriminate them from one another or from 0? The situation is really very different from that posed by intuitionistic analysis. There constructive meanings of the logical operators, disjunction, negation, the conditional, both existential and universal quantifiers, obviously do not sustain the formal LEM or related classically valid principles, e.g. quantifier conversions such as “not for every x $\phi(x)$” to “there exists x such that not $\phi(x)$”. Apparent conflicts with classical analysis are only apparent due to these radically different meanings. But none of this is applicable in interpreting SIA, for constructive meanings do not seem appropriate to the subject. Nilsquares, for example, are not constructed at all; indeed, their existence cannot be asserted any more than it can be denied, on pain of contradiction. Rather one must settle for the double negation of existence. While this in itself might seem compatible with a constructive reading, the KL axiom itself seems, if anything, more dubious on such a reading. (What method do we have for finding the slopes of the linelets?) Lawvere and others have taken failure of LEM for nil-squares to express “non-discreteness”, perhaps in analogy with familiar kinds of vagueness. But, once we are speaking of objects at all, however invisible or intangible, how can the predicate ‘_ = 0’ itself be vague? Though from a setting he never contemplated, one harks back to Quine: “No entity without identity!”

Indeed, with the tendency to speak of ≠ as “distinguishable” (Bell [1998]), it is natural to seek an interpretation of ‘= ‘ in SIA as an equivalence relation broader than true identity, and this suggests trying to recover SIA in a *classical* interpretation. Such an interpretation has actually been carried out in detail (Giordano [2001]). Certain differences emerge: the class of functions treated is narrower than all continuous ones (a Lipschitz condition is invoked), but the KL axiom and much of the theory are recovered on a fully classical basis. Whether proponents of SIA and SDG will plead “change of subject” remains to be seen.

Turning to “category of categories”, efforts towards axiomatization are at least grabbing the bull by the horns, laying down explicit assertory axioms on the mathematical existence of categories and providing a unified framework for a large body of informal work on categories and toposes (hence mathematics, generally). Three questions demand our attention:

(1) What concepts are presupposed in such an axiomatization?

(2) Are these such as to sustain the autonomy of CT *vis-à-vis* set theory or related background, 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”?

On (1) and (2), it is clear that these axioms (as in McLarty [1991]) are not employing the CT primitives (‘object’,. ‘morphism’,‘domain’, ‘codomain’, ‘composition’) schematically, as in the algebraic defining conditions, but with intended meanings presumably supporting at least plausible truth of the axioms. The objects are *categories*, the morphisms are *functors between categories*, etc. Commenting on this, Bell and I recently wrote:

> “Primitives such as ‘category’ and ‘functor’ must be taken as having definite, understood meanings, yet…they are in practice treated algebraically or structurally, which leads one to consider interpretations of such axiom systems, i.e. their semantics. But such semantics, as of first-order theories generally, rests on the set concept: a model of a first-order theory is, after all, a set. The foundational status of first-order axiomatizations of the [better: a] metacategory of categories is thus still somewhat unclear.” (Hellman and Bell [forthcoming])

In other words, when we speak of the "objects" and "arrows" of a metacategory of categories as *categories* and *functors*, respectively, what we really mean is "structures (or at least "interrelated things") *satisfying* the algebraic axioms of CT", i.e. we are using "*satisfaction*" which is normally understood set-theoretically. That is not to say that there are no alternative ways of understanding "satisfaction"; second-order logic or a surrogate such as the combination of mereology and (monadic) plural quantification of modal-structuralism would also suffice. But clearly there is some dependence on a background that explicates *satisfaction* of sentences by structures, and this background is not "category theory" itself, either as a schematic system of definitions or as a substantive theory of a metacategory of categories. But this need for a background theory explicating *satisfaction* was precisely the conclusion we came to in our [2003] paper, reinforcing the well-known critique of Feferman from [1977], which exposed a reliance on general notions of "collection" and "operation". It was precisely to demonstrate that this in itself does not leave CT structuralism dependent on a background set theory that I proffered a membership-free theory of large domains as an alternative. Although the reaction, "Thanks, but no thanks!", frankly did not entirely surprise me, it will also not be surprising if a perception of dependence on a background set theory persists.

As to the third question of scope, I think it is salutary that McLarty calls his system "*a* (meta) category of categories", rather than "*the* category of categories", which flies in the face of general extendability. No structuralist framework should pretend to "all-embracing completeness", in Zermelo's [1930] apt phrase. And we certainly had better avoid such things as "the category of exactly the non-self-applicable categories"!

But it is, I believe, an open question just what instances of impredicative separation should be allowed.

To conclude this section, it seems a fair assessment to say that, while axioms for a (meta) category of categories do make some progress toward providing answers to some of our five questions put to the various versions of mathematical structuralism, we are left still well short of satisfactory, full answers, even to the first four.

Awodey's "Hilbertian" Response

In contrast to the foregoing, this response takes as its point of departure an "anti-foundationalist" stance: mathematics should not be seen as based on a fixed universe of special objects, the elements of domains of structures, the relata of structural relations, as on the set-theoretic view. Instead, mathematics has a *schematic* character, which seems to mean two things: any theorem includes hypothetical conditions, which govern just what aspects of structure are relevant; and, in any case, the particular nature of individual objects is irrelevant. Moreover, whereas modal structuralism tries to get at this by open-ended modal quantification (which is *not* to be interpreted as ordinary quantification over a fixed background domain of *possibilia*, as Awodey seems to recognize), category theory itself provides a more direct expression, standing on its own without need of any further (assertory) background principles.[4] The central, general but flexible primitive notion is "morphism", capable of grounding talk of relations, operations, etc. (A summary list of relevant "arrows only" categorical concepts illustrates this.) A "top-down"

[4] It should be clear that modal-structuralism, although it does provide such background principles, is also quite explicitly "non-foundationalist" in Awodey's sense. To avoid confusion, I have preferred to speak of "structuralism frameworks" rather than "foundations", but I would certainly plead guilty to "foundational concerns", much in the spirit of Shapiro's *Foundations without Foundationalism*. Clearly, this is reflected in the questions (1) – (5) we have been putting to the various versions of structuralism.

metaphor, as opposed to “bottom-up”, is used; it seems that it is sufficient simply to *describe* whatever ambient background structure we deem relevant to the mathematical purpose at hand, without needing to worry about any absolute claims of existence.

(Clearly, this is reminiscent of Hilbert’s view that sought to eliminate metaphysics from mathematics by, in effect, replacing absolute claims of existence with a combination of proofs from formal axioms, as defining conditions, together with a proof of formal consistency of the relevant axioms, although presumably, since we are in a post-Gödelian era, the latter demand is omitted.)

I see a dilemma in understanding all this. Either mathematics is adequately understood as just a complex network of deductive and conceptual interconnections, or it is not. On the first horn, what we are really presented with is a kind of formalism, in which theorems in conditional form, together with definitions, are all there is to mathematics, that is, we just give up on the notion of mathematical truth as anything beyond deductive logical validity. In this case, we really need not worry about primitives with meanings supporting basic axioms in the Fregean sense. Questions of truth (beyond first-order logical truth of conditionals) would arise only in certain limited cases, typically in applications of mathematics where we might be in a position to assert that the antecedent conditions are indeed fulfilled (e.g. that certain finite structures, say, are actually instantiated, or that even certain infinite ones are, say space or time or space-time as continua). Whether this is intended and is viable, after all (i.e. after all the criticisms that have been levelled against deductivism), remain to be determined.[5]

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[5] I have been taking as a ground rule for articulating structuralism that it should not collapse to formalism or deductivism. If CT structuralism is playing by different rules, that certainly should be made explicit.

On the other horn of the dilemma, we take seriously the idea that ‘morphism’ is a primitive with definite, if multifaceted, meaning, giving genuine content to mathematics beyond mere inferential relations. But what is that meaning? And what is the content beyond inferential relations? Awodey himself [1996] has stressed the algebraic-structural character of the CT axioms, and, unlike McLarty, he does not appeal to any special topos axioms as assertions, nor does he appeal to a special category of categories. It seems clear, then, that the notion of morphism—which, unlike the notion of ‘part/whole’ employed in modal-structuralism and also exhibiting a kind of "schematic character", is a mathematical term of art, not a familiar one in ordinary English—depends on the context, viz. *on the category or categories presupposed or in which one is working*. As indicated at the outset, arrows (i.e. morphisms) need not be ordinary functions. They need only *satisfy the conditions on "arrows" of the CT axioms or extensions thereof*. Surely, this is what should be said in explaining "what morphisms are". Moreover, *functors* do more that ordinary functions, and they are centrally involved in "the usual language and methods of category theory" (Awodey [2004], p. 62). But then what we really have as primitives are "*satisfaction of axioms*"" and "*functor between categories*"; i.e. we are presupposing "*category*" as a primitive as well. But this brings us right up against the same problem that confronted the previous view, namely that we are falling back on *prima facie* set-theoretic notions after all. The main difference seems to be that, whereas on the McLarty view we were at least being given axioms asserting the mathematical existence of various categories, here we are not even being given that. In any case, the CT "arrows only" explications of "relations", "operations", and so forth, are of no avail until we first understand "morphism" (i.e. "arrow"), "functor" and "category", i.e. until we already understand *satisfaction* or equivalent (second- or higher-order) notions. It would be plainly circular to appeal to “morphisms” to explain this!

Conclusion

The contrast between Fregean and Hilbertian axioms seems to present us with a stark choice. But really, unless we go back to formalism, mathematics requires both. For all the axiom systems of ordinary mathematics, for number theory, analysis, algebra, pure geometry, topology, and surely much of category and topos theory, i.e. for all commonly studied structures and spaces, not only is the Hilbertian conception appropriate, it is part and parcel of standard modern practice. But when we step back and contemplate fundamental and foundational issues—when we ask questions about what principles govern the mathematical existence of structures generally, or when we consider the closely related “unfinished business” of Hilbert’s own program (as Shapiro puts it), the place of metamathematics, questions of absolute and relative formal consistency, questions of (informal) higher-order “consistency” or “coherence”, relative interpretability, independence, etc.—then we are in the realm of outright claims, not mere hypotheticals as to what holds or would hold in any “structures” satisfying putative algebraic (meta) axioms of metamathematics. Rather we are seeking assertory axioms in the Fregean sense.[6]

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[6] This is highlighted in Shapiro [forthcoming]. As he reminds us, Hilbert clearly regarded the claims of proof theory as “contentful”, closely related to statements of number theory (insofar, “mathematical” as well as “metamathematical”), and not conditional with respect to implicitly defined (meta) structures of metatheory

Thus, in connection with category theory, the advice of Berra [1998], “When you come to a fork in the road, take it!”,[7] is quite apt, and the theory of large domains I sketched in my [2003] was one way of taking the advice (and the fork!). The alternative responses considered here, categories of categories (Fregean) or category theory as schematic mathematics (Hilbertian), lead us straight back to *prima facie* set-theoretic notions, only slightly beneath the surface, and so do not sustain category theory as providing an autonomous structuralist framework adequate to the needs of both mathematics and metamathematics.

[7] One road leads to G`ttingen, the other to Jena.

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