Recent developments in the philosophy of category theory

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The consistency of ZFC

- undecidable (Gödel)
- In particular, ZFC could be inconsistent
- but this could only be proved by finding one day a contradiction.
- Bourbaki: trust in the consistency of ZFC on "empirical" grounds (no contradiction discovered so far)

Bourbaki's line of argument

1954 introduction of Ensembles (my translation):

"In the 40 years since one has formulated with sufficient precision the axioms of [set theory] and has drawn consequences from them in the most various domains of mathematics, one never met with contradiction, and one has the right to hope that there never will be one. (. . .) We do not pretend that this opinion rests on something else than experience." [Bourbaki(1954)]

talk "Foundations of mathematics for the working mathematician" delivered 1949 by André Weil:

"Absence of contradiction, in mathematics as a whole or in any given branch of it, (. . .) appears as an empirical fact, rather than as a metaphysical principle. The more a given branch has been developed, the less likely it becomes that contradictions may be met with in its further development." [Bourbaki(1949)]

Bourbaki seems to suggest a certain way in which contradictions are met with in the everyday work of mathematicians. Is this truly the usual way they have been met with historically? [Krömer(2012)]

"As far as sets occur and are necessary in mathematics (at least in the mathematics of today, including all of Cantor's set theory), they are sets of integers, or of rational numbers (. . .), or of real numbers (. . .), or of functions of real numbers (. . .), etc.; when theorems about all sets (or the existence of sets) in general are asserted, they can always be interpreted without any difficulty to mean that they hold for sets of integers as well as for sets of real numbers, etc. (. . .). This concept of set, however, according to which a set is anything obtainable from the integers (or some other well defined objects) by iterated application of the operation "set of", and 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 "naive" and uncritical working with this concept of set has so far proved completely self-consistent." [Gödel(1947), p. 518f]

Grothendieck universes

"It is certain that one needs to be able to consider categories, functors, homomorphisms of functors and so on . . . as mathematical objects on which one can quantify freely and which one can consider as in turn forming the elements of some set. Here are two reasons for this necessity: to be able to carry out for functors the types of properly mathematical reasoning (induction and so on. . .), without endless complications installed in order to save the fiction of the functor which is nothing but a specific metamathematical object; because the sets of functors or of functorial homomorphisms, with the various natural structures which one has on them (group of automorphisms of a given functor and so on) obviously are mathematically important, and because many structures (semi-simplicial structures and so on) are most naturally expressed by considering the new objects to be defined as functors."

(Grothendieck in Bourbaki manuscript n^{o} 307, July 1958-March 1959)

Grothendieck universes and consistency

- usual way of allowing for large categories: postulating the existence of Grothendieck universes
- equivalent: existence of strongly inaccessible cardinals ("Tarski's axiom")
- Tarski's axiom independent of ZFC [Drake(1974), p.67]
- relative consistency is undecidable [Kunen(1980), p.145]
- no Bourbaki-type empirical justification

Intended models vs. formalism

My central focus: the relation between formal definitions and intended uses of mathematical concepts.

Already in the early days of axiomatic mathematics, Poincaré stressed that the formal can't stand for itself:

"[. . .] A selection must be made out of all the constructions that can be combined with the materials furnished by logic. The true geometrician makes this decision judiciously, because he is guided by a sure instinct, or by some vague consciousness of I know not what profounder and more hidden geometry, which alone gives a value to the constructed edifice" [Poincaré(1914), 148].

In the case of non-standard models of formal systems, the formal definition is "overcomprehensive": it has more models than just the intended one(s).

Kreisel: we have the capacity to cope with this situation:

"Many formal independence proofs consist in the construction of models which we recognize *to be different from the intended notion. It is a fact of experience that one can be honest about such matters! When we are shown a 'non-standard' model we can honestly say that it was not intended. [. . .] If it so happens that the intended notion is not formally definable this may be a useful thing to know about the notion, but it does not cast doubt on its objectivity [. . .]." [Kreisel(1970), 25]*

In the case of category theory, the situation is complementary: usual formalizations are too restrictive to capture the naive theory. It seems at first glance that this situation provokes but another instance of the capacity stressed by Kreisel:

"The restrictions employed [Grothendieck universes or NBG*] seem mathematically unnatural and irrelevant. Though bordering on the territory of the paradoxes, it is felt that the notions and constructions [as the category of all structures of a given kind or the category of all functors between two categories] have evolved naturally from ordinary mathematics and do not have the contrived look of the paradoxes. Thus it might be hoped to find a way which gives them a more direct account" [Feferman(1977), 155].*

The view of the working mathematicians

"The well known fact that some basic constructions applied to large categories take us out of the universe seems to me to indicate that the constructions are not yet properly presented. The discovery of proper presentations is too difficult, though, for all work on these constructions to wait for it." [Isbell(1966)]

"Categoricians have, in their everyday work, a clear view of what could lead to contradiction, and know how to build ad hoc safeguards." [Bénabou(1985)]

"Our intuition tells us that whenever two categories exist in our world, then so does the corresponding category of all natural transformations between the functors from the first category to the second." [Lawvere(1966), 9]

These quotations suggest the following situation:

- the workers in the field are convinced that category theory is a clear and reliable conceptual framework for a certain type of mathematical work, the absence of a "proper presentation" notwithstanding;
- (Grothendieck universes being part of an unproper presentation, replacing the *obscurum* by the *obscurius*);
- they feel able to make judicious choices — by some "instinct"?
- Epistemological questions pursued in my 2007 book:
  - On which grounds do workers in the field feel justified in using various category-theoretic constructions; on which grounds do they find them "intuitive" ?
  - Would a set-theoretical foundation be appropriate to justify this feeling? (is this feeling related to the hoped-for possibility to reduce the constructions to trusted-in set-theoretical axioms?)

Topos-theoretic foundations and Feferman's objection

Feferman quotes Mac Lane (personal communication):

"mathematicians are well known to have very different intuitions, and these may be strongly affected by training" [Feferman(1977), 153]

Feferman replies:

"I believe our experience demonstrates [the] psychological priority [of the general concepts of operation and collection with respect to structural notions such as 'group', 'category' etc.]. 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." [ibid.]

- A composer makes judicious choices without hearing, because he or she has *learned* how to do that.
- My thesis in 2007 was that psychological priority is in the last analysis irrelevant for the problem of proper presentation:
- one needs to take into account the effects of training because without the training one is not able to judge the "properness".
- Kreisel pointed out that we manage to restrict uses of a formal concept to those in agreement with an intended model, even if this intended model is not formally definable in the sense that it is not (yet) grasped exactly by the formal concept.
- The existence of such a capacity presupposes a corresponding training.
- In order to use the concept competently, you need to know both the formal definition and the intended model.
- I tried to work out a conception of intuition in agreement with these observations by focussing on intuitive *uses* (following Peirce)

Unlimited category theory

Feferman already in 1977 suggested the following requirements and in [Feferman(2013)] gave a foundational system (set-theoretical in nature and based on Quine's "New foundations") meeting them "nearly" (p.9):

- (R1) Form the category of all structures of a given kind, e.g. the category **Grp** of all groups, **Top** of all topological spaces, and **Cat** of all categories.
- (R2) Form the category B^A of all functors from A to B, where A, B are any two categories.
- (R3) Establish the existence of the natural numbers N , and carry out familiar operations on objects $a, b, \ldots$ and collections $A, B, \ldots$, including the formation of $\{a, b\}$, (a, b), $A \cup B$, $A \cap B$, $A - B$, $A \times B$, B^A, $\bigcup A$, $\bigcap A$, $\prod B_x[x \in A]$, etc.

(R1)-(R3) spell out what workers in the field *intend* a foundation to provide.

Ernst's result

- M. Ernst [Ernst(2015)] showed that "unlimited category theory" in the sense of [Feferman(2013)] is inconsistent, i.e. that presupposing (R1)-(R3) leads to a contradiction.
- Ernst's strategy: build a proof in analogy to the proof that there can be no set of all sets using Cantor's theorem.
- Cantor's theorem says that there is no surjection between any set and its powerset; established by a diagonal argument
- In Ernst's proof (relying on Lawvere's 1969 work on transporting diagonal arguments to the language of category theory) the set-theoretical notion of "surjection" is replaced by a notion of onto mapping in a category-theoretic sense, and "set of all sets" and "powerset" by corresponding objects in a certain category of graphs.

Some comments

- Ernst's proof does not easily generalize to other candidates for contradiction:
  "There are certain barriers to extending this result to other categories beyond the aforementioned categories of graphs." [Ernst(2015)] p.319
  (neither for a category of all sets nor for a category of all categories)
- The way Ernst actually arrived at the contradiction underlines my criticism concerning the Bourbaki viewpoint
- Since Feferman's 2013 system is based on Quine's NF — can Ernst's result be of any help in the investigation of the consistency of Quine's system?

A suggestion of mine from 2007 dismissed?

"there may be a shortcoming in the usual claims about the set-theoretical illegitimacy (or inconsistency) of **Cat***. For* **Cat** *contains "itself" not as the entire complicated building of points, arrows and labels (its inside) but as a single point connected to certain arrows (the functors between other categories and this category). Thus, to speak about "self-containing" here seems quite simplifying. This is naturally no proof for the claim that a category of all categories is consistent but a remedy to the usual arguments in favor of its illegitimacy."* [Krömer(2007)] p.280

- But Ernst takes this difference into account. The situation is the following:
  - there is a problem with self-containing pointed out by the standard proof that there can be no set of all sets using Cantor's theorem;
  - Ernst shows that this problem stays put when the category-theoretic equivalents of all set-theoretical notions intervening in this proof are used (when the category-theoretic equivalent of self-containing is used, so to say)
- (My idea was too naive with respect to the notion of cardinality, after all.)
- One should ask: on which grounds is Lawvere's theorem from [Lawvere(1969)] proved?

A failure of category-theoretic foundations of math?

"One could take the failure of unlimited category theory as an indictment of categorial foundations more generally. This is a tempting conclusion to draw, but it fundamentally misunderstands the role of unlimited category theory with respect to foundations. It is true that unlimited category theory cannot be a foundation for mathematics because it is inconsistent. However, unlimited category theory is not one of the proposals for a categorial foundation of mathematics. Its inconsistency could be used as a case against categorial proposals only if those proposals could provide a foundation for unlimited category theory.

This is certainly false for the most well known proposals."
[Ernst(2015)] p.318

In my opinion, Ernst's result rather shows the strength of category-theoretic tools in foundational research (since his proof relies successfully on these tools)

Other foundational proposals reinforced?

> *"[. . .] many different foundations fail to found unlimited category theory. This failure has been taken as an argument against the use of those foundations. For example, ZFC has been considered inadequate as a foundation for mathematics because it cannot provide a foundation for unlimited category theory. What the result of this paper shows is that such objections are ultimately unfounded. No foundation can underwrite unlimited category theory and so one cannot reject a foundation for such a failure. Thus, the inconsistency of unlimited category does not produce new objections to foundational proposals, it actually removes objections. Whatever foundation one wants to accept, that decision cannot be made based on the relationship between that foundation and unlimited category theory."* [Ernst(2015)] p.318

I would argue only (and still) that ZFC is inadequate as a foundation for any version of category theory of any use in actual mathematics.

Is my epistemological investigation now pointless?

- Ernst's result at first glance seems to show that the epistemological question posed earlier is ultimately only of historical interest: people when introducing the constructions felt justified to do so, but finally the feeling turned out to be misleading. (And one might wonder why they erroneously had that feeling.)
- First of all, this would be a wrong description of the situation since obviously the workers in the field did not confound their convictions with a consistency proof.
- I maintain that my question is still of philosophical interest. I (still and even more) think that
  - thinking in terms of set membership is not the only form of thinking leading to mathematical knowledge
  - the debate on category theory and foundations should make us think about what we expect a foundation of mathematics to accomplish

- A foundation is often thought of as an axiom system (set-theoretical, for instance) to which we reduce deductively other mathematical propositions and theories; it is the possibility of this reduction which ascertains that we indeed have obtained mathematical knowledge
- If we think of a foundation like this, we ultimately come down to the problem why we believe in these axioms.
- Pen Maddy in her early two-part paper on "Believing the axioms" [Maddy(1988)] investigates justifications (nondemonstrative arguments) in favour of set-theoretical axioms
- (Unfortunately, I was not aware of this important paper when writing my 2007 book)

Maddy and extrinsic vs. intrinsic justifications

Maddy in particular distinguishes between intrinsic and extrinsic justifications for axioms.

"The suggestion is that the axioms of ZFC follow directly from the concept of set, that they are somehow "intrinsic" to it (obvious, self-evident)" p.482

"I have argued elsewhere [. . .] that we acquire our most primitive physical and set-theoretic beliefs when we learn to perceive individual objects and sets of these. [. . .] The simplest axioms of set theory, like Pairing, have their source in this sort of intuition. [. . .] Given its origin in prelinguistic experience, the best indication of intuitiveness is when a claim strikes us as obvious [. . .]" p.758

Types of extrinsic justification (Maddy p.758f)

(1) confirmation by instances (the implication of known lower-level results)
(2) prediction (the implication of previously unknown lower level results)
(3) providing new proofs of old theorems
(4) unifying new results with old, so that the old results become special cases of the new
(5) extending patterns begun in weaker theories
(6) providing powerful new ways of solving old problems
(7) providing proofs of statements previously conjectured
(8) filling a gap in a previously conjectured "false, but natural proof"
(9) explanatory power
(10) intertheoretic connections

- It is only with respect to extrinsic arguments that Maddy says: *"A careful analysis of the structure of such arguments must precede what we hope will be an explanation of why they lead us toward truth"* (p.759)
- I think that there is not really a difference between intrinsic and extrinsic arguments when we are searching for such an explanation. (Maddy labelled the distinction as "vague" herself.)
- In *Tool and Object* I presented an (admittedly very wide) conception of intuition (of experts) and tried to present evidence supporting the claim that in the case of category theory, experts are using such an intuition in their mathematical work. I have the impression that such an intuition of experts includes appeals to many nondemonstrative arguments Maddy would have classified as extrinsic (in the sense that they can be seen as belonging to some type of (1)-(10)).
- My claim is that it depends on the context in which a concept is used whether such an argument counts as intrinsic or extrinsic to the concept.

- More precisely, Maddy with (1)-(10) focusses on the roles some axiom can play in some mathematical working situation. I would rather focus on why the fact that the axiom can play one of these roles makes it more convincing.
- To label a proposition as "obvious" or "self-evident" is done by relying on a capacity called intuition. While Maddy concentrates on "our most primitive physical and set-theoretic beliefs" stemming from "prelinguistic experience", we have seen many much less primitive beliefs labelled as intuitive by experts.
- How do experts develop such an intuition? I think: it is developed during exactly such specific manipulations of theories like those yielding results of the types (1)-(10).
- I do not see why to privilege prelinguistic experience with respect to expert knowledge.

Mathematical intuition has been described as stored experience by [Davis and Hersh(1980)].

At another place, they stress the effect of training on intuition:

"We have defined [velocity] v by means of a subtle relation between two new quantities, ϵ and δ, which in some sense are irrelevant to v itself. [. . .] The truth is that in a real sense we already knew what instantaneous velocity was before we learned this definition; for the sake of logical consistency we accept a definition that is much harder to understand than the concept being defined. Of course, to a trained mathematician the epsilon-delta definition is intuitive; this shows what can be accomplished by training." [Davis and Hersh(1980), 245f]

(Think of Feferman's composer argument again).

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