A discussion between Colin McLarty and Andrei Rodin about Structuralism and Categorical Foundations of Mathematics.

CM: Colin McLarty
AR: Andrei Rodin

CM (in the course of discussion started by other people): I myself am also confident that people will calm down and notice that axiomatic categorical foundations such as ETCS and CCAF work perfectly well, in formal terms, and relate much more directly to practice than any earlier foundations. One hundred and fifty years of explicitly foundational thought has made this progress possible.

(Note: ETCS stands for Elementary Theory of the Category of Sets, see F.W. Lawvere. “Elementary theory of the category of sets”. *Proceedings of the National Academy of Science*, 52(6):1506-1511, 1964.; CCAF stands for Category of Categories as a Foundation of Mathematics, see F.W. Lawvere. “The category of categories as a foundation for mathematics”. *Proceedings of the La Jolla Conference on Categorical Algebra*, pages 1-21, 1966.)

AR (joining the discussion): I do NOT believe that ETCS and CCAF "work perfectly well". Each of these involve two foundational "layers", namely, the classical "bottom" and a categorical "superstructure". By the classical bottom I mean NOT an underlying Set theory but the "Elementary theory of categories" (ETC), i.e. a theory of categories using the usual First-Order Logic (FOL) and relying on the standard Hilbert-Tarski-style axiomatic method. I agree with John Mayberry and some other people who argue that this axiomatic method alone assumes a basic notion of set or collection. Unlike Mayberry I don't think that this fact implies that the project of categorical foundations, as a alternative to and replacement for set-theoretic foundations, is futile. Recall that the axiomatic method we are talking about (which is, of cause, quite different from Euclid's method and other earlier versions of axiomatic method) emerged together with Set theory.

In order to make categorical foundations into a viable alternative of set-theoretic foundations we still need to provide Category theory with a new axiomatic method rather than use the older axiomatic method as do ETCS and CCAF. Elements of this prospective axiomatic method are found in what I just called the "categorical superstructure" of ETCS and CCAF but as far as these theories are concerned the classical background (FOL+ETC) is indispensable.

This is why I say that ETCS and CCAF do NOT work perfectly well as categorical foundations. Building of "purely categorical" foundations remains an open problem. It is not a matter of a ideological purity but a matter of complete "rebuilding" (Manin's word) of foundations: in my view, such a rebuilding is healthy and refreshing in any circumstances (unless it clashes severely with practice).

CM: The basic notions are in fact not very articulate in themselves, and throughout the history of mathematics it has taken further ideas to articulate them. Bill saw how to articulate these and many more, quite directly, in categorical terms not assuming any prior set theory. That articulation works even if you do not take it as foundational. But it gets a natural foundational character in the framework of the category of categories -- thus CCAF, the axiomatic theory of the category of categories as a foundation.

AR: I agree with you about generalities concerning articulation of concepts. But I disagree with your conclusion. A reason why I say CCAF is not a satisfactory categorical foundation is different. ETC is the formal basis of CCAF and ETC relies on a pre-formal notion of set or collection just like ZF or any other axiomatic theory built with Hilbert-Tarski axiomatic method. Elements of a new properly categorical method of theory-building are present in the "basic theory" (BC) that follows ETC. (I mean, in particular, the "redefinition" of functor in BC as 2-->A, etc. The standard definition of functor given earlier in ETC never reappears in BC.) However in CCAF these new features are not yet developed into an autonomous axiomatic method - or into a new way of formalisation of pre-formal concepts, if you like. In my understanding, such a method should meake part of categorical foundations deserving the name. CCAF remains in this sense eclectic, it is a half-way to categorical foundations.

CM: Do you mean that every formalized axiom system uses arithmetical notions such as "finite string of symbols"? This is why that formal axioms cannot be the real basis of our knowledge of math, but it has no more bearing on categorical axioms than any others.

AR: No I did not mean this. Agree that this argument has no more bearing, etc.

CM: Or do you think that pre-formal notions of "set" or "collection" are all based on iterated membership and Zermelo's form of the axiom of extensionality, so that CCAF is less basic than ZFC? That is a common belief among logicians who have not read Zermelo's critique of

Cantor (where Zermelo points out that Cantor did not hold these beliefs) and who know a great deal more of ZFC than of other mathematics.

AR: No. I certainly do NOT think that pre-formal notions of "set" or "collection" are all based on iterated membership and Zermelo's form of the axiom of extensionality. I explain in the next entry what I do think about this matter.

CM: In fact, long before mathematicians could analyze the continuum into a discrete set of points plus a topology, they were well aware of collections like the collection of rigid motions of the plane -- and that "collection" is a category. It is not just a ZFC set of motions but comes with composition of motions and with an object that the motions act on.

AR: True, the most general notion of “collection” one can imagine may cover “category” and whatnot. But, I claim, the preformal notion of colection *relevant to the axiomatic method in its modern form* is more specific, and does NOT cover the preformal notion of category. I’m talking about “systems of things” in the sense of Hilbert 1899 rather than sets in the sense of ZFC or of any other axiomatic theory of sets. The idea of *this* axiomatic method (not to be confused with other versions of axiomatic method like Euclid’s) is, very roughly, this. One thinks of collection of “bare” unrelated individuals and then introduces certain relations between these individuals through axioms. Objects of a theories obtained in this way are sets provided with relations between their elements, i.e. “structured sets” (or better to say “structured collections”.

The principal feature of the preformal notion of collection involved here is that elements of such a collection are unrelated. Because of this feature the collection in question is not a general category. (It may be, perhaps, thought of as a discrete category but this fact has no bearing on my argument.)

The idea of building theories *of sets* using the version of axiomatic method just described is in fact controversial: it amounts to thinking of sets as bare preformal sets provided with the relation of membership. I mention this latter problem (which is not relevant to my argument) only for stressing that the notion of set or collection I have in mind talking about categorical foundation is NOT one that has any specific relevance to ZFC or any other axiomatic.

In ETC (the Elementary Theory of Categories in the sense of Bill’s 1966 paper) categories are conceived as collections of things called “morphisms” provided with relations called “domain”, “codomain” and “composition” (I hope I nothing forgot). The notion of collection involved in this construction is MORE BASIC than the resulting notion of category simply because this very axiomatic method is designed to work similarly in different situations - for doing axiomatic theories of sets and of whatnot. Even if there are pragmatic reasons to build theories of sets like ETCS and other mathematical theories on the basis of ETC rather than use axiomatic theories of sets like ZFC for doing category theory and the rest of mathematics, this doesn't change the above argument.

CM: What is a "formal basis" of a theory T?

AR: I called ETC "formal basis" of BT ("Basic Theory of Categories" in the sense of Bill's 1966's paper) meaning the two-level structure of BC. BC is ETC plus some other axioms. Conceptually the order of introduction of these axioms matters. My point (or rather guess) is that BC involves a prototype of a new axiomatic method (different from one I described above), which, however, doesn't work in the given form independently. I'm not quite prepared to defend any general notion of formal basis - I didn't mean to introduce such a general notion and didn't think about a general rule.

CM: The Eilenberg-MacLane axioms are a subtheory of CCAF and also have a natural, conceptually central interpretation in CCAF. I consider this an insight, Bill's insight, and I do not see how it becomes any kind of objection to CCAF.

AR: The subtheory you are talking about is what I call ETC in these postings, right? I hope I understand it correctly what you mean by "natural, conceptual central interpretation in CCAF" - the fact that any object in CCAF is a model of ETC, right? Now, the objection is this: ETC involves the preformal notion of collection that can NOT be thought of as a category (for the reason I tried to explain above).

In addition to the above argument my conclusion about CCAF is also based on the following historical observation. Every major historical shift in foundations of mathematics so far involved a major change of the notion of axiomatic method. (I can substantiate the claim if you'll ask.) But ETC (and, formally speaking, the whole of CCAF) relies on the old Hilbert-Tarski-style axiomatic method.

CM: Andre.Rodin@ens.fr suggests a better take on CCAF than the one he has been taking. That would be a take based more on Bill's published work on CCAF, and less on the philosophical objection that Geoff Hellman used to make about CCAF. Geoff himself has given up this objection.

AR: I don't know about Hellman's objection to CCAF and would be grateful for the reference. Talking about CCAF I mean first of all Bill's 1966 paper (leaving aside the problem noticed by Isbell as irrelevant to my story), rather than later versions of CCAF.

My argument is supposed to be based on its own (as, in my understanding, any philosophical argument should be) but not on works of other people.

CM: This is <an objection based on> the Hilbert conception where axioms are not asserted as true but offered as implicit definition; and so they are not about any specific subject matter but may be applied to whatever satisfies them. < > His preferred terminology was to say that categorical axioms are "not assertoric," meaning exactly what you mean by calling them axioms in Hilbert's sense. Stewart Shapiro made the point explicit in his article.

Lawvere from 1963 on has always been clear that his first order axioms ETCS and CCAF can be taken this way for metamathematical study -- but that he does assert them as true specifically of actual sets and categories. (Now Bill is not talking about any idealist truth or objects. He takes a dialectical view. But that is another topic.)

AR: This is an interesting aspect of the issue, about which I didn't think earlier. It might have a bearing on what I'm saying but so far I cannot see that it does. I am saying this: the axiomatic method in its modern form - which has been pioneered by Hilbert (among other people including Dedikind, et al.) and then further developed by Zermelo, Tarski et al. - involves a preformal notion of set or collection. Whatever first-order theory is built by this method objects of such a theory form preformal sets. In particular, when this method is used for building ETC then primitive objects of this theory called "morphisms" form preformal sets called "categories". In THIS sense the preformal notion of set remains a foundation of ETC. As far as I can see this situation doesn't depend on whether one thinks about axioms of ETC (or any other first-order theory) as assertive or as implicit definitions.

CM: But even take the interpretation corresponding to any one object A of CCAF. That amounts to specifying X as A in the parametrized interpretation. This interpretation does not deal with "the collection of objects of A" and "the collection of morphism of A". It never refers to any such collections. It deals with categories A,1,2,3, and functors among them.

AR: Right. This is exactly the reason why I say that CCAF has two well-distinguishable foundational "layers". At the first layer (ETC) a category is a collection of morphisms; at the second layer (i.e., in the core fragment of CCAF called in 1966 paper "basic theory"), as you rightly notice, a category is no longer a collection. My problem with this is actually twofold.

(1) The second layer depends on the first but not the other way round. Formally speaking, this simply amounts to the fact that axioms of ETC are axioms of BT but not the other way round. In THIS sense, once again preformal sets remain a foundation of CCAF.

(2) The joint between the two layers remains for me unclear. From a formal viewpoint this looks trivial: CCAF is ETC plus some other axioms. But this doesn't explain the switch from thinking about categories as collections to thinking about categories as identity functors. In Bill's 1966 paper this switch is described as a new terminological convention made in the middle of the paper (that cancels the earlier convention). This change of notation points to but doesn't really addresse the issue, as far as I can see.

CM: You would do better to notice the novelty of these parametrized and single-category interpretations of ETC in CCAF and take this as the kind of major change that you expect to see.

AR: I do see this as a great novelty. But I claim that this novel approach in the given setting (i.e. in CCAF) doesn't work *independently* of the older approach; moreover there is a sense in which the older approach remains basic while the new one is a "superstructure".

CM: This different axiomatic method is explicit in CCAF, and does work independently there. Specifically what is supposed to "not work" about it?

AR: ETC is built with the older Hilbert-Tarski's method. CCAF as a whole involves a genuinely new idea of how to build mathematical theories , I agree with you on this point. But since ETC is indispensible in CCAF - and morever since ETC is a starting point of CCAF the new categorical axiomatic method in the context of CCAF does not work *independently* (I am not saying that it doesn't work at all.) This is why I say that CCAF is only a half-way to genuinely categorical foundations of mathematics (that is only natural in case of such a pioneering work as Bill's 1966's paper).

For a possible development of CCAF into a better categorical foundation my hopes are for developing the diagrammatic reasoning of the second layer of CCAF into a genuine logico-mathematical synatax, which could serve independently of the usual first-order syntax. I'm particularly interested in this respect in recent work of Charles Wells, Zinovy Diskin, Dominique Duval, René Guitart and other people. Actually I would be quite interested to hear from these people what they think about a possible relevance of their work to foundations of mathematics and, more specifically, to CCAF.

A more general point: in my understanding, a dialectical attitude to foundations amounts to looking at them as a subject of further rebuilding - rather than looking at them as what is accomplished in principle and needs only working out some further technical details.

CM: Do you mean that Bill likes your idea that CCAF is inadequate as foundation because it is just HIlbert-style axiomatics? That would surprise me.

AR: Bill agreed with my distinction between the two foundational layers in CCAF - and this is the core of my argument.

CM: Yes of course. But he actually distinguishes three, and so do you. You call them ETC, BT, and CCAF. ETC comprises the elementary Eilenberg-MacLane category axioms. This would be "Hilbert style axiomatics" if the axioms were taken abstractly, but CCAF takes them concretely as applying to "the" category of categories and functors -- this is the chief mistake of Hellman and Mayberry and also of you so far as I can see. And in fact Hellman has given it up.

AR: I understand your point but I don't think that the issue concerns only one's favorite philosophical interpretation of these axioms; I claim that the very axiomatic technique (which is due to Zermelo, Tarski and others rather than Hilbert himself) also matters. In other words I don't believe that by taking these axioms "concretely" or "abstractly" one can really change the substance.

CM: The Basic Theory (BT) is pretty much as you describe it in your paper. This gives the E-M axioms a second role, which is "Hilbertian" in the sense that it applies the axioms to any and all categories, but is radically different from Hilbert's axioms in that it does not refer to discrete "collections" of objects and arrows. It refers directly to categories and functors taken as primitive, with the given relations between any category A and the categories 1,2,3 (an early step in categorical logic generalizing Tarski's set theoretic semantics). If you want to hold that every new foundation comes with a new concept of axioms then i think you should say this level is the new conception of axioms associated with CCAF.

AR: I agree with what you say about the second level but I claim that this second level doesn't work here without the first. As far as intentions and philosophical interpretation are concerned the role of E-M axioms maybe indeed seen as secondary. But this inteded view is not built in BT (and hence in CCAF) at the formal level. This is exactly the problem with CCAF that I stress.

CM: The remaining CCAF axioms give particular existence claims. The distinction between BT and CCAF is analogous to saying the basic layer of ZF is extensionality, and the other axioms assert particular existence claims (pair sets, powersets, an infnite set and so on).
So do you mean to say CCAF is unsatisfactory as a foundations since it includes ETC as a fragment, while the ETC axioms also have Hilbertian uses?

AR: Yes, you can put it this way if you like. I believe that the way in which one may "use" a given formal technique is by far less important than the technique itself. I'm talking about objective features of given axiomatic systems, not about their pragmatic uses and their possible interpretations.

CM: That would be like claiming that airplanes are unsatisfactory as a means of flying since airplanes use tires to take off and land, while tires are also used for ground transportation.

AR: That's a nice metaphor. To continue it note that a spaceship doesn't use tires (let alone the failed space shuttle project :)

Talking more seriously. The very idea of taking the standard first-order logic as a pre-given framework for developing foundations of mathematics seems me inappropriate for categorical foundations. The assumption of such an universal logical framework is, by the way, a common feature of Hilbert's and Frege's (otherwise different) approaches. It remains at place whether you think about E-M axioms abstractly or concretely.

CM: The space shuttle does use tires. It lands on them. The international space station uses the space shuttle (and I'll bet it uses tires itself in many other ways, but I do not know).

AR: Am I not right that the shuttle project is officially has been stopped and in the near future NASA plans to use return landing (or "watering") into the ocean as it did earlier? More seriously, my point is this: if a given foundation combines older and newer elements the further task is to replace the older elements by new ones.

CM: So you can now "prove" that these are unsatisfactory ways of orbiting the earth because they are not entirely different from bicycles. That will not stop people orbiting the earth in them.

You can "prove" that CCAF is unsatisfactory because not everything about it is new. But who has ever considered that a problem for any theory, of foundations or anything else? And who has ever seen a theory in which everything was new?

AR: Hilbert's foundations of geometry of 1899 were entirely new in the sense in which CCAF is not.

CM: The Space Shuttles will fly through 2010. This just makes a point that we both agree on: people constantly use "unsatisfactory" things, if "unsatisfactory" means that better ones will come along some day. Certainly CCAF is unsatisfactory in this sense. Every idea is.
Do you know Hegel's position that once you prove some conclusion is necessary, it remains to prove the conclusion is also true? That is, you can prove any conclusion you like if you make up the terms -- and everyone is free to make up their own terms at any time. But after you do that, it remains to show that your terms have actual content.

AR: I think that for future historians CCAF will be a good example of a transitional theory that combines older and new features almost 50/50.

CM: Let us hope so. Which fossils are transitional? Every one of them that left descendants. Every theory that is not a complete dead end is "transitional."

AR: Yes, of course. The issue of this discussion is what kind of decendent one wish to obtain and what one should do for it. Here we disagree.

AR: It seems me that Bill no longer endorses structuralist slogans of his 1966 paper.

CM: You seem to think that "the structuralist agenda announced by the author in the beginning of this paper (Lawvere 1966)" refers to what you call "traditional structuralism." But it does not.

I had trouble understanding you because I learned ETCS and CCAF before I learned what you call "traditional structuralism."   So I saw the difference as soon as I learned what you call the "traditional" form. And I learned Bourbaki's structuralism before Hellman and Shapiro had formulated theirs, and so I am aware of a great loss in their later version.

AR: In my paper I tried to describe Structuralism in most general terms. I didn't distinguish between different types of Structuralism; I describe Structuralism as "traditional" meaning Structuralism in general, not its specific version that I call "traditional". As far as this general notion of Structuralism described in my paper is concerned, Bill's slogan is perfectly structuralist.

CM: You also suffer that loss. You try to improve on Bill's formulation by saying:

"The subject matter of pure mathematics is covariant transformation, not invariant form"

Do you see the problem? Why only "covariant" transformation? Aren't contravariant transformations just as important? Of course they are.

AR: Your point about contravariant transformations is interesting but I didn't try to develop it in my paper (it is already too long). This point is more specific. My claim is that the notion of "invariant form" comes from the older structuralist thinking and that it is not appropriate for categorical thinking. For obvious historical reason structuralist thinking was abundant in the early category theory but I claim that it should be wholly abandoned in the future category-theoretic mathematics. The notion of invariant is relavant only to reversible transformations (hence invariants of groups) but not to transformations in general.

CM: Even Bourbaki knew that contravariance is as important as covariance. Once you have a fixed Bourbaki echelle de structure, to define the morphisms you still have to decide which aspects will be preserved covariantly and which contravariantly. (In traditional logician's terminology: which will be preserved and which will be reflected.)

This was obvious to them, because open sets are contravariant for continuous functions. Continuous functors do not preserve open sets, but reflect them. A topology on a set S is a suitable set of subsets of S, and so is a simplicial structure on a S (with a different sense of "suitable"). So both are Bourbaki structures. Continuous maps of topological spaces reflect but do not preserve open sets --while simplicial maps of simplicial sets preserve but do not reflect simplices. There is no general rule for what should be preserved and what should be reflected. (Bourbaki may have thought they had such a general rule. Did they? If they did then it has not become standard.)

It was also obvious to Bill from his first serious exposure to categories. You probably know that came when he was an undergraduate in Truesdell's seminar and gave a presentation on some exercises in Kelley's TOPOLOGY dealing with rings of continuous functions. Kelley shows there is a contravariant equivalence between a certain category of topological spaces and continuous functions, on one hand, and a certain category of rings and ring homomorphisms on the other. When the philosophical structuralists began giving simplified version of Bourbaki structuralism they all focused on "structure preserving functions." At the very least, their program cannot work for math until they also use structure reflecting functions. Does that seem like a small technicality?

AR: The whole idea to make Mathematical Structuralism into a philosophical doctrine AFTER it has been worked out in serious mathematics seems to me futile. I have a lot of respect to Bourbaki Structuralism but say that it is not appropriate for categorical mathematics.

CM: Well the more important fact is that MacLane was way ahead of that whole framework already in 1945, and so was Bill by 1959. They both knew the whole framework of "structured sets and structure preserving or reflecting functions" was far too limiting on a conceptual level. Many familiar objects in math are not normally thought of as structured sets. And it is even too narrow on a formal-technical level, although some objects of math that are not best thought of as structured sets *can* be forced into that mold if you try. Indeed they all can, by using the Yoneda lemma, if you like ZF foundations with the addition of Grothendieck universes and you don't mind cutting math up into as many pieces as there are universes. But CCAF gives a simpler way of getting around that.

AR: I agree with this but this doesn't have a bearing on my critical arguments against CCAF.

CM: Certainly it would be a great project to spell out what is and is not "structuralist" about CCAF -- in each of several different senses of "structural."

AR: I tried to do this in my paper - but without distinguishing between different senses of "structural". To make such a more refined analysis would be indeed useful. But a more challenging task seems to me to articulate a new alternative to Structuralism without reviving earlier outdated views.

(Note: See Andrei Rodin “Categories without structures”: arXiv: 0907.5143)

CM: But do not think that Bill in 1966 was talking about Bourbaki structures.

AR: Perhaps he had in mind his own version of Structuralism there. But I cannot see that this has a bearing on my principle argument.

CM: Not "perhaps." Certainly. And if you want to critique Bill's 1966 concept of structuralism without knowing what it was, then you must expect people to react badly.

AR: I would like to learn more about Bill's Structuralism of 1966 but he left not so much evidences. His today's thinking about Structuralism is different. Anyway I don't see why some specific features of one's Structuralism can be crucial. If Bill's Structuralism of 1966 doesn't fall under what I call Structuralism in my paper, then I'm wrong. But textual evidences (modulo my reading, of course) shows that it does. If you think that it doesn't please explain why.

I have a question concerning your note about the importance of covariant functors.When in CCAF the notion of functor is taken as primitive such a functor is by default covariant, right? Does this mean that in this framework the notion of contravariant functor is NOT primitive. Or perhaps it is even redundant?

CM: This remains a research topic, in my opinion. One way or another CCAF has to accommodate the notion of the dual to a category. Bill in 1966 gets it from his comprehension axiom scheme. So with that axiom scheme, dualizing is redundant.

But I would like to know better axioms for the categorical dual not using the comprehension axiom scheme. I believe that in my JSL article on CCAF I axiomatized dualization as an operator in the theory, and I think I proved that is not redundant by finding a non-standard model of my basic axioms which does not allow any interpetation of the dualizing operator. I think it was an interpretation using categories with some kind of further order relation on the arrows which is NOT definable from the category structure. (If I did not, then I should.)

(Note: See C. McLarty. “Axiomatizing a category of categories”. *The Journal of Symbolic Logic*, 56(4), 1991.)

Another promising approach is to axiomatize duals by axiomatizing Yoneda functors in some way, perhaps actually axiomatize the Yoneda profunctor on a category A. So: each category A has a dual A^(op) and a subcategory Y>->A^(op)xA with some suitable property. I have no reason to believe this is hard, but I have only ever spent a few days now and again thinking about it.

AR: Before I comment on your last remarks let me make another point. I think we both agree that Bill's paper of 1966 is an important achievement, so this is not the issue we are debating. The real issue seems me this: what to do with the content of this paper today, that is, more than 40 years later. Here our views and strategies are very different. You want to do two things about it , as far as I can see. First, to improve on this paper by fixing technical problems like that noticed by Isbell. Second, to promote the obtained improved version in mathematical and philosophical community as a working foundations of mathematics. I, in my turn, want to push further the same dialectical movement that brought about Bill's paper of 1966. When I say that CCAF is not satisfactory I mean exactly this: the need to rebuild it over again. I suggest the direction of such a rebuilding even if I don't have a ready result in hands.

(Note: See J.R. Isbell. “Review of Lawvere:1966”. *Mathematical Reviews*, 34, 1967.)

In a very general sense I agree that every new theory has some older elements. But I make a difference between how develop foundations and how develops the rest of science. Foundations are continuously *renewed*, not continuously grow. This renewal of foundations allows for progress, i.e. continuous growth, of science. In the history you can easily find foundations combining older and newer elements (I think of Clavius geometry). But the following development doesn't build on these older elements, it renews foundations further.

CM: I think that is an interesting idea and I really think you should develop it. But to to apply it to CCAF you need to understand CCAF better as it was written in 1966.

AR: Your efforts seem to me similar to efforts of people who earlier canonized set-theoretic foundations of mathematics. I don't think that CCAF or any other foundations of mathematics or science needs such a canonization. I think that canonization of foundations slows their renewal and as a consequence slows progress in science and mathematics. I, in my turn, try to push the renewal further. This is the point where we really disagree. Please correct me if I misinterpret your purposes.

CM: We do not disagree so much. We agree better ideas than CCAF will appear some day. We agree that you have not found these ideas yet. I think I have advanced CCAF some whereas you think I have merely "canonized" it. I would need to know more about what you mean by "canonizing" before I could say whether we disagree.

AR: I claim that the notion of "invariant form" comes from the older structuralist thinking.

CM: And this is false, as I have documented in many articles. For that matter so have Cartier and Corry and Kroemer and Mashaal, if by "older structuralist thinking" you mean Bourbaki.

AR: I didn't mean specifically Bourbaki, I meant structuralist thinking in general. I cannot see why distinctions between different varieties of Structuralism might shed a light on the issue I'm stressing. Even if such distinctions can be useful for some purposes they also can easily hide the very general notion of Structuralism. For obvious historical reason structuralist thinking was abundant in the early category theory.

CM: You yourself argue that the early category theory explicitly and frequently contradicts this.

AR: Yes - but so far category theory didn't suggest a clear philosophical alternative to Structuralism. The notion of invariant is relevant only to reversible transformations (hence invariants of groups) but not to transformations in general.

CM: And will you say publicly that Eilenberg and MacLane did not know this in 1942 (let alone 1945)? Will you say Lawvere did not know it in 1966? Or that he did not think about it?

AR: These people knew and thought about it but they didn't manage to get systematic philosophical consequences of this thinking and still embraced Structuralism that gives invariance and reversible transformations a distinguished status.

CM: If your principle argument is to show that CCAF does not achieve what Bourbaki, or Hellman and Shapiro, sought -- then you are quite right. And that point might be worth making to philosophers. But it is no critique of CCAF.

AR: I never said that my argument is to show this.

CM: Frankly I think you could do much better rebuilding if you did not spend so much time misreading the work that has been done.

AR: However you judge my reading of the recent history of maths I cannot agree with you, Colin, on this methodological point. My notion of rebuilding implies that I shouldn't take for granted any "work that has been done" but cannot avoid doing - or rather re-doing - this work myself. I don't deny that building one's work on works of other may be quite legitimate in science. But I don't think that it is legitimate in philosophy - or at least it doesn't work in philosophy in the same way as in the rest of science. In philosophy I stick to what can be called a Cartesian style. Of course, a philosopher must know stuff written by older generations. But at certain point he must put this stuff off - off his desk and off his mind - and develop his own reasoning without using earlier works as a support. This is a methodological, not a psychological or sociological point, this is simply how philosophy works, in my understanding. I should also add that I stick to the traditional view according to which foundations of mathematics and science is a philosophical subject. I can see that this methodological point is a serious point of our disagreement.

When Pierre Nicole says in the Preface to Arnauld's "New Elements" that the author drafted this book during few weeks in a country house without having any printed source at hand, he is very likely cheating. (Just like allegedly did Descartes when friends visited him in Holland and Descartes claimed he had no books in his house.) But this intention - that I don't take to be a practical maxim - well represents for me what I mean by rebuilding of foundations. Stressing this point I surely don't mean that the result of one's attempted rebuilding cannot or shouldn't be severly judged by others. I insist here on a metodological point but not defend my attempts of rebuilding from your critics. If you disagree with my reading of the recent history, please, show where and why it is wrong. I'm certainly not prepared to take one's reading of this history for granted. This may be appropriate in some contexts but not in the context of rebuilding of foundations. With all reservations in mind one can hardly deny that rebuilding of foundations of a subject implies a rebuilding of the history of this subject. Or putting it more mildly: it implies a new reading of the history of the subject.

CM: Thinking things through for yourself is the *opposite* of misreading others and misattributing views to them. You have to decide how important it is to you to understand Bill 1966. You could decide it is vital or irrelevant. I only say you would do better to either disregard him, or read him more carefully.

AR: So far I didn't get from you an evidence that my reading is wrong - even if I see that your reading is different. You didn't point so far to where my reading Bill is not careful (even if it is indeed not careful). You object on general grounds (in particular, by saying that I shouldn't split Bill in half) and I don't find these general arguments convincing. Since my analysis of Bill 1966 serves my project I don't want to disregard it.

CM: I don't think you should say Structuralism is quite "worked out." That would mean that people who don't use categorical methods no longer produce new theorems. Yet they plainly do. What about work on finite simple groups, notably on the Monster? No new theorems there?

AR: I'm exagerating saying that Structuralism is "worked out", so I'm agree with your critics on this point. In fact, even if my thinking about development of science is Hegelian-like, I certainly don't buy the understanding of this development as a single uniform global process (let's now leave aside the question which picture is "truly" Hegelian). Such an understanding is a part of what Popper calls Hegel's "historicism" - and I agree with Popper's critics of Hegel (or Hegelians) at this point. I see the development of science rather like the biological evolution, that is quite ramified. So the fact that structural thinking still brings new scientific results doesn't really contradict my picture. What I wanted to stress in this passage - and this explains the exaggeration - is that philosophy must be active rather than reactive with respect to science and suggest tentative new foundations even if this doesn't bring immediate scientific results.

CM: Andrei, you have Bourbaki Structuralism on the brain. Calling it "Structuralism" actually makes it worse since most of what philosophers of mathematics mean by "Structuralism" hadn't even been dreamed up yet in 1966.

AR: I can see well that Bourbaki Structuralism is something more specific than what I tried to describe in my paper as Structuralism tout court. My claim is that Bill in his slogan in the beginning of 1966 and in ETC - but not in the rest of his paper - followed this Structuralism tout court. Anyway I would like to understand better what you see as my confusion here.

CM: Here is your key point: Lawvere implicitly followed the Bourbakiste concept of

Structuralism, although his theory CCAF contradicted this concept.

AR: My strategy of analysing Lawvere's 1966 is to separate its structuralist content from its non-structuralist content. Your strategy seems to be to ascribe to Lawvere's 1966 a specific version of Structuralism. 1966 is a mathematical paper containing little of philosophical prose and no systematic philosophical account at all. The same applies to other mathematical sources you referred to earlier. Such stuff can inspire a systematic philosophical work but cannot replace it. So I don't think that this material by itself provides a sufficient ground for making a justified choice between the two strategies I just mentioned. The mathematical writings are open to both. You ascribe to 1966 a coherent philosophical position (that I would like to understand better); I ascribe to 1966 an eclectic and contradictory philosophical position. I opt for the latter because I believe that the contradiction I'm stressing is a very profound and fertile dialectical contradiction (certainly I don't mean here a contradiction in the sense of formal logic!) that still waits to be resolved through a new significant synthesis.

CM: Yes, you take all the evidence you have of Bill's view in 1966, and you decide to disregard the mathematical half when you interpret the philosophical half. You should spend less time splitting his ideas in half.

AR: Even this would be worth doing - showing a tension between philosophical slogans and mathematical content. But in fact what you say is not quite correct since the split goes through mathematics too. I mean the conceptual tension (or even gap) between "Elementary theory" and "Basic theory" that I stressed in my paper. The gap is marked by a terminological change. Generally I think that splitting contents in half and more than in half is called *analysis* and that such an analysis is (not only legitimate but also) useful when one reads texts - mathematical, philosophical or others.

CM: You merely show that if you do not try to relate the "two parts", then you can interpret them as contradictory -- especially by reading that the introduction as if it were Bourbaki (and thus bringing it neatly into line with philosophical views of "structuralism" in the 1990s) . That is easy.

AR: I don't read this introduction as Bourbaki but I think that it shares with Bourbaki what I call Structuralism in this paper - and what historically is by far an older idea than Bourbaki. Let's take at least Hilbert 1899 as a relevant reference. When I explain to a non-mathematical reader the example of group I indeed rely on Bourbaki. I must more stress perhaps at this point that this is a version of Structuralism, not Structuralism in general. But anyway this part of the paper is not supposed to have a bearing on my claim that Bill's introduction is structuralist.

CM: When you decide to read Bill 1966 as incoherent, you substitute your initial view of each separate part for any evidence you could gain from the coherence of the parts.

AR: I provide my own reading and I think that other people including you may provide another reading and then I can see what additional evidence I can get of it. I think that the dialectics of ideas works better when it involves different people rather than one. Actually my reading serves the purpose of pushing categorical foundations further: this is the main reason why the "incoherent" reading seems me appropriate.

CM: You decide the parts do not bear on each other.

AR: Not really. I rather interpret the second part as built on the first part but having a potential to stand on its own.

CM: For example, you could read Eilenberg-Cartan and Grothendieck, and find that their work is no kind of "structuralist" in your sense -- and yet insist that Bill simply did not know this. You can insist that it is "math" and not "philosophy" and so you could even say Bill used it in "math" and yet keep saying he did not know it in his "philosophy". But you only impoverish your view by doing that.

AR: I don't see why I impoverish my view by doing that. You describe here correctly what I'm trying to do but I still think that I'm doing this right. Nobody of people you just mentioned was a systematic philosopher. So they used - directly or indirectly - the philosophical background developed by systematic philosophers in the past (the historical part of your paper only confirms what I say here). Mathematical work of these people is more impressive than their philosophical work. They didn't invest as much efforts into philosophy as they did invest into maths. Not surprisingly their maths went ahead their philosophy. So I see it as my philosophical task to make this philosophy explicit and develop it further in a systematic way. I think, Colin, you confuse two different things here: doing philosophy and doing maths that provides philosophical insights. As a philosopher I see my task in making these insights into philosophy. I also think that this may have a backward effect: new philosophy may provide mathematical insights. But then it is up to mathematicians to develop such insights into mathematics. Philosophy cannot be replaced by philosophical insights just like mathematics cannot be replaced by mathematical insights.

CM: You might want to re-read Hegel's Philosophy of Nature and see whether he believes that the physicists, geologists, botanists, etc that he talks about have "used - directly or indirectly - the philosophical background developed by systematic philosophers in the past." He certainly believes their concepts need systematic development, but this is precisely because they contain what is *new.*

AR: I tend to agree with those of Hegel's critics who say that his analysis of his contemporary science is the weakest point of his philosophy that spoiled the relationships between science and philosophy and made many scientists idiosyncratic to philosophy and particularly to dialectical philosophy. I think that issues concerning science risen by Hegel are extremely important but I also think that he didn't manage to deal with them. Perhaps nobody really managed since then.

Concerning your point that concept waiting for a systematic development already contain everything what is new implicitly - yes, but making this new content explicit is anything but automatic and it requires more than an accurate interpretation. I think that an implicit content can develop explicitly in many different directions. Darwin might have a better insight about this than Hegel.

Colin, you confuse two different things here: doing philosophy and doing maths that provides philosophical insights. As a philosopher I see my task in making these insights into philosophy.

CM: That is a very short refutation of a very large body of work. Still, as long as I know that, when you tell me I confuse doing philosophy with doing math, you only mean I am as naive as Hegel was, then I can live with that.

But you do not want to make CCAF into philosophy. Rather you want to improve CCAF to incorporate philosophical insights that you think are not yet in it.

AR: First of all I want to develop a philosophy of maths that would be more adequate to today's mathematical practice than what you call "philosophical structuralism". I think of this new philosophy as an alternative to Structuralism, not as an improved version of Structuralism. Indeed I don't want to make CCAF in the form of Bill's 1966 or similar "into philosophy" but I want to use it for developing the new philosophy. More specifically, I want to distinguish between an older part of CCAF (remind the space shuttle) from a newer part, and then use only the newer one for the new philosophy. I think that CCAF as a whole doesn't fit the job because it brings much of older philosophy with it - I mean here the Hilbert-Tarski style axiomatic method. I don't see that interpreting this method in general philosophical terms in one way rather than another resolves the problem. Even if such a halving of CCAF in some aspects may look artificial it may help to make a conceptual brake with Structuralism; the historical continuity can be easily restored afterwords.

CM: Indeed it may help you even if it is entirely false to the text you are discussing.

AR: To improve on CCAF is a further task for which I perhaps is not quite qualified - but I shall try anyway.

CM: By all means try. But, in the meantime, to declare CCAF "inadequate" on the grounds that
it does not meet your high standards -- which in fact no existing foundation can meet according to you, including any foundation you have conceived, is wearisome and unoriginal.

CM: You make a very good point that Hilbert (and in fact also Bourbaki) tended to think of "isomorphism" as a relation between structures: two groups might be "isomorphic." And on the other hand category theory makes "isomorphism" a kind of transformation: a given homomorphism may be an isomorphism. This shift in perspective is already apparent in Emmy Noether's algebra, where she constantly relies on specific constructions of homomorphisms to prove theorems -- and often the key step is to show that some homomorphism is in fact iso. The "transform" perspective was prominent much earlier in topology: already Riemann is interested in mappings between spaces and takes "homeomorphism" as a derivative notion based on continuous (or holomorphic) transform (but the details are complicated since 19th century geometers often took the "birational" viewpoint so we must look at meromorphic maps and birational equivalence.)

Noether may not have been explicitly aware that she was doing anything new. She tended to read her ideas back onto whoever she was reading, most famously Dedekind. But Eilenberg and MacLane certainly did know they were doing something new.

AR: Right. However in my understanding the new "categorical" thinking is related to taking non-reversible morphisms on equal footing with reversible ones, not to a mere thinking of isos as transformations rather than relations. By "equal footing" I mean that transformations are taking for primitives; then one shoud, of course, distinguish between reversible and non-reversible transformation, which are obviously not the same. Structuralism gives invariance and reversible transformations a distinguished status.

CM: Well "reversible" is a distinction! Of course we distinguish isomorphisms from non-isomorphisms!

AR: Yes, obviously. But the distiction I'm talking about is an epistemological, not merely a mathematical or merely conceptual distinction. Structural mathematics uses isomorphisms for constituting its basic notions of structure and invariant and then takes structures and invariants to be mathematical objects, i.e. takes them to be its subject-matter. My claim is that this way of building mathematical objects is not appropriate for Category-theoretic mathematics (just like it is not appropriate for Euclid's mathematics).

CM: The very logic of Noether's procedure requires working with non-iso transformations equally with the iso. The reason she can draw conclusions (say, in number theory) from a proof that *some given* transformation *is* iso, is that she starts working with all morphisms and not only the isos. And the reason she began doing this was that she saw (following ideas of Dedekind) she needed to use all morphisms to solve problems.

Certainly Eilenberg and MacLane felt that their crucial step was to take *all* morphisms (in a given category) seriously -- and their first great theorem was to show that a certain class of morphisms (induced morphisms between certain colimits of cohomology groups) are in fact isomorphisms.

Grothendieck had decided by 1954, in his work on functional analysis, that the "natural" way to look at any class of topological vectors spaces (or any mathematical objects) was to look at the category of all morphisms between them. Often a key fact will be that certain morphisms are iso (e.g. the comparison map between two different kinds of tensor product of two fixed topological vector spaces). And he quickly followed Cartan and Eillenberg to say that isomorphism itself is often better expressed by saying some sequence is exact. In short AG decided that "zero-morphisms" are more central to important math than "isomorphisms."

In short you are quite right about what is new in categorical thinking, but you are wrong if you think Eilenberg or MacLane or Grothendieck or Lawvere were not perfectly aware of this from the start of their work using these tools.

I hope you understand that Eilenberg and Mac Lane were already quite conscious of this difference in 1945 when they had only very special examples of universal objects (limits and colimits). Grothendieck based huge amounts of his work on this idea, in the form of "representable functors" which he constantly used to define *all* the morphisms to (or, from) any given representing object. Bill was all the more conscious of it as he defined natural number objects in terms of *all* their morphisms to pointed endomorphisms, and cartesian closedness in terms of *all* morphisms to an exponential B^A.

AR: I don't say they didn't know about it. I say they didn't sufficiently develop philosophical consequences of what they new about new tools and instead relied on older philosophical views. Their mathematics went ahead their philosophy. In fact I do not make historical claims in my paper at all; I don't try to guess or to find out what these people knew and what they didn't new. I simply read their papers and observe conceptual tensions between their philosophical claims and their mathematics.

CM: What amazes me is how you even see that the mathematical side of CCAF relies on this new idea, and yet you insist that Bill somehow didn't know this when he wrote the introductory comments.

AR: I don't think that Bill in 1966 was an exception from what I've just said. Bill's maths went ahead his philosophical slogans (but not completely ahead because of ETC). It seems that later Bill also abandoned his Structuralism of 1960ies. In particular, his recent introduction to his thesis, if I remember it correctly, doesn't any longer involve his earlier structuralist views.

CM: MacLane and Lawvere do sometimes speak of "isomorphism invariance" as crucial. And you are right that this understates the facts. But they knew it was just a handy summary. By 1963 they both more often spoke of "universal properties." Grothendieck all along spoke of representable functors, which amounts to the same thing as universal properties. This idea is explicitly about all morphisms and not only isomorphisms.

When MacLane talked about "structures" in the 1930s and 1940s he seems to have usually meant it Oystein Ore's way: the "structure" of a group is its lattice of subgroups. It was not a Tarski signature or a Bourbaki echelle. It was a lattice of subobjects.

The key point is his 1996 comment to philosophers: "‘there can be quite different views of structure—as something arising in set theory and then formulated in Bourbaki's typical structures, or as something located in some ethereal category".

For him in this paper, then, a "structure" simply means any object in any category.

AR: OK, but in this case his notion of structure has really nothing to do with Structuralism! But I cannot see that MacLane made this distinction clearly. I didn't see in MacLane a clear notion of mathematical object as an object of a category - as clearly distinguished from the notion of mathematical object as a thing determined up to isomorphism.

CM: Indeed Saunders (as well as myself) fails to use your conception of structuralism, although we both talk about it under the names of Bourbaki or Hellman et al. Saunders's notion has a great deal to do with the generally "structural" character often noted in modern mathematics.

AR: I'm talking now NOT about MacLane's mathematics but about his explicit philosophy. This remark that you quote is an evidence that Saunders thought about mathematical structures other than Bourbaki structures but it is not an evidence that he had any alternative well-defined notion of structure. In Bourbaki we find a precise notion of structure - however inadequate it may be in certain aspects. "Something located in some ethereal category" doesn't qualify as an alternative notion.

CM: MacLane did not consider categories "ethereal" himself so far as I know, but he thought they must look ethereal to philosophers because a category is not (necessarily, or in general) "made of" anything set theoretically. Rather the objects in it "are related" by the arrows. And that certainly means all the arrows and not only the isomorphisms. The word "ethereal" here is just a stylistic effect that you need not worry about. A structure is an object of a category. This is an alternative well-defined notion of structure.

AR: As far as we take CCAF as a foundation (sorry for the pleonasm) than an object of a category is, generally, a category, right?

CM: Heavens no. Formally, an object of a category A in CCAF is a functor 1-->A from the terminal category to A. Conceptually an object of A is a group or a topological space or whatever A is a category of.

AR: That's an important point. A functor 1-->A *is* a category (with two objects). A group is a category (with one object) and so is a topological space (a site in Grothendieck's sense that covers the classical case too). Generally in CCAF any object of any category is a category. In my understanding this is crucial. Let me repeat the analogy with sets I used in the paper. (See “Categories without Structures”.) Talking about sets of points, numbers and whatnot one doesn't yet take sets as foundations. When sets are foundations every set is a set of sets (except the empty set that is not a set *of* anything). Similarly categorical foundations don't require - and even don't allow for - any other categories but categories of categories. If this doesn't work throughout this means that categorical foundations are not yet available. In CCAF there is indeed a problem about this noticed by Mayberry - but we have agreed to leave it for the moment.

Why then to call an object of a category also a "structure"? This cannot produce anything but confusion. Why MacLane call it structure but not a category? - I think, because he does NOT think about category theory as a foundation. Since I do want to think about category theory as a foundation I drop the redundant and confusing word "structure" (and suggest you to follow me in this) and call categories by their own name. "An object of a category" is is not an alternative well defined notion of structure - it is a definition of category (provided, of course, this self-referential description is interpreted correctly).

CM: It does produce a good bit of confusion. But 20th century math in general has produced very little more than confusion among philosophers of math. Logic is the only part of that math where any significant number of philosophers have tried to sort out the confusion -- and a great deal of confusion remains even on logic.

Why would someone say the Euclidean plane is an x-y coordinate plane? That idea produced confusion for some time. And if your goal is to understand Euclid (supposing there was a person Euclid) or even Descartes then you should not think of the plane this way. But if your goal is to understand plane geometry then you probably should define the plane that way even though it is a relatively recent idea. Mathematics advances by new explications, which generally confuse a lot of people at first.

AR: This brings us back to historiographical issues. I think that mathematics and science in general should incorporate a historical dimension and not to oppose any longer "understanding of Euclid" and "understanding plane geometry". The former is a part of the latter. Understanding Euclid (I mean here an "authentic" understanding) is a part of our understanding of plane geometry. And understanding it as a xy coordinate plane is another - and historically more recent - part of this understanding. What is essential in our understanding of plane geometry over and above the history of this subject is not its alleged a-historical Platonic aspect but the future of this subject. The presence of plane geometry today is an on-going transformation of the history of the subject (i.e., of all the knowledge about the subject acquired earlier) into the future of this subject.

AR: I asked Bill some time ago what he would call a structure. He told he would use this word to refer (not just to an object of a category but) to a functor from a small category to sets. This seems to me a better suggestion than Saunders'. Since Bill's definition of structure is more specific it doesn't allow one to say that a mathematical object is, generally, a structure.

CM: Yes, Bill would like to reserve the word "structure" for essentially what model theorists and Bourbaki mean, except getting it right -- so it will be such a functor. He uses this throughout his dissertation. I take that seriously.

AR: I am sympathetic to such a restriction of the meaning of "structure" because otherwise it may mean too much.

CM: The problem is that then a topological space or a Riemannian manifold is not a structure, and that seems to me excessive.

AR: However topological space and Riemannian manifolds are structures in Bourbaki's sense. So what you mean is that there is a technical problem here, namely, that Bill's definition of structure in fact does NOT cover all Bourbaki's structures, right? Can you elaborate?

If I say here that a topological space *is* Bourbaki's structure and earlier I've said that a topological space is a category and NOT a structure this is not a contradiction since I'm talking about two different version of the notion of topological spaces, which are built on different foundations.

CM: Actually it is wrong to say "Given a type of structures there is a standard way to define a general notion of map between structures of the given type." It is true for algebraic structures (structures defined just by operators and equations). But even for the category of models of a given first order theory T there are several important notions, depending on whether you want a map to preserve all the primitive relations or all the definable relations -- or you want it to preserve and reflect all primitive relations or all defined relations. It only gets more complicated in geometry (including topology).

AR: I agree with your point. But my intended notion of structure here is Bourbaki's while you counter-example is not Bourbakist. Do you think that for Bourbaki's structures (in the sense of their formal definition from the 4th chapter of "Set theory") my claim is true?

Actually your counter-example confirms what I'm trying to say: people tend to think of functors as morphisms (I mean - as structure-preserving maps) but this may be a *wrong* thinking.

CM: You define "structural mathematics" as being typified by Bourbaki's theory of structures, which was not published until 1957 and never did work. So it is absurd to say that category theory either emerged or was developed "in the context of structural mathematics." The reverse is far more true, as noted by Cartier, and Corry, and myself, and Kroemer: Bourbaki produced their 1957 theory of structures as a reaction to category theory. The 1939 sketch is not a theory.

AR: I see your point - I should less stress on Bourbaki as what typifies structural mathematics and more on Hilbert and other earlier authors. An excellent philosophical account of structural thinking in maths and physics is found in Cassirer's early works. In this wider sense the claim that category theory has been first developed in the context of structural mathematics seems to me right. Bourbaki's stance was indeed quite conservative in this sense. My claim about Bourbaki is that their systematic attempts to build new foundations of mathematics couln't digest CT without a radical change of the structu ralist philosophical background behind this project.

CM: I am sure most philosophers of math will agree with you that "The growing popularity of Category theory ... ... as well as the continuing efforts of building categorical foundations of mathematics are ... a further step of the structuralist project briefly described above." This is because they know recent philosophy of math very well, and history of 20th century math not at all. So they believe, as you do, that mathematicians up to now have been struggling to figure out Hellman-Shapiro structuralism. It is just not true.

AR: This seems me to be a plain misunderstanding, I certainly don't believe - and don't say that I believe - what you say I do believe.

CM: Andrei this is amazing! You actually say "Remarkably, Category theory did never make it into Bourbaki's Elements [3], which is the most systematic attempt to develop structural mathematics ever undertaken."

This demonstrates that Bourbaki as well as Eilenberg and MacLane knew very well that "categorical foundations of mathematics are not and cannot be anything like the structural foundations developed by Bourbaki" just as Cartier, Chevalley and Grothendieck argued at the time -- and as Cartier, Corry, myself, and Kroemer have shown long ago.

AR: Right, but what do you find so amazing? I suggest in my paper exactly what you say here explicitly. What made you to think that my view is different from yours? Perhaps I should change my wording not to be misunderstood by others.

I didn't study enough history to criticise the historical aspect of this story as it is described in your paper, so I take it for granted. But I am still not convinced by interpretation of these events given by MacLane in 1986 and further developed by you in your paper. MacLane says: "Categorical ideas might well have fitted in with the general program of Nicolas Bourbaki". Which program MacLane refers here to? If this is "Architecture of Mathematic" then MacLane is wrong for reasons I explained in the paper. Even if a project of categorical foundations coming with a variety of structuralism as a philosophical background is feasible there may be other ways of building of categorical foundation involving a different philosophical background. You convinced me by what you say about a somewhat conservative nature of Bourbaki's project (such as we know it). But explaining this merely by Weil's authority doesn't seem me correct even if your historical account of events is 100% correct. I think that categorical foundations would require a new philosophy - rather than a new variety of structuralism - that younger members couldn't provide. You are right pointing me that this new philosophy is implicit in the mathematical content of Bourbaki's volumes, so there is a tension between this content and Bourbaki's structuralism. However there is a huge difference between having this new philosophy implicitly and making its explicit. Hence the tension.

CM: MacLane means the project of a comprehensive grounding for all the branches of mathematics in the Elements de Maths. It is fashionable now to wave the Elements away and say they were a failure but in fact they had a huge impact (I am happy to say that I learned the commutative algebra of tensors and flatness from Bourbaki). They so thoroughly shaped the current conception of math that most people think they did nothing at all! This is like the Goths who took life in the Roman Empire for granted, and thought Rome itself was a minor annoyance that had nothing to do with it.

AR: Yes, quite like Goths! I'm certainly a fan of Elements - from Euclid to Arnauld to Bourbaki. What particularly fascinates me about Elements is how it is possible that the pattern survives when the content changes that much. This transformation of mathematical Elements through history is anything but structure-preserving!

CM: I believe it is true to say that Weil's opposition was decisive for Bourbaki as an organization. But in fact Bourbaki did commission first drafts of categorical approaches to mathematics, and these proved infeasible. It is not just that they needed a more philosophical development. Even on a purely technical level, a categorical approach to algebraic topology alone would need a vast preparation, and for commutative algebra -- well you can look at Grothendieck's EGA, and notice how radically incomplete that is. And Bourbaki's methodological perspective would combine these and other subjects into a general cohomology.... Indeed algebraic topology plus Cartan-Eilenberg grew quite directly into the general topos theory of SGA 4.

So, yes, indeed, a "huge difference." In math and in philosophy. And this is the difference that Bill set out to make as an undergrad in 1959 in Indiana! I am sure that at every step of the way he believed the task was enormous. And still at every step he underestimated it. And you are right it is not done yet. Not at all. Either in math or in philosophy.

AR: OK. But one thing is to explain events in terms of immediate historical causality and another thing is to analyse conceptual possibilities. By the latter I mean, in particular, the following question: Whether or not Bourbaki can be easily modified (rather than completely remade) to the effect of including category theory in a reasonable way? One may ask and answer such a question just having volumes of Bourbaki at hand and forgetting about controversies between members of Bourbaki group. I suggest the answer in negative. If I'm right this is also a reason why category theory didn't make it into Bourbaki: they couldn't make an easy compromise between the way they had taken and a category-theoretic way (moreover given the technical obstacles, which you mention below).

Do you also agree with my further claim that recognizing all morphisms implies giving up Structuralism altogether? If not, why?

CM: Well Bourbaki more or less recognize all morphisms in 1957. And I would call their approach structuralist. But they also "give it up altogether" in their own practice. They do not use their 1957 theory of structures.

AR: In 1957 Bourbaki distinguish between structures and types of structures. Agree that their approach is structuralist. But disagree that they "recognize all morphisms" treating them on equal footing in foundations. They prefer isomorphisms in the sense that for them basic mathematical objects are structures and structures are defined up to isomorphism. General morphisms are recognised in *types* of structures. But types of structures in Bourbaki are not objects in the sense in which structures are objects. This makes the whole difference. The notion of category makes a type of structure into an object. But this is nothing but a historical link since not all categories are categories of structures in Bourbaki's sense.

CM: No, an order structure on a set in Bourbaki 1957 is a specific order on a specific set, and they admit the general idea of order-preserving function between order relations on sets -- and similarly for all the more elaborate "structures."

If you wanted to write a thorough, clear exposition of Bourbaki's 1957 theory of structures that would be a valuable service. I can tell you I do not mean to do it.

CM: I think your summary is all too accurate: "I hope to have convinced the reader that the project of categorical foundations requires a new philosophical view on mathematics, which the traditional Structuralism cannot possibly provide." So in fact you agree with Bill from 1959 on, and with me and Steve Awodey. But you make it look as if you disagree. Then you "to summarize this new categorical view" without ever talking about your project to find new ideas for foundations.

AR: I certainly agree with all of you on this general point. But there are important disagreements about what to do next. Steve tries to invent some new Structuralism that would do better than the traditional Structuralism. His new Structuralism falls under the Structuralism tout court as I describe it in my paper (too much stressing on Bourbaki, as I can now see). I tried to show in this paper that this is a wrong strategy and suggest - or at least to hint to - a better one. Bill's 1966 was my major inspiration in my attempt to describe this better strategy. However it is essential for my project to separate in Bill's 1966 what I want to push futher forward from what I want to leave behind. Hence my critical attitude to this paper .
I would like to understand better your stance vis-à-vis Structuralism, Colin. I read what you wrote about this but so far didn't quite understood your position.

CM: Steve Awodey's difference from traditional structuralism is in foundations -- where the traditional had a foundation in membership-based set theory, Steve wants no foundation but a framework of variable algebraized set theory with classes. But yes I do find his view closer to traditional structuralism than CCAF is. I prefer CCAF.

As to you pushing further on from CCAF, I am waiting to see what you can do. You believe that something like sketches will be part of it. Okay, I have no interest in arguing that you *can't* do it. But you also say quite correctly that existing sketch theory is not foundational. I'll wait to see if you find some use of them that is.

AR: I realize that the project I'm taking about is waiting to be accomplished. However I'm not sure (neither I'm sure to the contrary) that I should invest my effort into this. There are examples of mathematical works made by philosophers but they are not very convincing. Think of Whitehead's book on universal algebra, for example. However interesting it may be van der Warden's book on algebra or Kurosh' book on universal algebra are more important for mathematics. What Whitehead achieved using English prose seems to me by far more important. So perhaps I should more invest into developing a philosophical view using ideas from category theory among other things - rather than try to built workable foundations of mathematics in the way you (and I myself in my paper) suggest. Maybe a good idea is to find a mathematical collaborator and convince him or her with my philosophy and make a joint work. I'll see what I can do about this in the future.

CM: You ask about evidence of what Bill <Lawvere> meant by "structural" in 1966 and I would say two things: First, you should suspect he means something coherent with his project in CCAF, but you suspect he did not. Second, and this one is really fun, you should look at the examples he knew and worked with. Have you read Grothendieck's Tohoku paper? Or Godement's book on sheaves, or the Eilenberg-Steenrod axioms for homology, or the Cartan-Eilenberg axioms for cohomology? Those are the things all the category theorists were talking about at Columbia when Bill arrived. And those are the things that Bill built on in his work, together with Grothendieck's SGA 4 once he heard of it. He did not build on Bourbaki.

AR: In Eilenberg-Steenrod I found what I see as a textual evidence confirming my claim concerning the role of irreversible morphisms:

"... homology theory parallels analytic geometry. However unlike analytic geometry it is not reversible. The derived algebraic system represents only an aspect of the given topological system, and is usually much simpler." (in the Preface).

In fact the correspondence between points and numbers in analytic geometry is not - or at least can be seen as not - reversible either. But this is a change of perspective rather than a bold mathematical fact that actually matters here.

So I agree with you that Eilenberg-Steenrod and Cartan-Eilenberg provide a background that is quite unlike Bourbaki background. But if I understand you right (otherwise correct) you describe this non-Bourbaki background as a specific form of Structuralism while I describe it as what is not Structuralism at all. By the way unlike Bill <Lawvere> Eilenberg and Steenrod don't make any structuralist slogan. Perhaps this is because they are less interested in philosophy than Bill, or because they think that philosophical slogans are not appropriate in mathematical papers.

CM: It is certainly not what you call "structuralism," and that is okay with me. Whatever you call it, it was Lawvere's conception of "structure" and of "structural mathematics" by 1963 when he wrote the first ETCS. By then Bill had been studying with Eilenberg and Eilenberg's students for 3 years. At that time they all took Eilenberg-Steenrod as a paradigm of topology, and based their own work on more sophisticated sources descended from it (Cartan-Eilenberg, Grothendieck, Godement).

Did Bill use the word "structuralism" in 1966? I do not recall him using it. Whatever words he used, he saw CCAF as a foundation expressing this concept of structure.

AR: Anyway I don't see why some specific features of one's Structuralism can be crucial. If Bill's Structuralism of 1966 doesn't fall under what I call Structuralism in my paper, then I'm wrong.

CM: Whether it is crucial for you to talk about Bill's structuralism, is up to you to decide. If you are going to talk about it, though, then it matters to get it right.

AR: It goes without saying that I do want to know more about what you call Bill's structuralism - whether it comes from you, or from Bill or from any other source. But as I have already explained I have a strong reason (which is at least partly methodological) NOT to take Bill's philosophical position in his 1966 as coherent. To repeat: the paper itself contains philosophical slogans and hints but does not contain any systematic philosophical account. It is open to philosophical interpretations just like any other mathematical paper. I cannot see any reason why interpreting Bill's postion in this paper as philosophically incoherent I'm doing wrong. Saying this I certainly don't mean to diminish the significance of Bill's work, moreover that this work is a source of my own inspiration. Moreover so since this incoherency reflects Bill's dialectical thinking behind his mathematical thinking and also reflects the great progress Bill made in this paper.

I also realise that what I call Bill's incoherent philosophical position is my reconstruction of this postion but I don't believe that a search for one's "genuine" position makes any sense in cases like this one. (Did you read Gadamer's Wahrheit und Methode? Please beware that I do NOT take Gadamer's philosophy to be a license for producing absurd interpretations of history, of mathematics or of anything else.)

I would be very interested to hear from Bill what he thinks about this issue today but I'm not prepared to take his word as decisive just because it comes from Bill. Creations live their own lives that their creators cannot possibly control. I would be equally interested to hear more from you about what you think Bill had in mind writing this paper. Even if this historical issue seems me less important than what comes next.

The title of your section 3 is "varieties of structuralism": Bourbaki-style, Categorical and Philosophical. (See Colin McLarty "The Last Mathematician from Hilbert's Gottingen: Saunders Mac Lane as Philosopher of Mathematics", The British Journal for the Philosophy of Science, 2007 58(1):77-112.) As this title suggests you are talking here about three species of the same genus. But you don't say what this genus except giving its name. Moreover this three-partition seems me odd because of the presence of the "philosophical" structuralisms. Comparing Bourbaki's structuralism with MacLane's Categorical structuralism seems me alright but putting philosophical structuralisms on equal footing seems me wrong. If philosophical Structuralism can be compared with the Bourbaki-style and with the Categorical structuralism the basis of such a comparison should be not the same as in the first case. In this first case we have different projects of building working foundations of mathematics; in the latter case we have writings of people trying to interpret ready-made mathematics in philosophical terms and tell us what mathematical objects *are* and the like. It seems we both agree that this latter enterprise as far as we know it is not quite methodologically sound (although our suggestions as to how to improve on it may be still different). But this seems to me to be a separate issue.

My question to this part of your paper is this: what do you mean by structuralism in general over and above its three varieties? What the three varieties really share except the common rhetoric?

CM: They are all conceptions of mathematics based on an idea of "structure" (rather than specifically on sets, or on intuition, or on logic, for example.) Bourbaki defined "structure" in a way that never worked, and categorists really have not tried to give any precise meaning to the term because it does not seem important. Recall MacLane's remark that he must have meant something by "structure" in the 1930's because he published a note announcing some result about "structures" -- but by 1996 he could not recall the result or what he meant by structure at the time. (I expect he meant Ore's structures.) When philosophers of math do try to define the term, they give some weaker version of Bourbaki.

AR: Right, but what is *your* general idea of "structure"? - in abstraction from its more specific "varieties"? In other words: What these varieties share in common except the term "structure"? What do you mean by structuralism when you talk about its different varieties?

CM: If we can say that twentieth century math had a general, growing "structuralist" character, then by far the greatest part of that was categorical and not Bourbakiste. On reflection, I think this is decisive. We want to say there is a general "structuralist" tendency that goes back to, say Riemann on Riemann surfaces. But holomorphic functions on Riemann surfaces are certainly not "structure preserving" in Shapiro's sense -- a holomorphic map is a function that reflects complex-valued meromorphic maps on the codomain.

AR: The fact that category theory allows for a (obviously anachronistic but) coherent reading of Riemann and of other older maths points to origins of category theory in this older maths; in other words, it shows that category theory and, more specifically, the idea of categorical foundation reflects an earlier mathematical practice. However I cannot take for granted the claim that " twentieth century math had general, growing "structuralist" character" - I know that this is often repeated but I don't know what people mean by saying this. We agree that if we take the "structuralist character" in Shapiro's sense the claim is wrong. But what is a better sense? Corry's book doesn't provide an answer even if it does provide a lot of very interesting material. I suspect that the claim is in fact wrong - in the sense that there is no reasonable general notion of structure that makes it true. At least if we are talking about the second half of the 20th century. (Note: See L. Corry. Modern Algebra and the Rise of Mathematical Structures, (Science Networks Vol. 17). Birkhauser, 2004)

CM: There is much better evidence than that. You can look at the original work in category theory and see they refer to predecessors, who refer to predecessors in turn. You can see which problems they set out to solve, and which earlier tools they took as useful, and see that category theory arose this way. It is better to study the history of a subject by looking at the work done in that subject in the past, than by seeing how you can use the subject today.

Just to be sure I understand you, though, do you consider it anachronistic to say that Riemann defined his surfaces in complex analysis by saying each small enough piece of such a surface is isomorphic to some piece of the complex plane? Or that he emphasized mapping relations between his surfaces? Or that his work was a major impetus to topology, through Poincaré and Weyl?

I am often accused of anachronism by people who have not read the original sources, but who feel it is obvious what the sources must have said.

AR: I don't *accuse* you of anachronism, Colin. In my understanding anachronistic doesn't imply wrong. Perhaps I had to say "modernizing" instead of "anachronistic". Generally, I think that hermeneutical issues are very pertinent but poorly understood in the history of maths. Did you come across the discussion between people calling themselves "antiquarists" and "presentists"? The issue was whether or not it is justified to use modern maths for interpreting older maths. This whole discussion stroke me when I read it as rather naive. Obviously the extreme antiquarism is absurd but it is also evident that one can use modern tools differently (and use different such tools) dependently on a given purpose. Your reading of Riemann with category theory is OK for me; calling such a reading anachronistic I only meant that the purpose of such a reading is to trace category theory back to its historical origins rather than try to understand Riemann in its authentic form (see reservations about the authentic form below). And the success of such a reading - provided, of course, that there is no cheating in it - indeed clarifies important historical facts. I didn't mean to say that there is any cheating in your reading of Riemann.

CM: Well, to read Riemann as basing his definition of his surfaces, and his major theorems about surfaces, on mappings is simply to read his Dissertation verbatim. It is not anachronistic. Only he does not say the mappings collectively form a category.

AR: At the same time, I think that a reading of Riemann with an intent to reconstruct his thinking in terms of mathematical notions of his own time is equally justified and equally important. As far as the historical authenticity is not understood naively a pursuit of such authenticity is OK. It serves a different purpose and requires a different qualification and different techniques than a modernizing or anachronistic reading.

When you say that category theory is implicitly in Riemann it sounds plausible for me - even if I don't check details. But once again it seems me that a source of our misunderstanding here is that I take the difference between implicit and explicit more seriously than you do.

CM: Yes, but I am tired of those who try to to do this in the firm belief that all mathematics before 1945 was somewhere between a current first year calculus course and set theory. There is a whole world of Dedekind’s work on algebra and number theory, which Emmy Noether quite rightly says was not truly appreciated "until recently" (she should have said, until her) and that stuff still awaits insightful explication. Emmy was not a patient expositor. This work by Dedekind was a major part of the structural trend in actual mainstream math. The arguments in Zahlen are adorable. I think they are quite correct. But they are not what most affected Weber and then Hilbert and then Noether .....

AR: I cannot take for granted the claim that " twentieth century math had general, growing "structuralist" character" - I know that this is often repeated but I don't know what people mean by saying this.

CM: The alternative to taking this for granted is to evaluate it yourself by learning a great deal of the math. Really this needs to be 19th and 20th century. It is a lot of fun but then a lot of philosophers are concerned that mathematical facts cannot bear on philosophical questions.

AR: I think that mathematical facts do bear on philosophical questions and I do appreciate your advise to learn more maths but I don't think that only learning maths can allow me, you or anybody else to justify or to refute the claim that "20th century maths is of growing structuralist character" (I hope I quote correctly). This is for the simple reason that "structuralist character" may mean anything; it needs a clarification and a precise definition - philosophical definition rather than mathematical.

CM: What you mean by a "general notion" of Structuralism is a formal definition.   Corry indeed reviews several such definitions arising in the past 100+ years of math and concludes that none was adequate.

AR: Yes, but he doesn't suggest any better replacement. I don't think of Structuralism as a formal definition - philosophical notions are not developed only through formal definitions even if such definitions can play some role in it.

CM: What I mean by the generally structuralist trend is not a formal definition, but an informal character. If you see no such character shown in Corry's book then there is no hope of me showing it to you in an e-mail.

AR: I don't think that an informal character can replace a genuine philosophical notion. Not for me in any event. And here is a reason why. If you say you see the character and I say I don't see such a character this cannot bring us (or anybody else) anywhere. But genuine philosophical notions allow for productive dialectics.

For me a philosophical analysis of mathematics amounts to making explicit what mathematicians discuss informally - rather than identifying a character. And I don't think about such making explicit as an innocent procedure.   "Informally" in this context is not the contrary to "formally" but rather the contrary to “systematically”. However suggestive and valuable can be remarks on general issues made by mathematicians in private talks and elsewhere they cannot replace a systematic philosophy.

CM: Anyway this "structuralist character" is what produced category theory, which worked, as well as Bourbaki's theory of structures which failed.

AR: Agree (with reservations about the fall of Bourbaki, which you, I guess, would share). But there is difference between being produced by something and being an instance of something. I agree that the "structuralist character" (whatever it can be) produced category theory but I don't think that because of this fact category theory is doomed to be structuralist. And this is exactly the kind of development I tried to trace in Bill's CCAF paper: he begins with a structuralist setting but then the dialectic of his thought brings him elsewhere.

We should invent other philosophy than Structuralism rather than yet another variety of Structuralism. When I say "invent" I assume, of course, that good philosophy of maths both reflects an existing mathematical practice and leads this practice into a new direction. In other words it must reflect tendencies rather than established patterns of practice.

CM: <I>f you think you can show that category theory not only *could* be in principle, but actually *is* in fact radically discontinuous with earlier math then go ahead and do that.

What you have argued so far, is that category theory is radically discontinuous with Bourbaki's or Tarski's notion of structure. But much of 20th century mathematics and already a lot of 19th century abstract math is quite different from Bourbaki's or Tarski's notions of structure.

AR: I am not a historical relativist but I think that the history of category theory or of any other branch of maths can be correctly described both as discrete and continuous. This depends of a purpose. An important point is this: I think that a renewal of foundations is (at least locally) necessarily discrete (or as you put this "radically discontinuous") process. Different foundations of mathematics are (pleonastically) radically different - otherwise they are either not foundations or they are not different.

This radical discontinuity certainly doesn't apply to the history of maths in general. As I have already said when you trace category theory back to Riemann I'm convinced by this. But I don't see this as an objection.

Take the notion of Euclidean space in its modern rendering, for example. It is true that this modern notion stems from the notion of geometrical space found in Euclid as a result of long continuous development. (Arguably there is no notion of space in Euclid at all but let's leave it.) This can be demonstrated by pointing to chains of references and by showing that Euclid can be interpreted in modern terms coherently. Such an interpretation can be used for explanation of Euclid's (to be distinguished from Euclidean) geometry. Such an explanation would be obviously anachronistic but not wrong; it can be useful for many purposes.
But it is also true that foundations of Euclid's mathematics and foundations of modern mathematics are radically different. In order to understand foundations of Euclid's mathematics one would need to interpret Euclid "authentically". Of course such an "authentic" interpretation would involve modern auxiliary concepts (and this is the reason why I put "authentic" in reverted commas) but it would be differently directed, so to speak. This kind of work one finds in Heath and other historians; I did it when I wrote my thesis.

CM: I am glad to see you wrestling with things like this. And you know we could refine your comments further ad infinitum.   When we say "Euclid's mathematics" perhaps we mean Theon of Alexandria's math, since we don't have Euclid's text -- and it is not clear whose text we have but perhaps Theon's.

Much more important: Once we decide that mathematicians are likely not to have clear concepts of their own work (as you say of Bill in 1966, and I would say of Newton on calculus for example) then it remains to know whether there ever was a clear conception of "Euclid's mathematics" in the first place.

AR: The example of Euclid well demonstrates that we cannot and don't really need to decide such things about mathematicians in either way. Whether or not Euclid had a clear conception of his maths we cannot possibly know about it. What we can do is to reconstruct such a conception as precisely as we can using available sources. And a verbatim reading of Euclid is obviously not sufficient for it. The ultimate responsibility for this reconstruction is, of course, on the reconstructor.

I don't think that the recent history requires a totally different approach. We cannot enter into somebody else's mind and we don't need it for doing history of science. When I analyse Bill 1966 I say I can distinguish two conceptually different parts there and then say that one of those two parts fits the pattern of works of other contemporary authors while the other part is genuinely original. It's great that Bill unlike Euclid can comment on this but this is not a reason to turn the whole thing into a psychological mode.

CM: What can make this kind of thing interesting is to make it work to produce new understanding. Critique of existing ideas has its place. And thinking about philosophic principles of historiography has its place. But again I urge you to take up Hegel's claim that after you prove something is necessary you must go on to prove it is true. Once you have shown your principles *must* be used, it remains to show they *can* be used.

CM: You really do need to be a lot clearer about what you mean by "structuralism." Bill and Saunders do not use the term. But they talk about "structures" and "structural properties." Those are Bill's terms since the 1960s -- and they correctly reflect how this approach relates to an awful lot of mathematics since the 1930s.

AR: I assume that the basic idea of Structuralism is replacing traditional equalities (I don't mean identities) by isomorphisms. Making categories into foundations (in the sense I'm thinking of) amounts to replacing equalities by general morphisms.

I use the term "structuralism" (even if I understandably hate "isms" because of my Soviet background) only because it is today popular in philosophical discussions about mathematics. I don't expect to find it in Bill's and Saunder's paper. But I nevertheless assume that a pattern of one's reasoning can be reasonably qualified as "structuralist" even if there is no word "structure" in it, let alone "Structuralism".

CM: Yes that is clearly your motivation. And so you take your sense of the word from recent philosophical discussion. Especially since you dislike "isms" it might be better to drop the word.

AR: If I want to refute the current "philosophical structuralism" how can I avoid calling it by name? However I didn't suggest a new ism for myself. Perhaps philosophers will not take me seriously unless I do this? :)

CM: I am not sure what you mean by identities versus equalities? There is no category theory today without equality. And I don't think there ever will be. Amicus <David> Corfield sed magis amica veritas.

AR: The whole strategy of "weakening" equality or identity - which is of "structural character" - cannot go far. A more interesting and challenging task, I think, is to reconstruct and perhaps generalise on these notions. The idea of replacing equalities by morphisms works in higher categories, which become progressively more important in physics and disciplines like knot theory, for example. (Some time ago I went at a conference in Uppsala organised by Oleg Viro - for people gathered at this conference Khovanov homology was apparently the first and for the time being the only evidence that category theory is more than an abstract nonsense for philosophers.) So it seems to me an important development.

Notions of equality and identity belong to foundations of mathematics. In categorical foundations of mathematics those notions should be rendered categorically rather than taken for granted from outside. As Bénabou puts this, they shouldn't be taken as "god-given". "To do without equality" is not really an issue, how to think about equality categorically is an issue.

A question: what happens if instead of thinking that two times two IS four we think that two IS TRANSFORMED into four as a result of this operation? There is a sense in which this transformation is irreversible (given 4 you might not know that it is obtained from 2), so it doesn't "resemble" the usual equality in the way in which an isomorphism does so.

CM: In the context of GR spacetime *no* trajectory can be reversed. Nothing goes backwards in time (unless tachyons, and they do not go *forwards*). Indeed the time-reversal of any possible process in GR is also a possible process -- but this applies just as well to a person falling into a black hole. A black hole could Hawking-radiate a person being ejected from it, though it would be a waste of time waiting to see this happen.

AR: Disagree. A friend and colleague of mine Marc Lachièze-Rey who is a GR expert says that GR just like Newtonian mechanics supports the view according to which all the fundamental physical processes are reversible.

CM: There is an ambiguity about "reversible." If an electron has traveled from some given event A to another B, then it absolutely cannot reverse that trajectory and "travel back" to B. Electrons only travel forward in time.

It is true that for every physically possible process in GR the time-reversal is also physically possible. But this is a comparison between pairs of different solutions of the field equation, and not a physical possibility within in one solution.

And if you mean time-reversal (rather than "reversing" a trajectory already taken), then black holes are *not* an exception. A person could fall into a blackhole and disappear forever -- but then, so could a person be Hawking-radiated out of a blackhole (complete with spacesuit) and go on to a long and happy life.

Of course it is extremely unlikely that any black hole will ever Hawking-radiate a person -- let alone the space suit -- but it is also unlikely that any person will ever actually reach the neighborhood of a black hole big enough to fall into. The latter is vastly more probable today, only because we already know the antecedently improbable fact that people exist.

I think it is extremely unlikely that any human will ever travel so far as, say, Jupiter; but you and your father must know much more about that.

AR: Marc says that black holes are things that GR cannot properly describe, so they are not yet reasonably well described (perhaps nor even well-constituted) physical objects.

CM: Yes, Hawking's work has made it clear that black holes have to be understood in quantum terms, and we have no GR quantum theory. But we have an excellent Special Relativistic quantum theory and so the general expectations for black holes and Hawking radiation are very clear.

AR: Marc's view about the reversibility and GR is standard: you can also find it in Prigogin and later literature on (ir)reversibility in physics.

CM: All of this refers to time-reversal, a comparison between different solutions to the field equation. It does not refer to "undoing" a trajectory that exists in some one solution.

AR: A time-like curve in the space-time of GR represents a physical process developing in its proper time. But this doesn't provide it with any preferred direction; such a choice can be is made on different grounds. In particular "the" global cosmological time (I learned a lot of interesting things from Marc about this classical-like notion in modern cosmology) is said to be directed from Big Bang to our present position in the space time rather than the other way round. In a sense, this is a (helpful) convention.

CM: Yes. And Einstein tried to convince himself on these grounds that the deaths of his friends with old age was not really "happening" -- from the viewpoint of space-time as a whole. But he found it did not help. It is not easy to escape from the helpful convention by which we say Saunders MacLane was alive "in the past" and will not be "in the future."