Email exchange between Colin McLarty and Andrei Rodin over the recent work by Sergei Artemov, on the Scholasticism in Analytic Philosophy, on the distinction between Logic and its philosophical interpretation and on some other related issues.

Background: On August 17, 2024, Eric Schmid and Connor Tomaka with a generous support of Emily Harvey Foundation (NYC) organised a one-day workshop on Logic and Methodology of Science and Its Applications (https://sites.google.com/nyu.edu/logic/about) were Andrei Rodin gave a talk on the Philosophy of Homotopy type Theory, and Colin McLarty responded. In his talk Andrei referred to recent paper by Sergei Artemov https://arxiv.org/abs/2403.12272 on the provability of consistency of Peano Arithmetic by means of this very theory. In his public reply Andrei's talk Colin also commented on Artemov's results. This triggered the following email exchange, which continued several moths.

26.08.2024 Andrei to Colin:
Hi Colin,
thank you indeed for your comments after my talk at the meeting organised by Eric. Attached is the article by Sergei Artemov that I referred to in my talk. Indeed, Sergei does not introduce here a consistency *predicate* but uses a consistency *scheme* instead. Sergei explains and motivates this choice in the very beginning of the paper. Is this the same way to get around G2 that you mentioned in your reply?
best
Andrei

27.08.2024 Colin to Andrei:
Oh no, this is not what I expected. But with all due respect to Sergei Artemov, it is not new logic---just a new philosophical interpretation. And you will not get logicians to adopt this interpretation.

The key point is as Artemov says: "The consistency of PA refers only to numerals (standard natural numbers) as codes of finite PA-derivations. In particular, it is not concerned with the truth values of 'x is not a code of a derivation of (0 = 1)' for nonstandard x's in nonstandard models of PA."

But *provability in PA* is precisely about all x's in all models of PA, not just standard ones. The whole difference between truths of arithmetic, and theorems of PA, is (by the completeness theorem) that theorems are not just true in the standard model but in all models of PA. We all believe it is *true* that PA is consistent. We all believe, for each standard natural number n, PA proves n does not code a derivation of (0=1). And Artemov agrees that the consistency statement is not provable in PA ---which (by the completeness theorem) is the same as saying there are nonstandard interpretations where the consistency statement is false.

So, what would we gain by taking Artemov's advice and accepting the consistency schema as the right formalization in PA of Con(PA)?

Would that make you more confident in the consistency of PA? I hope not. We can go further into that if you like, but I hope you agree that PA proving its own consistency would not be any reason at all for believing it is consistent. Obviously, every *inconsistent* extension of PA *does* prove its own consistency *statement*--and a fortiori its own consistency scheme.

In exactly the same way, the unprovability in PA of the consistency sentence (which explicitly

depends on the assumption that PA is consistent) should in no way make you doubt the actual consistency. Just the opposite--if you believe there is any unprovable sentence at all in PA then you *must* believe PA is actually consistent.

People have known PA proves this consistency schema since the 1950s. The general conclusion (which I think is entirely right) is that this shows the consistency schema is a curious infinitary formality--while unprovability of the consistency statement is a pivotal fact for proof theory.

best, Colin

27.08.2024 Andrei to Colin:

I agree with your analysis but I evaluate Artemov's work differently. Taking the consistency schema to be the "right" formalisation in PA of Con(PA) (instead of the standard formalisation of Con(PA) as a formula), arguably, provides a pattern of formalising mathematical reasoning, which apparently better fits the common informal version of this reasoning. Given this alternative formalisation of Con(PA), the popular informal statement of G2 as "Arithmetic, if consistent, cannot prove its own consistency" turns out to be false. Given that most people outside of the logical community learn about G2 in this way, it is important to stress that this statement is not only imprecise but also conditioned by a dubious (and in no way mathematically approved) interpretation of Con(PA) as a formula. Formalisation of Con(PA) is an important example that shows, more generally, that mathematical theorems like G2 cannot, by themselves, prove or disprove philosophical theses like "a consistent theory cannot prove its own consistency". I used this example in my talk for making just that point.

Of course, this is not a new logic but a new philosophical interpretation, you're quite right. But at least for a *philosophical* logician, in my view, it is important to stay critical about philosophical interpretations rather than dogmatic. The situation when "all" logicians for some reason adopt basically the same philosophical interpretations of known logical calculi is known in the history and called a scholasticism. ("All" means here that these people do not qualify as members of their community those who do not comply.)

27.08.2024 Colin to Andrei:

« I agree with your analysis but I evaluate Artemov's work differently. »

I fear you and I have totally different views of "the" common informal understanding of mathematical reasoning. I do not even believe there is one single articulable common informal understanding of mathematical reasoning. Certainly math educators who specialize in elementary ed believe elementary students can and should be taught an understanding of math that they do not already have -- and that for the most part their parents also do not have. By age 17 or 18 students themselves often report they do not understand their math classes. Looking at a higher level, I expect (based on many, sometimes long, discussions) most research mathematicians have no idea of formalisation in PA at all, nor even of the PA axioms. Specifically they have no idea that an axiom scheme is different from an axiom, much less that a theorem scheme is different from a theorem. They have essentially no idea of consistency in any formal sense.

« Given this alternative formalisation of Con(PA), the popular informal statement of G2 as

"Arithmetic, if consistent, cannot prove its own consistency" turns out to be false. »

Correct. Any statement will turn out to be false given a suitable redefinition of its terms.

« Given that most people outside of the logical community learn about G2 in this way, it is important to stress that this statement is not only imprecise but also conditioned by a dubious (and in no way mathematically approved) interpretation of Con(PA) as a formula ».

Andrei I really urge you to reconsider. The standard treatment of Con(PA) is indeed dubious to you and Artemov. It is simply opaque to most professional mathematicians let alone nonmathematicians. But it is "mathematically approved" in precisely this way: Ever since Godel made his theorems public, the vast majority of logicians who claimed to understand them agreed with them. Russell and Hilbert, as notable examples, initially said they did not understand. Hilbert came to strongly agree with Godel--though he let Bernays do the work of spelling it out further and in more generality.

By 1950 there was a community of dozens of logicians whose main interest was spelling out G2, and indeed they debated (and still to this day study) exactly how best to formalise Con(PA). Today hundreds of specialists have a professional grasp of the issue. Everyone agrees it is important to understand that PA does prove consistency of each of its finitely axiomatized subtheories.

Much more: for any sentence phi of PA (including any conjunction of finitely many PA axioms with overtly inconsistent sentences like "0=1&0/=1") PA plus phi proves consistency of phi.

Nearly all the professionals agree this makes the consistency scheme (as Artemov calls it) a valuable technicality but a poor formalisation of actual consistency. What other kind of "mathematical approval" would you want to see?

I can direct you (using Google) to numerous people who consider the proof of Fermat's Last Theorem dubious. There is a grand tradition (which during one dinner in Montreal around 1980 anyway included Peter Freyd) who consider Quantum Mechanics and General Relativity worse than dubious --- and actually refutable.

Merely considering some claim "dubious" is easy, if you want to do it, and makes a poor argument.

« Formalisation of Con(PA) is an important example that shows, more generally, that mathematical theorems like G2 cannot, by themselves, prove or disprove philosophical theses like "a consistent theory cannot prove its own consistency". I used this example in my talk for making just that point. »

Yes. History proves this. Worse, no kind of argument at all has ever proved any philosophic thesis to everyone's satisfaction. Too many philosophers try to conceal this fact from our university colleagues and especially the university administration and we might as well be open about it. As long as each philosopher is free to redefine the terms in any other philosopher's claim, all philosophical claims will be refutable.

« But at least for a *philosophical* logician, in my view, it is important to stay critical about philosophical interpretations rather than dogmatic. The situation when "all" logicians for some reason adopt basically the same philosophical interpretations of known logical calculi is known in the history and called a scholasticism. ("All" means here that these people do not qualify as members of their community those who do not comply. »

I fear this is the basic difference between us on this issue. For you, the opposite of dogmatism is doubt. For me the opposite of dogmatism is giving articulate reasons for a belief.

And I will not agree that "scholasticism" means accepting the word of experts. I will side with Jamie Tappenden's sig file, a quote of Ramsey:

"The Chief Danger to our philosophy, apart from laziness and woolliness, is scholasticism, the essence of which is treating what is vague as if it were precise and trying to fit it into an exact logical category." --- Frank Ramsey ``Philosophy" in "Last Papers"

Colin

28.08.2024 Andrei to Colin:

« I fear you and I have totally different views of "the" common informal understanding of mathematical reasoning. »

Not really. I have a very similar picture of "the" common informal understanding of mathematical reasoning. (But I may still have difficulties with using the definite article in English.) I believe, the difference between us rather concerns the question: What to do with this picture (from a logical point of view)?.

« I do not even believe there is one single articulable common informal understanding of mathematical reasoning. Certainly math educators who specialize in elementary ed believe elementary students can and should be taught an understanding of math that they do not already have -- and that for the most part their parents also do not have. By age 17 or 18 students themselves often report they do not understand their math classes. Looking at a higher level, I expect (based on many, sometimes long, discussions) most research mathematicians have no idea of formalisation in PA at all, nor even of the PA axioms. Specifically they have no idea that an axiom scheme is different from an axiom, much less that a theorem scheme is different from a theorem. They have essentially no idea of consistency in any formal sense. »

Right. I believe that this common lack of interest to and knowledge of logic among mathematicians is a symptom of the fact that mathematical logic has little if anything to say about mathematics and mathematical reasoning in general. This is because it applies inadequate (mathematical) models of mathematical reasoning. The fact that the mathematical logic also shares a lot of mathematical methods with the rest of mathematics is a wholly different issue. Which model of mathematical is "adequate" is a delicate question, of course, because such a model is supposed to be normative rather than just descriptive. But anyway it should help mathematicians to reason, in particular, to build and verify mathematical proofs. I don't think, that PA can be helpful in the number theory in this way. Perhaps some version of Univalent Foundations will be more effective in this sense (but hardly the current version as far as arithmetic is concerned).

« Andrei I really urge you to reconsider. The standard treatment of Con(PA) is indeed dubious to you and Artemov. It is simply opaque to most professional mathematicians let alone nonmathematicians. But it is "mathematically approved" in precisely this way: Ever since Godel made his theorems public, the vast majority of logicians who claimed to understand them agreed with them. Russell and Hilbert, as notable examples, initially said they did not understand. Hilbert came to strongly agree

with Godel--though he let Bernays do the work of spelling it out further and in more generality. »

It sounds like a historical and sociological argument, that is, an appeal to authority, not like a mathematical argument. Saying that the standard treatment of Con(PA) is not mathematically approved I want to say that it cannot be mathematically approved in principle (but not that a mathematical proof is missing but should be found) because this standard treatment is not purely mathematical but involves a philosophical interpretation of what counts as mathematical proof, what counts as a contradiction, etc.. "Agreeing" with Godel's theorem may mean different things. Neither I nor Sergei challenge the mathematical part of this theorem, which tells us interesting things about syntactic structures. In that respect we all "agree with" Godel's theorems). Sergei and I after him challenge only the popular philosophical interpretation of this theorem (as you quite rightly attested yourself in your first message). Thus one may "agree" with the mathematical argument and disagree with its philosophical interpretation. This is what I'm talking about. By the way, it seems that Godel himself did not agree with the philosophical interpretation of G2 that later became an orthodoxy. But I should read more of Godel for making a historical argument.

« For any sentence phi of PA (including any conjunction of finitely many PA axioms with overtly inconsistent sentences like "0=1&0/=1") PA plus phi proves consistency of phi. Nearly all the professionals agree this makes the consistency scheme (as Artemov calls it) a valuable technicality but a poor formalisation of actual consistency. »

I cannot see how this fact supports your claim that the consistency scheme is a poor formalisation of actual (?) consistency. Of course, PA plus an inconsistent formula proves everything including the consistency of this formula. How this disqualifies the consistency scheme as a formalisation of consistency?

« What other kind of "mathematical approval" would you want to see? »

I do *not* want to see a mathematical approval (that is, a mathematical proof) in this case because I believe that it is impossible to justify a philosophical/epistemological choice mathematically, see above.

« Merely considering some claim "dubious" is easy, if you want to do it, and makes a poor argument. »

I claim that the standard treatment of Con(PA) is dubious because it hinges on a particular philosophical position (or rather a *disposition* because it involves many different things), which is not a subject of mathematical or any other conclusive proof by its very (epistemological) character. Below you seemingly agree with me on this point saying that all philosophical theses are refutable. This is exactly what I'm talking about. Any philosophical thesis is, and doomed to remain, dubious in this sense. This is not the same as saying that GR or the existing proof of Fermat's Last Theorem are dubious. Philosophical theses are dubious - or better to say controversial - by their special character.

Formalisation of Con(PA) is an important example that shows, more generally, that mathematical theorems like G2 cannot, by themselves, prove or disprove philosophical theses like "a consistent theory cannot prove its own consistency". I used this example in my talk for making just that point.

« I fear this is the basic difference between us on this issue. For you, the opposite of dogmatism is

doubt. For me the opposite of dogmatism is giving articulate reasons for a belief. »

I'm talking about dogmatism in philosophy, not in mathematics and science. The opposite of philosophical dogmatism for me is philosophical criticism rather than doubt.

« And I will not agree that "scholasticism" means accepting the word of experts. I will side with Jamie Tappenden's sig file, a quote of Ramsey: "The Chief Danger to our philosophy, apart from laziness and woolliness, is scholasticism, the essence of which is treating what is vague as if it were precise and trying to fit it into an exact logical category." --- Frank Ramsey ``Philosophy" in "Last Papers" »

Thanks for the quote, I didn't know it! This is exactly what I also mean by the scholasticism. Me too, I side with this quote without reservations. I believe that quite a few of "experts" in the philosophical logic make the mistake pointed to by Ramsey. In particular, they make this mistake when they treat and explain G2 in the usual way, which involves quite a bit of a dogmatic philosophical interpretation of this mathematical fact.

Andrei

03.11.2024 Colin to Andrei:
Andrei,

It took quite a while for me to respond to this. I had written to you about several decades of thought on G2, and you replied:

« It sounds like a historical and sociological argument, that is, an appeal to authority, not like a mathematical argument. »

Seriously? You want to tell me that the history of a school of mathematics is history? or that it is about people? Those are not news to me. As to "argument by authority," I assume you have read some number of those logician's works, and I am appealing not just to the authority of those logicians but to the arguments in those works. Of course if Tarski, Robinson, Feferman, Friedman are just authorities to you and you do not know their arguments, then you could only read what I wrote as an appeal to authority---but that is not my fault.

Maybe I can understand you better if you can tell me this: Can you give me an argument, that you consider "mathematical," and not merely historical or sociological, for the claim that Euclid's *Elements* give a description of shapes? Or for the claim that the Peano axioms describe the counting numbers 0,1,2,3,...? Is there a "mathematical" argument (according to you) to show either ZF or ETCS describe sets? If you can do that, then I can try to follow your example and give a "mathematical" argument to the now standard understanding of G2.

best, Colin

05.11.2024 Andrei to Colin:

Hi Colin,

it's great to hear from you! Before we go for the second round of this discussion, here is a proposal (I think I didn't tell you this earlier). Some time ago I discussed with Sergei Artemov and some other people a possibility to organise in NYC a workshop on philosophical and logical issues of formalisation. Sergei's paper could be a topic but I'm thinking about the theme of the workshop more broadly. Personally I'm interested in issues concerning formalisations of Euclid's geometry; another possible issue is formalisation of physical and other scientific theories. Since you and I represent such different attitudes and positions vis-à-vis the formalisation of mathematical theories and the place of logic in it - and at the same time share a lot of background in common - the discussion between us could be of public interest. What do you think?

I have no idea for the moment about a special funding for such an event but since a room in GC CUNY will be available perhaps participants can pay their own travel expenses (at least I have some travelling money for it) or participate by zoom.

OK, now for the second round.

« It sounds like a historical and sociological argument, that is, an appeal to authority, not like a mathematical argument. - Seriously? »

Look, I called it an "argument by authority" because you were not specific, and didn't point me to any specific argument - mathematical or not - made by the people that you mentioned in the support of what you call "the now standard interpretation of G2". But forget the "authority". I can be more specific myself and tell you what does *not* convince me in Tarski's works. (I pick up on Tarski simply in order to be concrete.) So you'll have a better understanding what I'm after, or so I hope.

In the very beginning of his "Introduction to Logic and Methodology of Deductive Sciences" (I use now the 4th edition, OUP 1994) Tarski writes:

"Every scientific theory is a system of sentences which are accepted as true and which may be called LAWS or ASSERTED STATEMENTS (sometimes one says, for short, simply STATEMENTS). In mathematics these statements follow one another in a definite order, and in accordance with certain principles which will be discussed in detail in Chapter VI; in view of these principles, the statements are generally accompanied by arguments whose purpose is to demonstrate their truth. Arguments of this kind are referred to as PROOFS, and the statements established by them are called THEOREMS. "
(The capslock is Tarski's, not mine).

Tarski postulates this without any critical discussion as if he would give a mathematical definition: the reader of the book (probably a student) is supposed to take these definitions for granted and use in what will follow. Yet, the representation of a theory and such its important ingredient as a proof is very controversial, to say the least, or even wrong (as I believe, it is). Judging that it is wrong, in fact, splits into a descriptive judgement and a normative judgement. To justify the descriptive judgement it is sufficient to take example of scientific theories including mathematical theories from the past and the present, and show that the proposed scheme hardly fits to scientific and mathematical theories presented in standard textual sources - or at least that one needs to allow too much of interpretational

liberty to achieve a fit (how much of liberty is "too much" is, of course, also needs to be discussed). The normative judgement is a different and more complicated form of judgement that requires for its justification a talk of epistemic values, etc., and also at least some pointers to how the norm is supposed to be applied in practice (so the two kinds of judgements, after all, are not wholly independent).

To soften my judgement that Tarski's notion of theory is wrong, I might say that a "theory" in his sense is a very simplistic *model* - which in what follows he turns into a fair *mathematical* model - of what mathematical and scientific theories really are. And perhaps even such a toy model can tell us something interesting about theories. It is crucial, however, to keep in mind that the model is oversimplified and keep clear from making wrong universal conclusions on its basis (i.e., via a mathematical study of the model). The idea that "a consistent theory containing arithmetic cannot possibly prove its own consistency" (which I take to be what you call the "standard understanding of G2") is an example of such a wrong conclusion. I agree with Sergei that this is a wrong and unwarranted message sent by logicians and philosophers to computer scientists discouraging them to work on self-verifying software and the like.

In any event the issue of "what is a theory and a proof" is not mathematical one in the sense that there is no possible mathematical proof (or disproof) that a theory and a proof are what Tarski says they are (in the above quote).

As you see, my disagreement with Tarski does not concern mathematical arguments. I might say that Tarski was a bright mathematician but a poor philosopher but I believe that the problem is deeper than that, and it concerns not only Tarski. It rather concerns a whole trend in philosophy and, more precisely, Analytic philosophy, that attempts to replace a philosophical reflection by mathematical reasoning using simple tricks like one in this Tarski's book. Mark Wilson from Pittsburgh in his recent book rightly criticises the trend and calls it an "imitation of rigour", see -

https://global.oup.com/academic/product/imitation-of-rigor-9780192896469?cc=us&lang=en&

Mark motivates his critique by examples from physics but his arguments also perfectly apply to mathematics. Someone, probably myself, should write a mathematical counterpart of Mark Wilson's book (and his earlier books).

Let me share a related personal memory. In 2014, about a week before his unfortunate death, I spoke to Grisha Mintz, then Philosophy professor in Standford (along with Sol Feferman). He was a speaker at a conference [in Saint-Petersburg] that I co-organised. I was impressed by Grisha's remark that he knew very little about philosophy and was never interested in this subject. When I asked him how he got then a philosophy professorship in Stanford, he laughed and said that such was the department's policy, and that he could'n care less how was called the department. Grisha was wise enough to critically describe his position in philosophy but I doubt that Tarski and other bright mathematicians pretending doing philosophy had the same self-consciousness about their situation. To repeat, the problem here, as I see it, is systematic and institutional, not personal.

« Maybe I can understand you better if you can tell me this: Can you give me an argument, that you consider "mathematical," and not merely historical or sociological, for the claim that Euclid's *Elements* give a description of shapes? Or for the claim that the Peano axioms describe the counting numbers 0,1,2,3,...? Is there a "mathematical" argument (according to you) to show either ZF or

ETCS describe sets? If you can do that, then I can try to follow your example and give a "mathematical" argument to the now standard understanding of G2. »

No, there are no mathematical arguments that support these statements. "Euclid's Elements give a description of shapes" is not a theorem that admits for a mathematical proof. If one tries to be more specific about the notion of shape -- presenting it as a Platonic form, Aristotle-style abstraction, cognitive construction or something else, the statement may acquire some non-trivial content but it'll not become a mathematical theorem anyway. The claim that PA describes the counting numbers is more interesting, particularly, in the presence of non-standard models, but once again I don't believe it admits for a mathematical proof. That ZF and ETCS "describe sets" is rather a linguistic convention (which is formalised by saying that sets are non-definable primitive objects). But this convention is not without a reason because behind it is an intuitive notion of collection. How this intuition relate to ZF and ETCS, is it innate and stable or educated and informed by these theories, are interesting and important questions which, however, belong to philosophy or cognitive science or perhaps some other disciplines but certainly not to mathematics. There is no possible mathematical argument (i.e. a proof) supporting the claim that ZF and ETCS describe sets..

The case of G2 differs from the above examples because the claim that "G2 describes mathematical proofs" is easier to analyse. The analysis shows that the claim is true only if one buys Tarski's notion of proof from the above quote along with some further specifications which rule out, in particular, schematic proofs used by Sergei in his article.

Best,
Andrei

P.S. There is a continuing discussion on "what is a scientific theory" that includes Suppes and Fraassen "non-statement view" and many other proposals. I contributed to this discussion myself in the Habilitation thesis. I had this in mind saying that Tarski's notions of theory and proof in his "Methodology" book are not adequate

17.11.2024 Colin to Andrei:
Sorry, I would not have much to say at the CUNY session you propose.

And really I think you should not worry about logicans or philosophers. If you see any way Artemov's idea can help develop self-verifying software then you should take that directly to computer scientists. The computer entrepreneurs have given vast resources to support even very speculative possibilities for breakthrough ideas.

Colin

18.11.2024 Andrei to Colin:

Sure, but I will not leave logicians and philosophers comfortably behind or aside. I'm more convinced today than I was 20 years ago that I touch upon here an important and very sensitive nerve of your style of thinking, and I regret that you are not ready to respond my arguments publicly.

Andrei

18.11.2024 Colin to Andrei:
Here are some arguments I will try to help you with, and might even reply to if you publish them, but no I will not debate them on a stage or on Zoom:

« The situation when "all" logicians for some reason adopt basically the same philosophical interpretations of known logical calculi is known in the history and called a scholasticism. ("All" means here that these people do not qualify as members of their community those who do not comply »

No one but you calls that scholasticism. Indeed the actual Scholastics in the middle ages did not all adhere to the same interpretation of logic. If we replace your phrase "known logical calculus" by "known scientific theory" then it is more often called "scientific consensus." Kuhn calls it "normal science." Everyone knows a consensus can be wrong. But ideas outside the mainstream can also be wrong.

I referred to a long line of logicians who developed modern views of logic, You replied

« It sounds like a historical and sociological argument, that is, an appeal to authority »

And you defended that reply by later saying

« Look, I called it an "argument by authority" only because you were not specific, and didn't point me to any specific argument »

Okay, I assumed you knew enough already about the consensus that you want to challenge, so I would not need to rehearse it for you. Do you know the history of G2 in the 1950s? I suppose you do know that logicians then knew PA proves the consistency of each of its finite subtheories, and much more PA proves consistency of its restriction to any finite quantifier complexity. These are not new ideas. And no I will not chase down references now. It is in Hajek and Pudlak's book. Fifteen seconds on Google produced

https://mathoverflow.net/questions/193680/does-pa-prove-a-sentence-asserting-that-all-of-i-sigman-theories-are-consisten

I very much hope that on reflection you do not consider all historical arguments to be appeals to authority -- and I seriously do not want to argue with you about that on a stage or on Zoom.

Now you want to turn to discussion of why you disagree with Tarski about what a theory is. I appreciate your sense of humor when you say

« I might say that Tarski was a bright mathematician but a poor philosopher. »

But unless you believe that every philosopher you disagree with is thereby "a poor philosopher" then you probably should not call Tarski one.

The arguments you give about what a theory is are familiar. You actually seem to know that. That does nothing particular to promote debate about Artemov's paper.

I will do you the courtesy of not calling the following an argument by authority:

« I agree with Sergei that this is a wrong and unwarranted message sent by logicians and philosophers to computer scientists discouraging them to work on self-verifying software and the like. »

But I wonder how concrete an argument it is. Do the two of you see any specific way his view could help developing self-verifying software?

Colin

21.11.2024 Andrei to Colin:

Thank you for the longer answer, Colin.

Concerning how I use the word "scholasticism". Of course I know that in the middle ages there were many competing interpretations of logic; I don't use here this word as a historical reference. In fact, I very much liked Ramsey's definition of scholasticism that you mentioned in an earlier message. Let's agree on that definition. Fortunately, neither today's philosophical logic nor the medieval philosophical logic reduces to scholasticism in that pejorative sense of the word.

Concerning the "scientific consensus" and the "normal science". Here lies our principal disagreement. I do agree that the scientific consensus plays a role in science but I deny that philosophy is or should be anything like a science. If consensus has any role in philosophy at all, it is a local and partial - I mean a consensus within competing philosophical parties and traditions - but not an universal or "mainstream" consensus like in sciences. A believe following Karl Popper that Kuhn's notion of normal science (and also that of "scientific revolution") is a serious misconception. But while in sciences this notion demonstrates at least some superficial descriptive adequacy, applying it in philosophy as an epistemic norm (as the term suggests) is a disaster.

My sensitivity to this issue is so idiosyncratic, probably, because before I learned about the "normal philosophy" within the Analytic tradition, I learned about another version of "normal philosophy" in the form of the "dialectical materialism" and "dialectical logic" developed by the official Soviet philosophers. They misused Hegel and Marx very much like their contemporary philosophers in the US misused mathematics. They similarly cared about consensus, institutions, and effectively marginalised anyone who disagreed. Sometimes even more effectively than in the US, I guess. And of course they also called their philosophy science. When I first visited US and spent a year in Columbia University in the 1990s I was stunned how philosophical establishments in the two countries were similar.

So my view is that mixing robust mathematical results and their philosophical interpretation is a mistake. Tarski makes this mistake when he treats mathematical proofs and theories as if they were themselves mathematical concepts like numbers, sets, categories and whatnot - *and* believes that in this way one can answer philosophical questions about truth and such like. I realise that treating proofs as mathematical objects is the essence of what Hilbert calls "metamathematics". What I refuse is not the metamathematics as such but the popular idea that it can a suitable substitute for philosophy, in particular, the philosophy of maths.

This is the first and more important point where I disagree with you and with Tarski. But there is a more specific and perhaps more interesting point: I do not believe that a mathematical or any other theory is a set of propositions. I don't quite follow here Suppes and other partisans of the "non-

statement view". I'm rather trying to develom Bill Lawvere's idea of "theory as a generic model", as in the conclusion of this paper: https://philsci-archive.pitt.edu/14434/1/MH1.pdf

I did not quite understand your point concerning the history of G2 in the 1950s. Are you implying that what Sergei presented in his paper has been already known in 1950? My own interest to this paper is philosophical rather than technical/mathematical: in my view, Sergei quite rightly opposed the orthodoxy - aka the consensus of "normal philosophy" - and argued that mathematical proofs admit for a different mathematical representation. Concerning the technicalities, I assume that you are right but it would be probably useful to check it with Sergei anyway.

As for the "message to computer scientists" - it was Sergei's remark. I believe he refers here to the fact that too many people including computer scientists take G2 to be an epistemological thesis proved mathematically. And this is indeed wrong and confusing, as far as I can see. To clarify this confusion is a philosophical rather than mathematical task. This is a general remark, not a CS project.

Andrei

21.11.2024 Colin to Andrei:
« Concerning how I use the word "scholasticism". Of course I know that in the middle ages there were many competing interpretations of logic; I don't use here this word as a historical reference. »

You said this situation "is known in the history and called a scholasticism." That is an explicit historical reference.

« I do agree that the scientific consensus plays a role in science but I deny that philosophy is or should be anything like a science. If consensus has any role in philosophy at all, it is a local and partial - I mean a consensus within competing philosophical parties and traditions - but not an universal or "mainstream" consensus like in sciences. A believe following Karl Popper that Kuhn's notion of normal science (and also that of "scientific revolution") is a serious misconception. »

Do you mean it is okay with you if logicans continue interpreting G2 in the now-usual way, as long as philosophers do not (all) agree with them? And you and Sergei think that if computer scientists learn about some philosophers disagreeing with how logicans describe G2 then some computer scientists might decide to follow those philosophers rather than the logicians and the other philosophers?

I think that here you are not saying what you think, because you have not finally decided what you think.

« My sensitivity to this issue is so idiosyncratic, probably, because before I learned about the "normal philosophy" within the Analytic tradition, I learned about another version of "normal philosophy" in the form of the "dialectical materialism" and "dialectical logic" developed by the official Soviet philosophers. »

Okay. For you current analytic philosophy is run like philosophy in the Soviet Union. I can respect your feeling without revising my view of Godel's Theorems in this light.

« So my view is that mixing robust mathematical results and their philosophical interpretation is a mistake. Tarski makes this mistake when he treats mathematical proofs and theories as if they were themselves mathematical concepts like numbers, sets, categories and whatnot - *and* believes that in

this way one can answer philosophical questions about truth and such like. »

First, I did not say I agree with Tarski. Second I hope you understand that Tarski's quoted definition of "theory" is stipulative, not descriptive. It defines what he will mean in that book by "theory," and he does not mean to say it is what others in the past meant by "theory." So there is really no question of "agreeing" with him or "disagreeing." If you do not want to use "theory" in his way, that's okay.

« I did not quite understand your point concerning the history of G2 in the 1950s. »

My last e-mail made no point about that history, but asked if you know that history. When I refer to things you do not know, you say I am "arguing by authority," so I need to ask you what you do know. I think most likely you do not know this history, but I could be wrong. If you do know it, you know that Sergei's distinction between a consistency formula and a consistency scheme was not only familiar then but logicians worked with it. And they worked with in full awareness of the distinction that Sergei makes between "verifiable in PA" and "uniformly verifiable in PA." Sergei says that, at least in discussions of the consistency of PA, we should take "provable in PA" to mean what he calls "verifiable in PA" and not require "uniformly verifiable in PA." The problems with that have been discussed since the 1950s. The most obvious is that would mean we give up Godel's Completeness Theorem. Do you understand that? Or do you want a fuller explanation?

« Are you implying that what Sergei presented in his paper has been already known in 1950? »

No. His specific idea of "selector proof" is new. But his whole discussion of consistency scheme versus consistency formula, and verifiable versus uniformly verifiable was widely known -- probably not in 1950 but by 1960. The issues were notably discussed at length in Sol Feferman's

```
@ARTICLE{FefArithGen,
  author =      {Feferman, Solomon},
  title =       {Arithmetization of metamathematics in a general setting},
  journal =     {Fundamenta Math.},
  year =        {1960},
  volume =      {49},
  pages =       {35--92},
}
```

« My own interest to this [Artemov's] paper is philosophical rather than technical/mathematical: in my view, Sergei quite rightly opposed the orthodoxy - aka the consensus of "normal philosophy" - and argued that mathematical proofs admit for a different mathematical representation. Concerning the technicalities, I assume that you are right but it would be probably useful to check it with Sergei anyway. »

Sure. But you know that every logician knows mathematical proofs admit for many different mathematical representations. One of the most widely discussed virtues of Godel's Incompleteness Theorems is their robustness under change of object theory and logical apparatus. So you need much more specific arguments than just a philosophical faith in the right to be "different" before you will change anyone's mind about Godel's Theorems.

« As for the "message to computer scientists" - it was Sergei's remark. [...] This is a general remark, not a CS project. »

Okay. It is a general remark about computer science that is not meant to apply to computer science. It is meant to be philosophical rather than true.

The impossibility of self-verifying software does not only rest on G2. It can as well be based on recursive function theory, or on Turing Machine theory, or other less widely pursued alternatives. You indeed have the philosophical freedom to say any and all of those might be irrelevant for whatever reason you like. I would just urge you not to invest your money in any computer company that claims to have self-verifying, general purpose software.

Colin

23.11.2024 Andrei to Colin:

« You said this situation "is known in the history and called a scholasticism." That is an explicit historical reference. »

I think about the scholasticism as a phenomenon of intellectual life, which appears and reappears in the intellectual history. Scholasticism is not a name of certain event making part of this history, which occurs once at certain time and place. I use this word as the name for a type, not a token. And yes, in is known in the history, both remote and very recent.

« Do you mean it is okay with you if logicians continue interpreting G2 in the now-usual way, as long as philosophers do not (all) agree with them? »

It depends on how you distinguish between logicians and philosophers. I also would like to understand better what you mean by the "interpretation" of G2 by logicians. Do you mean a philosophical interpretation or something different? And it is not clear to me who are those logicians you refer to. People who describe themselves as logicians and are recognised as such by some other people do not form a uniform group.

Many logicians don't have strong philosophical views or keep their philosophical views private (which means that they are not very serious about philosophy). Grisha Mintz whom I already mentioned in our discussion is just one example. Vladimir Andreevich Uspensky, Kolmogorov's student who was the Chair of logic in the math department of Moscow University for many decades (until his death in 2018), is another example. V.A. wrote a very useful book on G1-2 and made other contributions to logic. At the same time he believed that a mathematical proof is a subjective and psychological matter - a matter of being or being not convinced by another person's rhetoric. He wrote a lot of popular prose where he exposed this view but I doubt that he himself would describe this view as philosophical. Another person from whom I owe my knowledge of some logic (including some developments related to G2 in the 1950s and later, see his https://arxiv.org/abs/1602.05728v1) is Lev Beklemishev, who tries to distance himself from philosophy (or so I understand his position). In one of his Russian papers Lev argues that Godel himself didn't want to be involved in philosophical debates about his incompleteness theorems. Thus there is a large group of logicians who are primarily mathematicians and only then logicians but certainly not philosophers. In my understanding Godel belongs to this group in spite of the fact that he had also philosophical ambitions.

When such non-committed logicians use the word "proof" in the "usual" way, I have nothing to object. When Uspensky talks about mathematical proofs in general and about formal "proofs" involved in G1-2, he clearly talks about two different things, which in his view are wholly unrelated. I think that none of these mathematicians-logicians would object Sergei's proposal to reconsider what qualifies as a mathematical proof and, more specifically, as a formalised mathematical proof. This is not a mathematical but rather an epistemological and also a historical issue (when we think about mathematical proofs of the past). For some mathematical logicians like Uspensky (for whom mathematical logic is wholly unrelated to mathematical reasoning) this whole issue may appear unimportant. To myself it does appear as interesting and important because I'm interested in the history of maths and its philosophy.

My objections apply only when a logician provides logical technical terms (like "proof" in in the "usual" formal sense) with a strong epistemological/philosophical meaning and converts logical statements into epistemological once without any serious attempt to justify such a move and to enter into a fair philosophical debate. I read some time ago - but cannot find easily the reference - about a public discussion (if it can be so called) between A. Tarski and F. Gonseth some time in 1950s. During this episode Tarski simply pretended not to understand Gonseth's arguments and objections. This is a clear example of avoiding a fair philosophical discussion while making philosophical claims.

So it is okay with me if logicians use whatever terminology they prefer. But it is not okay with me when a logician makes strong philosophical claims but avoids to accomplish what philosophy requires to do, that is, (1) to understand that philosophical debates unlike mathematical problems are open-ended and don't admit for definite solutions, (2) to engage oneself in such debates and carry them faithfully and respectfully. It is not okay with me either when such philosophically-minded logicians try to sell mathematical proofs for philosophical arguments. (More rarely some logicians commit the dual error trying to sell a philosophical argument for a mathematical proof: I recently came across such an example in Bolzano's early writings.)

« And you and Sergei think that if computer scientists learn about some philosophers disagreeing with how logicians describe G2 then some computer scientists might decide to follow those philosophers rather than the logicians and the other philosophers? »

I think that computer scientists and all other people should carefully distinguish between the mathematical content of G2 and its philosophical interpretations. I also think that logicians, mathematicians and philosophers should jointly help other people to do that. Such a distinction is necessary for making an informed choice between available philosophical interpretations of G2. Finally, I think that the presence of multiple debated philosophical interpretations of G2 (some of which may be compatible and some other not) is fruitful (for logic and philosophy alike) while developing a consensual philosophical interpretation via academic policies such as ignoring or marginalising opponents is a harmful scholasticism.

«I hope you understand that Tarski's quoted definition of "theory" is stipulative, not descriptive. It defines what he will mean in that book by "theory," and he does not mean to say it is what others in the past meant by "theory." So there is really no question of "agreeing" with him or "disagreeing." If you do not want to use "theory" in his way, that's okay. »

The quote is from Tarski's book titled "Introduction to Logic and Methodology of Deductive

Sciences". As the author explains in the Preface, by Deductive Sciences he means primarily mathematics. The book is supposed to provide a guide of how to build mathematical theories. When Tarski wrote this book mathematics was already a big industry with a long history. So in order to say something important about mathematical theories and how to build them one would need to motivate one's notion of theory and check it against existing examples rather than simply stipulate it. I don't want particularly blame Tarski: if he did not this work in his book, perhaps other people had to that. But many people who were in such a position preferred either to take Tarski's stipulations as dogmas, or accept some other dogmas and build their own churches aside (like the intuitionists).

So I don't think, that just to *stipulate* what is a theory or a proof is a good idea. A good idea is to discuss various such notions, evaluate them against historical examples, and provide some epistemological arguments supporting this or that stipulation. This is, in my sense, a philosophical and historical element of logic.

« My last e-mail made no point about that history, but asked if you know that history. »

Of later developments concerning G2, I'm better aware about the following mathematical developments: Löb theorem, etc. As for serious philosophical interpretations, I read Mic Detlefsen's book on the Hilbert Program where he discusses G2 philosophically and refers to some earlier literature of the same sort.

No, I cannot understand how Serge's proposal contradicts the Completeness Theorem. Please explain or give me a reference. This is very interesting.

« But his whole discussion of consistency scheme versus consistency formula, and verifiable versus uniformly verifiable was widely known -- probably not in 1950 but by 1960. The issues were notably discussed at length in Sol Feferman's 1960 paper *Arithmetization of metamathematics in a general setting.*

Thanks for this reference.

« But you know that every logician knows mathematical proofs admit for many different mathematical representations. One of the most widely discussed virtues of Godel's Incompleteness Theorems is their robustness under change of object theory and logical apparatus. So you need much more specific arguments than just a philosophical faith in the right to be "different" before you will change anyone's mind about Godel's Theorems. »

I realise that G1-2 are robust, Löb's formalism shows this very well. But Sergei's argument nevertheless goes through. This argument shows that G2 is not robust with respect to changing one's way of formalising informal arithmetical proofs. This does not concern the specifics of the "syntactic sugar" but does concern considering schematic proofs as bona fide proofs.

« Okay. It is a general remark about computer science that is not meant to apply to computer science. »

It is meant to apply to CS and elsewhere as a critique that dispels a popular myth about G2 created

with a massive popular maths/logic/philosophy literature which involves biased (and often also incompetent) philosophical interpretations of Godel's results.

« It is meant to be philosophical rather than true. »

That's correct. A philosophical critique is not supposed to be true, it is rather supposed to be valid and formally correct.

«The impossibility of self-verifying software does not only rest on G2. It can as well be based on recursive function theory, or on Turing Machine theory, or other less widely pursued alternatives. You indeed have the philosophical freedom to say any and all of those might be irrelevant for whatever reason you like. »

It is not only philosophical but also a practical matter. In practice, to be a "self-verifiable software" may mean a thousand of different things, and there is no good reason to believe that all of those fall under the limitations of G2 or recursive function theory. When I studied some CS in the early 1980s we were told about "self-correcting codes" which was basically a simple evenness check. Such tricks work (I'm sure that today they are by far more sophisticated), and this fact obviously does not contradict any known metamathematical result. The idea of "self-correcting software" was very inspiring for me then, and I still love it.

At the very least Sergei shows in his paper that there is a sense in which on can talk about a proof of consistency of PA formalised in PA. This suggests that there can be a software which in some appropriate practical sense of the word can be called "self-verifiable" - and its existence is not ruled out by the recursive function theory.

Andrei

24.11.2024 Colin to Andrei:
Okay It is utterly commonplace to use "scholasticism" generally for a type, which has occurred both remotely and recently in history. That is no problem.

I am concerned with your specific use of scholasticism to describe the situation when "all" logicians for some reason adopt basically the same philosophical interpretations of known logical calculi. Do you mean this situation is known in both remote and recent history? If that is what you mean, then I'd like to know some remote examples you have in mind, and someone besides you who called those examples scholasticism.

I will comment on your very numerous other concerns after you answer this.

Colin

24.11.2024 Andrei to Colin:

I assume that in the middle age Europe certain writers assumed uncritically certain logical and metaphysical principles that they attributed to Aristotle, and then developed their speculations on that basis in a manner of Kuhn's normal science without ever questioning these principles and persecuting those who tried to do that with the Inquisition and such like means. I assume that this is what gave the

Scholasticism its bad fame from the 17th century on when Galileo and other founders of the modern science won the social battle and gained an increasing support of those in power. This bad fame is responsible for how the title of Scholasticism has been used ever since, in particular, by Ramsey.

Having said that I realise that the history of logic and philosophy in the medieval Europe is by far richer and more involved and interesting than the above caricature suggests, and that I'm not an expert in this history. So I cannot provide your more details here. It would be interesting indeed to learn how different writers used the word "scholasticism" during the last centuries but I'm not ready to engage myself into such a research right away.

In fact, this historical reference plays no important role in my line of argument. Since we both agreed on Ramsey's definition of the Scholasticism (which is a rare point we agree upon in this discussion) I propose you just to stick to this definition, and leave the European middle ages aside. If we do that then I can show you that building a group of philosophically-minded logicians in an academic establishment who all share the same philosophical interpretation of their favourite logical calculi and pursue their studies after the model of normal science is an instance of scholasticism in Ramsey's sense. And moreover so when they tend to describe themselves as "all" logicians that are around.

Andrei

24.11.2024 Colin to Andrei:
« I assume that in the middle age Europe certain writers assumed uncritically certain logical and metaphysical principles that they attributed to Aristotle, »

That is quite correct.

« and then developed their speculations on that basis in a manner of Kuhn's normal science »

That is remote from fact.

« without ever questioning these principles »

The leading figures very explicitly questioned their own ideas---question and answer was a standard format for philosophy (just as it was the normal format for catechisms). And each one accepted his own answers (just as each priest accepted the answers in their own preferred catechism). What they could not do was accept each other's answers.

« and persecuting those who tried to do that with the Inquisition and such like means. »

Sometimes. But the population of scholastics was not high. Direct conflict between them was not very common. The famous wars of religion and inquisitions had more to do with princes and peasant heretics than with scholastic philosophers.

The most famous direct conflict between scholars pitted Petrus Ramus and his followers against others. Both sides believed they were the true Aristotelians. And the conflict was over much more than logic. See https://plato.stanford.edu/entries/ramus/

Compare https://plato.stanford.edu/entries/ockham/ and https://plato.stanford.edu/entries/ockham/

« In fact, this historical reference plays no important role in my line of argument. »

Yes, that is how it seems to me. But you want to keep calling the view you dislike "scholasticism," while shifting the grounds for that to whatever they need be.

« Since we both agreed on Ramsey's definition of the Scholasticism (which is a rare point we agree upon in this discussion) I propose you just to stick to this definition, and leave the European middle ages aside. If we do that then I can show you that building a group of philosophically-minded logicians in an academic establishment who all share the same philosophical interpretation of their favourite logical calculi and pursue their studies after the model of normal science is an instance of scholasticism in Ramsey's sense. »

I am entirely certain you will succeed at proving that to your own satisfaction. Feel free to try proving it to my satisfaction.

My counter proposal is that you continue using the term "scholasticism" any way you like. And I will continue ignoring that term in favour of directly relevant descriptions like "vagueness, wooliness, laziness, precision, rigour" and "consensus," plus specific discussion of G2 and Artemov's arguments.

I do hope no one actually says "all" logicians disagree with Artemov. I hope everyone one agrees Artemov himself is a logician. I notice Wikipedia does.

Colin

24.11.2024 Andrei to Colin:

I certainly can avoid using the word scholasticism - since it creates misunderstanding between us - and apply instead more direct descriptions as you suggest.

Some time ago you wrote:

« I fear this is the basic difference between us on this issue. For you, the opposite of dogmatism is doubt. For me the opposite of dogmatism is giving articulate reasons for a belief. »

and I replied that

« The opposite of philosophical dogmatism for me is philosophical criticism rather than doubt. »

You are quite right that this is a basic difference. This is why I believe that articulating reasons does not save one from dogmatism. A community can develop a system of reasons and ways of reasoning that are convincing for its members. And it can successfully hire novices converting them not only into their system of beliefs but also into their system of reasoning. Unless the community is open for debates with members of other similar communities, I would qualify it as dogmatic.

The case of philosophical logic is particularly difficult because one may argue that in order to have a rational discussion all parties should share the same logic, say, first-order classical logic. I don't have a formal solution for that problem but I think that in order to critically discuss foundations of logic we

need something else than logic, some form of dialectics. And of course philosophers should not stick to a single logical calculus in order to have a rational discussion.

I cannot see how a community of philosophers can possibly reach and sustain a consensus (about foundations of logic or anything else) unless it goes dogmatic, whether with reasons or not. This is why I'm very suspicious about the very notion of philosophical consensus. Consensus in philosophy appears to me as a sign of its weakness, not of its strength. Philosophy and sciences (including mathematics) work differently in that respect: while reaching a consensus in science is normal, typical, and in some sense even mandatory (when we talk about established scientific knowledge) in philosophy it is not. A consensual philosophy appears to me a sort of bad science.

Peter Ramus is indeed a fascinating character. I came across presentations of some of his mathematical works in the secondary literature, which was very intriguing. And earlier scholars are also very interesting. I would wish I have more time for reading. So far I'm trying to catch up my understanding of French intellectual environment of the 16th century reading Rabelais. It takes me even stronger than Thomas Pynchon's novels.

Andrei

26.11.2024 Colin to Andrei:

« I certainly can avoid using the word scholasticism - since it creates misunderstanding between us - and apply instead more direct descriptions as you suggest. »

I explicitly suggested you continue using the term "scholasticism" any way you like. Please do not censor yourself on my account. Since we have discussed the word at such length, I am sure the mere word is not causing misunderstandings. I am sure we actually disagree about whether the reasons you have given for using it are good reasons.

As a practical matter, on a larger scale, I believe that if you publicly depict the numerous logicians publicly discussing Artemov's ideas as practicing a "scholastic" repression of new ideas, then that will not create any misunderstanding. It will not even create any impression. Most readers will simply pass over it as a banal cliche used by people who feel their ideas are underrated. But don't take my word for it. I could be wrong. You have to express yourself in the words you choose, whether you are writing to me or others or for print.

You wrote:

«The case of philosophical logic is particularly difficult because one may argue that in order to have a rational discussion all parties should share the same logic, say, first-order classical logic. I don't have a formal solution for that problem but I think that in order to critically discuss foundations of logic we need something else than logic, some form of dialectics.»

If you ever arrive at such a form of dialectics let me know. And give me some real examples using it to discuss Artemov's selector proofs, so I can follow them. Until then I think you and I have exhausted our capacity for useful discussion of philosophical logic in general (let alone "rational" or "critical" discussion by your here-stated criteria).

Later today, or tomorrow, I will send an email (as promised) replying to your specific thoughts about

Artemov in your email of Sat, Nov 23, 1:45 PM.

Colin

26.11.2024 Andrei to Colin:

« I explicitly suggested you continue using the term "scholasticism" any way you like. Please do not censor yourself on my account. »

Yes, but in the next sentence you wrote me that you "will continue to ignore that term". I cannot see that you've really ignored that term up to this point, but I certainly don't want to use the word that the addressee of this word is going to ignore. At least not in our exchange. I don't feel it like censoring, it's okay.

« Since we have discussed the word at such length, I am sure the mere word is not causing misunderstandings. I am sure we actually disagree about whether the reasons you have given for using it are good reasons. »

agreed

«As a practical matter, on a larger scale, I believe that if you publicly depict the numerous logicians publicly discussing Artemov's ideas as practicing a "scholastic" repression of new ideas, then that will not create any misunderstanding. It will not even create any impression. »

At least the experience of our recent exchange suggests me just the opposite: this single word of mine produced more of your comments and more my replies than any other single word. So we indeed discussed it in a great length. But I agree with you that general arguments like "scholastic repression" are not particularly appropriate when any particular idea like Artemov's is at stake. In my public zoom talk on which you then publicly commented I talked about (what I described as) the scholasticism in the Analytic philosophy in a more general context referring to Artemov's work only in passim. Artemov's work certainly deserves a special discussion involving more specific arguments.

« If you ever arrive at such a form of dialectics let me know. And give me some real examples using it to discuss Artemov's selector proofs, so I can follow them. »

I think that such a dialectic can be only forged as a form of some collective experience, no single individual myself included can possibly "arrive" to it.

« Until then I think you and I have exhausted our capacity for useful discussion of philosophical logic in general (let alone "rational" or "critical" discussion by your here-stated criteria). »

I agree. Thanks for your time, Colin

Andrei

27.11.2024 Colin to Andrei:

« It depends on how you distinguish between logicians and philosophers. »

By logicians I mean people pursuing logical problems and logical explanations. Philosophers are people pursuing logical problems and interpretations. Perhaps you can prove to your satisfaction that logicians should all also be philosophers. I cannot prove it to my satisfaction.

« I also would like to understand better what you mean by the "interpretation" of G2 by logicians. »

You see, I thought you were complaining to me about how logicians and analytic philosophers interpret G2, as if you already knew what that meant. For example

« My objections apply only when a logician provides logical technical terms (like "proof" in in the "usual" formal sense) with a strong epistemological/philosophical meaning and converts logical statements into epistemological once without any serious attempt to justify such a move and to enter into a fair philosophical debate. »

When you say this, what do you mean by "logicians"?

« No, I cannot understand how Serge's proposal contradicts the Completeness Theorem. Please explain or give me a reference. This is very interesting. »

Well, this is the Completeness Theorem in the comprehensive sense (as for example in Goldstern and Judah *Incompleteness Phenomenon*): A first order theory proves all and only the sentences that are true in all models. Others separate that into a Soundness Theorem and Completeness Theorem, and in those terms it is actually the Soundness Theorem that is lost when you take on selector proofs. Selector proofs prove some sentences that are not true in all models. That is the whole point. Sergei explains repeatedly that "PA is consistent" fails in some non-standard models of PA, and yet there is a selector proof.

Of course Sergei argues "PA is consistent" should not be understood as a sentence. It is a sentence scheme. But it really does look like a sentence.

« But his whole discussion of consistency scheme versus consistency formula, and verifiable versus uniformly verifiable was widely known -- probably not in 1950 but by 1960. The issues were notably discussed at length in Sol Feferman's 1960 »

Sergei knows this paper. You should ask him. Two things are obvious: Sergei believes he can show his approach to a fundamental understanding of G2 is better than Feferman's here, and a great many other experts (who assuredly also know this Feferman paper) believe Sergei is wrong about this.

Feferman 1960 and Sergei 2024 deal with all the same issues of arithmetization, formulas versus formula schemes, and enumeration of axioms and of proofs. One key point for both is how to understand the formally-clear fact that PA proves consistency of all its finite fragments. Feferman also emphasizes the problem of how to best express Godel's Completeness Theorem inside of arithmetic. Feferman cites a huge number of other then-recent discussions of these issues (published and unpublished).

The most systematic difference from Sergei is that Feferman gives explicit reasons (as well as a long practical demonstration by showing how well it works) why the most revealing way to organize the metatheory of First Order theories and especially PA is to standardize one sense of "proof," and put all non-standardness into the enumeration of axioms. This Feferman paper persuaded the great

majority of researchers of this point at the time. Obviously Sergei now disagrees and wants to vary the notion of "proof," at least for PA. So Sergei defines "selector proofs," which are his novel contribution on this topic.

I will not try to argue the case between Feferman and Sergei here. That would be a lot of work. I have not even read the FOM discussion. I will say that the grounds for the current consensus view of G2 are, in historic fact, much more extensive than Sergei deals with in this 2024 paper. Many more than Sergei addresses are found in Feferman 1960 and its references.

« A philosophical critique is not supposed to be true, it is rather supposed to be valid and formally correct. »

If you emphasize this point in publications, you will have to explain that "formally correct" is not to be understood in terms of first order logic, and you are not yet sure how it should be understood.

« In practice, to be a "self-verifiable software" may mean a thousand of different things. [...] When I studied some CS in the early 1980s we were told about "self-correcting codes" which was basically a simple evenness check. »

And so, as a practical matter, you want logicians and philosophers to stop discouraging computer scientists from developing self-verifying software. I fear that is the exact opposite of practical.

Since you know Self Correcting codes have been a reality, widely used, for decades, you must know logicians and philosophers have done nothing at all to discourage computer scientists from developing those. Self correcting codes have nothing to do with G2 or with self-verifying software.

Program-verification means taking a program and verifying that it does what it is supposed to. This description is too vague to matter much. The practical version of it today is "Formal Verification," for which Wikipedia makes a good starting point. A very special case of program verification would be taking a given program that is supposed to accept an input, do some calculation and then halt, and verifying that for each input the program will at least halt. The halting problem is a special case of program verification---and even this special case is recursively unsolvable in general.

Self-verifying software is software which does take inputs, and is able to tell for itself whether it will ever halt on that input---so that it will reject any input that would never halt. Some special purpose software actually does that. But any software which is capable of defining all recursive functions will have an unsolvable halting problem and so certainly cannot be self-verifying.

Can Microsoft Word define recursive functions? Yes, it can, if you know how to use its macros. So can LaTex. Even PowerPoint.

https://stackoverflow.com/questions/2968411/ive-heard-that-latex-is-turing-complete-are-there-any-programs-written-in-late

Programs that can do those jobs cannot be self-verifying---and indeed this is known by recursion theory, or by G2, or by Turing Machine theory, and many other ways.

« At the very least Sergei shows in his paper that there is a sense in which on can talk about a proof of

consistency of PA formalised in PA. This suggests that there can be a software which in some appropriate practical sense of the word can be called "self-verifiable" - and its existence is not ruled out by the recursive function theory. »

That is easy to say, if no one asks you to explain any concrete sense of how Sergei's idea could possibly help develop self-verifying software.

If Sergei is right, and he (or you) can find an actual way to make that connection for software like MS Word, then the CUNY research laboratory for logic and computation will make headlines around the world and its efforts will have huge funding. I would be delighted for you. But I am not betting money on it.

Colin

27.11.2024 Andrei to Colin:

« When you say this, what do you mean by "logicians »? »

I understand terms like "logician", "mathematician" and "philosopher" as liberally as possible. This means that anyone who self-describes as logician, mathematician or philosopher *and* has at least a tiny circle of colleagues who accept and use the same description, falls under such description also in my words. So my notion is basically sociological. A remarkable sociological fact about philosophers is that there exist many philosophers (in the above liberal sense) who do not recognise each other as such. Mathematicians tend to recognise each other universally. I'm not sure about logicians.

« In those terms it is actually the Soundness Theorem that is lost when you take on selector proofs. Selector proofs prove some sentences that are not true in all models. That is the whole point. Sergei explains repeatedly that "PA is consistent" fails in some non-standard models of PA, and yet there is a selector proof. »

What gets lost here is, once again, not the theorem itself but rather its standard interpretation. In order to distinguish between the theorem and the interpretation it is sufficient to replace in the theorem the "provability" by the (syntactic) "derivability". Given the Soundness/Completeness theorem one may tentatively interpret the derivability as provability. But this is a problematic move, which needs to be discussed, evaluated from an epistemological viewpoint and checked against facts from the current mathematical practice and from the history of maths. I would go further and find a different and more neutral term for the "truth in a model". Because as it stands, the term is also heavily laden with epistemological and metaphysical connotations, which are are too easily (and wholly uncritically) smuggled into students's minds when they first learn that stuff.

I can see that pointing to the Soundness/Completeness is relevant and important for discussing Sergei's proposal. It shows that his proposal indeed diverges from the mainstream very significantly, and problematises the whole Hilbert's distinction between a formal theory and its models (in case of number theory). We know how Frege challenged this distinction. It would be interesting to compare Frege's challenge with Sergei's proposal. The Frege-Hilbert debate is a standard topic at today's philosophy departments, right? Why not to promote more pertinent philosophical debates in, on and around logic? Is the time distance of 120+ years just sufficient for protecting living people's beliefs

and interests from possible critical disturbances? Is this time distance necessary?

« Of course Sergei argues "PA is consistent" should not be understood as a sentence. It is a sentence scheme. But it really does look like a sentence. »

It is a sentence of the natural language but, as Sergei argues, it is best formalised in PA as a scheme but not as a single formula. In order to defend or attack Sergei's proposal one needs an account on formalisation. This is why Sergei's paper motivates me to organise a workshop on that topic. In fact, there are many people interested in the issue of formalisation (in maths and beyond), as well as some ongoing events and papers published on the topic. But they are, typically, not interested in sorting out things like ones discussed in Feferman 1960 and Sergei 2024. Perhaps because that stuff appears to many of them too technical and unimportant. I do believe that it is important.

« The most systematic difference from Sergei is that Feferman gives explicit reasons (as well as a long practical demonstration by showing how well it works) why the most revealing way to organize the metatheory of First Order theories and especially PA is to standardize one sense of "proof," and put all non-standardness into the enumeration of axioms. This Feferman paper persuaded the great majority of researchers of this point at the time. Obviously Sergei now disagrees and wants to vary the notion of "proof," at least for PA. So Sergei defines "selector proofs," which are his novel contribution on this topic. »

Feferman's paper is not an easy read. What I learned from the first page is the fact that it is a summary of his Ph.D. made under Tarski. I guess that the thesis itself should be more readable since it should be less compressed. I'l try to find it, it should be, among other things, a useful overview of earlier works.

Here is another significant difference between Feferman 1960 and Sergei 2024, which I can identify even without knowing all the content of Feferman 1960. In his Ph.D. (as I can judge using the 1960 summary) Feferman reviews a great amount of earlier works and makes his improvements. This is what a good Ph.D. student is supposed to do in order to demonstrate his/her knowledge of the research area as well as his/her ability to proceed the "normal science" on that ground in the direction approved by the supervisor. This is normal for the academia, and I will not use the s-word to qualify it. Sergei 2024, on the other hand, is a work of a mature logician who challenges some established beliefs of his community. In that respect the two papers are not at all on equal footing. Unlike Sergei 2024, Feferman 1960 does not challenge anything but proposes improvements.

« I will not try to argue the case between Feferman and Sergei here. That would be a lot of work. I have not even read the FOM discussion. I will say that the grounds for the current consensus view of G2 are, in historic fact, much more extensive than Sergei deals with in this 2024 paper. Many more than Sergei addresses are found in Feferman 1960 and its references. »

What upsets me is not the fact that a significant number of logicians disagree with Sergei but rather the fact that I fail to see the same openness on their side. This is what I characterise by using the s-word. I hope that I managed to convince you that Sergei's work at least worths considering.

I realise that expressing upsets is generally not a good idea, and not an effective public strategy. This is why I'm trying to push Sergei's agenda - not just defending his interpretation of G2 but rather trying

to create a room where it can be decently discussed. This is certainly more interesting than expressing upsets.

« A philosophical critique is not supposed to be true, it is rather supposed to be valid and formally correct. - If you emphasize this point in publications, you will have to explain that "formally correct" is not to be understood in terms of first order logic, and you are not yet sure how it should be understood. »

Yes, sure. Perhaps I should leave out "formally correct" here altogether.

Andrei

29.11.2024 Colin to Andrei:

Here is an important difference: For you, diverging and problematizing are rare enough that if someone has done one or both then that person per se deserves wide attention. (I mean, Artemov's selector proofs have gotten wide attention, but not enough to satisfy you.)

For me, current G2 research itself contains no shortage of divergent approaches and wide open problems.

As to Frege, by all means, ask Frege scholars to take up Artemov's ideas. I expect they won't, but I could be wrong. If you do that, though, do not ask them whether the time distance of 120+ years is necessary or sufficient for protecting their beliefs and interests from possible critical disturbances. That will not make a good impression on them.

Alternatively, you could compare Frege's challenge with Sergei's proposal yourself.

« It is a sentence of the natural language but, as Sergei argues, it is best formalised in PA as a scheme but not as a single formula. In order to defend or attack Sergei's proposal one needs an account on formalisation. »

You are eager to attack and/or defend. But before that, *giving* Sergei's proposal needs a fuller account of formalization.

The proposal would be of no interest if it only applied to Con(PA). So he looks at other uses. For example, his page 6 discusses PA formalization of "the product of polynomials is a polynomial." On page 5 he seems to say this "can[not] even be formulated by a single arithmetical formula, let alone conventionally proved in PA." But his selector proof of "the product of polynomials is a polynomial" uses arithmetization, and indeed arithmetization does let us express "the product of polynomials is a polynomial" by a single PA sentence and give that sentence a conventional PA proof. Or does Sergei mean something different by "arithmetical formula" than the rest of us? In that case he needs to say what.

Or consider "every sentence of PA can be relettered so that each quantifier introduces a new variable." How should that best be formalized in PA, according to Sergei? and why?

Indeed the standard textbook proofs of G1 and G2 depend on the fact that many arithmetized statements in the metatheory of PA are expressible by sentences in PA and have

conventional proofs in PA. Until we get some reasonably general explanation of which sentences in natural language are "best" treated schematically in PA there is not a lot of a proposal to attack or defend.

Practical questions of schematic versus sentential formalization in arithmetic are bread and butter topics in Reverse Mathematics. Personally, if I wanted to know more about this I would go to Friedman and Simpson.

« In fact, there are many people interested in the issue of formalisation (in maths and beyond), as well as some ongoing events and papers published on the topic. But they are, typically, not interested in sorting out things like ones discussed in Feferman 1960 and Sergei 2024. Perhaps because that stuff appears to many of them too technical and unimportant. I do believe that it is important. »

I am very sure many people who haven't actually worked on the subject would consider Feferman 1960 too technical. Are they your target audience for a conference?

People like Friedman and Visser (and me) consider Feferman 1960 so successful at what it does, and so productive of still-open problems, that there is no reason to cast about for new problematizations.

« Feferman's paper is not an easy read. What I learned from the first page is the fact that it is a summary of his Ph.D. made under Tarski. I guess that the thesis itself should be more readable since it should be less compressed. I'l try to find it, it should be, among other things, a useful overview of earlier works. »

Feferman 1960 is famously difficult. Albert Visser with some younger logician tried to write a commentary on Feferman 1960, but gave up. I tried the same before I knew Visser had. I am entirely confident the project looks easier to someone who has not tried it than to anyone who has.

I fear the dissertation will be less readable because it is a work in progress, and an earlier draft than the published one. But by all means look for it.

No, Andrei, you cannot tell without reading Feferman 1960 that it is a less mature work than Artemov 2024. You can only tell the author was younger.

« Here is another significant difference between Feferman 1960 and Sergei 2024, which I can identify even without knowing all the content of Feferman 1960. In his Ph.D. (as I can judge using the 1960 summary) Feferman reviews a great amount of earlier works and makes his improvements. This is what a good Ph.D. student is supposed to do in order to demonstrate his/her knowledge of the research area as well as his/her ability to proceed the "normal science" on that ground in the direction approved by the supervisor. This is normal for the academia [...]. Unlike Sergei 2024, Feferman 1960 does not challenge anything but proposes improvements. »

Good point. You are judging what is "normal for academia." Being a Tarski student at Berkeley was in many ways not normal for academia.

« What upsets me is not the fact that a significant number of logicians disagree with Sergei but rather the fact that I fail to see the same openness on their side. »

I have written very much more to you about Sergei than I have read by Sergei on those issues (but I haven't read his posts on FOM)..

« A philosophical critique is not supposed to be true, it is rather supposed to be valid and formally correct. ... Perhaps I should leave out "formally correct" here altogether. »

But then the same issue arises for "valid." They are just less important for "valid" since that word is so commonly used as a content-free term of approval.

Colin

04.12.2024 Andrei to Colin:
« Artemov's selector proofs have gotten wide attention, but not enough to satisfy you. »

If you know about some traces of discussions on and about Sergei's selector proofs (beyond FOM) please let me know. This is not for challenging you on what is "enough" in terms of attention, I'm really interested to learn about the content of such discussions. I may miss them.

« Alternatively, you could compare Frege's challenge with Sergei's proposal yourself. »

I think that Frege's view on logic including his challenge on Hilbert is very much behind constructively-minded logicians making part of the "Proof-theoretic semantics" trend. Sergei's take on logic shares a lot with these people's view (he also uses the concept of witness) but socially he prefers to stay independent. At least this is how I see the place of his work on today's map. Anyway, if we talk about the legacy of Frege in today's logic it would make sense to discuss it in a broader context that Sergei's PA paper.

My observation is that these days logicians prefer not to dramatise differences between different philosophical interpretations of logic but be ecumenists. This is good as far as it allows 100 different flowers to blossom. But at the same time this policy downplays interesting controversies like that between Frege and Hilbert, which could be still interesting and productive today and the new contexts. I like and support Sergei's independent position because it makes his work open to attacks from all parties; if he would join the PTS club (or perhaps some other appropriate club) he could easily protect his non-orthodox approach to G2 from such attacks, they would simply not happen. Within such a protecting club his work (perhaps modulo some changes in wording and notation) might be seen as orthodox or at least not deviant. And this would eliminate the opportunity to go into interesting and important discussions in a broader logical community that his work presently provides.

« Until we get some reasonably general explanation of which sentences in natural language are "best" treated schematically in PA there is not a lot of a proposal to attack or defend. »

I fully agree that Sergei's proposal needs a fuller account on formalisation. This is my major motivation to organise a workshop on this topic about which I wrote you earlier. I don't have immediate answers to your questions. Those are good questions.

« Practical questions of schematic versus sentential formalization in arithmetic are bread and butter topics in Reverse Mathematics. Personally, if I wanted to know more about this I would go to Friedman and Simpson. »

It would be great if these people would agree to discuss these issues with Sergei. I didn't look at FOM either so far, perhaps there are some interesting reactions from their part. (I had a bad experience from how these two people reacted on FOM to Volodya Voevodsky's ideas about PA some 15 years ago, I unsubscribed then from FOM for that particular reason. But this is an old and hopefully irrelevant story, and I hope that with Sergei it may work better than that.

« I am very sure many people who haven't actually worked on the subject would consider Feferman 1960 too technical. Are they your target audience for a conference? »

There is a relatively large community of people interested in formalisation outside of the community of logicians reading Feferman 1960. It includes people working in math education, computer science, philosophy and perhaps some other people. Most (if not all) of them would consider Feferman 1960 too technical. I myself can read and understand such papers with an appropriate effort only because in my childhood I had a strong mathematical training (which had nothing to do with logic or philosophy, of course).

« People like Friedman and Visser (and me) consider Feferman 1960 so successful at what it does, and so productive of still-open problems, that there is no reason to cast about for new problematizations. »

This is exactly why I have reservations about the "normal science". It may be very productive indeed to pursue the research of open problems set by Feferman 1960 but ruling out "new" (which historically may be well very old) problematizations is a sort of blindness that hardly makes anything good for science (for logic in this case). Of course such new problematization can be done by some new people who will not wait for Friedman's and Visser's (and your) approvals for that. This is very much what repeatedly happens in history. But everybody would gain if these new people with their new problems learn something useful from Feferman, Friedman, Visser, and you - rather than simply ignore their (your) works and start from scratch.

« I have written very much more to you about Sergei than I have read by Sergei on those issues (but I haven't read his posts on FOM). »

I really appreciate it, Colin.

« A philosophical critique is not supposed to be true, it is rather supposed to be valid and formally correct. ... Perhaps I should leave out "formally correct" here altogether. - But then the same issue arises for "valid."

Essenin-Volpin defines proof as any honest attempt to convince another person (I can find the exact quote). "Honest" is the key word here, so it is an ethical rather than formal or any other category. I tend to think that he is right on that point.

Andrei