This document is a lightly edited version of the mathematically substantive comments posted from April 6 - April 19, 2020 at this blog posting. The original source should be consulted for comments that might be of interest posted before and after the ones gathered here. For context, please consult the original blog posting as well as this later one. This document was provided to the editors of PRIMS who acknowledged its receipt but have decided that the Mochizuki proof should be deemed correct and complete and published, despite the serious objections raised here and elsewhere.

For a detailed background to this discussion, one should first consult the 2018 documents written by Scholze/Stix and Mochizuki (as well as Mochizuki/Hoshi) available at Mochizuki's website. David Roberts points out there is a more detailed version of what he has to say here.

Peter Scholze *April 6, 2020 at 9:28 am*

I may have not expressed this clearly enough in my manuscript with Stix, but there is just no way that anything like what Mochizuki does can work. (I would not make this claim as strong as I am making it if I had not discussed this with Mochizuki in Kyoto for a whole week; the following point is extremely basic, and Mochizuki could not convince me that one dot of it is misguided, during that whole week.) It strikes deep into my heart to think that in the name of pure mathematics, an institute could be founded for research on such questions, and I sincerely hope that this will not come back to haunt pure mathematics.

The reason it cannot work is a theorem of Mochizuki himself. This states that a hyperbolic curve X over a p-adic field K (maybe with some assumptions, all of which are always satisfied in all cases relevant to IUT) is determined up to isomorphism by its fundamental group $\pi_1(X)$, and in fact automorphisms of X are bijective with outer automorphisms of $\pi_1(X)$. Thus, the data of X is completely equivalent to the data of $\pi_1(X)$ as a profinite group up to conjugation. In IUT, Mochizuki always considers the latter type of data, but of course up to equivalence of groupoids this makes no difference. (The passage back and forth is even constructive, by another result of Mochizuki.)

Mochizuki claims that by replacing X by $\pi_1(X)$, things can happen that cannot otherwise happen. Examples are given concerning the action of $\pi_1(X)$ on certain associated monoids. We discussed this at very great length in Kyoto, but none of these examples carried any actual content. Note that any potential non-commutativity of some diagram that results from identifying $\pi_1(X)$'s via isomorphisms of X's could not possibly be resolved by using some other isomorphism of $\pi_1(X)$'s – all of them come from isomorphisms of X's! Mochizuki considers infinitely many distinct isomorphic copies of $\pi_1(X)$'s, but could not tell us what goes wrong if we simply identify all of them with one another, and with $\pi_1(X_0)$ for some fixed X_0 – there is no diagram that commutes in his situation but does not commute under this further identification. (In my manuscript with Stix, we simply went through Mochizuki's argument with this further identification, pinpointing what goes wrong. If this further identification causes problems, just tell us which diagram it is whose commutativity is rescued by not explicitly identifying $\pi_1(X)$'s.)

However, what I really want to do with this comment is to point out that there seems to be significant confusion over just the above point on X's vs $\pi_1(X)$'s. Recently,arXiv:2003.01890v1 appeared, in which the author (Kirti Joshi) gives some survey on results related to Mochizuki's work. In the introduction, on page 7, he explicitly claims that one could find non-isomorphic X's giving rise to the same $\pi_1(X)$, and even more, in Remark 2.1 on page 14 he explains that my reading of the situation is a common misunderstanding. Even more, in Corollary 21.2 on page 47, he states something "well-known to everyone at RIMS" giving an explicit example of this phenomenon of non-isomorphic X's giving rise to the same $\pi_1(X)$.

With this appearing on arXiv, I was indeed quite confused – did I in fact misunderstand this basic point all this time? If the above claims would have been true, I would see how Mochizuki's strategy might have a nonzero chance of succeeding. But I was quite sure that in our discussions in Kyoto, Mochizuki agreed with me on that basic point; and the proof of Theorem 21.1 in that survey (of which Corollary 21.2 is indeed a corollary) was wrong. In any case, I emailed Joshi indicating my confusion, and he has since checked back with Mochizuki and retracted all of these claims (he told me a new version will be on arXiv soon). In particular, the fact "well-known to everyone at RIMS" is wrong, and in contradiction to this earlier correct anabelian theorem of Mochizuki.

Taylor Dupuy *April 6, 2020 at 1:00 pm*

Hi Peter!

First, hope your pandemic is going well. Mine is going ok. Hard to get things done without daycare.

Second, let me say that for hyperbolic curves over a p-adic field K (with no extra hypotheses like strictly Belyi type or Belyi type or canonical lift) that $\pi_1(Z)$ "determines" Z is open. Also, I personally would advocate against using words like "determines", "reconstructs", etc that have been causing sooo many problems in discussing all of these things. For the uninitiated, let me say that what Peter claims here is that outer isomorphisms of fundamental groups as topological groups are in bijection with isomorphisms of curves over a field K. The difference between what Mochizuki did in his relative case together with his interpretation of G_K and the absolute Grothendieck conjecture I claim is subtle (maybe I am missing something though, I'm a little intimidated saying this so publicly to be honest).

–to see this is a non-trivial topic consider for example the introduction to this manuscript here:

`http://www.kurims.kyoto-u.ac.jp/preprint/file/RIMS1892.pdf`

Mochizuki's theorem states that for Z and W hyperbolic curves over a p-adic field K, outer isomorphisms $\pi_1(Z) \to \pi_1(W)$ *over* G_K — meaning they morphisms in an overcategory where $\pi_1(Z) \to G_K$ and $\pi_1(W) \to G_K$ — are in bijection with isomorphisms between Z and W.

To prove this, it suffices for example to show $\pi_1(Z)$ interprets the field K. I have written down an unreviewed proof which I don't think it is so difficult which I can share if you want.

Anyway, I think what you are thinking about is that $\pi_1(Z)$ admits an interpretation $GG(\pi_1(Z))$ naturally isomorphic to G_K and since we have both the fundamental group and the augmentation map we should be in the setting of Mochizuki's theorem and are done. This is not quite correct. What is at issue is that given $f : \pi_1(Z) \to \pi_1(W)$ one does not necessarily know that GG(f) is inner (which would puts you into the hypotheses of the Mochizuki's proof of the *relative* Grothendieck conjecture).

Also, Mochizuki has conjectured in print it is absolute Grothendieck over p-adic is not true—-See Remark 1.3.5.1 of this paper:

`http://www.kurims.kyoto-u.ac.jp/~motizuki/Absolute%20Anabelian%20Geometry.pdf`

but seems to be nonspecific nowadays. I don't know. We would need to ask him.

I personally believe if it is true then there is sort of a Zilber trichotemy/diochotemy thing going on. That is "absolute Grothendieck over p-adic" iff $\pi_1(Z)$ interprets a field. Maybe I am using Zilber trichotemy/dichotemy wrong, but I stand by the statement. The reason for this is because in all the special cases where absolute Grothendieck over a p-adic field holds, (canonical lifts, Belyi type, strictly Belyi type) there is an interpretations of fields. Maybe this is wrong though and a reader can point out an example where we know the absolute Grothendieck conjecture over p-adic fields without some special hypothesis being imposed.

Third, your counterpoint is a heuristic and not a disproof. For example, Dieudonné modules are equivalent to finite group schemes and we don't call them worthless. We could go blue in the face coming up with examples of two equivalent objects one of which is useful and the other which is not. Maybe I am missing something, but I'm not sure how productive these meta discussions are. I think your point is that he needs to "use something"/"do something". I agree, but it is not a disproof.

Alternatively, we can argue who has the burden of proof here... which I think is a more compelling argument for rejecting the whole thing. I think we can all agree the notation in his manuscripts are a big dumpster fire.

Fourth, regarding the deformation theory, his entire theory is up to a generic isomorphism (=polyisomorphism) the base G. That is the fundamental objects he considers are (like) a fundamental group Π with a map $\Pi \to G_K$ where G_K is considered up to automorphism. In fact, any time he does one of these polyisomorphisms (which I think we should be calling generic isomorphisms), we should probably be thinking of some sort of poor man's universal family.

Next we can ask "does this *DO* anything"? Well, for one, it certainly provides a formalism for talking about how things change under automorphism. There are a lot of interesting representations of automorphisms of fundamental groups acting on various interpretations which seem to me to be vedry non-trivial... etale theta being a principal example. Here is another thing I haven't been able to puzzle out but maybe you can help: Let Z be a hyperbolic curve over a p-adic field. Fix the augmentation map $f : \pi_1(Z) \to G_K$, let $g : G_K \to G_K$ be an outer isomorphism. What is the base change of f by g?

$$\pi_1(X) \times_{G_K, g} G_K = ???$$

It is like some sort of Frobenius twist looking thing—except not twisted by the Frobenius but by some outer morphisms of the fundamental group of the base. Is this the fundamental group of a curve? Anyway, it seems to me you can apply many of his interpretations to this object and that constructions are uniform in this sort of thing. This I am not 100% on and wish I didn't have to talk about these things I don't understand so well publicly. Maybe the readers can tell us what breaks here.

So, that's all I have to say about that for now.

Best, Taylor

W *April 6, 2020 at 1:56 pm*

@ Taylor Dupuy

Your clarification is very helpful for arbitrary curves, but as you say at the beginning, is relevant when there are no extra hypotheses like strictly Belyi type. Since IUT focuses on étale fundamental groups of once-punctured elliptic curves, and these are of strictly Belyi type, I don't see why they are helpful.

In case anyone following is confused about what theorem of Mochizuki is meant, it is Corollary 1.10 of his paper "Absolute Anabelian Cuspidalizations of Proper Hyperbolic Curves". (Not his earlier theorem about arbitrary curves.) [*see next comment for correct reference*]

Even if this "strictly Belyi type" condition were somehow avoided, then the existence of extra isomorphisms of the abstract fundamental group would still be an open problem. It seems hard to imagine how these isomorphisms could ever be used in a proof of some concrete inequality between two real numbers without proving that at least one exists.

It is probably possible to make rigorous a lot of what Peter is saying, that any proof of XYZ form cannot possibly work, or maybe more precisely that Mochizuki's proof is equivalent to the wrong proof sketched by Scholze and Stix.

But one also should include not-completely-rigorous evidence when deciding on how much burden of proof to assign to the author of a paper. The fact that every plausible use of a particular mathematical construction would not help in a particular argument, but the construction adds unnecessary complexity to the argument, and thus would make it harder to see any mistake, and two smart mathematicians have tried to remove the construction and found an incorrect argument, is strong evidence against the paper, even though it is possible that (1) there is a different way to remove the construction which leads to a correct argument or (2) there exists a clear explanation of how the construction is used to solve a problem which is not possible without it.

> Fix the augmentation map $f : \pi_1(Z) \to G_K$, let $g : G_K \to G_K$ be an outer isomorphism. What is the base change of f by g?.... Is this the fundamental group of a curve?

Isn't the content of Mochizuki's theorem that, if Z of strictly Belyi type, then this is not the fundamental group of a curve? Of course this may be a curve in some other case.

Vesselin *April 6, 2020 at 2:28 pm*
@ W

Just to fix your precise reference for the convenience of others following along: You mean either Corollary 2.3 on page 500 of that paper (Absolute anabelian cuspidalization of proper hyperbolic curves); or Corollary 1.10, part (iii) on page 43 of the subsequent paper (Topics in absolute anabelian geometry III).

Those disprove Corollary 21.2 of Joshi's preprint.

Peter Scholze *April 6, 2020 at 4:54 pm*

Taylor, thanks a lot for your answer!

Regarding your first point: Yes, I'm doing fine in these strange times; I hope you are too. Fortunately we are still allowed to enjoy the incoming spring.

Regarding your second point: As W observes, your objection only seems relevant when the curves are not of strictly Belyi type, but all the relevant ones for IUT are. So this objection is a red herring. (Meanwhile, it is clearly interesting to sort out whether strictly Belyi type is necessary, and I wish you luck in improving Mochizuki's anabelian results!)

Regarding your third point: Of course I am very well aware of the power of category equivalences. But there must be something you can do on the other side that you can't do on the first. As I said, during one week Mochizuki was not able to give a single relevant example. So to me it seems like a category equivalence that simply obfuscates things.

Regarding your fourth point: I am at a complete loss what one wants to do with full poly-isomorphisms. For the convenience of readers following along: A full poly-isomorphism between two isomorphic objects A and B of a category C is the set of all isomorphisms between A and B. Mochizuki often says that he identifies two objects A and B along the full poly-isomorphism. To be clear, this is (up to equivalence of categories) no data at all, there is a unique full poly-isomorphism, so you can't "pick one" (or rather, you can always pick one, and only one). On the other hand, to identify to objects in a category, you need to pick a specific isomorphism! Of course, you can just pick any one of them, but you can't pick all of them! If A and B are sets and $a \in A$ is an element, then what is the image of a in B under the full poly-isomorphism? It makes no sense. Mochizuki has repeatedly told us in Kyoto that because some diagram does not commute when A and B are identified via the obvious isomorphism (that usually exists in his situation), he has to identify them only along the full poly-isomorphism. But (in his situation) there is not a single isomorphism between A and B that makes the diagram commute! So how does the full poly-isomorphism help? You can't make the logarithm map into a map of fields by pre- and postcomposing with field automorphisms!

(To be clear, I am willing to accept that there is a nonzero chance that some of these things might make sense under certain circumstances. Again, let me stress that we discussed these very matters for one week in Kyoto.)

From what I understand, the objection to my manuscript with Stix is that we did some identifications that are not allowed. This is just the identification I was talking about: Mochizuki considers infinitely many distinct copies of $\pi_1(X)$ and is only allowing himself to identify them along the full poly-isomorphism. We do not see any diagram that commutes with this choice, but does not commute when we identify them along the identity map, once taking all $\pi_1(X)$ to be equal to the actual $\pi_1(X_0)$ for our fixed curve X_0. In any case, the passage from X's to $\pi_1(X)$'s is giving you no extra flexibility, as discussed in my previous comment.

Best wishes, Peter

Taylor Dupuy *April 6, 2020 at 6:39 pm*

Hopefully this is productive...

second point (and W's comment on the base change thing):

I'm not sure how much to engage here since this isn't strictly logically necessary for a disproof. Since I think this is an important point in terms of our understanding of what is going on I'm going to say a few words. First, for the readers, Peter and W are focusing on the bad non-archimedeans cases, I believe. In this situation, I'm not 100% sure that all the curves in what I would call the "zoo of covers" are SBT but certainly most of them are as they are analytifications of base changes of hyperbolic curves from number fields. This is Belyi's Theorem. There are a number of other points that we should consider: there are log structures too which should make things even more rigid (my understanding is that you need this information for reconstruction of the special fiber), you have these punctured tempered universal covers (these are certainly not algebraic), there are groups involved in mono-theta environments going on, and stacks fundamental groups. My point is that it is still conceivable to me that there is some sort of deformation theory going on and I don't want to confirm or deny this. But yeah, a lot of them are. The base fields certainly are not.

In terms of uniformity of constructions in G or "strange base changes"; Mochizuki performs many constructions with respect to (Π, G) where for a pair we consider a generic isomorphism $\mathbf{G}(\Pi) \to G$ – here $\mathbf{G}$ is the interpretation of a structure isomorphic to G_K in Π. This makes G "up to automorphism". You need Lemma 1.1.4.ii of "Absolute Anabelian Geometry of Hyperbolic Curves" for the interpretation $\mathbf{G}$.

W: "Isn't the content of Mochizuki's theorem that, if Z of strictly

Belyi type, then this is not the fundamental group of a curve?"

I would say no, this is not the content of his theorem. But I don't know what you mean here. Mochizuki's theorem is a statement about morphisms not objects. The begining of the claim is

> "*given* that $\Pi \to G$ and $\Pi' \to G$ are augmented fundamental groups of hyperbolic curves..."

There may be statements that allow you to classify when a group is the fundamental group of something but you will have to ask Daniel Litt or Emmanuel Lepage or Jakob Stix about this. They certainly will be much more knowledgeable than me. By the way, this is what Mochizuki means when he calls things "bianabelian".

Let $\Pi = \pi_1(Z) \times_{G_K,g} G_K$. I think you can run the $\mathbf{G}(\Pi)$ interpretation on this thing. I haven't worked out what this does... it is unclear if $\mathrm{pr}_2 : \Pi \to G_K$ is naturally the same as to $p_{\mathbf{G}} : \Pi \to \mathbf{G}(\Pi)$.

Also, Peter, don't hold your breath waiting for new results in this direction. What I said is pretty much the extent of what I "know" about absolute Grothendieck conjecture over p-adic fields.

Also, I think we can all agree whether something can change or not due to Mochizuki's consideration of potentially lossy functors is one of the more interesting parts of this story and it is important that we as a community clarify the situation. I think Kirti's emphasis on this is a good idea.

fourth point:

`https://imgflip.com/i/3vr2vv`

Full polyisomorphisms should be considered as "generic isomorphisms". I completely agree with this viewpoint. There are some instances where you have polyisomorphisms which not full and these keep track of finite indeterminacy rather than an arbitrary choice.

For better or for worse Mochizuki decided this is the language he wanted to use to describe these things. When things are omitted without justification the statements made are no longer Mochizuki's. We can get mad at him all we want for over questionable style choices but it doesn't change his assertions. This, I believe, is grounds for rejecting a paper, but it doesn't disqualify the proof.

I want to give a couple comments on polyisomorphisms for the readers.

Comment A) First, I want to point out a really really really bad style problem that comes with this choice of "polyisomorphism language": Many commutative diagrams involving full polyisomorphisms are tautologically commutative which makes many claims vacuous.

example: Theorem 3.11.iii.a, the $\vdash \times\mu$ prime strip commutativity statements.

These are everywhere.

This style choice forces the readers to search unnecessarily for non-full polyisomorphisms which, frankly, is a big pain (an example of a non-full polyisomorphism can be found in the definitions of the bridges of the Hodge Theaters for example). This doesn't make him wrong though. Just not the best expositor.

Comment B) If A and B are isomorphic objects then $\mathrm{Isom}(A, B) = f\,\mathrm{Aut}(A) = \mathrm{Aut}(B)f$ for any fixed isomorphism $f : A \to B$. In my head I always "push" the generic isomorphism into an arbitary automorphisms of one object. This is pretty tautological and Peter already does this but I just wanted to say that applying this systematically allows you to reduce a lot of things and perform computations.

Comment C) I have spent a bit of time literally identifying objects if they had a polyisomorphisms between them. Although you can actually get some pretty interesting "global objects" this is not what Mochizuki had in mind. One example comes from the so-called full polyisomorphism mono-theta environments and the log-linked fields. If you do this you end up identifying a bunch of roots of unity in a bunch of different fields – this is well defined because the fields are all log-linked. Anyway, my point here is that this is not what Mochizuki had in mind so you maybe don't want to do this.

(I can talk at length about other style issues. Another example is invocation of interpretations "in Hodge Theaters". This is one I feel that just sends readers on a wild goose chase reading page after page of definitions. Things are typically defined by much less and in an optimal presentation one shouldn't consider superfluous structure when a reduct will do.)

Ok, so what do generic isomorphisms do besides confuse readers?

First, they are intended to keeps track of automorphisms. Mochizuki's theory is really an investigation of the behavior of interpretations under automorphisms and permutations of the interpreting structure. I say permutations because sometimes they don't respect the category and are maps of sets. The stupidest example I can think of: Let $G \to G'$ be a map of groups with kernel N. Set theoretic permutations of G which fix cosets gN setwise induce the identity on G'.

Here are some example questions Mochizuki addresses: –How does the Kummer class of (an l'th root of a pullback of) the Jacobi theta change when you take an automorphims of $\pi_1^{temp}(\underline{\underline{X}}^+)$? –What about the evaluation points (conj classes of decomposition groups)? –What types of automorphisms/actions stabilize what construction? (I am thinking about mono-theta environments here and the purpose they serve) –What happens to a the measure space we construct when we take automorphisms of the interpreting G? (this is what Ind2 is)

Second, generic automorphisms can serve as a sort of "poor person's deformation space". I think Kirti discusses this well in his updated manuscripts so I'm not going to talk about this so much. I think, as Kirti has suggested, we should be asking ourselves which of these generic isomorphisms are actually representable. Emmanuel Lepage, if he is reading, might be able to say more about the usage of full and essentially surjective functors from the perspective of Gerbes. He told me something about this once.

Third, regarding your "what is the image of an element under a full polyisomorphism". This goes back to what the point of all these things are. We are really investigating interpretations under automorphisms. At the end of the day we are looking to construct a "multiradial representation of the theta pilot object" which is a region obtained by a procedure involving certain orbits. This is supposed to be 1) independent of any choices and 2) relatable back to our original P_q. We partially address (1) in my first manuscript with Anton which should be available soon. You can find some stuff on my vlog about indeterminacy diagrams. Anyway, the point of these comments is that the polyisomorphisms are Mochizuki's way of dealing with choices, it is also safe to make a choice but you need to make things independent of this choice.

fifth point:

–Regarding your manuscript with Jakob, the construction as you have stated imposes two normalizations that are alleged to be simultaneously enforced which lead to a trivial contradiction of the form A=B and A!=B. I think we all agree on this. Mochizuki says this is a straw man. I can't find these assertions in the manuscript.

I do however think that statements of the form "All proofs that use X must have property Y" could be very useful provided 1) we can make X completely rigorous and 2) Mochizuki's language can be pinned down in a way that makes X verifiable.

Aside: Actually, this was part of my motivation for looking at these interpretations. If you can show that two objects are equivalent (e.g. Frobenioids and pairs (Π, M)) then if there exists a proof using Frobenioids then there exists a proof using pairs (Π, M). These sort of reductions allow you to make assumptions about the structures that are used in the proof. This is what safely allows us to get rid of unappealing constructions.

I personally find a lot of the language very hard to falsify/parse.

–Regarding the sentences "This is just the identification I was talking about: Mochizuki considers infinitely many distinct copies of $\pi_1(X_0)$ and is only allowing himself to identify them along the full poly-isomorphism. We do not see any diagram that commutes with this choice, but does not commute when we identify them along the identity map, once taking all $\pi_1(X)$ to be equal to the actual $\pi_1(X_0)$ for our fixed curve X_0."

First, I don't think this is a faithful presentation of Mochizuki's setup but one thing I can say that might make you feel better: in this particular example of bad non-arch primes setting $X_0 = \underline{\underline{X}}_v$ (this should be a double underline) then automorphisms of the fundamental group do all sorts of things to the zoo of covers. All of those dihedral symmetries act, all of the $\underline{\mathbb{Z}}$ symmetries act etc. Also, all of the stuff that is not interpreted from a fundamental group stays fixed. This seems particularly relevant in the context of "monotheta cyclotomic synchronization", but again, if I could finish the proof we wouldn't be having this discussion at all.

Sorry if I made any mistakes anywhere...

DL *April 7, 2020 at 8:09 pm*

Taylor — you've written quite a lot, but frankly I don't really see why anyone should continue discussing this until someone can point to a specific spot in the IUT papers which defeats Peter's objection. For example, if I understand correctly, you suggest some juice might be obtained by looking at maps of π_1 which are not over G_K. If so, where are such maps used in IUT? And Peter gives a specific challenge — point to some diagram whose commutativity is rescued by not making the identifications Peter makes. If there is such a diagram, where is it?

David Roberts *April 7, 2020 at 10:19 pm*

> not making the identifications Peter makes

I'm convinced there is no such diagram, because that is not how category theory works. The whole issue with identifications (as in: demanding objects are distinct copies vs having them be the same object) is a red herring. Mochizuki doesn't want to do it for spurious technical reasons that one can ignore (or alternatively, humour him and agree to play along). I'm more suspicious that people could be thinking about objects that are living in different categories. But what do I know?

DL *April 7, 2020 at 10:32 pm*

David — indeed there are objects in different categories being identified here (eg a curve and its fundamental group). That's because we're speaking English, not trying to make formal mathematical statements. Formally, one might (for example) look for a diagram which is not commutative but whose image under some functor becomes commutative, say. That is something that can happen in "category theory." In any case, I agree it's likely there's no such diagram.

David Roberts *April 7, 2020 at 11:21 pm*
@DL

There are more people reading here than just experts on arithmetic geometry, or even mathematicians. I'm also writing for their benefit. I'm just pointing out that your interpretation of Peter's challenge is not really the question one should be asking—or at least, the way it comes across to me.

But oh well. I should keep my head down, perhaps. Much easier to discuss this not via the internet, but we are all locked away after all.

W *April 8, 2020 at 8:45 am*
@Taylor Dupuy,

> I would say no, this is not the content of his theorem. But I don't know what you mean here. Mochizuki's theorem is a statement about morphisms not objects. The begining of the claim is

> "*given* that $\Pi \to G$ and $\Pi' \to G$ are augmented fundamental groups of hyperbolic curves..."

Despite what DL mentioned, I just want to respond to this bit…

OK but if we're asked if something happens and we're trying to prove that it doesn't happen we can try a proof by contradiction.

We have a curve X over K and a map $\pi_1(X) \to G_K$. We fix K' and an isomorphism $G_K \to G_{K'}$ which does not come from an isomorphism $K \to K'$. You ask, does the composition $\pi_1(X) \to G_K \to G_{K'}$ arise from a curve X' over K'?

Suppose it does. Then we have two hyperbolic curves X, X' and an isomorphism between $\pi_1(X)$ and $\pi_1(X')$. Then Mochizuki proves that, if X is of strictly Belyi type, this isomorphism arises from an isomorphism between X and X'. In particular, it implies the isomorphism on the Galois part arises from an isomorphism K to K', contradicting our assumption.

In fact, given Mochizuki's previous proof of the p-adic Grothendieck conjecture theorem, the new theorem (except for the part about open injections, maybe) is precisely equivalent to the statement that the "base changes" of fundamental groups of Belyi curves are not fundamental groups of curves, plus the fact that the geometric part is identifiable as a normal subgroup.

kirti joshi *April 8, 2020 at 10:15 am*

Dear Professor Woit,

This is to clarify the claims made about my paper in the context of your abc blog-post. First of all let me say that my paper (referred to by Scholze in his comments) is not a survey of Mochizuki's work (though it may initially appear so because I state a number of standard results without proofs) and contains a number of new and original results (Scholze agrees with me on this). I start with one of Mochizuki's ideas and I build upon it in my paper. This is the paper which is cited in Scholze's comments and which can be found on the arxiv (though the update is not yet ready). In our correspondence Scholze has agreed (mostly) with all the changes in the new version and we (i.e. Scholze, Hoshi and myself) continue to correspond to resolve any persisting issues (of which there are very few). These issues and changes, at any rate, do not pertain to main results of the paper but to how the contents relate to IUT. Since comments about the retracted section of the paper may add to the confusion in a topic which is already quite complicated for many reasons, so all references to my paper (below) will be to the forthcoming version.

To explain one of Mochizuki's important ideas, let us begin with a classical result which says that there exist p-adic fields (i.e. finite extensions of the basic p-adic field $\mathbf{Q}_p$) which are not isomorphic but which have topologically isomorphic absolute Galois groups. For examples of such fields see my paper. (In the 1990s this was refined by Mochizuki: a p-adic field is determined by its topological absolute Galois group equipped with its ramification filtration (see the section: five fundamental theorems of ... in my paper) for references to proofs of these results).

Two p-adic fields with isomorphic absolute Galois groups have distinct additive structures (the multiplicative groups of non-zero elements of such fields are even topologically isomorphic). So the additive structure is the one which is changing (even as the absolute Galois group remains fixed). Because of Mochizuki's Theorem one can view the (upper) ramification filtration as the Galois theoretic manifestation of the additive structure of a p-adic field.

One of Mochizuki's ideas, simply stated, is to treat the p-adic field as a dynamic variable while keeping its absolute Galois group fixed. Because of the above remarks, this should be seen as treating the additive structure of the field as a (dynamic) variable. If readers are uncomfortable with this idea, they can simply think of allowing the p-adic field to vary while its absolute Galois group remains fixed. This makes complete sense and comes with highly non-trivial consequences as my examples illustrate:

(1) I show in my paper (with explicit numerical examples) that two p-adic fields with isomorphic absolute galois groups do not have the same different and discriminants (these are standard measures of complexity of fields in number theoretic contexts). These examples can be easily verified by any one with a computer equipped with SAGEMATH and importantly more can be found by my methods.

(2) I show that if E/F is an elliptic curve over a p-adic field F and if L,K are two p-adic fields with isomorphic absolute Galois groups and both containing F, then the base changed curves E_K and E_L (i.e. E considered as curves over L, K respectively) do not have the same list of numerical invariants (in general). This is done by means of explicit examples computed using SAGEMATH (with no additional programming needed). Notably my examples establish quite clearly that the additive structure of the p-adic field controls many subtle invariants of elliptic curves over p-adic fields. At any rate computing these examples does not require any of Mochizuki's theory.

Important realization on which my paper is based is this: the upper numbering ramification filtration is a Galois theoretic stand-in for the additive structure of the field and through this stand-in, the additive structure leaves its imprint on Galois representations.

(3) Notably invariants in (1) and (2) are also the sort of invariants which are crucial in Szpiro's conjecture and my work shows that these quantities are affected by the changes in additive structure and so Mochizuki's idea of using the variation of the additive structure to understand Szpiro's conjecture might have significant merit (there are several other new ideas in Mochizuki's paper as he has reminded me on a number of occasions). Note that I am not claiming that this is exactly what happens in the context of IUT, but I am simply reporting my observations that these quantities are not determined by the isomorphism class of the absolute Galois group of the relevant p-adic field. I do not know how to use my examples to illustrate changes in the specific context of IUT.

(4) I also demonstrate that the idea of changing the additive structure (which I have called anabelomorphy in my aforementioned paper) can be used in the

theory of Galois representations. In this theory L,K are two p-adic fields which have isomorphic absolute Galois groups, so one can pass from representations of G_K to G_L. This does not affect many broad aspects of the representations as these two groups have equivalent categories of finite dimensional representations. However this operation of considering a G_K representation as a G_L representation via any given isomorphism of these two groups does not preserve (p-adic) Hodge theoretic properties of a representation (for example an Hodge-Tate representation of G_K may not remain Hodge-Tate when considered as a G_L representation via an isomorphism of G_L with G_K). However I prove that an important subcategory, namely ordinary p-adic representations is preserved under this operation. This operation of viewing G_K representations as G_L representations is not the identity functor in general nor is it so on the subcategory of ordinary representations. One can say such things because I also demonstrate that important numerical invariants of a Galois representations, for example its Swan conductor changes (again there are explicit numerical examples which I provide). The fact that this operation does preserve ordinary representations is of importance not only in Mochizuki's work (which uses two dimensional ordinary representations arising from Tate elliptic curves), but also in the broader theory of Galois representations (Wiles, Taylor and most results in the area since then). This result opens up the possibility of wider applicability of Mochizuki's ideas to other areas of number theory. For additional results readers are referred to my paper.

These ideas, proofs and examples have nothing to do with the one theorem of my paper which I have admitted was incorrect (and the new version will appear on the arxiv on thursday or friday this week) and my error should not be viewed by the readers as an example of what is wrong in this business. [I sincerely apologize to my colleagues and friends in Kyoto and Japan for the incorrect statement in the old version of my paper and my assertion that this (incorrect statement) was "well-known to everyone in Kyoto".] Again let me be clear that my errors (if any) should not in any case be attributed as issues emanating from Mochizuki's paper.

(5) I explain in my paper that even though K,L have topologically isomorphic absolute Galois groups, it is possible to communicate meaningful (arithmetic) information between them. For p-adic fields this idea is due to Mochizuki. There is no direct interaction between the additive structures of these fields at any point.

(6) In my paper I have also pointed out the analogy between Scholze's work (deeply extending earlier work of Fontaine) and Mochizuki's idea (see the section on perfectoid spaces in my paper) and in our personal correspondence Scholze has said that he sees no issues with my claims in that section (modulo minor corrections). In particular I point out in that section that Scholze's work is founded on a similar idea of changing perfectoid fields, perfectoid varieties (instead of p-adic fields and curves over p-adic fields) while keeping the absolute Galois group (of the perfectoid field) fixed (resp. etale fundamental group fixed). In the parlance of perfectoid geometry this corresponds to moving from one untilt to another untilt–see my paper for details (or Scholze's paper). Deepening of

this analogy (as Taylor Dupuy and I hope to do in an ongoing project) should provide further insights into this difficult topic.

(7) The following way of remembering Mochizuki's idea may be useful:

A p-adic field wiggles and moves around in the isomorphism class of its absolute Galois group. This wiggling is a (new) degree of freedom in number theory and in algebraic geometry.

(This is illustrated in my paper with explicit examples and also see Mochizuki's paper and Hoshi's work and other members of the Kyoto school).

Peter Scholze *April 8, 2020 at 11:46 am*

Dear Kirti Joshi,

thanks for chiming in here, and I'm sorry for concentrating on the parts of your paper/survey that were wrong.

The issue of non-isomorphic p-adic fields that have isomorphic absolute Galois groups is potentially interesting, and it is worthwhile to study which invariants are (un)changed under such an isomorphism. However, it is unclear to me how this enters into the actual content of IUT. In particular, your example of taking an elliptic curve E over a field p-adic field F and base-changing to p-adic fields K/F and L/F with isomorphic absolute Galois groups is not relevant to IUT. Namely, $\pi_1(E_K)$ and $\pi_1(E_L)$ are not usually isomorphic, although $\pi_1(E_K)$ and $\pi_1(E_L)$ both can be computed as pullbacks $\pi_1(E) \times_{G_F} G_K$ resp. $\pi_1(E) \times_{G_F} G_L$ where all terms are isomorphic – but not the fibre product, as the maps $G_K \to G_F$ and $G_K \cong G_L \to G_F$ are not the same. (This was the essential mistake in Joshi's first version.)

Regarding (6), perfectoid geometry gives nontrivial examples of such relations, which however require a strong "softening" of algebraic varieties (to pass to perfectoid spaces). Let me stress that perfectoid spaces have precisely this flexibility of changing geometry while preserving topology (like π_1), while Mochizuki's theorems I alluded to in the first comment prove that for the hyperbolic curves he considers, the geometry is determined by the topology (in fact, by π_1). It is clear that being able to change geometry while fixing topology can be interesting – but Mochizuki is just not in a setup where this is possible!

Finally, regarding (7), while Mochizuki claims that his proof must use this fact that p-adic fields are not determined by their Galois groups in some way, it never actually enters – in particular, no construction of such exotic isomorphisms is given or cited. In this sense, as you also say at the end of (3), this whole discussion seems tangential to IUT.

Best wishes! Peter

Taylor Dupuy *April 10, 2020 at 4:58 pm*
W:

I agree with your observation. Nice.

Here is my summary (slightly modified).

Lemma. *Let X be an SBT curve over a p-adic field K. Let $\Pi = \pi_1(X) \times_{G,g} G$ where $g : G \to G$ is an outer automorphism (=not inner).*

The map $\mathrm{pr}_2 : \Pi \to G$ is not isomorphic to the map between a fundamental group of an SBT curve to its base field. (Maps between maps are taken to be the obvious pair of maps satisfying the usual commutative diagram).

Proof. Suppose it is. First the map $\Pi \to G$ can be viewed as $\Pi \to \pi_1(X) \to G$, or pr_2 (indexing starting at 1 and not 0). Call this map h. In this case we have a diagram

$$\begin{tikzcd}
\Pi \arrow[r, "\mathrm{pr}_1"] \arrow[d, "h"'] & \pi_1(X) \arrow[d] \\
G \arrow[r, "g"] & G
\end{tikzcd}$$

Where the bottom map is the outer g. If they were both SBT by Mochizuki's relative Grothendieck theorem + interpretability of the field we have that pr_1 is geometric (the Grothendieck theorem is not just about isomorphisms but all morphisms). But the bottom map is outer. This gives a contradiction. (I'm assuming the p-adic relative Grothendieck for morphisms here... which I need to double check on). $\square$

I can't parse your last remarks completely...

> In fact, given Mochizuki's previous p-adic Grothendieck conjecture theorem, the new theorem (except for the part about open injections, maybe) is precisely equivalent to the statement that the "base changes" of fundamental groups of Belyi curves are not fundamental groups of curves, plus the fact that the geometric part is identifiable as a normal subgroup.

Here are a couple more points (I'm not trying to be a jerk):

a) What do you mean by "the new Theorem"?

b) We still don't know if $\mathrm{pr}_2 : \Pi \to G$ is the same as $\Pi \to \mathbf{G}(\Pi)$ where $\mathbf{G}$ is the interpretation I referenced previously.

c) I think we only get that $\mathrm{pr}_2 : \Pi \to G$ is not the natural map from an SBT curve to its base not that it is not the fundamental group of any hyperbolic curve.

I believe we can resolve (b) and (c) with the Lemma below.

Lemma. $\mathrm{pr}_1 : \Pi \cong \pi_1(X)$ *is an isomorphism. In particular* Π *is isomorphic to the fundamental group of an SBT curve.*

Proof. Let $f : \pi_1(X) \to G$ be the map from the fiber sequence. Since $\Pi = \{(a, b) : f(a) = g(b)\}$ the kernel of the map pr_1 contains elements of the form $(1, b)$. Since $f(1) = g(b)$ this means b is in the kernel of g. Since g is an isomorphism $b = 1$ and hence the map pr_1 is an isomorphism. $\square$

This means the composition $\pi_1(X) \to G \xrightarrow{g} G$ is something that Mochizuki considers but it not isomorphic (as maps) to some $\pi_1(X) \to G_K$ of "geometric origin".

Also, I think you made an earlier comment about verifying the inequalities. I personally think people should be looking at the Mochizuki's inequalities after 3.12 but before Theorem 1.10 type inequalities. In my manuscript with Anton we point to a couple places where improvements can be made; there seems to be a lot of room between the two inequalities. To do direct computations with Cor 3.12 it seems you need to work directly with Division Fields as in the work of Harris Daniels, Alvaro Lozano-Robledo, and Drew Sutherland. Stuff like this:

`https://alozano.clas.uconn.edu/wp-content/uploads/sites/490/2014/01/lozano-robledo_minimal_ramification_Rev1_v2.pdf`

(Maybe you can email me and we can talk about this more if you are interested.)

DL:

More comments on p-adic Grothendieck:

The existence of some isomorphisms of fundamental groups of curves over p-adic fields inducing isomorphisms of absolute galois groups of p-adic fields "not of geometric origin" seems to be related to the section conjecture but I couldn't figure out if there was an "obvious" implication. See the results here:

`http://www.kurims.kyoto-u.ac.jp/preprint/file/RIMS1892.pdf`

I wanted to say something like "if the section conjecture is False then there exists a hyperbolic curve Z over a p-adic field K and some automorphism σ of $\pi_1(Z)$ such that $\mathbf{G}(\sigma)$ is not inner." but I may be totally totally off here. Maybe you can salvage this? I'm trying to get a sense of how difficult this should be by reducing this to a "really hard" problem.

Also, while the pro-p relative Grothendieck conjecture is true the pro-p section conjecture is False. This is a Theorem of Hoshi. I don't understand these counterexamples but it is my understanding that Jakob views these as "accidents" or "lucky". (pg 192 of his evidence for the section conjecture book.)

I'm going to postpone any more remarks about missing monoid structures in the setup for a later post (if I'm going to say anything at all) because I want to get it right for everyone.

UF *April 11, 2020 at 9:02 pm*

As far as I understand the main objection brought up here by Peter Scholze is: Mochizuki considers infinitely many distinct isomorphic copies of $\pi_(X)$'s, but can not tell us what goes wrong if we simply identify all of them with one another, and with $\pi_1(X_0)$ for some fixed X_0 - there is no diagram that commutes in his situation but does not commute under this further identification.

It seems to me that this is adressed in (C7) in Cmt2018-05. Mochizuki claims there that if one (once and for all) identifies the various $\pi_1(X)$'s in one column (using the non-archimedean logarithms), then there is no switching symmetry between the two neighboring columns. On the other hand, if one identifies the $\pi_1(X)$'s just as topological groups via poly-isomorphisms, then Mochizuki claims that there is such a switching symmetry (i.e. some diagram commutes at this weak level).

It seems to me that in the proposed proof both viewpoints are used: The ability to rigidify the relationship between the $\pi_1(X)$'s in one column when needed (e.g., I think, on both columns for the log-Kummer correspondence), and then later the ability to forget about the rigid structure, to pass from the LHS to the RHS.

Peter Scholze *April 12, 2020 at 4:58 pm*

As UF is trying to point to a specific objection to my manuscript with Stix, and it's the first real try in this thread, let me try to answer this.

Actually, the objection is bizarre. If all your copies of $\pi_1(X)$ are the same, then how can the situation be less symmetric? It's totally symmetric. It's not symmetric if you also look at some other structures present in Mochizuki's setup, like the log-link: This just means that the logarithm map is a different map from the one you obtain by switching source and target, which is self-evident (the logarithm is not its own inverse). It also cannot be salvaged by using some other/indeterminate identification of $\pi_1(X)$'s – this would at most change source and target by some field automorphism. So it's unclear to me how the passage from X to $\pi_1(X)$, and to $\pi_1(X)$ up to indeterminate isomorphism, is helping in this matter.

W *April 13, 2020 at 9:49 am*

@Taylor Dupuy

I'm just saying that you can reverse the argument to an extent. Given two curves X_1, X_2, with an isomorphism of their fundamental groups, if you check that the geometric π_1 of X_1 is sent to the geometric π_1 of X_2 by this isomorphism, then it follows that this isomorphism arises by "base change" from an isomorphism of Galois groups of local fields. If this isomorphism of Galois groups of local fields arises from an isomorphism of fields, then the fact that X_1 is isomorphic to X_2 comes from Mochizuki's first p-adic Grothendieck conjecture theorem. If this isomorphism of Galois groups of local fields does not arise from an isomorphism of local fields, then we can get a contradiction if we know that base changes of strict Belyi type curves are never a curve. So Mochizuki's theorem that strict Belyi type curves are determined by their π_1s, is basically equivalent, modulo these prior results (I think…), to a special case of your question.

Taylor Dupuy *April 13, 2020 at 12:07 pm*

W's comment has insprired me to write a little bit more since he seems to be paying close attention (thank you W).

It seems to me that Peter S. has two claims:

1) One involving the diagram in his manuscript with Jakob in section 2.2. That the setup here gives a contradiction is undisputed. The fact that the setup is valid is disputed (see C8 of Mochizuki's comments). It also seems very dubious to me that the contradiction would be so tautological and I can't find these claims in the manuscript.

2) That one can identify all fundamental groups at bad non-archimedean places using the identity and this will not change anything (maybe he wants to assert this for every structure in a "base Hodge theater"?). This includes across the theta link and across log links. This is in say Footnote 8 of their manuscript.

Peter, how do you propose to show that (2) implies (1)? I also don't understand the level of your assertion. Do you want to conjecture (2)? Do you want to claim (2) as a theorem? Do you want to claim/conjecture that (2) is true and implies some contradiction?

There is something in footnote 12 of your manuscript that I spent a little time with and if it is supposed to claim this is the explanation of (2) implies (1) then it seems to be squeezing around 15 assertions into one footnote and omits the explicit claim that (2) implies (1). In particular I don't see how to recover the normalization that needs to be simultaneously in force in (1) to derive their contradiction.

Anyway, I still see Peter's arguments as still being in the realm of a meta argument.

Maybe it is obvious to him and everyone else how to apply the definitions in IUT3 and convert these into proofs but this is not obvious to me. Again, for me 3.11 is made up of about 15 different assertions and uses a large number of definitions that I don't have at my fingertips...

Peter Scholze *April 13, 2020 at 4:25 pm*

Let me take Taylor's comment as an opportunity to more clearly state several related but distinct criticisms that are explicit or implicit in my manuscript with Stix, and the previous discussion on this thread.

(1) The non-necessity of passing from X to $\pi_1(X)$

(2) The non-necessity to replace $\pi_1(X)$ with infinitely many distinct copies of it

(3) The inconsistency of the identification of various copies of ordered 1-dimensional real vector spaces

Off these, only (3) points to (what seems to us) an actual mistake. (1) is about the question whether anabelian techniques – which are supposed to be at the heart of the matter – are of relevance. (2) is about whether the huge diagrams that Mochizuki considers are actually relevant to the argument. Neither (1) nor (2) alone would really falsify anything; but if non-necessary, not much is left of these manuscripts.

As only (3) is about an actual mistake, we focused on this in our manuscript.

(1) is Remark 9, and (2) is Footnote 8 in my manuscript with Stix. In my first comment here, I concentrated on (1), while point (2) was addresses in my second comment in response to the fourth point raised by Taylor. [I should apologize that while my current (2) corresponds to the (2) of Taylor's last comment, it is my (3) that corresponds to his (1).]

The reason I brought up (1) and (2) here is that if one only stresses (3), as we did in our manuscript with Stix, then it may seem plausible that we simply misunderstood something at this point of the argument, but some quite powerful machinery had been built and one could plausibly finish the argument differently. However, (1) and (2) mean that quite the opposite, all machinery that is in place seems to have no power.

To me, (1), (2) and (3) seem logically independent. For (3), we explained everything in Section 2.2 of the manuscript with Stix: What the various ordered 1-dimensional real vector spaces are, what identifications one wishes to do, and that these identifications lead to nontrivial monodromy, i.e. are inconsistent. The loop one has to take in order to get the monodromy is rather large (6 identifications). I have seen no convincing argument that this nontrivial monodromy does not lead to problems. Of course, one could just decide to cut the loop at any point, but then one has to make sure that the argument never uses that disallowed identification. In particular, one has to decide in advance where to cut – if you know where, please let me know. (That in various parts of the argument only small, locally consistent, parts of the diagram are relevant, does of course not help as the argument altogether must be consistent. So the only way to achieve a consistent argument is to decide not to use one of these identifications.) I agree that this contradiction is very tautological. It seems the more surprising to me that it can be altogether neglected. (Yes, Mochizuki can't possibly mean this. But regardless of what he means, this inconsistency is simply there!)

About (1), Mochizuki's anabelian theorem states that relevant X's are equivalent to relevant $\pi_1(X)$'s, which is as much as can be asked for regarding a proof.

About (2), it's hard for me to prove this (and it's not required for the main point, (3)), as Mochizuki of course asserts that this is false, and there are thousands of diagrams one might have the occasion to look at, and I can't look into Mochizuki's mind to find all of them. What I can say, and this is basically Footnote 12 in my manuscript with Stix, is that we checked that Theorem 3.11 holds up with (2) in place, and in fact is completely tautological, so if anything Footnote 12 is meant to "prove" (2) to the extent this seems possible. More generally, (2) seems extremely plausible to me, and with all diagrams I've looked at in Mochizuki's papers it was holding up; and if you doubt it, you can just answer to my challenge of pointing to a single diagram whose commutativity is rescued by allowing some indeterminate isomorphism.

I do not claim that (2) implies (3) or anyway leads to a contradiction, although my recollection is that Mochizuki said that (2) alone would contradict his papers – he agreed that it is impossible to think that his argument might work if all of these copies are simply the same. (Maybe the idea is that one of the identifications we use to get nontrivial monodromy in (3) is omitted by having distinct copies of $\pi_1(X)$'s. But that's not actually the case.)

As I said, it's very easy to convince me that (2) is wrong: Just point to one diagram whose commutativity is rescued by allowing this indeterminate isomorphism of $\pi_1(X)$'s.

David Roberts *April 13, 2020 at 8:31 pm*

I'm sorry to keep banging on about it, but Peter S's (2) is an artefact of a weird approach to diagrams in categories that Mochizuki is using. Reading M's 2018 Report closely, he claims things like that you can't define manifolds if you build them out of colimits of diagrams where the objects are all the same 'copy' of $\mathbf{R}^n$ (this is LbEx5). Or that you incorrectly calculate the perfection of a ring if in the usual sequential colimit you don't create separate copies of the ring in advance (this is LbEx3). This is patently absurd, but makes sense if one assumes that diagrams *must* be injective functors, or rather, literal subcategories. Recall that M assumes that he is identifying isomorphic functors, so that the concept of a diagram qua functor is severely underdetermined. Working up to isomorphism like this, and replacing a diagram with one that produces an isomorphic (co)limit, one can safely assume that diagrams are subcategories—but it is super weird, and it took me ages to realise that was what he was thinking. This is why he talks about things like "forgetting histories", because he is thinking that you need to somehow create fresh, distinct copies of objects in order to not collapse the subcategory down, and thereby give a different diagram. So when someone versed in standard category-theoretic language says "let's identify these objects", he seems to hear it as "let's collapse this subcategory to something trivial". And when he says "I need distinct copies", it seems totally weird and unmotivated. So when I look at LbEx3 in the 2018 Report it looks like the sort of mistake a student would make, when learning category theory for the first time. The problem is his conceptions of basic notions seem to be so idiosyncratic that without a serious translation filter, what he is saying seems to be completely off the wall.

If one takes category theory seriously, and DL poked a bit of fun at me for this (we talked privately afterwards), then one can *at the level of foundations* forget the equality predicate on the objects of categories, so that the equality or otherwise of random objects of a category is not even something you can consider. Alternatively, one can pass to a skeleton of the category at hand, in which case you just don't have distinct copies at hand, but *nothing breaks*. So there can be zero mathematical effect of the whole issue of distinct copies or otherwise, it is merely a psychological crutch. Once one realises this, the actual mathematics can then be discussed, for instance something like creating a formal colimit of a diagram all of whose objects are $\pi_1(X)$ (or whatever), with appropriate gluing maps. But this is not what I wanted to address, and is out of my sphere of expertise.

UF *April 13, 2020 at 10:16 pm*
@Peter Scholze

Thank you very much for addressing these comments of Mochizuki relevant to your (2). Let us consider the simplest situation where we just consider two neighboring columns of log-links and disregard for the moment the theta-link (I think we agree compatibility with theta link at most creates more trouble).

Then even in this simple situation (omitting theta-, but keeping log-links) Mochizuki seems to claim (C7) in Cmt2018-05 that if one rigidifies the $\pi_1(X)$'s in the columns (by identifying them using identity maps, making the logarithms Galois-equivariant), there is no "switching-symmetry" permuting the two columns.

As you say, this seems quite bizarre, since one has two columns of isomorphic data, so why should one not be allowed to switch?

I think a way out may be the following: It seems quite likely Mochizucki uses "switching-symmetry" in a technical sense, synonymous with "multiradiality" of some algorithm reconstructing the data (here a rigidified column of log-links) at hand from some choric data, as he often does, compare e.g. [Alien, p.51].

His statement would then mean that if we rigidify the vertical columns, then there is (unlike in the non-rigidfied case) no multiradial algorithm to recover this column from certain choric data. This does not sound so absurd anymore (to me).

Now which multiradial algorithm does he mean here? I would suggest it may be the multiradial algorithm in [IUT III, Cor 2.3], more specifically, the first part of 2.3 (ii) which concerns its compatibility with log-links. Note that close by, [IUT III, Rem 2.1.1 (ii)] the issue we are talking about "why π_1 only upto indeterminate iso?" is discussed. For further discussion see also in [IUT II, Rem 3.6.4 (i)].

In any case, I agree this is an important issue to track down.

Taylor Dupuy *April 14, 2020 at 5:30 pm*(written before seeing next comment)

I think we are mostly on the same page now. No worries about the indexing. I'm going to use your indexing in what follows.

–Regarding (3): The "cut" is supposed to occur at identification of the theta side of the theta link; in that global realified frobenioid we don't normalize that Picard group according what you would want the degree of the theta pilot object to be. For the interested reader, details are in my manuscript with Anton after we introduce theta pilot divisors. I think Mochizuki includes this in one of his responses too.

–Regarding (1) and (2) not implying a contradiction (directly): we agree. I would consider (1) and (2) open as well. We agree on that too. Also, as I'm sure everyone will agree, statements need to be pinned down a bit more and directly tied to what Mochizuki has written. So I think even *exact* statements of (1) and (2) are also open. I will say a little more about (1) below.

For reasers who want to look at this, the obvious thing to do is to isolate a single claim in IUT and just start running with it, then build out from there.

–Regarding not using automorphisms of fundamental groups: we should observe that without indeterminacies, there are no indeterminacies. This is tautological, yes, but indeterminacies appear in the statement of Cor 3.12 and without them we are really talking about something else entirely.

Also, this all goes back to how the indeterminacies allegedly afford us the ability to compare the "volume" of the hull of the multiradial representation of the theta pilot region with the degree of the q-pilot divisor (definitions can be found in my paper with Anton)—this is the infamous "Mochizuki switcharoo".

I could talk more about this more but right now but I will be stating a bunch of (useful) isolated facts that readers would need to assemble for themselves (if it is even possible to do so). I think I might be burned at the stake for this as well as run the risk of making mistakes in public!

–Regarding (2): I think we need more language about "infinitely many copies" but basically I'm of the same mindset as David on this. I think everyone agrees we shouldn't think about "different copies" so much in the same way that you don't need two copies of $\mathbb{R}$ to talk about $\mathbb{R}^2$. I'm not sure this is what you or Mochizuki means though Peter. On the other hand there certainly are cases where you need multiple monoids.

–Regarding (1): we basically agree here. I think groups may be much more convenient and I think the representations are definitely important. The statements Mochizuki makes involve etale theta and the reconstruction of evaluation points and the representation of $\mathrm{Aut}(\pi_1^{\mathrm{temp}}(\underline{\underline{X}}_{\underline{v}}))$ on these interpretations (here we need to take an analytification or formal scheme with log structure). Technically speaking, I suppose $\mathrm{Aut}(\underline{\underline{X}}_{\underline{v}})$ acts on the same objects but maybe it isn't as easy for me to see these actions.

–Getting your hands dirty in the definitions like UF has begun doing is the way to proceed in these investigation. I can give some basic definitions of "switching" but I believe they are not adequately developed for discussing IUT3.3.11. Maybe we should take that offline? Whatever you guys want. Mochizuki discusses the formalization of multiradiality in a remark following IUT3.3.11 but that remark mostly says "you can do it" without any details as I recall.

–As a side remark, and I know Mochizuki would hate me saying this, it does feel like mind reading at times. I strongly agree with this sentiment. He tries though. I believe attributions of malintent are misplaced. But still, lots of mind reading.

Peter Scholze *April 14, 2020 at 5:17 pm*

David Roberts: Thanks for your thoughts on this!

Actually, something related happened in our Kyoto discussions: We realized that we could not get on common grounds regarding the issue of whether one needs separate copies of a ring to form its perfection, so we decided that we simply have different psychological crutches (as you call it) on this, and that we better focus on some actual mathematical statement where related issues indisputably become important. However, no such focal point ever appeared, despite us going through the essence of the IUT manuscripts. So it seems to us that to the common mathematician, his whole big log-Theta-lattice essentially comes down to one Hodge theater – which is really just the elliptic curve you started with (the category of Hodge theaters is equivalent to the category of elliptic curves isomorphic to your given one) – together with the p-adic logarithm map ("the log-link") and some isomorphism ("the Theta-link") of two copies of a local Galois group acting (trivially) on a monoid isomorphic to $\mathbb{N}$ (I'm obviously simplifying, but not too much – I'm basically considering only one bad place, while you have to consider all places, but I'm already telling you about the most interesting place). The generator of this monoid $\mathbb{N}$ is one time regarded as the value of the Tate parameter q, one times as the value of the Theta-function (a collection of such values, really, but never mind). But of course this isomorphism of abstract monoids is totally incompatible with these "interpretations". Initially, one might think that Mochizuki claims that there is some isomorphism of the pair of (local Galois group acting on local units) that takes the q-value to the Theta-value – this would obviously have great consequences, and would probably require the use of exotic isomorphisms of local Galois groups – but this is just not the case, one can easily give counterexamples; and it can't even be true locally "up to blurring", only a global statement, averaging over all places, can be true. The isomorphism is just on the level of (local Galois group acting trivially on monoid isomorphic to $\mathbb{N}$) and the relation of this monoid isomorphic to $\mathbb{N}$ to the local units is completely external.

So for all we can see we simply followed the procedure you suggested, and reinterpreted his distinct copies in the way usual mathematicians think. We recorded the outcome of this in our manuscript: You can read there the details of what everything is, in particular that his log-Theta-lattice really boils down to essentially the simple data above. How could one possibly go from here to any nontrivial result? Of course, as also W suggests, all of this is much harder to see through in his language.

@UF

The remarks from IUT that you cite make heavy reference to his paper on etale theta-functions, which seems to play a key role in the IUT papers. This paper gives some neat algorithm to start from the fundamental group of a once-punctured elliptic curve with bad semistable reduction, and recover its Tate parameter q and some Theta function; I forget the details. While this is all good and well, I don't see the relevance: Mochizuki's more general anabelian theorems, discussed previously on this thread, tell you that from the fundamental group you can simply recover the whole curve. In these comments of Mochizuki that you reference, Mochizuki is discussing some nitty-gritty details of this algorithm, but this seems completely besides the point if you just remember that relevant $\pi_1(X)$'s are equivalent to relevant X's, so of course you can recover all invariants of X, and you can do so functorially in $\pi_1(X)$.

Generally, a point seems to be made that Mochizuki's algorithms have some magic power and that really the content of the algorithms is critical, so in the context of the previous paragraph, it would matter in some way how I invert the functor $X \mapsto \pi_1(X)$ using some explicit construction (or that I don't actually invert it but only read off Theta-values using some other roundabout algorithm). This seems very surprising to me.

Regarding Mochizuki's algorithms, let me add that I was surprised that the following procedure counts as an algorithm for Mochizuki.

In IUT-3, Theorem 3.11 (i), an algorithm is discussed, that does the following. The input is data concerning only p-adic fields; it is basically a profinite group isomorphic to the absolute Galois group of your given p-adic field. The output is something like the Theta-value of the elliptic curve you chose at the beginning of the IUT papers (cf. part (b) of that data).

How is this possible? The input data doesn't even know anything about the elliptic curve! This is completely magic!

The resolution is that the elliptic curve has indeed been fixed once and for all in these papers, and so of course you can produce that Theta-value – simply look at the elliptic curve you have fixed, and take its Theta-value.

Of course, there is some packaging done around this, but this is the essence of this "algorithm"; Mochizuki confirmed this.

Taylor Dupuy *April 14, 2020 at 6:07 pm*

Quick comments:

–Peter, I think the $\mathbb{N}$'s should be regarded as embedded inside a monoid with enough roots of unity and nth roots to do Kummer theory.

–You definitely need more than one part of the "Frobenius-like" objects of the Hodge Theater — the abstract monoids. I also think you should think about overloading free variables; this thing is some sort of F'd up quotient of a free construction. This of course isn't precise.

–The term "algorithm" is trash. I think it is a big part of the problem. Also, we should note that has been going on in anabelian geometry for a very long time and it isn't a "Mochizuki thing". For the most part I have found that algorithms = interpretations in infinitary first order model theory. And I very very emphatically agree with you that it does *not* suffice to treat "functorial algorithms" as functors alone, you need more. This is certainly omitted in Mochizuki's exposition.

Also, this interpretations stuff also goes off the rails a little bit...

1) you need to be able to consider structures up to automorphism as well.

2) There is also a backwards version of this where he considers "lifts" of structures that interpret lower structures. I talk about this a little in my first manuscript with Anton.

–Regarding 3.11.ii this is an independence results. He is saying it doesn't matter which lift of all these things that interpret the absolute galois groups "at the bottom" you take. This thing you construct independent of the lift (by the way this points to the automorphisms of fundamental groups you were looking for).

Here is a funny observation: there exists a “shitty multiradial representation” where you can take the union of all images of the theta pilot region in measure spaces cooked up from absolute galois groups of p-adic fields at $\underline{v} \in \underline{V}$. This is ALSO independent of the lift. Proving that it is “multiradial” is tautological.

Yet, this one doesn’t use log-links and it isn’t claimed that this ”shitty multiradial representation” can be related back to the minimal discriminant.

Peter Scholze *April 14, 2020 at 7:05 pm*

Dear Taylor,

first, thanks for explaining where to do the cut. In your comment, I don’t actually understand where you want to cut, so I’ll have to look at your note with Hilado. (In my manuscript with Stix, which direction does the isomorphism go? Horizontal/slanted? In the lower or bottom half? In the left or right half?)

About (2) not leading to a contradiction, you actually made me realize that probably it does: I don’t see why part of Mochizuki’s indeterminacies, I believe for example (Ind2), is necessary, but omitting it leads to a form of ABC that is provably too strong. I agree it would be hard to “prove” a strong enough form of (2), but I think the burden of proof is on Mochizuki here, to show where the argument needs indeterminacies like (Ind2) – which is basically the issue (2) we’re discussing.

Reading your first comment above, I actually don’t really see where we disagree.

Regarding your second comment, the relevant $\mathbb{N}$’s are manifestly not part of monoids in which you can do Kummer theory! Yes, Mochizuki includes some extra factor, but that’s just along for the ride.

About the last bit, to be sure we’re on the same page: I was talking about part (i), not (ii), of Theorem 3.11.

Peter Scholze *April 14, 2020 at 7:37 pm*

OK, I quickly looked at your manuscript. If I understand it right, that’s the upper left slanted arrow? In our terms, that’s the difference between the “abstract” Theta-pilot – a generator of an abstract monoid $\mathbb{N}$ – and the “concrete” Theta-pilot – the actual Theta-values of your elliptic curve. (Mochizuki seemed to conflate the two originally, or so it seemed to us.) If you cut there, then the Theta-link no longer links anything to actual Theta-values! This of course removes any inconsistencies, but it also removes what’s supposed to be the key, namely the identification of q-values with Theta-values, in some form.

On the other hand, I read say on page 140 of IUT-3 that Mochizuki considers the Theta-intertwining, which I believe simply means this identification of abstract Theta-pilots with concrete Theta-pilots. He wants to be very careful with using this etc., but I do believe he wants to (and has to) use it somewhere. So I don’t think you can simply cut there. I think the closed loop that

Mochizuki discusses on page 143 of IUT-3 is also relevant here. See also on page 144 the simultaneous Theta-intertwining and q-intertwining he wants to have (up to indeterminacies etc...).

Taylor Dupuy *April 14, 2020 at 8:37 pm*

Yep, the theta pilot doesn't map to the actual theta values *on the theta side*. On the q-side it does.

so yeah, you don't normalize the degree on the left hand side of your diagram. It is like we are all saying you can't something be equal to two different things at the same time. . .

Also there is only *one* copy of the real numbers.

Taylor Dupuy *April 15, 2020 at 5:17 am*

Peter: Below is a more detailed response.

To recap, you made three posts. One in response to David and UF, and then two smaller ones. I'm going to address the newer ones first and then go back and address those older comments.

Responses to Newer Comments

Regarding the "cuts": I think I addressed this. Let me know if you want to talk about it more. I think if you are not taking two log-linked strips of Hodge Theaters and only have one theta monoid it is going to get rough. He wants all the computations in on particular Hodge Theater in the log-linked strip on the q-side to be the "usual normalization" (sorry for referencing such large objects, I know this is super abusive... I'm sure there is a better way to talk about this than to make blanket references to Hodge Theaters — really the relevant "official constructions" here use the $\prec$ prime strips (I think) in the Hodge Theater... that is not a fun reference chase.)

Regarding (2) [infinitely many fundamental groups]: So I think we need to clarify this what you mean by (2). I'm not sure which indeterminaciees you want to get rid of. At some points I see that you are thinking about using "one fundamental group" and at some points you are saying "use one Hodge theater". I think these have different consequences — one is about representations on monoids and one is about the groups solo. Are you wanting to get rid of ind3 and this log-kummer correspondence? What do you want to kill exactly?

I agree with you that tracing the proof for simplifications which removes or modifies inequalities to the point where they are false is a good strat for finding flaws.

Aside: I will always agree with you that the burden of proof is on the writer to explain things. In modern arithmetic geometry there is too much flexing on the reader. IUT is a bit of a weird flex in some ways.

Regarding $\mathbb{N}$'s and Kummer Theory in the $\Vdash\!\blacktriangleright \times\mu$ prime strips: As a recap: You stated that they are isolated, I said they have a Kummer theory. I want to clarify: technically you are correct the monoids in a components at a bad place of $\Vdash\blacktriangleright \times\mu$ prime strip are just $\mathbb{N}$'s. BUT... they are interpreted structures. So they come from other structures where it does make sense. Note also that in isolation, there is no action of the $\mathbb{N}'s$ on the corresponding copies of $\mathcal{O}_{\underline{v}}^{\times\mu} \otimes \mathbb{Q}$. (Just as a reading note: Mochizuki sometimes calls this his "holomorphic structure". I think he also uses the words "embedding" or "link between unit group and value group portions"). I can speculate a little about what is going on on either side of the theta link if you want.

*Subremark. This is sort of an ongoing theme I want to highlight: Given a structures A and B there may exist two ways of interpreting B in A (called them $\mathbf{B}_1$ and $\mathbf{B}_2$) which are not equivalent. This for example could for example be distinguished by the representations $\mathrm{Aut}(A) \to \mathrm{Aut}(\mathbf{B}_i(A))$ (I'm not saying this is the case here, I'm just trying to give a concrete example).

*Subremark. Sometimes I find it useful to think about there being a single $\Vdash\blacktriangleright \times\mu$ strip that lives in different "charts" (after applying the correct automorphisms). I am not sure how helpful this is, but it is kind of fun to think about.

Regarding My Remarks on 3.11: A quick summary: You had stated how ridiculous it is to construct anything involving Tate parameters from absolute galois groups of p-adic fields. I said some words about lifting G's to an interpretation so we view G as $\mathbf{G}(\Pi)$ for some fundamental group Π and said these constructions are uniform in Π (let me say this is a cartoon picture right now, we actually need a lot more structure on this. In particular what I had in mind was a lift of the structures "at the bottom" of 3.11.i to log-linked collections of prime strips — I made need to modify the structures in this statement to make it exactly correct). I had made some remarks about 3.11.ii being an independence result. Let me clarify, I was talking about $U_\Theta \subset \mathbb{L}^{\vdash,et}$ (the "coarse) multiradial representation of the theta pilot region" inside the so-called "mono-analytic etale version of the log shells" all put together) being independent of the lift. Let me think about 3.11.i. I need to refamiliarize myself with this $\mathfrak{R}^{LGP}$ bullshit. If we are really going there right now then I think we need to introduce "abc-modules" (the primary data discussed 3.11.i and 3.11.ii). These are sort of pre U_Θ structures (used to construct U_Θ). I need to think about 3.11.i and get back to you. Also, we are in territory where I am pretty shaky. So... no promises...

Regarding "Theta Interwining": Can you give me a reference with respect to a Theorem/Remark/Environment of some kind? I think we are reading different versions of the manuscript. (Let me just make one side remark— there needs to be a stable version of these documents somewhere in print; it has obviously become an important historical document. I'm not saying where and how, it just need to be immutable.)

Response to Response to David Roberts and UF

Let me just say, I don't think your simplifications were an unreasonable guess (and I want to clarify to the readers that this is actually not what Mochizuki meant in his setup. I'm just saying the whole thing is so crazy you need to start somewhere.) I think we all agree though if you take one Hodge theater it won't work as per your manuscript with Jakob.

Regarding Magic Powers: I really like this comment. It is a very interesting thought regarding looking at the "conjugate constructions" from the perspective of curves. It does look funny. What is in this fundamental group sauce?! Here are some possible explanations:

1) There is more to what is going on than just fundamental group vs curves; Mochizuki uses the monoids and cyclotomes extensively. In particular he applies many "orthogonality" results.

2) Fundamental groups are first order structure: they are a topological space with a binary operation. The formalism of interpretations applies here (maybe by slightly increasing the signature), but you get to put everything in this nice box where represenations become automatic and you get to see how everything varies with respect to automorphisms. (Full disclosure, Mochizuki objects to the language of model theory for this stuff, he thinks it is unnecessarily complicating things. This is a matter of taste maybe.)

I think we need to unpack the comment "The resolution is that the elliptic curve has indeed been fixed once and for all in these papers, and so of course you can produce that Theta-value — simply look at the elliptic curve you have fixed, and take its Theta-value." What Mochizuki means is 'fixed in initial theta data' usually. I also want to repeat that I object to the usage of a single Hodge Theater. If $\overline{\mathscr{H}}$ is a log-linked collection of Hodge Theaters (which we view as a single massive infinitely sorted structure) the definable set we need is in like $\overline{\mathscr{H}}^2$ (I said "like").

Regarding the comment: "it can't even be true locally 'up to blurring', only a global statement, averaging over all places, can be true...." This is correct. His claim is a purely global statement. But... it is even WORSE than this! You need to take the hull and then take the volume and only then does he claim you see the comparison.

In this comment you also reminded me of some things concerning the shape of the inequality. I'm not going to do it now, but I'm going to make a note to try to talk about:

*Ind2 and moving around $q^{j^2/2l}$ – why you need Ind3

*Analytic number theory, what happens at large discriminants, and toy phenomenology for the inequality.

IOU: 3.11.i discussion.

UF *April 15, 2020 at 9:04 am*

I now think a good reason to for not identifying π_1's along log-links is as follows: (It is very similar to [IUT-2, Rem 1.11.2 (ii)] for the theta-link; I am less precise here, taking the relevance of some reconstructions on faith, but it is a intuitive story, and precision can come later)

First, let us step back and look at the log-theta-lattice: It consists of Hodge-theaters and the log/theta-links as maps/functors between them. Now a formulation of (2) is: Why do we, say for the log-links, not just take the "identity" isomorphism between the relevant π_1's instead of the full poly-isomorphisms between them? Surely it is also a completely valid map between the theaters?

Yes, but: let us recall what we want to do with this log theta-lattice: we want at "suitable times(?)" glue the Hodge theaters along these links and then apply certain multiradial algorithms.

I think that if we would glue with the "identity" on the π_1's (and all the other prescriptions of the log-link), then this gluing will be inconsistent/not well defined (when combined with some multiradial algorithms), in some vaguely similar sense as if you try to define a complex structure on a manifold by charts, but in some region the procedure leads to two different, incompatible complex structures, i.e. the transition map is not holomorphic.

Indeed, in one Hodge theater, the π_1 is not independent of the rest of the data; it acts via the quotient to the Galois group on the monoids. Thus π_1 is "related" to the monoids, and it is not apriori clear we are allowed to glue two Hodge theaters by gluing π_1 and the monoids separately in whatever way we wish (and then follow certain multiradial algorithms). In the above analogy, a certain region of the Hodge theater is supposed to be "determined" jointly by the π_1 and the monoids. The multiradial algorithm we consider is roughly as follows: The action of π_1 determines on the (from the Galois group reconstructed) local monoids (with 0) in addition a ring structure (a "holomorphic" structure). The output is roughly some étale version of some of the data originally present in the Hodge theater, and the reconstruction algorithm yields a canonical isomorphism between the original data and the reconstructed one (see e.g. [Alien, §2.12] for essentially such constructions).

If one now glues two Hodge theaters along a log-link (with in addition "identity"-gluing for π_1's), then this yields no consistent holomorphic/ring structure compatible with the above algorithm on some region: Namely, consider the region of the ring (local units), which is glued via log (the actual log part of the log-link) to the other Hodge theater; this yields one definition of "holomorphic" for this region. On the other hand, we can embed this region in the reconstructed monoids and use for the "holomorphic" structure on the reconstructed monoids the one induced by π_1 on both Hodge theaters and glue by the "identity".

The above roughly describes two incompatible holomorpic/ring structures on some part of the glued Hodge theater; they are incompatible, "because the transition map, essentially the log, is not a ring map". Such a situation is presumably avoided by taking the full polyisomorphism between π_1's, where the log map on the units alone defines the holomorphic structure on the relevant region.

Peter Scholze *April 15, 2020 at 2:05 pm*

Dear UF,

thanks for your answer. (Taylor, I will answer in a separate comment later.)

Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.

The only way I see to make this into a valid thing is if the indeterminacy somehow allows you to map the logarithm map into a map of rings – after all, this is what you critized in the first place. But any automorphism of π_1's will only ever replace source and target by isomorphic rings. So the logarithm will still not be a map of rings. Note that I'm simply repeating what I already said in my second comment in this thread.

Peter Scholze *April 15, 2020 at 4:31 pm*

Dear Taylor,

addressing your last comment.

First half: I was refering to the current version on his webpage. I realize that this is dangerous, but it's the only thing that at least currently makes it possible for everyone to follow. In any case, to me it sounds like you are saying that the Theta-pilot has nothing to do with Theta-values. Well, strictly speaking (modulo minor issues) what Mochizuki does is the following: He takes the Theta-value (some nonzero p-adic number, not a root of unity), and takes the (multiplicative) submonoid M of the p-adic numbers generated by it, and then considers M as an abstract monoid. As an abstract monoid, it is then of course isomorphic to $\mathbb{N}$, necessarily canonically so; its generator is referred to as a Theta-pilot. Does the abstract monoid M know anything about the Theta-value? Of course not! But that is all that ever seems to enter the story of the Theta-link. At some point you do need to remember the interpretation of the Theta-pilot in terms of Theta-values – this is what Mochizuki calls the Theta-intertwining, and it plays a key role as I tried to reference. So I don't think that Mochizuki simply cuts our diagram there.

Now David Roberts previously mentioned that Mochizuki seems to have a nonstandard point of view on what a mathematical object is, requiring separate copies when they are not actually needed; he often refers to some "history" of these objects. Now maybe one could try to argue that because M was built out of Theta-values, it (the abstract monoid) still knows something about them, that can be even transported via an isomorphism of abstract monoids. (You made some similar remarks in your paragraph on $\mathbb{N}$ and Kummer theory, referring to "interpreted structures".) But that's evidently not the case. (That the abstract monoid M knows something about Theta-values might be debatable if for you an abstract monoid is really encoded – as it is in ZFC – in terms of its actual set of elements. But even then, this structure ("the interpretation") is clearly not transported by an isomorphism of abstract monoids.)

About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, except because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that? (There was actually a point where Mochizuki was surprised that by "forgetting the history of a group" one still has more than a group up to isomorphism: Namely, a group. That the datum of a group is strictly more than the datum of a group up to isomorphism seemed new to Mochizuki. I believe this is the (psychological) main reason he considers these full poly-isomorphisms. But really, forgetting "the history of the groups" you still have groups and they may still have natural commuting isomorphisms between them. Of course, for groups up to isomorphism you can't ask for natural commuting isomorphisms between them, then you only have "full poly-isomorphisms".)

Regarding the rest of your post, I don't see how you're really objecting to anything I said.

UF *April 15, 2020 at 4:57 pm*
@Peter Schoze

I agree that the above should be surrounded by big caveats about the nature of the effects of passing to full poly-isomorphisms and I may interpret those incorrectly.

What my post above attempts to show, is: if passing to poly-isomorphism has the effect of doing no gluing/no identification of ring structures (arising from π_1, just the gluing from the actual log map), then the only gluing left is the actual log map, which gives one global chart, and no transition functions needed, essentially(?) since just one chart. I before never really seriously considered that full poly-isomorphism could have the effect of "no gluing arising from this part" (instead of "choose your favourite gluing"), but a similar thing is (I think) asserted for the case of the theta-link in [IUT II, 1.11.2(ii)], where the message is: full polyiso on π_1 means no sharing of ring structure arising from this.

Clearly this does not show by itself it really works like this, but it seems to reduce an argument for the log-link to some degree to an analogous one for the theta-link.

Peter Scholze *April 15, 2020 at 5:10 pm*

Dear UF (and Taylor and everyone),

I won't comment any further here on statements of the form "well, maybe Mochizuki actually meant (vague statement)".

A few comments up I summarized the situation with claims (1), (2) and (3). I have seen no valid objection to (1) and (2), and (2) alone would lead to a contradiction (as one gets too strong a form of ABC). To (3), Taylor indicated where to cut the diagram, but I really don't think this is what happens, as this would isolate Theta-pilots from Theta-values and effectively remove the actual Theta-values from the proof; while Mochizuki does consider this "Theta-intertwining" which is the association of the Theta-pilot with the Theta-values.

I will only comment further here if either a valid objection to (1) or (2) is mentioned, or further clarification is given regarding (3). Any further technical discussions are probably best done via e-mail.

Best, Peter

Taylor Dupuy *April 17, 2020 at 12:17 pm*

Hi Everyone,

I'm just going to make one last post to close out some loose ends for interested readers. I also want to point out some things that we haven't covered that I think are important. I've omitted the discussions of asymptotics of Mochizuki's formula but other than that, I think I covered my IOUs. If there are any analytic number theorists who were really looking forward to that please email me.

Sorry for the length.

Regarding (3): Is Mochizuki's Proof Falsified?

There is no *proof* that Mochizuki's method doesn't work.

The following is what the Scholze-Stix manuscript proves:

Theorem. Assuming that one may identify Hodge Theaters in Mochizuki's theory and simultaneously impose "concrete normalizations" of q-pilot and theta-pilot degrees then there is a contradiction.

Remarks.

1) The hypothesis of "simultaneously imposing concrete normalizations" is equivalent to $l(l+1)/12 = 1$ for a fixed prime l. In the manuscript no machinery from Mochizuki's theory is used in this derivation. A protest to this identification is made in C12 of Mochizuki's response. Despite this, Peter S. insists this is what Mochizuki means. See his comments above, or the quoted comments below.

2) The hypothesis that one can identify Hodge Theaters is also protested by Mochizuki and runs counter to his stated objective. It is prudent to observe that the thesis of the Gaussian integrals survey is that sometimes in Mathematics one can introduce "alien copies" of objects to prove something about your original object. This runs counter to the assumption of identifying Hodge Theaters in the above theorem.

I am not taking the position that Mochizuki's proof is correct or will turn out to be correct but that, as it stands, the Stix-Scholze manuscript should not be held up as a reason to reject Mochizuki's proof. On the contrary, I am just saying that a falsification of the proof requires a fluency in the definitions which seems hard or impossible to achieve. Many of the definitions use nonstandard terminology and come off as ambiguous to many readers (I'm not sure if I want to get into a definition of ambiguous). Case in point: questions about the "embeddedness" (or in Mochizuki's terms "holomorphic structures") of the monoids involved in the $\Vdash\blacktriangleright \times\mu$ prime strips; these came up in Peter's previous post. Moreover, because of certain ambiguities in these definitions, readers are forced to search a large space of possible meanings. While these difficulties do not falsify the possibility of a proof, they make it so that what Mochizuki has written is not a proof in traditional terms. To speak plainly, flexing on the reader by omitting details is not something new in Arithmetic Geometry and IUT is a weird flex.

Finally, it is not outside the realm of reality that there could be 5 top notch international referees who have understood the proof as complete and correct. PRIMS has traditionally been a journal of very high quality and members of the editorial board have shown integrity in the past (look at how senior mathematicians are responding when asked by journalists to comment). Let's see what the print version looks like (hopefully it has a table of contents and remove bolding and italics).

What can we do?: I think we should all think carefully about the damage omissions have next time we are tempted to do it. Additionally, we should praise the writers whose writing we find "easy" and useful. Because of these difficulties I personally hold Mochizuki's document as a program for proving ABC and consider Mochizuki's formula a conjecture (until I can see this with my own eyes). Then again, there are a lot of Theorems I know about and use but can't prove. Also, be nice to everyone, we are all just people.

Regarding (1)

I see nothing wrong with (1). By this I understand replacing all constructions in IUT involving the fundamental group of an SBT curve with the corresponding construction in an actual curve. I think this is possible, but we would need to check that this doesn't break somewhere. If the claim is that all fundamental groups in the theory need to be eliminated, I would be more reluctant. There are a lot of weird curves. I think I need to see what a more precise statement looks like in practice to say for sure.

Regarding (2)

Regarding (2), to be clear, we should repeat that this is not what Mochizuki does (only using one fundamental group). Also, the statement as Peter has stated is a moral one not to be taken literally – there are many more groups appearing in the construction that are not isomorphic to $\pi_1(X)$ (profinite etale) so, like, it doesn't literally parse but I think I know what he means. Regarding its validity, I have reservations related to conjugacy synchronization and construction of "the diagonal" (points in Mochizuki's theory are replaced by conjugacy classes of decomposition group and evaluation of functions is replaced by restriction of cohomology classes to decomposition groups – since we want to evaluate at many points simultaneously one needs them to be determined up to conjugation simultaneously – we need a diagonal action of a conjugation not an action by a product of conjugations. This process involves multiple fundamental groups.) — BUT if we are going to dispose of these things entirely as in (1) you can probably scrap a lot from section 2 of IUT2. I need to think about how this construction works in order to say something precise. I personally would ask Emmanuel Lepage or Jakob Stix about this.

A more middle ground statement is that one can replace all "base Hodge theaters" by a single "base Hodge theater"—which have more than just $\pi_1(X)$ and a bunch of maps between them. Yet another variant of this would involve contracting even more fundamental groups in the base Hodge Theater (and this is where I think you might run into conjugacy synchronization and diagonal problems).

Also, there are a lot of monoids built into the theory which are excluded from the statement and I don't know if these are implicitly assumed to be identified as well. I think not.

Finally, to derive a contradiction, I think some things need to be said about *how* one proposes to reduce Mochizuki's construction to "a single fundamental group".

All in all to build this into an actual counter-example, as Peter claims, one needs to make the statement more precise.

Remark. In a post I read someone emphasized that Peter S. read "hundred of pages", or that he met with Mochizuki "for a week". Similarly, Fesenko has emphasized the number of hours people have studied the theory. These are personal reasons not to undertake something and not acceptable substitutes for proofs.

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Now more comments...

Intertwining

> On the other hand, I read say on page 140 of IUT-3 that Mochizuki considers the Theta-intertwining, which I believe simply means this identification of abstract Theta-pilots with concrete Theta-pilots. He wants to be very careful with using this etc., but I do believe he wants to (and has to) use it somewhere. So I don't think you can simply cut there. I think the closed loop that Mochizuki discusses on page 143 of IUT-3 is also relevant here. See also on page 144 the simultaneous Theta-intertwining and q-intertwining he wants to have (up to indeterminacies etc. . .).

There is more of this on page 188 too (April 16, 2020 copy)

In the quoted statement above the "up to indeterminacies" is the point.

Mochizuki asserts the existence of TWO theta monoids you need to be thinking about relating. One that identified with q-monoid and the other which has been interpreted alongside q in the same Hodge theater. In Mochizuki's language one is in a "alien holomorphic structure" and the other is in interpreted from the same structure. Mochizuki's game is then relate the auxillary theta pilot using anabelian techniques (amphoricity in the language of Joshi's manuscript) in order to derive a relation between the two. This relation is a uniformity statement. All the thetas of the world can be blurred.

At one point I was confused about what is going on with the interpretations in absolute galois groups of p-adic fields. It is helpful to know that the measure spaces constructed from absolute galois groups of p-adic fields have measures which are well defined—in the initial construction in AAG3 (section 5?), the measures take values in an ordered one dimensional real vector space obtained by the process of 'perfection' (colim M, nodes = $\mathbb{N}$, morphisms=($a \mapsto ab$) realized as multiplication by b) and completion. It turns out that because of initial theta data, the we know how to normalize the measures to give real numbers.

My point: the constructions of these measure spaces may look weird, but from the perspective of either interpreting structure these things give real numbers, and the values of these measures don't lie in some abstract ordered one dimensional real vector space. Also, there exists a region $U \subset \mathbb{L}^{\vdash, et}$ (this is my notation for the monoanalytic étale like big log-shell) that is invariant under automorphisms of log-linked collections of Hodge Theaters. In Mochizuki's terminology, "this region can be seen from both sides of the theta link".

> In any case, to me it sounds like you are saying that the Theta-pilot has nothing to do with Theta-values.

On the contrary, all theta pilots are defined *in* structures which includes an evaluation map like (theta monoid)×(evaluation points)→(constant monoids). Look at the diagram in IUT Remark 3.10.2 and read it clockwise starting from gau and ending at etale $\mathfrak{lgp}$. Each of these $\Vdash\blacktriangleright \times\mu$ strips are parts of a larger structure where multiplication of the "value group portions" may act on the "unit group portions". Eventually these "Kummer maps" lead to the construction of the region U_Θ described previously.

> Well, strictly speaking (modulo minor issues) what Mochizuki does is the following: He takes the Theta-value (some nonzero p-adic number, not a root of unity), and takes the (multiplicative) submonoid M of the p-adic numbers generated by it, and then considers M as an abstract monoid. As an abstract monoid, it is then of course isomorphic to N, necessarily canonically so; its generator is referred to as a Theta-pilot. Does the abstract monoid M know anything about the Theta-value? Of course not! But that is all that ever seems to enter the story of the Theta-link.

This goes back to my comments about his objects not having good black boxes (without some sort of language like the of interpretations). This also points to my point about his confusing writing style in the beginning. It is typically not sufficient to perform his constructions without knowing about the interpretation.

> At some point you do need to remember the interpretation of the Theta-pilot in terms of Theta-values — this is what Mochizuki calls the Theta-intertwining, and it plays a key role as I tried to reference. So I don't think that Mochizuki simply cuts our diagram there.

Again, from the horse's mouth, read C12 of his responses. He says he does not do this. Also, please look at the displayed diagram below. I think this may clarify something for you. Also, in this "interwining", this is where the theorem about the amphoric/characteristic nature of the interpretation of the Jacobi theta function is supposed to be applied. I made some remarks before on this and refer to those.

> Now David Roberts previously mentioned that Mochizuki seems to have a nonstandard point of view on what a mathematical object is, requiring separate copies when they are not actually needed; he often refers to some "history" of these objects. Now maybe one could try to argue that because M was built out of Theta-values, it (the abstract monoid) still knows something about them, that can be even transported via an isomorphism of abstract monoids. (You made some similar remarks in your paragraph on N and Kummer theory, refering to "interpreted structures".) But that's evidently not the case. (That the abstract monoid M knows something about Theta-values might be debatable if for you an abstract monoid is really encoded — as it is in ZFC — in terms of its actual set of elements. But even then, this structure ("the interpretation") is clearly not transported by an isomorphism of abstract monoids.)

I think this quoted text reinforces my points about "depth searches" and the ambiguities (percieved or not) in Mochizuki's writing. Peter I am going to use the words "My understanding" so you may want to skip this response.

Regarding ZFC, while this is true, we don't go this deep. That is not what is going on here. (for the uninitiated see `https://stacks.math.columbia.edu/tag/0009`)

My understanding of the "history of objects" is different, and I don't think this saves the day. "Forgetting history" is Mochizuki-speak for forgetting about the interpretation as you have suggested. Automorphisms of objects are no longer coming from the interpreting structure and this structure is no longer "going along for the ride".

In Mochizuki's language or the language of naked categories Mochizuki's theory does not have many good black boxes. This makes parsing the examples even harder because you need to carry around these large definitions in our small human brains (or use a large amount of paper). If you pick up Hodge's a "a Shorter Model Theory" or Olivia Caramello's book, or MacLane's Sheaves in Logic book, you will find perfectly fine definitions of interpretations that allow you to package all of that sloppy anabelian geometry/ "reconstruction algorithms" into nice little formulas.

Here are some of David Marker's notes:

`http://homepages.math.uic.edu/~marker/math512-F13/inf.pdf`

Exercise: Look at the first page of Marker's notes then show that the torsion of an abelian group is definable using infinitary formulas.

Exercise: People often complain about the non first orderizability of topological spaces. Read this post from stack exchange on how to define topological spaces: `https://math.stackexchange.com/questions/46656/why-is-topology-nonfirstorderizable`

> About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, except because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that?

I'm not sure what "that" is. I'm going to assume you mean ind2. I need to break this down in order to give you a coherent response. For me, ind2 is a representations of automorphisms absolute galois group of a p-adic field $G \cong G_K$ on a $\mathbb{Q}_p$-vector space $\mathcal{O}^{\times\mu}(G) \otimes \mathbb{Q} \cong (K, +)$ induced functorially. There are interpretations given from local class field theory (I'm going to release a table of these soon in my manuscript with Anton, alternatively you can find these in some papers of Hoshi). These preserve the $\mathbb{Z}_p$-lattice $\mathcal{O}^{\times\mu}(G)$ inside this vector space.

I'm not sure what sort of diagrams you want but here is one: Let K be a finite extension of $\mathbb{Q}_p$ with uniformizer π where $\log(1+\pi) = \pi$ and $\log(1+\pi^2) = \pi^2$. Let $\{x\}$ be a one point set. Let $f, g : \{x\} \to K$ be given by $f(x) = \pi$ and $g(x) = \pi^2$.

$$\begin{tikzcd} {} \arrow[r] & \{x\} \arrow[d, "g"] \\ \{x\} \arrow[u] \arrow[r, "f"'] & K \end{tikzcd}$$

(I don't know how to make the diagonal arrow) Considering K up to ind2 makes the diagram commute. Something tells me this is not what you had in mind. Ind2 has the property of "mixing valuations".

In terms of automorphisms of G these give you the freedom to move around the higher ramification groups. The reference for this is Joshi's manuscript.

Here is the standard warning: this will not save the day and you will NEED ind3 to make the formulas work as this sort of doesn't mix very much. The $\mathbb{Z}_p$-submodules $p^j \log(\mathcal{O}_K)$ will always be preserved under ind2, so

$$K = \coprod_{j \in \mathbb{Z}} p^j \log(\mathcal{O}_K) \setminus p^{j+1} \log(\mathcal{O}_K)$$

and each annulus is preserves setwise under ind2. This is saying, in some sense, that ind2 (and ind1) do not do too much. Moral: in order for Mochizuki's inequality to be true, you *do* need ind3.

The purpose of ind2 (and the other indeterminacies) is to perform a construction that is independent of the structure that it was constructed from.

> (There was actually a point where Mochizuki was surprised that by "forgetting the history of a group" one still has more than a group

> up to isomorphism: Namely, a group. That the datum of a group is strictly more than the datum of a group up to isomorphism seemed new to Mochizuki. I believe this is the (psychological) main reason he considers these full poly-isomorphisms.

Classic Mochizuki.

> But really, forgetting "the history of the groups" you still have groups and they may still have natural commuting isomorphisms between them. Of course, for groups up to isomorphism you can't ask for natural commuting isomorphisms between them, then you only have "full poly-isomorphisms".)

To be honest, I'm not sure what you mean by "history of groups" and it actually hurts my head to think about this or to think about you thinking about this. If you want to email me a reference I might be able to say what is going on here but I would need to see it. I'm also don't want to defend the usage of "history" in these documents.

> A few comments up I summarized the situation with claims (1), (2) and (3). I have seen no valid objection to (1) and (2), and (2) alone would lead to a contradiction (as one gets too strong a form of ABC).

I addressed this in the introduction.

> To (3), Taylor indicated where to cut the diagram, but I really don't think this is what happens, as this would isolate Theta-pilots from Theta-values and effectively remove the actual Theta-values from the proof; while Mochizuki does consider this "Theta-intertwining" which is the association of the Theta-pilot with the Theta-values.

I addressed how Mochizuki proposes to bring theta values back into the proof; using the anabelian geometry. How this actually works is unclear to me. Also, I think Mochizuki would object to your characterization "theta intertwining" here. He would say something about the "strong anabelian properties of the étale theta function" which might be equally imprecise. He would also point to a number of other constructions which would include MOD vs $\mathfrak{mod}$ constructions, and the theory of cyclotomic synchronizations. How the q and Θ pilot objects are tied is the mystery and has been the subject of my conversations with everyone for the last several years (which is embarassing to admit outloud).

What is missing to me is the comparison between the U_Θ and q. Mochizuki and Hoshi have very patiently been trying to explain this to me for a long time now. Maybe you want to call this a gap. Maybe I'm dense. Mochizuki will undoubtedly call this a "fundamental misunderstanding" and point to some aspect of his manuscript. Me (and Emmanuel Lepage and others) have been talking about this since 2017 and are stumped.

Some "Refereeing" of the log-links discussions between Peter and UF

@UF April 11, 2020 at 9:02 pm

–C7 of the 05 comments actually says that you *can* switch. –I don't think this rigidifying/derigidifying should be emphasized and will just confuse people. Calling a pair $(\Pi, \overline{M}) \in [\pi_1^{temp}(Z^+), \mathcal{O}_K^{\triangleright}]$ a "rigidified fundamental group" is weird.

@Peter Scholze, April 12, 2020 at 4:58 pm

In this Peter chooses to address the log-links as an example of indeterminate copies doing nothing. Here he claims these can be replaced by logs.

My understanding of the situation for log-links is different. I'm going to give an abstract setup and I claim something like this occurs in IUT. Fix $\overline{M} \cong \mathcal{O}_{\overline{K}}^{\triangleright}$. This can be decomposed as $\overline{M} \cong \pi^{\mathbb{Q}_{\geq 0}} \cdot \overline{M}_{tors} \cdot F$ where $\overline{M}_{tors}$ is the torsion subgroup and F is the free part (at finite level these are free $\mathbb{Z}_p$ modules). Furthermore for the subgroup of units we have $\overline{M}^{\times} \cong \overline{M}_{tors} \cdot F$. Suppose now that one only knows $\overline{M}$ up to automorphisms of $\overline{M}_{tors}$. In order to get a well-defined object, one thing that we can do is mod out by torsion $\overline{M}^{\times} \to \overline{M}^{\times\mu} = \overline{M}^{\times}/\overline{M}_{tors}$ this is now well defined.

Mochizuki would say the converse. That we want to manipulate $M^{\times\mu}$ (via ind1 and ind2) and this manipulation needs to be independent (lifted to?) from the $\overline{M}_{tors}$, which plays a role in the exterior cyclotome of the frobenius-like mono-theta environment, which is used for passing structures "down to the bottom" in IUT3 3.11.

@UF, April 13, 2020 at 10:16 pm

It seems that you are both talking past each other. Log links are about relationships between monoids *not* fundamental groups.

> It seems quite likely Mochizucki uses "switching-symmetry" in a technical sense, synonymous with "multiradiality" of some algorithm reconstructing the data (here a rigidified column of log-links) at hand from some choric data, as he often does, compare e.g. [Alien, p.51].

There is a technical sense. Given a morphism of connected groupoids $F : \mathsf{A} \to \mathsf{B}$ one can form the category $\mathsf{A} \times_{\mathsf{B}} \mathsf{A}$ whose objects are (A_1, A_2, f) where $f : F(A_1) \to F(A_2)$. When F is full and essentially surjective the switching functor is $(A_1, A_2, f) \mapsto (A_2, A_2, f^{-1})$. In practice, as I have said before, this seems to be note enough for applications as objects in categories seem to be more. Also, I think there is a problem because we literally want a map of sets at the end of the day. I haven't worked out how to use this formalism effectively.

> His statement would then mean that if we rigidify the vertical columns, then there is (unlike in the non-rigidfied case) no multiradial algorithm to recover this column from certain choric data. This does not sound so absurd anymore (to me).

Minor typo: “choric” should be “coric”

> Now which multiradial algorithm does he mean here? I would suggest it may be the multiradial algorithm in [IUT III, Cor 2.3], more specifically, the first part of 2.3 (ii) which concerns its compatibility with log-links. Note that close by, [IUT III, Rem 2.1.1 (ii)] the issue we are talking about “why π_1 only up to indeterminate iso?” is discussed. For further discussion see also in [IUT II, Rem 3.6.4 (i)]. In any case, I agree this is an important issue to track down.

This is addressed in the comments at the very very bottom.

@Peter Scholze, April 14, 2020 at 5:17 pm

> The remarks from IUT that you cite make heavy reference to his paper on etale theta-functions, which seems to play a key role in the IUT papers. This paper gives some neat algorithm to start from the fundamental group of a once-punctured elliptic curve with bad semistable reduction, and recover its Tate parameter q and some Theta function; I forget the details. While this is all good and well, I don’t see the relevance: Mochizuki’s more general anabelian theorems, discussed previously on this thread, tell you that from the fundamental group you can simply recover the whole curve. In these comments of Mochizuki that you reference, Mochizuki is discussing some nitty-gritty details of this algorithm, but this seems completely besides the point if you just remember that relevant $\pi_1(X)$’s are equivalent to relevant X’s, so of course you can recover all invariants of X, and you can do so functorially in $\pi_1(X)$’s.

If we are collecting complaints about exposition I have another: As Peter has mentioned this and many other pieces of text in theorem environments punt you WAAAY back to topics in other papers. As Peter is mentioning this goes back to Etale Theta which I think is at least 5 papers back (IUT3>IUT2>IUT1>AAG3>AAG2>AAG1>EtTh) to be fair one could argue that AAGX and EtTh are independent. Either way, this is a big dependence, and we haven’t even touched the dependencies of the AAGX papers.

I responded to the second part of this remark already. You asked about 3.11.i. Also, this $\mathfrak{R}^{LGP}$ is actually not so complicated. The one in Cor 2.3 $\mathfrak{R}$ that is the bad one (no pun intended). I’m going to postpone this dicussion until the end.

@UF April 15, 2020 at 9:04 am

> I now think a good reason to for not identifying π_1’s along log-links is as follows:

(It is very similar to [IUT-2, Rem 1.11.2 (ii)] for the theta-link; I am less precise here, taking the relevance of some reconstructions on faith, but it is a intuitive story, and precision can come later)

> First, let us step back and look at the log-theta-lattice: It consists of Hodge-theaters and the log/theta-links as maps/functors between them. Now a formulation of (2) is: Why do we, say for the log-links, not just take the "identity" isomorphism between the relevant π_1's instead of the full poly-isomorphisms between them? Surely it is also a completely valid map between the theaters?
>
> Yes, but: let us recall what we want to do with this log theta-lattice: we want at "suitable times(?)" glue the Hodge theaters along these links and then apply certain multiradial algorithms.
>
> I think that if we would glue with the "identity" on the $\pi_1's$ (and all the other prescriptions of the log-link), then this gluing will be inconsistent/not well defined (when combined with some multiradial algorithms), in some vaguely similar sense as if you try to define a complex structure on a manifold by charts, but in some region the procedure leads to two different, incompatible complex structures, i.e. the transition map is not holomorphic.
>
> Indeed, in one Hodge theater, the π_1's not independent of the rest of the data; it acts via the quotient to the Galois group on the monoids. Thus π_1's "related" to the monoids, and it is not apriori clear we are allowed to glue two Hodge theaters by gluing π_1 and the monoids separately in whatever way we wish (and then follow certain multiradial algorithms).

I'm going to refer you to my forthcoming manuscript with anton (the prequel to the one with the IUT4 style computaions). There is some stuff on my vlog that is a (poor) template for this stuff if you don't want to wait.

> In the above analogy, a certain region of the Hodge theater is supposed to be "determined" jointly by the π_1 and the monoids.

The multiradial algorithm we consider is roughly as follows: The action of π_1 determines on the (from the Galois group reconstructed) local monoids (with 0) in addition a ring structure (a "holomorphic" structure).

> The output is roughly some etale version of some of the data originally present in the Hodge theater, and the reconstruction algorithm yields a canonical isomorphism between the original data and the reconstructed one (see e.g. [Alien, §2.12] for essentially such constructions).

This *is* a thing.

> If one now glues two Hodge theaters along a log-link (with in addition "identity"-gluing for π_1's, then this yields no consistent holomorphic/ring structure compatible with the above algorithm on some region: Namely, consider the region of the ring (local units), which is glued via log (the actual log part of the log-link) to other Hodge theater; this yields one definition of "holomorphic" for this region.

I can't parse this sentence.

> On the other hand, we can embed this region in the reconstructed monoids and use for the "holomorphic" structure on the reconstructed monoids the one induced by π_1 on both Hodge theaters and glue by the "identity".

This is true. You definitely have one structure from the base and one structure from pulled back from log. The diagrams commute though.

> The above roughly describes two incompatible holomorpic/ring structures on some part of the glued Hodge theater; they are incompatible, "because the transition map, essentially the log, is not a ring map". Such a situation is presumably avoided by taking the full polyisomorphism between π_1 where the log map on the units alone defines the holomorphic structure on the relevant region.

I'm not sure I parse this completely. I will say there is a way to get the a commutative diagrams of fields in the log-kummer correspondence. It depends on what maps you take. Any "log" has a backwards map which is an isomorphism of fields. I will refer to the forthcoming paper with Anton for details.

@Peter Scholze April 15, 2020 at 2:05 pm

> Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.

The log-link is about the monoids and not the base (you use really only use the fundamental group to use Mochizuki's interpretation of a field in order to pullback this structure so you can take the logarithm).

I don't know if this helps but there is a "post-logarithm" that IS an isomorphism of rings and this IS used the definition of the log link. I think it is nice to factor the actual logarithm as $\log^{post} \circ \log^{pre} = \log$ where $\log^{pre}$ is the map in IUT. One can then use the map $\log^{post} : \mathcal{O}^{\times\mu} \otimes \mathbb{Q} \to K$ and pull back the field structure to get a new field $K_{\log}$ which is $\mathcal{O}^{\times\mu} \otimes \mathbb{Q}$ as a set but with this new field structure.

@UF April 15, 2020 at 4:57 pm

> What my post above attempts to show, is: if passing to poly-isomorphism has the effect of doing no gluing/no identification of ring structures (arising from π_1 just the gluing from the actual log map), then the only gluing left is the actual log map, which gives one global chart, and no transition functions needed, essentially(?) since just one chart.

Yes, but I think the point is what I was saying about killing indeterminacies. Doing log-links isn't "for sport" as Mochizuki would say.

> I before never really seriously considered that full poly-isomorphism could have the effect of "no gluing arising from this part" (instead of "choose your favourite gluing").

I don't think I understand how you are thinking about this.

Remarks on IUT3 2.3.ii

We are looking to see if this statement says something non-trivial about the bases of log-linked hodge theaters and multiradiality (I actually think this is barking up the wrong tree since, as I've said before, the bases in log-linked hodge theaters do nothing except impart ring structures to the monoids). Indeed this is the case because of assertion (2) below. Let me just remark though for Peter, that the maps between the $\times\mu$ prime strips are what are going to encode everything in Galois groups and what eventually gets the theta back to the other side.

Anyway, at the beginning of the proposition there is a certain amount of setup. BUT, notice in the setup, most of the structures are interpreted! This means that their morphisms are just functorially induced by the interpreting structures, in this case there is a prime strip of the $\times\mu$ variety, and a base hodge theater (actually, it is even simpler than this... see the note below). Looking at my notes... this looks tautological.

Also, just as a tip from as a person who has wasted his life doing this, the thing to look out for are maps that are NOT full polyisomorphisms.

In this item there are a total of three assertions.

1) log-links induce a full polyisomorphism of the $\mathfrak{R}$'s;

2) Theta links on log-links pairs of hodge theaters have $\mathfrak{D}^{\vdash}_{\Delta}$'s interpreted in $\mathfrak{D}^{\vdash}_{\Delta}$ which are isomorphic.

3) Using (1) and (2) the $\mathfrak{R}$ across is generically isomorphic across log-links; the $\mathfrak{D}^{\vdash}_{\Delta}$ data is generically isomorphic across Theta links.

Proof of 1: This only could be a problem because of the nature of the way the monoids are linked and the dependence of $\mathfrak{R}$'s on these monoids. In the data for $\mathfrak{R}$ there is exactly one place where the monoids interact. This is in the $\times\mu$ prime strips. Everything else is functorially induced. For example, in the definition he takes the monotheta environment interpreted from the base (this actually has well-definedness issues of its own!).

Proof idea of 2: As described somewhere in this note ind2 is isomorphisms of $\mathcal{O}^{\times\mu}(G)$'s induced by isomorphisms of G's. If you look up the definition of $\mathfrak{D}^{\vdash}_{\Delta}$

you will see that it is just a bunch of G's. This means the generic isomorphism of from the G's induce an ind2 on the $\times\mu$ monoids they interpret; the $\times\mu$ monoid isomorphisms in the theta link are the same thing.

Expositional Note: This is an example of a proposition where too much structure was invoked for my taste. In the fine print you notice that the interpreters are $\mathfrak{D}_{>}$ and $\mathfrak{D}^{\vdash}_{\Delta}$ (=fundamental groups at each place and absolute galois groups at each place respectively). In the statement he references the full base hodge theater. That's baggage. Also, you will notice that this could have been broken into many much simpler assertions each which can be individually checked.

Remarks on IUT3 3.11.i

Peter had asked (essentially) how can galois groups interpret theta values? How can they do anything?!

Well, I claim that Theorem 3.11.i and 3.11.ii are not the difficult parts of Theorem 3.11; Theorem 3.11.iii is the weird part. There are some subtle differences between the (abc)-module structure in both of these statements. In the first, the actors (b) and (c) are defined as subset of the $\mathbb{L}^{\vdash,et}$ (at the appropriate places) in part i. In part ii there is a clear module structure.

In the situation of (i) you lift to an richer stronger and more powerful structure that allows you to define (b) and (c) then then you stick them inside the log shell. What the proposition is saying is that this is well-defined up to ind1,2 — so everything that was stuck inside these log shells now is considered up to a jumbling of the type described in a previous response. I will refer readers to my manuscripts with Anton Hilado for these formulas.

A subtle difference that I want to point out to everyone here which they may not have noticed is the difference between the usage of the MOD, LGP constructions and the $\mathfrak{mod}$, $\mathfrak{lgp}$ constructions in the later.

****

Sorry if I made any errors anywhere. I didn't mean to and tried to proofread this. Hopefully we learn as part of the corrections.

Best, Taylor

Peter Scholze *April 17, 2020 at 6:25 pm*

Dear Taylor,

thanks for these final comments! I think I should answer to this. Let me first say that I agree with much of what you write, and for the sake of keeping this short, I only jump at the few places where I disagree.

> **Regarding (3): Is Mochizuki's Proof Falsified?** [...]
>
> Finally, it is not outside the realm of reality that there could be 5 top notch international referees who have understood the proof as complete and correct.

Really? I would have hoped that in that case at least one of them – not in their role as a referee, but simply as a mathematician who wants to share insight – would have come around and explain the key ideas in a way that is understandable.

> Because of these difficulties I personally hold Mochizuki's document as a program for proving ABC and consider Mochizuki's formula a conjecture (until I can see this with my own eyes). Then again, there are a lot of Theorems I know about and use but can't prove.

I very strongly object to the implicit (I believe) assertion that Mochizuki's result should now be considered just another of these really difficult theorems whose proof we never understood.

> **Regarding (2)**
> Also, there are a lot of monoids built into the theory which are excluded from the statement and I don't know if these are implicitly assumed to be identified as well. I think not.

Well, I think I would want to identify some, too. My mode of thought is that starting from your elliptic curve, you can easily cook up all the data that forms a Hodge theater, including those monoids. Simply take that collection, and always the same one. Later, when you study log-links, try to understand what they do, which diagrams do not commute, and if necessary enlarge your diagram (of course you get a non-commutative diagram if you want that the logarithm equals the identity). My issue lies with possible non-identity isomorphisms of Hodge theaters being relevant at any point; they are not for all I can see, as all possible non-commutativity can't be restored in terms of internal isomorphisms of Hodge theaters.

> Finally, to derive a contradiction, I think some things need to be said about *how* one proposes to reduce Mochizuki's construction to "a single fundamental group".

As the final step in Mochizuki (proof of Cor 3.12) is so unclear, it also seems impossible to fully justify where and how it breaks.

Intertwining

> Again, from the horses mouth, read C12 of his responses.

OK, I reread this. He talks about the q- and the Theta-holomorphic structure. This indeed sounds like there must be two distinct Hodge theaters around – Hodge theater is, I believe, the technical version of "ambient holomorphic structure". But there are not! All of them are isomorphic. And if I identify them (using whatever isomorphism) I can see plainly that this does not make any sense.

> He says he does not do this. Also, please look at the displayed diagram below. I think this may clarify something for you.

Details seem to be off in the example, but in any case I totally see that some non-commutative diagrams become commutative up to these indeterminacies – otherwise they would not be indeterminacies. But I'm asking about a relevant one.

> Also, in this "interwining", this is where the theorem about the amphoric/characteristic nature of the interpretation of the Jacobi theta function is supposed to be applied.

The "amphoric" nature here is just that you can recover X from $\pi_1(X)$, so in particular the theta function (and all else). I'm still failing to see the importance of étale theta functions in all of this.

> Exercise: Look at the first page of Marker's notes then show that the torsion of an abelian group is definable using infinitary formulas.
>
> Exercise: People often complain about the non first orderizability of topological spaces. Read this post from stack exchange on how to define topological spaces: `https://math.stackexchange.com/questions/46656/why-is-topology-nonfirstorderizable`

I'm totally lost about where you're trying to go here.

Thanks again for the example in reply to this [*from earlier comment by Scholze*]

> About the indeterminacies: I'm focusing on (Ind2); (especially) the part concerning automorphisms of local Galois groups (acting on local units if necessary). I don't see any diagram that doesn't commute when you take the identity isomorphisms, but does commute when you conjugate by an automorphism of a local Galois group (acting on local units). Why do you need that, except because "you need to forget the history of your objects in order to apply reconstruction algorithms" or some magic like that?

but I'm asking about a relevant diagram in the relevant abstract setting.

> The purpose of ind2 (and the other indeterminacies) is to perform a construction that is independent of the structure that it was constructed from.

This slogan I understand, but where does Ind2 help with anything? Why do you need to introduce it?

> I addressed how Mochizuki proposes to bring theta values back into the proof; using the anabelian geometry. How this actually works is unclear to me. Also, I think Mochizuki would object to your characterization "theta intertwining" here. He would say something about the "strong anabelian properties of the etale theta function" which might be equally imprecise. He would also point to a number

of other constructions which would include MOD vs $\mathfrak{mod}$ constructions, and the theory of cyclotomic synchronizations. How the q and Θ are tied is the mystery and has been the subject of my conversations with everyone for the last several years (which is embarassing to admit outloud).

> What is missing to me is the comparison between the U_Θ and q. Mochizuki and Hoshi have very patiently been trying to explain this to me for a long time now. Maybe you want to call this a gap. Maybe I'm dense. Mochizuki will undoubtedly call this a "fundamental misunderstanding" and point to some aspect of his manuscript. Me (and Emmanuel Lepage and others) have been talking about this since 2017 and are stumped.

Well, I guess you are just pointing your finger to the same problem that I'm trying to point at. It is completely unclear how you get the actual Theta-values back in the game. You started this paragraph with "using the anabelian geometry", but as I argued long before, it is completely unclear how anabelian geometry helps with anything in his setup (by (1)).

> *@Peter Scholze April 15, 2020 at 2:05 pm*
>
> Removing all language from your message, I end up with the following message: The logarithm map is not a map of rings. This is why you have to consider source and target up to some indeterminacy.
>
> The log-link is about the monoids and not the base (you use really only use the fundamental group to use Mochizuki's interpretation of a field in order to pullback this structure so you can take the logarithm).
>
> I don't know if this helps but there is a "post-logarithm" that IS an isomorphism of rings and this IS used the definition of the log link. [...]

My main question in the discussion with UF has always been the question of full poly-isomorphisms vs. identities between $\pi_1(X)$'s, i.e. issue (2). All you are writing is completely tangential to this. I think you actually agree that the log-link has so little to do with $\pi_1(X)$'s that it can't possibly be the reason to consider $\pi_1(X)$ up to full poly-isomorphism. Yet Mochizuki was trying to make that exact point when we were discussing.

> **Remarks on IUT3 2.3.ii**

Why are you discussing this? I'm lost. Is it saying more than Theta-values being the same in all Hodge theaters?

> **Remarks on IUT3 3.11.i**
>
> Peter had asked (essentially) how can galois groups interpret theta values? How can they do anything?!
>
> Well, I claim that Theorem 3.11.i and 3.11.ii are not the difficult parts of Theorem 3.11;

You are simply ignoring my point!
Best wishes! Peter

Peter Scholze *April 17, 2020 at 7:15 pm*
PS: I just realized that maybe the following information is worth sharing. Namely, as an outsider one may wonder that the questions being discussed at length in these comments (e.g., the issue of distinct copies etc.) are very far from the extremely intricate definitions in Mochizuki's manuscripts (his notation is famously forbidding, some of it surfaced in Taylor's comments), and feel almost philosophical, so one might wonder that one is not looking at the heart of the matter.

However, the discussions in Kyoto went along extremely similar lines, and these discussions were actually very much led, certainly initially, by Mochizuki. He first wanted to carefully explain the need for distinct copies, by way of perfections of rings, and then of the log-link, leading to discussions rather close to the one I was having with UF here. He agreed that one first has to understand these basic points before it makes sense to introduce all further layers of complexity. (I should add that we did also go through the substance of the papers, but kept getting back at how this reflects on the basic points, as we all agreed that this is the key of the matter.)

UF *April 17, 2020 at 8:59 pm*
@Taylor Dupuy
Thanks for your comments. The context for comment (C7) is Mochizuki's interpretation of footnote 5 in [Scholze-Stix]. Mochizuki gives this interpretation in the first display of (C7): the rigidification mentioned there is (I think) not (some kind of) rigidification of π_1 in one Hodge theater (which you seem to mention, by considering pairs (π_1, monoid)), but the rigidification among the different π_1's in one vertical column by choosing a specific isomorphism ("identity") instead of full poly-isomorphism between the different π_1's in one column.

Mochizuki then says that one can consistently identify the π_1's as above, if one forgets about both log- and theta-links. (Mochizuki does not say so explicitly right here, but I think in this situation there would be a switching-symmetry. He says so in a closely related situation after the second display, which in my understanding amounts to the same situation).

In the second display in (C7), he seems to say (my "" are not meant here as quotes): In contrast, if one keeps log-links (or maybe similarly theta-links), then the rigidification of the π_1's "depends" on the other data, say "0-column Frobenius-like data (at different vertical spots) with log-relations", which do not admit a switching-symmetry between two vertical columns.

Here in my understanding "depends" indicates that the log-identifications between the 0-column Frobenius data cannot be made completely independently from the identity-identifications of the π_1's in the 0-column, since at a fixed vertical position the π_1 and the monoids are "related/not independent" (via the action). Implicit here (and relevant for the context of this C7) seems to be the assertion that because the rigidification of the π_1's "depends" on the 0-column Frobenius data (which does not admit a switching-symmetry), something has to go wrong with switching (when π_1's are rigidified in a column), "because" it goes wrong for the related/dependent 0-column Frobenius-like data. (If this is *not* implicit here, then what in Mochizuki's response in C7 is the objection to the vertical rigidification of π_1's?)

After the second display he says: If one forgets about the 0-column Frobenius data (in particular the log-relationship between them at different vertical spots), then there is a switching-symmetry between two neighboring columns.

This is how I understand Mochizuki's C7; I may certainly understand it wrong in parts, and I would be happy to understand it better. All the rest I wrote is an attempt make this (from my point of view) "implicit objection to rigidifying π_1's" in the second display more explicit; but if my understanding concerning this is incorrect, then this is largely meaningless.

W *April 19, 2020 at 9:53 am*

There has been some discussion about this comment thread that focused on the importance of the differentiating between fact and opinion. There has also been discussion that the long thread may obscure the points that Peter Scholze and Taylor Dupuy agree on by focusing on their areas of disagreement. I have written a (very long) comment to try to help with these issues.

The way this comment is intended to work is that the numbered paragraphs (1), (2), (3) give facts that are difficult or impossible to dispute and the paragraphs after them give opinions building on those facts.

(1) The first couple hundred pages of the IUT papers introduce a lot of difficult new terminology. Scholze and Stix demonstrated that none of this new terminology is necessary to prove any of the formal statements in these pages. Moreover, when the new terminology is ignored, the proofs all become simpler and easier to understand. They all follow quickly from Mochizuki's previous work on the p-adic Grothendieck conjecture.

As a reader, this should already make you skeptical of the accuracy of the paper. How can dressing up simple statements in confusing and difficult terminology for hundreds of pages represent progress in turning the solution of one problem (the p-adic Grothendieck conjecture) into a completely different problem (ABC)? There are mathematical arguments that begin by introducing a lot of terminology (e.g. work of Grothendieck) but nothing like this ratio of terminology to content has ever been successful before. However, you certainly shouldn't rule out Mochizuki's work based only on this evidence.

As an editor or referee, you should also be very skeptical at this point. Setting aside concerns about correctness, you have some duty to ensure papers published in your journal are clear and get to the point. Having hundreds of pages that can be compressed to much less by the removal of new ideas rather than their introduction is not normally consistent with good mathematical writing, but one could imagine that the remainder of the paper justifies this sufficiently.

(2) Many people have tried to read the papers, read and understood the first few hundred pages, and then were unable to verify a key point – Corollary 3.12. This point is either the first or the second statement in the paper which doesn't fit into the hundreds of pages mentioned in (1).

As a reader this should make you very worried! This is is not a pattern one typically sees when people try to understand a correct but difficult argument (regardless of how well or poorly it is written). Instead, different people almost always get stuck at different points. This is certainly true if the reason people are getting stuck is a lack of background in anabelian geometry, or a refusal to put enough work into it – both reasons suggested by defenders of IUT. You would then expect to see different confusions in different places from people with different levels of background, or from people who made careless errors at different points. Of course, what we see is exactly what you would expect to see if the proof of Corollary 3.12 is not a valid argument but the rest of the paper is valid.

As an editor or referee, this should be completely unacceptable. Even if the proof is technically correct in the sense that Mochizuki or someone else can give a clear and precise argument which the proof of Corollary 3.12 could be said to summarize, allowing the paper to continue in its present form would be a failure at their duty to ensure papers in their journal properly explain their arguments. A paper such that everyone who studies it seriously, except a small inner circle, gets stuck at the same point simply isn't a proper explanation.

Some defenders of IUT like to point out that Scholze and Stix didn't give their precise objection until 2018. But this phenomenon, given that it was noticed by most people who read the paper seriously, should have been turned up by the refereeing process before then. This is, I think, the starting point for ethical concerns about the refereeing process. (For instance, OP's comment suggests that the editors could have asked a series of referees, ignoring those who have negative commentary, until they found someone willing to say it is good.)

Is it possible that the final version makes substantial changes to this argument and answers this objection? Everything is possible, but it seems unlikely. It is common practice in mathematics to edit the online version of the paper to the final submitted version, or something close to it, without the journal's formatting. As far as I know the main reasons not to do this are because one doesn't post online versions or doesn't edit them often, neither of which apply to Mochizuki.

Even if the editors and Mochizuki want to avoid this for some reason (like they want to make people read the journal to see the complete proof, I guess. . .) they could easily post some comment online like "Corollary 3.12 has been replaced with a series of simpler statements, each with a detailed proof, culminating in the key inequality, totaling about 35 pages" and get much less criticism from mathematicians and mathematical physics bloggers. Does anyone like receiving criticism? Is there any reason not to do this?

(3) Two serious mathematicians have come up with a precise objection to the proof of Corollary 3.12. Despite the fact that it is not clear exactly what Mochizuki means, they have come up with a plausible interpretation, and shown that, in this interpretation, the inequality that Mochizuki proves is off from the inequality he states by a factor of j^2. This is important because the improvement of the stated inequality over a certain trivial inequality is exactly by a factor of j^2. They have come up with additional arguments, which are unsurprisingly not completely rigorous, that any plausible interpretation of what Mochizuki means will have the same problem.

(One could say that it is not plausible that Mochizuki made such a simple mistake, but I think it is plausible in light of the previously discussed information).

Neither Mochizuki nor his defenders have come up with a convincing rebuttal to this. Mochizuki's response focused on arguing that it is possible for an argument like his to work, and that Scholze and Stix misunderstood it, rather than making basic clarifications to the argument that would aid in understanding whether the objection is valid, and that would certainly be possible if the argument is correct. For instance it is *provably* possible that any valid argument proving a concrete inequality using facts about affine spaces over the real numbers can be replaced with one that proves the concrete inequality using other concrete inequalities (or identities). Doing this would allow a more focused discussion because Scholze could either dispute one of the building-block concrete inequalities or dispute the implications from them to the desired statement.

For me this objection, combined with the other stuff, is devastating. Of course it is not the case that one must stop the presses of a journal every time a mathematician objects to an argument, and keep them stopped until that mathematician is satisfied. But combined with the other very worrying facts about the proof, this is an objection that must be answered for the paper to be published ethically. As mentioned earlier, if a better answer than what is in the currently online version has been delivered by Mochizuki to the journal, I would expect it, or at least an advertisement for its existence, to be posted online.

Despite the long comment thread, Peter Scholze and others have not exhausted all the reasons that a mathematician examining Mochizuki's argument should be skeptical that it, or even any argument like it, could possibly work. I could list these additional reasons, but they are not so relevant when the objections outlined above already mean that it would be inappropriate to publish the article in its current form.