REPORT ON THE CURRENT SITUATION SURROUNDING INTER-UNIVERSAL TEICHMÜLLER THEORY (IUT)

Shinichi Mochizuki
(RIMS, Kyoto University)
October 2025

Contents

§1. Journalism/story-telling versus the practice of mathematics 1
§1.1. Mathematical accountability and the EMSCOP 2
§1.2. Professional training versus ChatGPT-type "hallucinations" 2
§1.3. Disregard for fundamental democratic principles 4
§2. Detailed analysis of mathematical misunderstandings 5
§2.1. The issue of intrinsic contradictions in the set-up of IUT 6
§2.2. Issues of identification: set-theoretic copies versus universes 9
§2.3. The issue of set-theoretic paradoxes 14
§2.4. Relation to recent developments in anabelian geometry 15
§3. Lean, the EMSCOP, and IUT 20
§3.1. Social/political dynamics versus mathematical truth 20
§3.2. The fundamental importance of Lean in the context of IUT 26

§1. Journalism/story-telling versus the practice of mathematics

In September 2025, J. D. Boyd published an article [Ctv] on his SciSci Research website exposing his views concerning the present situation surrounding *inter-universal Teichmüller theory (IUT)*. This article is a **highly irresponsible** piece of amateur journalism by an individual who lacks professional training in the mathematics relevant to the article and is replete with **mathematically false** and **factually incorrect** statements (cf. §2 below for more details), as well as numerous *absurd conclusions* based on such statements. The purpose of the present report is to survey the current situation surrounding IUT with an eye to

- minimizing the **entirely unnecessary confusion** that could potentially be caused by this article [Ctv] (cf. the remainder of the present §1, as well as §2 below, for more details), as well as to
- discussing **recent progress** toward the important goal of **Lean-style formalization** of IUT (cf. §3 below for more details).

In the following, I will use the notation "p. nXY/8" for references to (the September 26, 2025 version of) [Ctv]. Here, $n \in \{1,2,3,4,5,6,7,8\}$ denotes the *page* "n/8" of [Ctv]; $X \in \{L,C,R\}$ denotes the *horizontal position* (i.e., Left/Center/Right) on the page; $Y \in \{T,M,B\}$ denotes the *vertical position* (i.e., Top/Middle/Bottom) on the page. Thus, searches for these references may yield inaccurate results when applied to other versions of [Ctv]. On the other hand, since we include in the present report direct quotations of all portions of the text of [Ctv] that are discussed in the present report, the present report is *entirely self-contained*, i.e., may be read without referring directly to [Ctv].

§1.1. Mathematical accountability and the EMSCOP. First, it should be noted (cf., e.g., the introductory portion of [Rpt23]) that IUT is a *well established mathematical theory* that currently consists of five mathematical papers published in two internationally recognized mathematical journals and, in particular, has passed peer reviews for these journals. There are also five (four published and one unpublished) survey papers on IUT. That is to say, IUT is mathematics that is well understood by quite a number of professional mathematicians and has been verified countless times since its release in August 2012. In particular, *this piece of amateur journalism [Ctv] does not have any effect whatsoever on the mathematical validity of IUT.*

Frequently, in discussions of the responses by various people toward IUT, I quote the following passage from Article 6 of the section "Responsibilities of authors" of the Code of Practice of the European Mathematical Society (i.e., the [EMSCOP]), which oversees the publishing company that publishes the mathematical journal PRIMS, where the four original IUT papers were published:

> Mathematicians should make public claims of potential new theorems only when they believe they are able to provide full details in a timely manner, to avoid unnecessarily blocking an active line of research.

This passage may be understood as expressing the fundamental importance of **mathematical accountability** when making mathematical assertions in public, i.e., the fundamental importance, for the practice of mathematics, of avoiding the creation of **blackholes** with respect to mathematical accountability, where the validity of mathematical assertions becomes a matter of *social/political dynamics*, such as *"faith en masse"* (cf. the discussion of §3.1 below), rather than detailed, rigorous mathematical proofs (cf. the discussion surrounding (BlkAcc) in [EssLgc], §1.12).

It is precisely for this reason that I have been very conservative in the past with regard to engagements with journalists and deeply critical of preprints such as those discussed in [Rpt24]. That is to say, articles written by journalists, as well as preprints such as those discussed in [Rpt24], lie in a realm that is fundamentally external to the *infrastructural apparatus* that exists in the field of professional mathematics for *long-term digestion* and *examination* of the details of proofs of mathematical ideas and assertions, namely,

- *internationally recognized mathematical journals with a functional peer review system* and
- *lectures and workshops on the mathematics involved*

(cf. the discussion surrounding (BlkAcc) in [EssLgc], §1.12).

§1.2. Professional training versus ChatGPT-type "hallucinations". Typically, journalists reporting on mathematics have no or little professional training in the mathematics that they are reporting on. In the case of [Ctv] (or, more generally, SciSci Research, which, as I understand, is an organization that consists of a single member), Boyd (according to the information that I was given) holds a masters degree in a discipline whose precise relationship to the field of pure mathematics is not clear; he does not hold a doctoral degree and has not published any research papers in an internationally recognized mathematical journal in any field of mathematics closely related to arithmetic geometry (much less IUT). In the case of the preprints discussed in [Rpt24], the author is a professional mathematician in a field of arithmetic geometry that is not directly relevant to the preprints, but has not published any research papers in an internationally recognized mathematical journal in either of the two fields that are directly relevant to the subject matter discussed in the preprints, namely, p-adic Hodge theory and IUT-related anabelian geometry, hence cannot be considered a professional mathematician in either of these fields.

In all of these cases, the fundamental lack of a *mathematical accountability apparatus* underlying the writing of the articles/preprints involved means that their content becomes nothing more than an exercise in *story-telling*, or *arbitrary text generation*, in the style of the so-called *"hallucinations"* generated by LLM's such as ChatGPT (cf. the discussion of [Rpt24], §1). In this context, I am reminded of the phrase *"linguistic trickery"* that was applied by one mathematician to describe the use of the term "arithmetic holomorphic structure" in the preprints discussed in [Rpt24], as well as the discussion of the ideas of H.G. Frankfurt in [Fsk] concerning the difference between a person who simply lies while being explicitly aware of the discrepancy between what he says and the reality of the situation and a person who is locked in a behavioral pattern of mass generation of content that is completely divorced from reality, but who has little or no explicit awareness of this discrepancy between the content generated and the reality of the situation. I am also reminded, as a result of my experiences as an editor of a mathematical journal, of the numerous manuscripts submitted to journals by amateur mathematicians that claim to prove famous conjectures in a few pages using elementary techniques.

In the context of ChatGPT-type "hallucinations", it is interesting to note that it appears that currently research is underway to link LLM's such as ChatGPT to Lean, so that interaction and text generation by the LLM can be supported by rigorous mathematical verification via Lean. I heard about this research in talks by Chinese researchers during a Lean workshop in Tokyo in July 2025, and, as I understand, this sort of technology is still in a rather initial stage of development. On the other hand, this sort of technological development is exciting in that it potentially (not in the immediate future, but perhaps in the not so distant future) opens up the possibility of preparing expository reports by LLM's on mathematics formalized in Lean that are not plagued by the inaccuracies of journalists who do not have a rigorous understanding of the mathematics involved. It also opens up the possibility that in the future, LLM's linked to Lean can be used by professional mathematicians to generate reports such as the present report or [Rpt24] to address problematic documents such as [Ctv] or the preprints discussed in [Rpt24], thus freeing mathematicians such as myself from the burden (or perhaps dramatically reducing the burden) of preparing manuscripts such as the present report or [Rpt24] by hand. We refer to §3 below for a more detailed discussion of Lean-related aspects of IUT.

§1.3. Disregard for fundamental democratic principles. As discussed in [Ctv], p. 1LT/8, the release of the article [Ctv] follows the release of another article [Cnv] that surveys the ideas, as well as the history of the development of the ideas, underlying IUT. Here, it should be noted that I consented to participate in the series of interviews/discussions that lead to [Cnv] precisely on the basis of Boyd's repeated assurances that SciSci Research represented a *fundamentally new kind of scientific journalism*, one that would indeed take a *responsible stance* with regard to *mathematical accountability* for the mathematical content of the articles. Typically, journalists prioritize *political* or *profit-related* motives over accountability for mathematical accuracy, often as a result of a business model that derives profit (e.g., in the form of internet clicks) precisely from sowing the seeds of *controversy*, e.g., by using *factually inaccurate content to generate entirely unnecessary confusion* (cf. the discussion of the *internet/mass media as an apple of discord* in [EssLgc], Example 1.5.2). The *highly irresponsible* and *mathematically inaccurate* content of [Ctv] thus amounts to an **egregious breach of trust** — i.e., an *outright betrayal* — relative to the repeated assurances that I received from Boyd in the period of time (between fall of 2023 and fall of 2024) leading up to the interviews of [Cnv]. This content also explicitly contradicts Boyd's assertion in [Ctv], p. 1CT/8, of the purely mathematical nature of his intentions in writing [Ctv].

The views espoused by Boyd in [Ctv], most particularly concerning the content of the Scholze-Stix manuscript, are in *direct contradiction* to the views that he expressed on multiple occasions during our interviews (cf., e.g., the discussion of [Ctv], p. 8RB/8), namely, to the effect that that it was obvious even to an amateur observer that the main assertions of the manuscript were absurd and meaningless. We also discussed the mathematical content of the Scholze-Stix manuscript and related mathematical topics such as redundant copies in some detail, so there was ample opportunity for him to discuss the issues that he raises in [Ctv], but he never did so. That is to say, it is *entirely inconceivable* that the person that I thought I knew by the name of "James Douglas Boyd" and interacted with, both electronically and in person, between the fall of 2023 and the fall of 2025 could have written an article such as [Ctv]. Put another way, the very existence of this article [Ctv] implies that the person that I thought I knew by the name of "James Douglas Boyd" and interacted with, both electronically and in person, between the fall of 2023 and the fall of 2025 was nothing more than a **cynical work of fiction**.

It is difficult for me to recall any time in my career as a mathematician when I experienced a comparable level of *overt fraud/duplicity/charlatanism* in a professional situation, and it has left me in a state of *deep shock*. Typically, interaction between professional mathematicians is conducted in a candid, forthright fashion, at least with regard to *substantive mathematical issues*. If Boyd feels strongly about the sort of issues that he raises with regard to IUT in [Ctv], there is absolutely no reason why he could not have resolved them smoothly and essentially painlessly in one-to-one mathematical discussions between the two of us. This is typically the way in which mathematical issues are resolved between professional mathematicians and indeed the way in which numerous mathematical inaccuracies in preliminary versions of [Cnv] were resolved between Boyd and myself during the period between the fall of 2024 and the summer of 2025.

In this context, it is important to recall that the Scholze-Stix manuscript has **not** been **published** in any internationally recognized mathematical journal with a functional peer review system. The manuscript does **not** contain **any proof** — or indeed *even any precise statement* — of the *main assertion* of the manuscript, namely, that there is some sort of **logical relationship** between the simplified version of IUT exposed in the manuscript and the original theory of [IUTchI, II, III, IV] (cf. [EssLgc], §3, for more details). Rather, the manuscript simply states (in a passage that may be found in the PDF file of the manuscript by performing a search for the character string "restor") that the authors are confident that restoring all the details of the original theory would not affect the validity of the conclusions asserted in the manuscript. As discussed in [Rpt23], Scholze-Stix have refused to respond to numerous offers on my part by e-mail to engage in further discussions (via e-mail/Zoom or in person) concerning the content of their manuscript. As a result,

(Myst) it remains a **complete mystery** *why* — i.e., in the sense of a *precise mathematical statement* supported by a *complete mathematical proof with full details*, in the spirit of the [EMSCOP] passage quoted above — Scholze-Stix or indeed anyone else, including Boyd (cf. the discussion of §2.1, §3.1 below!), assert(s) the existence of some sort of **logical relationship** between the Scholze-Stix version of IUT and the original theory of [IUTchI, II, III, IV].

During my interaction with Boyd between the fall of 2023 and the fall of 2025, I repeatedly emphasized the importance of *mathematical accountability* and the [EMSCOP] passage discussed above, as well as the more general, but nonetheless closely related, **fundamental democratic principles** of the **rule of law**, **due process of law**, and **burden of proof** (cf. the discussion at the beginning of [EssLgc], §1.10; the discussion of [EssLgc], Example 1.10.1, (v); the discussion surrounding (DngPrc) in [EssLgc], §1.12). Instances of **dramatic** and **egregious flip-flops** in position and policy are often closely linked to a **flagrant disregard** for such fundamental democratic principles/norms, and indeed this sort of phenomenon appears to be increasingly prevalent — i.e., a sinister sort of *"new normal"* — in the culture and governing bodies of certain parts of the world today.

§2. Detailed analysis of mathematical misunderstandings

The mathematical misunderstandings of [Ctv] are **deeply embedded** in the **fabric** of the narrative of [Ctv]. They are so numerous and fundamental that it is a formidable task to categorize and arrange these misunderstandings in a form that is readily digestible for analysis. In §2.1, §2.2, §2.3, we discuss *three categories* of misunderstandings, corresponding to **three distinct** (but not entirely unrelated) **mathematical issues**. Interestingly (perhaps even ironically!), these misunderstandings concern the logical structure not only of IUT, but also of the assertions of the Scholze-Stix manuscript that is often cited, as well as of various elementary and classical aspects of arithmetic geometry and indeed set-theoretic mathematics/mathematical reasoning in general. As discussed extensively in §1, these misunderstandings stem essentially from Boyd's *short-sighted "hit-and-run" journalistic/story-telling* approach to discussing mathematics, an approach that is entirely divorced from any sort of **functional apparatus** for ensuring **long-term mathematical accountability**. Indeed, I was entirely unable to detect *any evidence* whatsoever in [Ctv] to the effect that Boyd has either the will, the explicit intention, or the technical capacity to give precise statements or complete detailed proofs for any of the mathematically false assertions discussed in the remainder of the present §2. Finally, in §2.4, we discuss misunderstandings surrounding the developments in anabelian geometry related to IUT that were discussed in [Ctv].

§2.1. The issue of intrinsic contradictions in the set-up of IUT. Perhaps the *most fundamental misunderstanding* of [Ctv] lies in the following **false assertion**:

(FA1) the Scholze-Stix manuscript shows the existence of a *contradiction* intrinsic to the *set-up* of the *log-theta-lattice* of IUT.

This false assertion (FA1) may be seen in the following passages of [Ctv]:

- "Although the manuscript does not use the algorithms that Mochizuki uses in the papers, it does indeed, taking the setup in which the algorithms are applied (i.e., the log-theta-lattice), show that a contradiction can be derived from this setup due to the inter-universal approach that IUT applies to Diophantine inequalities." ([Ctv], p. 1RT/8);
- "The Scholze-Stix argument shows that if one simply removes the labels from the beginning (by identifying the various copies of $\mathbb{R}$), and then looks at the theta-links, the contradictions become immediately manifest." ([Ctv], p. 1RB/8);
- "The answer is that, although the overall setup clearly harbors these contradictions, suspended by inter-universality, Mochizuki has argued that the papers are misunderstood," ([Ctv], p. 2LT/8);
- "On the other hand, the Scholze-Stix argument is essentially that it's unnecessary to consider such advanced algorithms since if one just takes the setup and simplifies it down, one finds it to contain a contradiction-inducing mapping protected by labels which one might just as soon remove." ([Ctv], p. 2LM/8);
- "it's about the fact that the contradiction is there in the lattice ... with the theta-link, whereas Mochizuki's algorithms essentially promise a way around it. ... it's about whether mathematicians are confident enough in the basic setup, shown in simplified terms by Scholze-Stix, to even consider the algorithms. ... it would essentially require the mathematical community to agree to abstain from deriving the contradiction from the setup, despite the ease of doing

so, ... I think they're unlikely to abstain from deriving the contradiction, since mathematicians can remove labels as they so choose ... Presenting IUT with the caveat that the setup is sensitive to set-theoretic contradiction and one can only perform a key algorithmic step under highly specific circumstances has been unconvincing." ([Ctv], p. 2LB/8 $\sim$ 2CM/8);

· "One algorithm might avoid those statements, but if one can find another algorithm that reaches a contradiction, one shows the theory to be inconsistent. ... if Scholze and Stix have an algorithm that reaches a contradiction, then the theory of the lattice is inconsistent. I think the key issue is that Mochizuki views the algorithms as part of the theory, such that other contradictions are mathematically extrinsic. Nonetheless, I think most mathematicians view the ease with which an immediate contradiction can be derived from the setup as a sign to move on." ([Ctv], p. 5CM/8 $\sim$ 5RT/8);

· "My own outlook is that Lean won't help in this case, since at issue is this matter of label-removals and $\mathbb{R}$-identifications. Lean admits distinct type-theoretic universes, which, as Carneiro discusses, if viewed in a set-theoretic framework, are indeed Grothendieck universes. So, on the one hand, I can imagine one trying to formalize the multiradial algorithms using type-theoretic universes with 'distinct labeling', perhaps put in by hand. The IUT papers symbolically label the Hodge theaters, q parameters, and other data (e.g., with † or ‡). So, formalizing IUT in a manner consistent with the papers would require encoding labels to prevent data from being identified. One could give them labels, perhaps, with irreducible definitions (or something like that), in order to make them resistant to equivalences. On the other hand, to formalize the Scholze-Stix argument, one would make the data readily amenable to identification. I don't foresee Lean being good for resolving a dispute such as this. Whether or not data is identified or kept distinct is a coding choice, just as it is a symbolic choice in pen-and-paper math. I can imagine both sides finding a way to code up their approach, only to dispute their respective approaches." ([Ctv], p. 5RM/8 $\sim$ 5RB/8).

Before proceeding further, it should be stated categorically that there is **no contradiction** in the set-up of the log-theta-lattice of IUT. The log-theta-lattice consists of Hodge-theaters glued together via log-links and theta-links. The construction of Hodge-theaters, which is the main topic of [IUTchI], is a *simple formal procedure*, provided that one accepts results in anabelian geometry and the theory of Frobenioids that are established in earlier papers. In a similar vein, the construction of log-links and theta-links follows *formally* from the fact that the prime-strips that appear in the domain/codomain of the log-link and theta-link are *isomorphic*, which again follows formally from the various definitions involved.

If this false assertion (FA1) were true, then it would follow formally that

(FA1.1) the *log-theta-lattice does not exist*, hence, in particular, is *not well defined*.

Here, I should emphasize that *never* in the period of time from August 2012 (when the IUT papers [IUTchI, II, III, IV] were released) till the present have I seen any assertions by any professional mathematician to the effect that (FA1.1) holds. Note that this includes the Scholze-Stix manuscript, my discussions with Scholze-Stix and Hoshi in March 2018, and subsequent internet/mass media comments by Scholze-Stix.

In this context, it is also important to note that if the false assertion (FA1.1) were true, then it would follow formally (i.e., since the multiradial representation for the theta-pilot in IUT *logically depends*, in an essential way, on the *existence/well-defined nature* of the log-theta-lattice) that

(FA1.2) the *multiradial representation for the theta-pilot* in IUT (i.e., the algorithm summarized in the statement of [IUTchIII], Theorem 3.11) *does not exist*, hence, in particular, is *not well defined*.

Again, I should emphasize that *never* in the period of time from August 2012 (when the IUT papers [IUTchI, II, III, IV] were released) till the present have I seen any assertions by any professional mathematician to the effect that (FA1.2) holds. Note that this includes the Scholze-Stix manuscript, my discussions with Scholze-Stix and Hoshi in March 2018, and subsequent internet/mass media comments by Scholze-Stix.

On the contrary, Scholze-Stix repeatedly emphasized — i.e., during the March 2018 discussions, as well as in their manuscript — that they do *not contest* the mathematical validity of [IUTchIII], Theorem 3.11 (i.e., the algorithm for the multiradial representation for the theta-pilot). Rather, their *central assertion* — both during the March 2018 discussions, as well as in the argument involving identifying copies of $\mathbb{R}$ in the final two pages of their manuscript — is the assertion that

(SSA1) they do not see *how to derive any nontrivial diophantine consequences*, i.e., such as height inequalities, from the theory of [IUTchI, II, III, IV].

That is to say,

(SSA2) the argument involving identifying copies of $\mathbb{R}$ in the final two pages of their manuscript consists of their *interpretation/simplification* of the set-up (i.e., involving the log-theta-lattice) and argument of [IUTchIII], Corollary 3.12.

As discussed in (Myst) (cf. also the discussion surrounding (Myst) in §1.3, as well as [EssLgc], §3, for more details), there is in fact **no logical relationship** between the Scholze-Stix interpretation/simplification referred to in (SSA2) and the set-up (i.e., involving the log-theta-lattice) and argument of [IUTchIII], Corollary 3.12.

Finally, we note that **formalization via Lean4** of the sort of mathematics that appears in the construction of Hodge-theaters or the multiradial representation of [IUTchIII], Theorem 3.11, is, at least at a theoretical level, an **entirely straightforward issue** (cf. the discussion of (NoObs) in §3.2 below). Of course, it would require a tremendous amount of work, perhaps on the order of several years, depending, for instance, on how much of the anabelian geometry and theory of Frobenioids of earlier papers one is willing to assume as a "blackbox" (cf. the discussion of (BBxFm) in §3.2

below for more details), but the *fundamental point* that must be emphasized in this context is that the **falsification** (i.e., verification of the falsehood) of false assertions such as (FA1.1) and (FA1.2) is (at least at a theoretical level) precisely the sort of task at which **Lean4 excels**. That it to say, it is **entirely inconceivable** that, as is asserted in [Ctv], p. 5RM/8 ∼ 5RB/8,

(FA1.3) Lean4 is fundamentally incapable of confirming the falsity of ((FA1.1) and (FA1.2), hence also of) (FA1).

In particular, it is **entirely absurd** to assert, as is done in [Ctv], p. 5CB/8 ∼ 5RT/8, that

(FA1.4) the falsification of (FA1) is a sort of *subjective issue* whose validity depends on the *point of view* of the mathematician in question.

Indeed, assertions such as (FA1.3), (FA1.4) *directly contradict* the *fundamental nature of logical reasoning* in mathematics. In this context, it is also important to note, again as a matter of *elementary logic*, the following **Existence Principle**:

(ExPr) the *existence* of an algorithm (i.e., such as the Scholze-Stix interpretation/simplification of (SSA2)) that is *unsuccessful* in yielding any nontrivial consequences (i.e., such as diophantine inequalities) does **not**, in any way, imply the *non-existence/falsehood* of an algorithm (i.e., such as the actual content of [IUTchIII], Theorem 3.11 and Corollary 3.12) that is *successful* in yielding nontrivial consequences.

§2.2. Issues of identification: set-theoretic copies versus universes. Another *fundamental misunderstanding* of [Ctv] lies in the following **false assertion**:

(FA2) *contradictions* in IUT are avoided by means of the *"extraordinary/novel"* approach of assigning *distinct labels* to *different universes*.

This false assertion (FA2) may be seen in the following passages of [Ctv]:

- "Otherwise-contradictory relations between data imposed by certain links are offset by assigning data to different universes with distinct labels, with this inter-universal setup giving rise to many copies of $\mathbb{R}$." ([Ctv], p. 1RM/8);
- "The answer is that, although the overall setup clearly harbors these contradictions, suspended by inter-universality, Mochizuki has argued that the papers are misunderstood, ..." ([Ctv], p. 2LT/8);
- "It's an extraordinary situation, due to the novelty of using inter-universality to suspend contradictions with labels." ([Ctv], p. 2LB/8);
- "amounts to working with differently labeled data assigned to different universes and independently manipulating them to behave as they would in a contradictory relationship without letting the contradiction occur, unless one removes the labels (the crux of the dispute with Scholze-Stix)." ([Ctv], p. 3CT/8);
- "I think the answer, in part, is that 1) the theory depends on inter-universality, 2) the general necessity of universes is already debated in mathematics today, and 3) IUT's use of universe-based labeling for

suspending contradictions fails under general label-removal." ([Ctv], p. 4CB/8);

· "In IUT, however, universes have a further role: to protect against contradictions. Mochizuki even distinguishes his work from the Grothendieck school in that IUT uses labels for relations that do not respect the labels (which means contradictions ensue without them)." ([Ctv], p. 5LM/8).

This sort of **provocative/sensationalist/exoticist/apocalyptic narrative** may make for great entertainment from the point of view of many internet users, but has **absolutely nothing** to do with the rather *mundane mathematical reality* of what is actually done in IUT:

(LB1) contradictions of the sort that occur in the argument on the final two pages of the Scholze-Stix manuscript are avoided in IUT *not by assigning distinct labels to different universes* (i.e., as in (FA2)), but rather by assigning **distinct labels** to **different elements/sets**, i.e., as is the standard practice when working with any sort of *set-theoretic object* in mathematics.

That is to say, the distinct labels in question in (LB1) are precisely the (entirely set-theoretic!) *labels* (n, m), where n, m are integers, of the vertices of the oriented graph underlying the log-theta-lattice (each of which corresponds to a distinct Hodge theater). This is an *entirely standard set-theoretic convention/apparatus* that is applied throughout mathematics for working with distinct elements/sets. There is **absolutely nothing** that is *"extraordinary/novel"* about this use of distinct labels.

Here, we note that it is true that at these distinct labels (n, m), *distinct rings/schemes* arise, which give rise to *distinct Galois categories*, hence, in particular, to *distinct universes*. On the other hand, there is **absolutely nothing** that is *qualitatively different*, at a foundational level, about this situation from the situation that arises classically in SGA1, where set-theoretically distinct schemes occur that give rise to distinct Galois categories, hence, in particular, to distinct universes. That is to say, at a foundational level, the circumstances in IUT that trigger the introduction of distinct universes in IUT are *precisely identical* to the circumstances in SGA1 that trigger the introduction of distinct universes in SGA1; moreover, these circumstances in both cases are entirely *classical* and *set-theoretic* in nature, i.e., the use of *distinct labels* when working with distinct set-theoretic objects, such as schemes. Here, we recall for the reader that in any classical discussion of schemes that involves, say, *two distinct schemes*, it is *entirely standard mathematical practice* to use *distinct labels*, e.g., "S" and "T" for the two distinct schemes, rather than, say, the *same label* "X" for the two distinct schemes.

One *fundamental misunderstanding* of [Ctv] that is closely related to (FA2) — and indeed may be regarded as a sort of "corollary" of (FA2) — lies in the following **false assertion**:

(FA2.1) IUT suffers (as a consequence of (FA2)) from the *fundamental defect* of exhibiting a *"vulnerability"* or *lack of "robustness"* when subject to arbitrary label-removals.

This false assertion (FA2.1) may be seen in the following passages of [Ctv]:

- "The Scholze-Stix argument shows that if one simply removes the labels from the beginning (by identifying the various copies of $\mathbb{R}$), and then looks at the theta-links, the contradictions become immediately manifest." ([Ctv], p. 1RB/8);
- "I think they're unlikely to abstain from deriving the contradiction, since mathematicians can remove labels as they so choose ... Presenting IUT with the caveat that the setup is sensitive to set-theoretic contradiction and one can only perform a key algorithmic step under highly specific circumstances has been unconvincing." ([Ctv], p. 2CT/8 $\sim$ 2CM/8);
- "Their algorithm shows that the necessity of inter-universality is also a vulnerability in the lattice; it's not robust against arbitrary label-removals." ([Ctv], p. 5CT/8).

In fact, this sort of *"vulnerability"* assertion (FA2.1) reflects a **fundamental misunderstanding**, on the part of [Ctv], of the way in which *set-theoretic mathematics works* (cf. the discussion following (LB1)!). Indeed, this "vulnerability" phenomenon is by no means limited to IUT; rather, it is an immediate consequence of the very nature of set theory that

(LB2) **no nontrivial set-theoretic mathematical structure/theory** (where, in this context, "nontrivial" may be understood as meaning that the structure/theory involves at least one set of cardinality ≥ 2) is *"robust"* with respect to **"arbitrary label-removals"**.

This phenomenon may be seen in

- the discussion of the *projective line/sphere* in [EssLgc], Example 2.4.7, as well as in
- the discussion of *"$\wedge$-/$\bigvee$-crystals"* in [EssLgc], §3.5

— i.e., examples that are particularly pertinent in light of the interesting structural analogies of the situations that arise in these examples with the situation that arises in IUT. On the other hand, as discussed above, this phenomenon is by no means limited to these examples and indeed appears *in essentially any discussion of set-theoretic mathematical objects*. For instance, one may witness this phenomenon explicitly in the following very elementary example involving the *additive structure* of the set $\mathbb{N}$ of *natural numbers*:

Example 2.2.1: The effect of arbitrary label-removals on addition of natural numbers. Write $\mathbb{N}^\dagger$ for the quotient set of $\mathbb{N}$ obtained by identifying the elements $2 \in \mathbb{N}$ and $3 \in \mathbb{N}$, i.e., by *"removing the distinct labels"* from the two distinct elements of $\mathbb{N}$ constituted by "2" and "3". Thus, if one *assumes* that addition of natural numbers in $\mathbb{N}$ is sufficiently *"robust"* in the sense that it is *unaffected* by the *label-removal* that gives rise to $\mathbb{N}^\dagger$, then one immediately obtains the *contradiction* $1+3=3$. In fact, of course, this "contradiction" does not imply the existence of any intrinsic defect in the theory of addition of natural numbers; rather, it simply implies that there is **no logical relationship** between addition in $\mathbb{N}$ and the operation induced on $\mathbb{N}^\dagger$ by addition in $\mathbb{N}$, i.e., *no logical relationship* that allows one to deduce statements concerning addition in $\mathbb{N}$ from corresponding statements concerning the induced operation on $\mathbb{N}^{\dagger}$.

Before proceeding, we recall that the phenomenon of *"distinct labels for different universes"* (cf. (FA2)) appears classically in the theory of

(DfUv1) the *profinite fundamental group* associated to a *Galois category*, developed in SGA1, as well as

(DfUv2) the general nonsense of *homological algebra* surrounding derived functors such as *cohomology functors*

(cf. the discussion of (AlgCl), (HomRs) in [EssLgc], §3.8). Here, we note that in both of these cases, the *necessity* of passing to a larger universe in order to obtain a *canonical construction* of the object of interest (i.e., profinite fundamental group or cohomology module) is a *very substantive issue* that is classically well understood and by no means "a matter of taste". Moreover, in both of these cases (DfUv1), (DfUv2), identification of the *larger universe* with the *original universe* — i.e., *removal* of the *distinct labels* for *different universes* (cf. (FA2)) — would result in a **contradiction**, namely, in the form of a **clear violation** of the **axiom of foundation**. That is to say, the classical theory of, say, *étale fundamental groups* of schemes (which is the main topic of *étale homotopy theory*!) does not satisfy *Boyd's "robustness" criterion*, despite the fact that Boyd actively endorses (cf., e.g., the discussion of [Ctv], p. 8CT/8 $\sim$ 8CM/8) étale homotopy theory as a topic of mathematical research that, unlike IUT, he finds to be "acceptable". Presumably, Boyd would also endorse the use of a very classical tool such as cohomology modules in mathematical research as "acceptable".

In the case of (DfUv1), this necessity arises from the *nonexistence of a canonical choice of basepoint*, which, in the very classical setting of the Galois theory of fields, corresponds to the *nonexistence of a canonical choice of algebraic closure* of a perfect field. Indeed, if one assumes the existence of a (functorially) canonical choice of algebraic closure, then, as discussed in [EssLgc], Example 3.8.1, it is very easy to derive a *contradiction*. Here, we note that canonical constructions play an important role in the theory of the *étale fundamental group* — hence, in particular, in *anabelian geometry* and *étale homotopy theory* — for instance, in situations where one wishes to consider *functorially induced canonical outer actions of groups* on étale fundamental groups.

In the case of (DfUv1), the nonexistence of a canonical choice of basepoint manifests itself concretely in the theory of étale fundamental groups developed in SGA1 in the form of **inner automorphism indeterminacies**. As I have emphasized in countless workshop talks and expository texts (cf., e.g., the discussion of [EssLgc], §3.8), as well as in the discussions with Boyd that led to [Cnv] (cf. [Ctv], p. 4RT/8),

(SGAIU) there is **absolutely no qualitative difference**, at a foundational level, between

- the use of universes/inter-universality phenomena in the classical theory of *étale fundamental groups* developed in *SGA1* and
- the use of *universes/inter-universality phenomena* in *IUT*;

the only difference lies in the particular sorts of set-theoretic objects that appear, namely,

- *rings/schemes* in the case of SGA1 versus
- *"non-ring-theoretic" abstract topological groups/monoids* (i.e., objects which are typically *weaker* than ring structures and hence give rise, in IUT, to more substantial indeterminacies, such as (Ind1), (Ind2), than the relatively minor inner automorphism indetermacies of SGA1) in the case of IUT

(cf. the discussion of [EssLgc], §3.8, preceding Example 3.8.1, as well as the discussion of [EssLgc], §3.8, following Example 3.8.4).

In the case of (DfUv2), the necessity of passing to a larger universe in order to obtain a canonical construction arises from the *nonexistence of a canonical choice of (injective/projective) resolution* for a module. That is to say, any sort of attempt to give a canonical construction, e.g., by taking the product over "all possible resolutions" in some model of set theory, immediately leads to a violation of the axiom of foundation, unless one works in a larger universe. This nonexistence of a canonical choice of resolution manifests itself concretely in the "indeterminacies" constituted by homotopy equivalences of resolutions, i.e., "indeterminacies" that may be factored out precisely by passing to *cohomology modules* (or, more generally, higher derived functors). Moreover, it is interesting to note in passing that these two *classical examples of inter-universality/indeterminacies* (i.e., (DfUv1), (DfUv2)) constitute, when taken together, a classical example of two theories that, on the one hand,

- exhibit *qualitatively essentially identical phenomena* at the level of *foundational* considerations concerning *universes/inter-university*, but, on the other hand,
- involve the use of *different types* of *set-theoretic objects*, as well as different types of *indetermacies*

— i.e., in a fashion that is reminiscent of the above comparison (SGAIU) of universes/inter-universality in SGA1 and IUT.

In the context of (DfUv1), (DfUv2), we observe in passing that if one is willing to sacrifice the *canonicality* of the construction, then one can always *avoid* the use of *universes* by working with *small Galois categories* (in the case of (DfUv1)) or *small collections of resolutions* (in the case of (DfUv2)), where the term "small" is to be understood as being relative to a fixed model of ZFC that is fixed throughout the discussion. On the other hand, as discussed above, this does *not* allow one to avoid the substantive *"inter-universal"* phenomenon of *indeterminacies*, i.e., *inner automorphisms* (in the case of (DfUv1), which, as discussed in [EssLgc], Example 3.8.1, are *unavoidable*) or *homotopy equivalences of resolutions* (in the case of (DfUv2)). The same phenomenon exists in the case of IUT (cf. the discussion of (SGAIU) above):

(SmCat) by *sacrificing canonicality*, one can avoid the use of universes by working with *small categories*, but again the **unavoidable** substantive aspect of the situation lies in the **"inter-universality"** of the various situations considered, i.e., which manifests itself concretely in the various **indeterminacies** that occur.

Finally, it is interesting to note that the very substantive classical phenomena discussed above in the context of (DfUv1), (DfUv2) are **faithfully reflected** in **Lean4 formalizations**. That is to say, I personally found it fascinating to learn, during the Lean workshop in Tokyo in July 2025, that first of all, Lean4 allows for the use of countably nested universes, and, moreover, that this apparatus for nested universes is *actually applied* in the *recently achieved Lean4 formalizations* of the classical constructions of (DfUv1), (DfUv2) (cf. also the discussion of (NoObs) in §3.2 below). I found this aspect of Lean4 formalization to be particularly impressive on account of the fact that these two classical situations (i.e., (DfUv1), (DfUv2)) are *precisely* the situations that I have been emphasizing for many years (of course, *completely independently* of any considerations related to Lean4!) in the context of explanations (such as the explanation given in [EssLgc], §3.8) of the use of universes/inter-universality in IUT.

§2.3. The issue of set-theoretic paradoxes. The final type of *fundamental mathematical misunderstanding* of [Ctv] that we discuss may be summarized in the following **false assertion**:

(FA3) Various objects, such as *prime-strips* and *theta-links*, that play a fundamental role in IUT *trigger set-theoretic paradoxes*.

This false assertion (FA3) may be seen in the following passages of [Ctv]:

- "This alone is an untenable method: an equivalence between q^{j^2} and q triggers a cascade of set-theoretic paradoxes, which Mochizuki calls '$\in$-loops'. However, one needs these '$\in$-loops', without contradiction." ([Ctv], p. 3LM/8);
- "Simulating '$\in$-loops' amounts to working with differently labeled data assigned to different universes and independently manipulating them to behave as they would in a contradictory relationship without letting the contradiction occur, unless one removes the labels (the crux of the dispute with Scholze-Stix)." ([Ctv], p. 3CT/8);
- "So, the theta-link threatens to trigger set-theoretic paradoxes, ..." ([Ctv], p. 3CM/8);
- "one responds to the failure of the isogeny method with a simulation method (e.g., prime-strips, theta-links); one responds to the set-theoretic paradoxes of the simulation method with inter-universality (e.g., different labels, Hodge theaters); ..." ([Ctv], p. 3RT/8).

First of all, it must stated categorically that the use of *prime-strips* and *theta-links* in IUT does **not trigger** any set-theoretic paradoxes. Rather, the use of prime-strips and theta-links is simply *incompatible*, in a very elementary set-theoretic sense, with the additive portion of the ring structures involved, thus obligating one to work with various *non-ring-theoretic structures* such as multiplicative monoids or Galois groups regarded as abstract topological groups (cf. the discussion of (SGAIU) in §2.3).

The *"simulation of '$\in$-loops'"* in IUT does *not* involve any set-theoretic paradoxes whatsoever. It simply refers to phenomena in which there is a certain kind of equivalence between the behavior of a *certain portion* of some mathematical object and the behavior of the *whole* mathematical object, i.e., in a fashion that is reminiscent, at a *purely philosophical level*, of the relation '$a \in a$', but does not give rise to any set-theoretic paradoxes in the literal sense. Well-known classical examples of this sort of phenomenon include the following:

- the case of a *modular curve* (i.e., a finite étale covering of the moduli stack of elliptic curves over the rational number field) of *genus* 1, i.e., an "elliptic curve" whose set of closed points may be thought of as corresponding to the *entire* set of isomorphism classes of (characteristic zero) elliptic curves (equipped with some additional data such as a level structure);
- the classical theory of complex functions of a *single holomorphic variable* $z = x+iy$, which behaves like a *single (complex holomorphic) dimension* as a result of a sort of *coupling* of the (*a priori* distinct and independent) *two underlying real dimensions* corresponding to "x" and "y".

§2.4. Relation to recent developments in anabelian geometry. We conclude the present §2 by discussing various *misunderstandings* and *misleading content* surrounding the recent developments in anabelian geometry related to IUT that were discussed in [Ctv]. These misunderstandings and misleading content center around the following *main thesis* of the discussion of [Ctv], p. 6/8 $\sim$ 8/8:

(AnMs1) The *anabelian content of IUT* (cf. Boyd's use of the term "arithmetic Teichmüller theory") can somehow be *separated* and *isolated* from the portions/aspects of IUT that relate to the *abc inequality*, thus allowing one (such as Boyd) to take a *positive* position toward the former, while maintaining a *negative* position toward the latter.

This point of view (AnMs1) may be seen in the following passages of [Ctv]:

- "Scholze and Stix don't give much attention to the anabelian geometry in IUT, as they remark in their manuscript that they don't see how absolute anabelian geometry, which they suggest is indeed a remarkable development in anabelian geometry, is needed for the IUT proof." ([Ctv], p. 6LM/8);
- "p-adic Teichmüller theory had its own book; arithmetic Teichmüller theory never got its own treatment. So, the novel anabelian content in IUT, in which one might *a priori* have the most confidence, was overshadowed both due to the proof controversy and the fact that the IUT papers basically jump straight into building Hodge theaters and pursuing the proof without explaining what this new Teichmüller theory really is." ([Ctv], p. 6CB/8 $\sim$ 6RT/8);
- "The other issue, in my view, is that of distinguishing the arithmetic Teichmüller theory from the rest of IUT. I still think one should be able to distinguish the Teichmüller-theoretic aspects of IUT from the abc proof strategy and even the inter-universal setup." ([Ctv], p. 6RM/8);
- "The multiradial representation is supposed to be a very general result about the multiplicative/additive structure of scheme theory

that one learns from arithmetic fundamental groups via the study of non-scheme-theoretic maps and anabelian reconstructions, but if that's the case, it should be amenable to formulation in a manner independent of the setup involving Hodge theaters, prime-strips, and the theta-link. Very few people care about it because of the IUT baggage. I don't see why the main result of arithmetic Teichmüller theory couldn't just be formulated in the familiar terminology of schemes, non-scheme-theoretic morphisms, arithmetic fundamental groups, anabelian reconstruction, and so on, with a short and simple paper." ([Ctv], p. 6RB/8 $\sim$ 7LT/8);

· "... the notion of studying arithmetic fundamental groups under non-scheme-theoretic mappings and reconstructions is interesting. However, I doubt many will take an interest if it is instantiated as the multiradial representation, with is attached to the abc proof strategy; there must be ways to explain this research area in relatively plain arithmetic-geometric language." ([Ctv], p. 7LM/8);

· "Mochizuki has himself said to me that he's not particularly interested in abc. Other mathematicians who have engaged with IUT, such as Assistant Professor Emmanuel Lepage (from the Institut de Mathématiques de Jussieu-Paris Rive Gauche), have, according to Mochizuki, also said that they are not interested in abc. Usually, only mathematicians indifferent to abc and well-versed in anabelian geometry have walked away satisfied." ([Ctv], p. 7CT/8);

· "From an anabelian perspective, looking at non-scheme-theoretic mappings is intriguing, for one might see how arithmetic fundamental groups behave, namely by decoupling arithmetic fundamental groups, as groups, from schemes, and finding new functorial relationships. However, one doesn't need to consider the theta-link or Hodge theaters to study this, as evidenced by applications to topics like GT-construction. It should be generalizable, without the IUT baggage; that would make it an arithmetic Teichmüller theory." ([Ctv], p. 8LM/8);

· "This research area, when viewed in its own right, is so simple and curiosity-provoking. As I heard repeatedly that Mochizuki and colleagues aren't particularly interested in abc, I thought it such a shame that the mathematics of most interest to them has been swept up in the abc proof controversy, one which I doubt will end in Mochizuki's favor. On the other hand, within the high-trust confines of AHGT, which is a very well managed collaborative project, I think there's space being created for setting drama aside and instead forming connections over the anabelian and étale-homotopic core that underlies the arithmetic-Teichmüller-theoretic content that one sees implicitly in the IUT papers. I still see these forthcoming 'Interface Papers', whose contents I do not know, as an opportunity to spell out the étale-homotopic and anabelian core of arithmetic Teichmüller theory, and perhaps really define it as a theory, distinct from absolute anabelian geometry and distinct from IUT." ([Ctv], p. 8LB/8 $\sim$ 8CM/8).

In some sense, the *fundamental point* that seems to be missed here is that

(SmMth) the *mathematics* that gives rise to a proof of the abc inequality in IUT is **precisely** the **same mathematics** as the mathematics that involves the multiradial representation and absolute anabelian geometry and is closely related to the theory of GT in combinatorial anabelian geometry; that is to say, although it may seem to make sense to distinguish the two from a social, political, psychological, or rhetorical point of view, when regarded from the point of view of the actual mathematics involved, this sort of **distinction** is **mathematically meaningless**.

Here, it is interesting to note that the *meaninglessness* of this distinction (SmMth) is already evident in various assertions that appear explicitly in Boyd's argument. For instance, he speaks of separating, for instance, in a "short and simple paper", the "novel anabelian content in IUT" and the multiradial representation of IUT from the "IUT baggage" consisting of Hodge theaters, prime-strips, and the theta-link, but it is self-evident from the very statement/content of the multiradial representation of IUT (i.e.,. [IUTchIII], Theorem 3.11) that this is nothing more than a *meaningless self-contradiction*. Moreover, despite the fact that Boyd speaks of separating the multiradial representation of IUT from the "abc proof strategy" and the "inter-universal setup", he also doubts that people will be interested in anabelian aspects of non-scheme-theoretic mappings if they are introduced via the multiradial representation of IUT on account of the fact that the multiradial representation of IUT is "attached to the abc proof strategy". Indeed,

· the "Teichmüller-theoretic aspects of IUT",
· the multiradial representation of IUT,
· the "abc proof strategy" of IUT,
· the "inter-universal set-up" of IUT involving distinct copies of Hodge theaters, and
· the anabelian aspects of "non-scheme-theoretic mappings"

refer to different aspects of, or ways of thinking about, **precisely** the **same mathematical content**.

In this context, it is also important to note that the aspects that Boyd refers to as the "arithmetic Teichmüller-theoretic" aspects of the *theory of GT in combinatorial anabelian geometry* also involve the use of *distinct copies* of rings and *inter-universality* in a fashion that is *entirely similar* to the way in which these aspects appear in IUT. In particular, it is **entirely mathematically meaningless**, to hold up, as Boyd does, this theory of GT as an example of separation of the issues of distinct copies and inter-universality from what he refers to as "the étale-homotopic and anabelian core of arithmetic Teichmüller theory". Moreover, it is important to recall in this context (cf., e.g., the discussion of [EssLgc], Example 3.5.3, (vi)) that the "general nonsense" aspect of either IUT or the combinatorial anabelian geometry theory of GT that involves *distinct copies* of (arithmetic) holomorphic structures is *by no means* unique to arithmetic theories such as IUT or the combinatorial anabelian geometry theory of GT and may also be seen in *classical complex Teichmüller theory*. Indeed, this analogy with *distinct* *copies* in classical complex Teichmüller theory, as well as the analogy with distinct copies in the classical theory of *crystals* (the discussion of *"$\wedge$-/$\vee$-crystals"* in [EssLgc], §3.5), underscores the fact that *this aspect of IUT has already been amply exposed on countless occasions and for many years now in "relatively plain arithmetic-geometric language"*.

Recent developments relating enhanced versions of IUT (that are currently under development) to the *Section Conjecture* (for hyperbolic curves over number fields) in anabelian geomtery are another important topic — i.e., alongside the combinatorial anabelian geometry theory of GT referred to above — of recent anabelian geometry-related research that is also related to IUT. Such recent developments involving the Section Conjecture also (i.e., again alongside the combinatorial anabelian geometry theory of GT referred to above) form an important topic of the *"interface papers"* (which are currently under preparation) referred to by Boyd. On the other hand, in this context, it must be noted that the application of IUT to the abc inequality is **literally** a **special case** of the application of (enhanced versions of) IUT to the Section Conjecture. In particular, this aspect of these recent developments again underscores just how **mathematically meaningless** it is to try to separate anabelian aspects of IUT from aspects of IUT related to the abc inequality.

With regard to the many anabelian geometers, such as Emmanuel Lepage, who have achieved a rigorous mathematical understanding of IUT, it is indeed accurate to state that such researchers are primarily interested in the anabelian aspects of IUT and are not particularly interested in the abc inequality *per se*, and that this stance in also reflected in my own point of view regarding IUT. (Incidentally, it should be mentioned that Lepage holds the position of "maître de conférences". This position is a tenured position, which, in some contexts, may be translated as "assistant professor", but is more commonly translated as "lecturer" or "associate professor".) On the other hand, it is *substantially misleading* to summarize their involvement with a highly ambiguous phrase such as *"walked away satisfied"*. For instance, this phrase could be taken — especially in the context of the *main thesis* (AnMs1) of the discussion of [Ctv], p. $6/8 \sim 8/8$ — to mean that they completely concurred with the *fundamental misunderstandings* (FA1), (FA2), and (FA3) of Boyd, but were nevertheless *satisfied* with the purely anabelian content of IUT in a form that, as Boyd advocates, was completely separated from the aspects of IUT that relate to the abc inequality.

In the case of Lepage, the discussion referred to in [Ctv], p. 7CT/8, arose in response to a question posed by me concerning why he, unlike various other mathematicians, was able to study and achieve a rigorous mathematical understanding of IUT. One important portion of his reponse was that he was able to do so because his primary interest was (not in the abc inequality itself, but rather) in the anabelian geometry aspects of IUT, namely, in

(AnDio1) *seeing how such anabelian geometry aspects could give rise to diophantine consequences* such as the abc inequality.

This point of view is *essentially identical* to my own and also shared by other anabelian geometers who are substantively engaged with the mathematical content of IUT. That is to say, his response refers to the *psychological mindset* that distinguished him from various other mathematicians with regard to studying IUT and achieving a rigorous level of mathematical understanding that was such that, in response to another question of mine, he confirmed that he was unable to see any mathematical reason not to acknowledge the mathematical validity of IUT. In particular,

(AnDio2) at **no** time has he, or any other anabelian geometer substantively engaged with the mathematical content of IUT, ever expressed any opinion to me to the effect that it is possible to **separate/isolate**, at the level of mathematical content (e.g., logical dependence), the *anabelian aspects of IUT* from the *diophantine aspects of IUT* in such a way that one could confirm the mathematical validity of the former, while denying the mathematical validity of the latter (cf. (AnMs1)).

In this context, it is also interesting to note that the *"strategic ambiguity"* of Boyd's use of the phrase "walked away satisfied" is reminiscent of his phrasing to the effect that the mathematical validity of the false assertion (FA1) is a sort of *subjective issue* that depends on the point of view of the mathematician in question (cf. (FA1.3), (FA1.4)).

We conclude the present §2.4 with a discussion of the

(AnMs2) activities of the *CNRS and AHGT* and the role of Stix in these and related activities.

In the case of (AnMs2), the problem is *not* with clear-cut inaccuracies related to (AnMs2) that appear in the text of [Ctv] (which I was unable to find), but rather with the *potential for misunderstandings* by third parties that might arise from the discussion of (AnMs2) in [Ctv].

First of all, although IUT is one of the topics covered by the AHGT network, which is supported by the CNRS, it is just one topic among a large variety of topics in arithmetic geometry that are covered by the AHGT. Perhaps the most succinct way to summarize this relationship is by observing that the relationship of IUT Summit 2025 with AHGT is only mentioned in *small print* at the *bottom of the page* on the webpages of the website for IUT Summit 2025.

The involvement of Stix with AHGT activities is mentioned in the text of [Ctv], p. 7CB/8 and p. 7RB/8. In this context, we note that Stix was listed as an online participant in IUT Summit 2025. This listing occurred as a result of a very brief e-mail exchange in which he expressed an interest in looking at the videos of talks given in IUT Summit 2025. In fact, the total amount of time of his online participation in IUT Summit 2025 was, according to the data that we were given, "0 minutes". During the week following IUT Summit 2025, Stix participated in a workshop given at Osaka University, Japan, in honor of Hiroaki Nakamura's 60-th birthday. At this workshop, just as in the case of the Oberwolfach workshop referred to in [Ctv], p. 7RB/8, he interacted with many anabelian geometers who are substantially involved in activities related to IUT. According to what I heard from other participants in both the Osaka University and Oberwolfach workshops, he was quite often in close physical proximity to positive, friendly discussions by other mathematicians concerning IUT, but he himself was *never involved in discussions concerning IUT*. This stance is entirely consistent with his position (cf. the discussion of (Myst) in §1.3; [Rpt23]) of refusing to respond to numerous offers on my part by e-mail to engage in further discussions (via e-mail/Zoom or in person) concerning IUT.

§3. Lean, the EMSCOP, and IUT

In each of the various cases examined in detail in §2, the false or misleading assertions given in [Ctv] exhibit the common tendency of being more oriented toward **social/political dynamics** — e.g., more *sensationalist* and *internet click/entertainment-oriented* (cf. the discussion in §1.1, §1.3 of my conservative stance with regard to engagements with journalists) — than toward the **mathematical truth** that underlies the topic under consideration. In the past, I have responded to such situations by emphasizing the importance of **mathematical accountability** and the passage of the [EMSCOP] quoted in §1.1 (cf. also the discussion of §1.3). On the other hand, there are practical limits with regard to enforceability and efficacy of this sort of *"analog"* approach. As discussed in [EssLgc], §1.12 (cf. also §3.1 below), I also remain substantially skeptical about the practical efficacy of Lean-style computer formalization in the context of certain types of social/political issues. Nevertheless, I feel strongly about the **fundamental strategic importance** of Lean-style computer formalization in the context of the situation surrounding IUT (cf. §3.2 below) in the sense that, while formalization might not result in any sort of complete and definitive resolution of numerous social/political issues, it nonetheless represents the

(LbMth) **best** and perhaps the **only technology available** (either currently or for the foreseeable future) for achieving meaningful progress with regard to the fundamental goal of **liberating mathematical truth** from the *yoke of social/political dynamics*.

It is precisely for this reason that, in addition to recent important mathematical research developments in IUT and related anabelian geometry, the topic of Lean-style computer formalization has become an **increasingly central focus** of research activities in the anabelian geometry community at RIMS (Kyoto University) in recent years.

§3.1. Social/political dynamics versus mathematical truth. Although Lean-style computer formalization seems to represent the *best hope* that is currently available (cf. (LbMth)) for *bridging the gap* between the **social/political dynamics surrounding social acceptance** of *mathematical (or, more generally, scientific) ideas* and **mathematical truth**, it does not appear at the present time to constitute any of sort of **"magical cure"** for the *complete resolution* of *social/political issues*, especially in situations where there are very strong undercurrents of **"faith en masse"** involved. In this context, I am reminded of the (perhaps humorously anachronistic) phenomenon of *"flat-earthers"*, as well as the following stories that I heard when I was a graduate student in the late 1980's to early 1990's at Princeton University:

· some local government in the United States of America passed a law to the effect that, within its local jurisdiction, π (i.e., the length of the cirumference of a circle of unit radius) is equal to 3;
· some survey of the general population of the United States of America found that a surprising percentage of the population believe that one-third is greater than one-half (i.e., "$\frac{1}{3} > \frac{1}{2}$"), that is to say, since three is greater than two.

In either of these two cases, it seems highly unlikely that presenting a Lean-style computer formalization of the irrationality of π or of the inequality $\frac{1}{2} > \frac{1}{3}$ to the people involved would result in a change in the minds or hearts of these people.

In some sense, this phenomenon is closely related to a *more fundamental* and *more scientific* issue, namely, the issue discussed in [EssLgc], §1.12 (cf., especially, [EssLgc], Examples 1.10.1, 1.12.1) of the

(LnGp) potential **gap** between what is actually verified by a *particular piece of Lean code* and the *human interpretation/intentions* that are attached to the Lean code.

In fact, this issue in some sense plagues *any kind of computer code* — or, more generally, *any kind of technology* — i.e., the fundamental issue of

(TecGp) *whether or not the computer code/technology actually does what is it is intended/supposed to do.*

In this context, we recall that in [EssLgc], §1.12, the elementary example of the *Pythagorean theorem*, i.e., "$x^2 + y^2 = z^2$" (where $x \leq y < z$ denote the lengths of the sides of a right triangle), is discussed in detail (cf. [EssLgc], Examples 1.10.1, 1.12.1). The main point of this discussion is that

(PyGp) it is difficult to see how any sort of Lean-style computer formalization of the **truth** of the *Pythagorean theorem*, i.e., "$x^2 + y^2 = z^2$", can yield any progress of a social/political nature in a situation where there are very potent *social/political dynamics* in force, e.g., in the form of a *faith en masse*, to the effect that the Pythagorean theorem states that "$x^2 \cdot y^2 = z^2$", hence may be verified, e.g., by a Lean-style computer formalization, to be *false*.

Of course, the *logically appropriate interpretation* of the simultaneous *truth* of "$x^2 + y^2 = z^2$" and *falsity* of "$x^2 \cdot y^2 = z^2$" is that the mathematical validity of "$x^2 + y^2 = z^2$" is *logically unrelated* to (i.e., in the sense that this mathematical validity is not equivalent to and, moreover, does not imply) the mathematical validity of "$x^2 \cdot y^2 = z^2$", but the point here is that, depending on the social/political dynamics in force, this logically appropriate interpretation can be suppressed by means of some sort of *logically/technically absurd narrative*, as described in (PyGp).

In some sense, the *false assertions* (FA1.3), (FA1.4) of [Ctv] discussed in §2.1 amount to a *conflation* of this sort of *purely social/political phenomenon* (i.e., as in (PyGp)) with a *substantive mathematical issue*. As discussed in §2.1, in some sense, the *most fundamental mathematical/logical tool* that can be applied in this sort of situation is the **Existence Principle** (ExPr) discussed at the end of §2.1. On the other hand, it is by no means clear whether or not even this Existence Principle has the power to penetrate the *faith en masse* of quite substantial populations of people.

More generally (i.e., moving beyond the narrative framework of [Ctv]), the whole situation surrounding the assertions of Scholze-Stix concerning IUT constitutes a sort of *textbook example* of the phenomenon discussed in (PyGp). As discussed in (Myst) (cf. also the discussion surrounding (Myst) in §1.3; [Rpt23]),

(SSGp) repeated encouragements on my part to various mathematicians (i.e., not just to Scholze-Stix, but also to other mathematicians who invoke the assertions of Scholze-Stix), often involving references to the [EMSCOP] passage cited in §1.1, to provide **full details** of the **precise statement** and **proof** of any sort of assertion of a **logical relationship** between the Scholze-Stix version of IUT (cf. (SSA2)) and the actual mathematical content of IUT have been met with either **refusals to respond to e-mails** or **blind declarations of faith** in Scholze-Stix, e.g., on the grounds that Scholze-Stix are *"top mathematicians"*.

Often, in response to such *blind declarations of faith*, I have posed the following questions concerning the **precise content** of the person's professed **faith**:

(Fth1) Does your professed faith in Scholze-Stix consist of the belief that a *precise statement/complete proof* as in (SSGp) already exists somewhere in *explicit written form*, preferably, in the form of a paper published in an *internationally recognized mathematical journal with a functional peer review system*? If so, can you give me a precise reference for this explicit written documentation of a precise statement/complete proof as in (SSGp)?

(Fth2) If the answer to the first query of (Fth1) is negative, then does your professed faith in Scholze-Stix consist of the belief that the assertions of Scholze-Stix should be granted — say, in the spirit of **absolute monarchism** and the **"divine right of kings"** (cf. the discussion of the final portion of §1.3) — a priviledged status of **unconditional eternal absolute tautological mathematical validity**, i.e., that Scholze-Stix should be **perennially exempt** from any obligation whatsoever to provide explicit written documentation as in (Fth1)?

I have yet to receive a meaningful sincere answer to either of these queries (Fth1), (Fth2) from any mathematician who makes a *blind declaration of faith* in the assertions of Scholze-Stix concerning IUT.

In this context, it should be recalled that the fact that *Fermat* is generally regarded as a *"top mathematician"* of his day does *not by any means* — e.g., by any invocation of the *"divine right of kings"* as in (Fth2)! — imply that he did indeed have a rigorous mathematical proof of "Fermat's Last Theorem". The history of mathematics is in fact replete with similar examples that amply demonstrate the folly, as well as the *deeply destructive effect* on the *development of mathematics*, of social/political dynamics that bestow on any mathematician or mathematical assertion a priviledged status of *perennial exemption* from any obligation whatsoever to provide precise, explicit written statements and proofs (as in (Fth1) or indeed the [EMSCOP] passage cited in §1.1).

As was emphasized in the e-mails cited in [Rpt23], I find it *deeply puzzling* that, in the context of IUT, discussions (such as that of [Ctv], although this phenomenon is, of course, by no means limited to [Ctv]!) tend to be predicated on the narrative that there are **two opposing sides** involved. That is to say, my understanding of the **conceptual core** of the **discipline of professional mathematics** (cf. the [EMSCOP] passage cited in §1.1) is that

(CmGl) explicit written documentation of *precise statements* and *complete proofs* of *mathematical assertions* — and, hence, by extension, ultimately, the **Lean-style computer formalization** of mathematical assertions — is the **common goal** of all professional mathematicians, that is to say, all professional mathematicians are supposed to be **fighting** on the **same side**, namely, in the context of IUT, to achieve, ultimately, the complete Lean-style computer formalization *not only of the actual mathematical content of IUT proper* (cf. the discussion of §3.2 below), but also *of all mathematical assertions by other mathematicians concerning IUT* in as *timely* and *efficient* a manner as is technically possible.

In this sense, so long as efforts/progress is being made with regard to the Lean-style computer formalization of IUT (cf. the discussion of §3.2 below), it is *by no means* the case that the situation surrounding the assertions of Scholze-Stix concerning IUT is *"practically frozen"*, as is asserted in [Ctv], p. 1CB/8.

In the context of the phenomenon discussed above in (LnGp), (TecGp), (PyGp), (SSGp), despite the apparent possible lack of efficacy of Lean-style computer formalization in situations where this sort of phenomenon occurs, it is important to emphasize that even in such situations, there are nonetheless various ways in which Lean-style computer formalization can play a *meaningful role* in **certain social/political contexts**. In the remainder of the present §3.1, we would like to examine such *aspects of Lean-style computer formalization* in more detail.

First of all, it should be emphasized that one fundamentally important aspect of Lean-style computer formalization is the following:

(Asp1) Lean-style computer formalization yields a **permanent, explicit record** of the **logical structure** of the mathematics involved that **operates completely independently** of any **human beings**, i.e., such as the mathematicians whose work gave rise to the mathematics involved; in particular, it continues to operate even after all mathematicians involved in the creation of the mathematics involved are *deceased*, e.g., even at some time in the future when there *no longer exist any human beings who understand the mathematics involved.*

This aspect (Asp1), combined with the important *Existence Principle* (ExPr), are of *fundamental importance* for the **sustained development** of the discipline of professional mathematics in the **very long-term** (i.e., not just decades, but centuries, etc.).

The second fundamentally important aspect of Lean-style computer formalization that we discuss — which is in fact closely related to (Asp1) — is the following:

(Asp2) Lean-style computer formalization yields a sort of **ideal, universal colleague** to address when writing/exposing mathematics; that is to say, whereas directing activities in the writing/exposion of mathematics (i.e., such as writing papers, giving talks, etc.) toward other colleagues in the traditional way, which is *fundamentally predicated on the existence of suitable colleagues*, i.e., colleagues who have a high degree of professional training and mathematical understanding in the *(often, for unavoidable technical reasons, very narrowly defined) relevant field of expertise*, Lean-style computer formalization makes it possible for a mathematician to direct his efforts when writing/exposing mathematics toward a goal whose range of efficacy transcends any **particular mathematical field** or **social grouping** in the mathematical community.

In the future (cf. the discussion in the final portion of §1.2), combining Lean-style computer formalization with **artificial intelligence** technology may even make it possible to *generate, simply by inputing Lean code into a suitable artificial intelligence device*,

- *expository textbooks*,
- *expository video talks*, or even
- *ChatGPT-style dialogue software* that allows one to discuss and ask technical questions

concerning the mathematics involved.

In the case of IUT, the original IUT papers [IUTchI, II, III, IV] were written with the **anabelian geometry** community centered around (but by no means limited to!) RIMS, i.e., the mathematical community in which I had been active since the mid-1990's, in mind. In particular, it should by no means come as a surprise that this community has been able to engage with IUT in a mathematically appropriate fashion, i.e., to achieve a rigorous level of mathematical understanding of IUT and hence to confirm the *mathematical validity of IUT* (cf. the discussion surrounding (AnDio1)). Here, it should be noted that it is

(NwExp) entirely **standard practice** in professional mathematics for research papers to be written with a rather **narrowly defined circle of experts** in mind.

It seems that some *analytic number theorists*, as well as some *arithmetic geometers* who lie outside the school of anabelian geometry that centers around (but is not limited to) RIMS, have taken issue with the fact that the original IUT papers [IUTchI, II, III, IV] were written in a way that was directed toward this narrowly defined anabelian geometry community, but the point that must be kept in mind is that this is entirely *standard practice* (cf. (NwExp)) for a *very simple technical reason*:

(TcUnf) it is **simply entirely technically unfeasible** for every research paper in mathematics to serve also as a *complete, self-contained textbook* that contains pedagogical material that encapsulates the level of mathematical understanding that is usually obtained via a *complete graduate education and career as a professional mathematician* in some narrowly defined technical field of specialty.

Note, moreover, that *quite intensive attempts* over the years since the release of the IUT papers in 2012 both on my part and on the part of other mathematicians engaged with IUT via personal (e-mail/face-to-face) communications or workshops on IUT to engage with mathematicians from other fields of number theory or arithmetic geometry concerning the topic of IUT have, for the most part, not been successful. Thus, it is precisely for this reason that the goal of Lean-style computer formalization constitutes the *natural (and indeed perhaps the only realistic) "next step"* for making meaningful progress with regard to the task of *exposing/recording the mathematical content of IUT* (cf. (Asp1), (Asp2), as well as the discussion of §3.2 below).

The discussion of (Asp2) leads naturally to the discussion of the following final two aspects of Lean-style computer formalization:

(Asp3) Once it becomes practical to generate textbooks, video talks, and other expository material simply by inputing Lean code (cf. the discussion following (Asp2)), one could imagine a *fundamental revolution* of the **business model** underlying the **employment practices** of **universities**, whereby researchers in mathematics would be *liberated from (undesired) teaching duties* (especially concerning uninteresting elementary topics) in return for work devoted to Lean-style computer formalization of mathematics in their field.

(Asp4) The mathematical rigor that is essentially guaranteed by mathematics that has undergone Lean-style computer formalization could result in a *fundamental revolution* of the **business model** underlying the **hiring/promotion practices** of **universities**, as well as in **peer review practices** of **mathematical journals**, whereby individuals — i.e., even, potentially, *"complete amateurs"*, who have not received a formal education in mathematics — could be hired for/promoted to high-ranking positions (such as professor, associate professor, etc.) at universities or have their manuscripts published in prestigious mathematical journals, not on the basis of traditional criteria such as educational background, papers published, recommendations by senior mathematicians, etc. — all of which can be very strongly influenced by **social/political dynamics** — but rather on the basis of **Lean code** that is submitted to the university or journal in question.

Here, it is important to note that examples of the culture of basing hiring/promotion practices *not* on traditional criteria such as educational background, papers published, recommendations by senior scholars, etc., but rather simply on the *computer code* or *device* that an applicant submits already exist in the *information technology industry*.

When taken as a whole, these last two aspects (Asp3), (Asp4) have the potential, in the perhaps not so distant future, of paving the way to **fundamentally revolutionizing** the way in which **mathematics departments** at universities operate. Another aspect of this revolution may be an increasing shift to **exclusively (or principally) online universities**, such as the recently established Zen University in Japan. In this context, it is interesting to note that the Lean workshop held in July 2025 that was referred to in the discussion of §1.2, §2.2 was held at the **Zen Mathematics Center** (ZMC), an international research institute that was established at Zen University *"with the aim of promoting and developing modern mathematics with a focus on arithmetic geometry and the formalization of modern mathematics using computer languages"*.

§3.2. The fundamental importance of Lean in the context of IUT. As was mentioned at the beginning of the present §3, in addition to recent important mathematical research developments in IUT and related anabelian geometry, the topic of *Lean-style computer formalization, especially in the context of IUT,* has become an *increasingly central focus* of research activities in the anabelian geometry community at RIMS in recent years. This state of affairs is reflected, for instance, in the Lean workshop in July 2025 in Tokyo, which focused on *anabelian geometry* and *computer formalization*, and in which many members of the RIMS anabelian geometry community (including myself) participated. This state of affairs is also reflected in plans for a *special year*, to be held during the Japanese school year April 2027 $\sim$ March 2028 on various aspects of arithmetic geometry, in which both the mathematical content of *IUT and related anabelian geometry* and the *Lean-style formalization of anabelian geometry* are included as important topics. This special year includes plans for an IUT workshop in the spring of 2028, i.e., a sequel *"IUT Summit 2028"* to the IUT Summit 2025 held in March 2025, and it is expected that the *Lean-style formalization of IUT* will also be one of the main topics covered at this workshop.

I have also been *deeply impressed* and *encouraged* by the *entirely unanticipated enthusiasm* that has been exhibited in recent years by computer scientists deeply involved with the development of Lean who are not mathematicians, and whose work has no direct connections to arithmetic geometry (hence, *a fortiori*, to IUT!), but who have expressed a *keen interest* in learning more about the situation surrounding IUT and, in particular, investigating what can be done with regard to pursuing the goal of *Lean-style formalization of IUT*. Such computer scientists, despite being disconnected from the mathematical community in a strict professional sense, nevertheless have substantial personal interaction with professional mathematicians, and it is through such personal connections that this enthusiasm was communicated to me. Moreover, as a result of these ties between the Lean-development community and the mathematical community, I have been invited to give an online talk on the topic of *Lean-style formalization of IUT* at a workshop on formalization that has been scheduled to be held in the spring of 2026 in the United Kingdom.

In this context, we recall that, as discussed in detail in §2.2 (cf., especially, (SGAIU)), there is **absolutely no qualitative difference**, at a foundational level, between

- · the use of universes/inter-universality phenomena in the classical theory of *étale fundamental groups* developed in *SGA1* and
- · the use of *universes/inter-universality phenomena* in *IUT*.

It is for this reason that

(NoObs) the discussion, at the July 2025 Lean workshop in Tokyo, of the recent **Lean4 formalization** of the SGA1 construction of the *profinite fundamental group* associated to a *Galois category* such as the Galois category of finite étale coverings of a connected scheme (cf. the discussion of (DfUv1) in the final portion of §2.2) convinced me that, at least at a theoretical level (i.e., modulo the fact that it would require quite a lot of work!), there should be **no fundamental obstruction** whatsover to the **formalization of IUT** via **Lean 4**.

In the context of (NoObs), it is important to remember, however, that efforts toward the *Lean-style formalization of IUT* have only just begun in recent years, and it will still probably take *at least several more years of concerted effort* before meaningful results, even relative to suitable "blackboxes", can be achieved (cf. the discussion of (LnCom), (MthFm), (BBxFm) below). Indeed, it is perhaps useful to recall that, at least according to reports that I have received, the *Lean-style formalization of Wiles' work on Fermat's Last Theorem has still, at the time of writing of the present report, not been completed*.

In the case of IUT, *one particularly important aspect* of Lean-style formalization lies in the following *essentially self-evident observation*:

(LnIm) When *writing and executing Lean code*, there is **absolutely no need** to address such issues as the following:

(LnIm1) the issue of conforming to the **tastes**, either in *writing style* or in terms of *how the theory is developed*, of particular senior researchers in fields of number theory/arithmetic geometry that are somewhat far removed from the sort of anabelian geometry that is applied in IUT;

(LnIm2) the issue of responding to harsh criticism of the **entirely elementary** nature of the *essential logical structure* underlying IUT — as may be seen, for instance, in the $\wedge(\vee)$**-chain** that forms the central topic of [EssLgc] — on the grounds that this entirely elementary nature of central aspects of IUT is *"inconceivable"* for a theory such as IUT that has highly nontrivial diophantine consequences and, moreover, *"deeply insulting"* to the intelligence/expertise of certain senior researchers.

That is to say, the writing and execution of Lean code is **inherently immune** to these sorts of social/political issues that have substantially obstructed the dissemination of IUT in the past.

Here, we note that (LnIm1) has, in the past, been particularly **pernicious** in that it has often resulted in mathematicians who attempt to **reformulate IUT** in a way that conforms to their own personal tastes, only to produce a **fabricated version** of IUT that is **logically unrelated** to IUT in its original form (cf. the discussion of *"logically unrelated fabricated versions"* of IUT in [EssLgc], §1.8, §1.10, §1.11, §1.12) and (unlike IUT in its original form) is *entirely devoid of any meaningful mathematical content*. Such logically unrelated fabricated versions often result in a tremendous amount of **entirely unnecessary confusion** concerning IUT. Of course, the most central example of this phenomenon may be seen in the *Scholze-Stix manuscript* (cf. (Myst)), but another notable example may be seen in the preprints discussed in [Rpt24].

In the context of (LnIm), it is also interesting to note that,

(ImApx) relative to the property of **immunity** to the issues (LnIm1) and (LnIm2) discussed in (LnIm), *talented young researchers at elite universities* — such as Emmanuel Lepage at the IMJ-PRG in France or Yuichiro Hoshi and Shota Tsujimura at RIMS — who specialize in topics in anabelian geometry that are closely related to IUT constitute a **far better approximation** to Lean-formalization than senior researchers in fields of number theory/arithmetic geometry that are somewhat far removed from the sort of anabelian geometry that is applied in IUT.

Moreover, in this context, it is important to observe that the **overwhelming heuristic validity** of (ImApx) has been *confirmed countless times* over the years since the release of the IUT papers in 2012, despite the quite *harsh criticism* and *scornful condescension* often heaped on the IUT-related activities of such talented young researchers by senior researchers of the sort mentioned in (LnIm), (ImApx).

From a practical point of view, in some sense, the *main obstacle* to Lean-style formalization of IUT lies in the *complete nonexistence* (at the time of writing of the present report) of professional mathematicians who are equipped with both

· a *thorough, rigorous mathematical understanding of the mathematical content of IUT* and
· *professional expertise in writing Lean code.*

Thus,

(LnCom) the task of **communicating** the *mathematical content of IUT* to mathematicians (such as arithmetic geometers) with *professional expertise in writing Lean code* is one important area of currently ongoing efforts with regard to the goal of Lean-style formalization of IUT.

Such efforts, however, tend to be fraught with *precisely the same dilemmas* (cf. (LnIm1), (LnIm2)) that occur more generally with regard to the dissemination of IUT to other mathematicians.

On the other hand, one can take a somewhat different point of view with regard to the goal of Lean-style formalization of IUT — a point of view that is *far less dependent* on bridging the *communication gap* discussed in (LnCom) — as follows. This point of view is the point of view that led to the exposition of [EssLgc], §3, i.e., in particular, to the $\wedge(\vee)$-*chain*, which forms the central topic of [EssLgc]. Here, we recall that this exposition of [EssLgc], §3, consists of distilling the essential logical structure of IUT down to the following **central components**:

· the *coricity* and *symmetry/non-symmetry* properties of the *log-theta-lattice* (cf. [EssLgc], §3.1 $\sim$ §3.4);
· the formalism of *descent* (cf. [EssLgc], §3.9);

· the actual descent procedure surrounding the *multiradial representation of the theta-pilot* (cf. [EssLgc], §3.10, (Stp1) $\sim$ (Stp3));
· the adjustment operations surrounding the *holomorphic hull* (cf. [EssLgc], §3.10, (Stp4) $\sim$ (Stp6));
· the *"ladder argument"* consisting of a certain *symmetrization* procedure to adjust for the log-shift in the 1-column (cf. [EssLgc], §3.10, (Stp7) $\sim$ (Stp8)).

The *central problem* with this exposition of [EssLgc], §3, i.e., from the point of view of the goal of Lean-style formalization of IUT, is that the exposition in [EssLgc], §3, of these central components still contains quite a bit of nontrivial *anabelian geometry* and, more generally, *arithmetic geometry*, which acts as a sort of *barrier* from the point of view of mathematicians who lack the necessary expertise in anabelian geometry. Thus, to summarize, one can take the point of view that

(MthFm) the *essential task* that must be completed in order to expedite the achievement of the goal of Lean-style formalization of IUT does not lie primarily (or in any essential sense) in bridging the *communication gap* discussed in (LnCom), but rather in **mathematically reformulating/formalizing** the *central components* discussed above in **abstract, purely formal/combinatorial language** that does *not contain any mathematical content* from anabelian geometry (or even arithmetic geometry) — i.e., in the style of the $\wedge(\vee)$-*chain* of [EssLgc] or the notion of *descent* discussed in [EssLgc], §3.9 — and hence may be readily understood by essentially *any professional mathematician or computer scientist who is comfortable with such abstract, formal/combinatorial arguments.*

In particular, the **mathematical formalization** discussed in (MthFm) may be regarded as a sort of *natural extension* of the theory of [EssLgc], §3. Alternatively, (cf. the discussion of and following (SymIUT) in [EssLgc], §1.12; the discussion of [EssLgc], Example 3.10.2, (i))

(BBxFm) one may think of the *mathematical formalization* that is proposed in (MthFm) as the task of **reorganizing** the mathematical content of IUT into suitable **blackboxes** consisting of *relatively straightforward, classical mathematical content in anabelian geometry* (or, more generally, arithmetic geometry) in such a way that the remaining mathematical content of IUT, i.e., IUT regarded modulo those blackboxes, consists *solely* of **abstract, purely formal/combinatorial** arguments that may be readily understood by essentially *any professional mathematician or computer scientist who is comfortable with such abstract, formal/combinatorial arguments.*

The development of a *mathematical formalization of IUT* as in (MthFm), (BBxFm) is currently one of the **central research topics** of anabelian geometers at RIMS. Also, it is hoped that such a mathematical formalization of IUT, once completed, will also serve as a *useful learning resource* for arithmetic geometers studying IUT.

Here, we note that the sort of *reorganization* proposed in (BBxFm) in some sense goes in precisely the *opposite direction* to the way in which mathematical papers are typically written (cf. (NwExp), (TcUnf)), i.e., for consumption primarily by experts in some narrowly defined field of specialty. Typically,

(BBxDD) it is very difficult to write mathematical papers in the way proposed in (BBxFm) since the **reorganization into blackboxes** of the sort proposed in (BBxFm) can only come after a sufficient amount of time has elapsed for the mathematical content under consideration to be **digested** and **distilled** to the degree that this sort of reorganization comes to seem *very natural* and can be achieved *relatively painlessly*.

This last observation (BBxDD) brings to mind the following *historical observation*. The sort of *mathematical formalization of IUT* proposed in (MthFm), (BBxFm), taken together with the development over the years of expositions of IUT such as [Alien] and [EssLgc], is, in some sense, reminiscent of the development of **class field theory**. That is to say, subsequent to the work of Teiji Takagi on *global class field theory* in the early 20th century, the theory underwent various *reformulations* including the reformulation using *group cohomology* by Emil Artin and John Tate, an approach that was also applied to further develop *local class field theory*. More recently, in the 1980's, Jürgen Neukirch was involved in developing yet another approach to formulating both local and global class field theory that is based on a very *abstract, formal* formulation of class field theory and appears to have been closely related to his work on anabelian topics such as the *Neukirch-Uchida Theorem*. In this context, it is interesting to observe that, as was pointed out by Ivan Fesenko, the local portion of this formulation of class field theory by Neukirch is reminiscent in various respects of the *monoid-theoretic* way in which local class field theory is applied in IUT.