COMMENTS ON THE MANUSCRIPT BY SCHOLZE-STIX CONCERNING INTER-UNIVERSAL TEICHMÜLLER THEORY (IUTCH)

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In the following, we make various *Comments* concerning the manuscript [SS2018-05] by Scholze-Stix (SS).

(C1) Title, first two paragraphs, and §1: It is interesting to note that here, explicit mention is made of the ABC Conjecture, but not of IUTch. Although very strong assertions are made in the title and first two paragraphs concerning, in effect, the validity of IUTch, the remainder of [SS2018-05] only discusses various *doubts* or *questions* by SS in a *rather vague form without any rigorous proofs* — or even *sketches of the main ideas* of such proofs — of the negative assertions that appear (cf. the remaining Comments below for more details). A related observation is that the discussion of [SS2018-05] is only *very partially linked*, at the level of notation, terminology, or precise references, to the theory that is actually developed in [IUTchI], [IUTchII], [IUTchIII], [IUTchIV]. For instance, [SS2018-05] never mentions (or even discusses in different, but essentially equivalent, terminology) various *key notions in IUTch* such as "multiradial", "log-shell", "tensor-packet", "procession", or the indeterminacies "$(\text{Ind}1, 2, 3)$". In this context, it is worth noting that the *main assertion* of [SS2018-05] appears to be the assertion summarized at the end of §1, on the top of p. 4.

(C2) §2.1, (2): With regard to the following text:

> "Generally, the discussions in Kyoto were at a level only slightly more sophisticated than what is reflected in the simplifications below, and Mochizuki agreed that this does not result in an essential obfuscation of the ideas."

I should point out that whether or not a specific simplification results in "an essential obfuscation of the ideas" depends substantially on the *aspect* of those ideas that is under discussion. Given a *specific aspect*, it is quite possible that a certain specific simplification does not result in "an essential obfuscation of the ideas". On the other hand, the same specific simplification may in fact result in "an essential obfuscation of the ideas" with regard to *other aspects* of those ideas. Also, it should be stated clearly that *I did/do not at all concur (i.e., either during or subsequent*

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to the March discussions) with the latter portion (i.e., the portion following "and") of the following assertion:

> "We also discussed the deeper parts of the theory, and Mochizuki agreed that we had a good understanding of the substantial mathematical content."

I agree that SS had a good understanding of *certain limited portions* of the substantive mathematical content of IUTch (i.e., "the deeper parts of the theory"). On the other hand, there are *many quite substantial/central portions of IUTch* which are *completely misunderstood* by SS. Put another way, SS have assimilated *quite a number of the superficial technical details* of various constructions that appear in the theory, but, at least judging from many of the assertions that appear in [SS2018-05], still have only a *very minimal understanding* (cf. the discussion of [Rpt2018], as well as the various Comments given below) of **why** *those constructions are introduced*, or **how** *those constructions are used in the theory to draw various nontrivial conclusions*.

(C3) §2.1, (3): With regard to the following text:

> "When it comes to the more drastic simplifications indicated below, such as merely identifying the choice of a Hodge theater with the choice of a curve abstractly isomorphic to X, or simply identifying identical objects of objects along the identity, these are inessential to the point we are making, but Mochizuki was not able to explain during the week why such a simplification was not allowed."

It should be stated clearly that the assertion that "these are inessential to the point we are making" is **completely false**! I made numerous attempts to explain this during the March discussions, and it is most unfortunate that we were ultimately unable to communicate regarding this issue. At any rate, detailed explanations (which are somewhat more detailed and better organized than my "real time impromptu" responses during the March discussions) are given in [Rpt2018].

(C4) §2.1, (4): With regard to the following text:

> "We are certain that even with all subtleties restored, the issue we are pointing out will prevail, and it is easier to point to the key issue with these surrounding subtleties removed."

It should be stated clearly that this assertion is **completely false** and reflects numerous *fundamental misunderstandings*, as discussed in [Rpt2018]. Moreover, this sort of "blanket statement" does not in any sense constitute a *rigorous proof*, or even a *sketch of the main ideas of such a proof*, of the validity of the central assertions of [SS2018-05].

(C5) Remark 8: This Remark denies the *necessity* of applying *anabelian geometry* in IUTch. This denial is justified in this Remark by the existence of anabelian results that imply that isomorphisms of fundamental groups arise from isomorphisms of schemes. In fact, **étale-like structures**, i.e., such as Galois groups and arithmetic fundamental groups, play a **central role** in IUTch, on account of the **structural properties** — such as **symmetry properties** (i.e., with respect to *permuting the* 0- *and* 1-*(vertical) columns of the log-theta-lattice*; such symmetry properties form the basis for various *multiradial algorithms*) — that étale-like structures satisfy within the log-theta-lattice. Indeed, in this context, it seems natural to pose the following questions:

(Q1) Do SS wish to deny that the log-theta-lattice **fails** *to admit an automorphism* that *permutes the* 0- *and* 1-*columns*? We refer, for instance, to [Rpt2018], (LbΘ), (Lb$\mathfrak{log}$), (LbMn), for more details. If SS wish to claim the existence of such an automorphism, they should give the construction of such an automorphism explicitly.

(Q2) Do SS wish to deny that the *étale-like* (i.e., Galois group/arithmetic fundamental group) portion of the log-theta-lattice **does** *admit an automorphism* that *permutes the étale-like portions of the* 0- *and* 1-*(vertical) columns*? We refer, for instance, to [Rpt2018], (EtΘ), (Et$\mathfrak{log}$), (EtMn), for more details. If SS wish to deny the existence of such an automorphism, they should give an explicit proof of the non-existence of such an automorphism.

(C6) §2.1.3: One central assertion of §2.1.3 is as follows:

> *No substantive problems* occur in IUTch if one *identifies the domain and codomain of the* $\mathfrak{log}$-*link*.

This point of view may be seen in the use of the term "endofunctor" in the first paragraph of §2.1.3, as well as in the discussion of the final paragraph of §2.1.3 (i.e., to the effect that distinguishing the domain and codomain of the $\mathfrak{log}$-link may have some vague philosophical significance, but is completely devoid of any substantive mathematical significance). It should be stated clearly that this assertion is **completely false** (cf., e.g., the discussion of [Rpt2018], (LbEx1), (DfLb), (LbLp); the discussion of the *definition of the* Θ-*link* in the latter portion of [Alien], §3.3, (ii)). That is to say, (as discussed in [Rpt2018], (LbLp)) identifying the domain and codomain of the $\mathfrak{log}$-link obligates one to identify the Θ-links emanating from the domain and codomain of a $\mathfrak{log}$-link. On the other hand, such identifications of Θ-links are **fundamentally incompatible** with the **definition** of the **Θ-link**, which depends, in an essential way, on **fixing** the **multiplicative** structures of the rings involved (or, put another way, *fixing a* **basepoint** with respect to the *translation action of* $\mathbb{Z}$ on vertical columns of the log-theta-lattice — cf. the discussion of [Rpt2018], (LbEx1), (LbEx2), (LbLp)), i.e., is **not invariant** with respect to the **"rotations/juggling"** *of the additive and multiplicative structures* that occur as one executes various iterates of the $\mathfrak{log}$-link. This *non-invariance*, which is closely related to the **non-commutativity** of the log-theta-lattice, is a *very substantive mathematical obstruction* to identifying the domain and codomain of the $\mathfrak{log}$-link. In particular, in this context, it seems natural to pose the following question:

(Q3) Can SS give a proof of the **invariance** *of definition of the* Θ-*link* with respect to the operation of *identifying* Θ-*links* emanating from the domain and codomain of a $\mathfrak{log}$-link?

Such an assertion of invariance seems *entirely absurd*. Moreover, *nowhere* in [SS2018-05] can one find a *rigorous proof*, or even a *sketch of the main ideas of such a proof*, of this assertion of invariance.

(C7) Footnote 5: The central assertion of this footnote appears to be the following:

> The *arithmetic fundamental groups "Π"* that appear in the local nonarchimedean portion of the $\mathfrak{log}$-links may be *identified* with one another by means of *fixed rigidifying isomorphisms* (as opposed to poly-isomorphisms, as is done in IUTch) *without affecting the essential content of the theory.*

First of all, it should be stated clearly that this assertion is **completely false**. It is *true* (as was discussed during the March discussions) that, if one *forgets* about the Θ-links, as well as the $\mathfrak{log}$-links on the other side of the Θ-link, then these Π's may be consistently identified with one another by applying the Π-equivariance of the nonarchimedean logarithm. On the other hand,

> this sort of **rigidification** of these Π's in, say, the 0-(vertical) column of the log-theta-lattice **depends**, in an essential way, on the **data** of the action of the Π's on various local rings/fields that are related to one another by means of various iterates of the (nonarchimedean) logarithm — i.e., *(0-column Frobenius-like) data* that does **not** admit **switching symmetries** that permute the 0-, 1-columns (cf. (Q1) above; [Rpt2018], (SWE1), (LbMn)).

By contrast, if one *forgets* this 0-*column Frobenius-like data* on which these rigidifications depend and regards the various Π's in the 0- and 1-columns of the log-theta-lattice as **abstract topological groups** (i.e., that are *not equipped* with such *rigidifications* and hence can only be consistently to one another by means of *full poly-isomorphisms*), then one obtains, in both the 0- and 1-columns, collections of isomorphic topological groups, related to one another within a fixed column by means of "some indeterminate isomorphism" and via the Θ-link to the opposite column by means of the surjections to an **abstract topological group** isomorphic to "G" (i.e., the absolute Galois group of the local field) — that is to say, data that **does** indeed admit **switching symmetries** that permute the 0-, 1-columns (cf. (Q2) above; [Rpt2018], (SWE1), (EtMn)).

(C8) §2.1.4, "$\mathbb{R}_v \cong \mathbb{R} \cdot \gamma_v$", "$\gamma_{\mathrm{can}} \in \mathbb{R}_{\circledcirc}$": If one is only given a *global realified Frobenioid*, then it is important to note that such elements γ_v, γ_{can} *exist*, but are *not uniquely/canonically determined.* In fact, in the situations that one is interested in, one is given *not only a global realified Frobenioid*, but also an $\mathcal{F}^{\Vdash\blacktriangleright\times\boldsymbol{\mu}}$*-prime-strip.* That is to say, if the global realified Frobenioid under consideration is regarded as the global realified Frobenioid that appears in some $\mathcal{F}^{\Vdash\blacktriangleright\times\boldsymbol{\mu}}$-prime-strip, then such elements γ_v, γ_{can} are *indeed uniquely/canonically determined* (cf. the discussion of the "factorization of the forgetful functor" in the final sentence of §2.1.5).

(C9) §2.1.5, "Frobenius element of $G_v/I_v \cong \mathbb{Z}$": There are *two minor technical errors* here. First of all, the local generators of the value group in this context are obtained by using the *unique generator* of the (*Frobenius-like*!) monoid $\mathfrak{o}^{\blacktriangleright}_{k_v}$ ($\cong \mathbb{N}$)

(in the notation of §2.1.5). That is to say, *étale-like* data such as G_v *cannot be related*, in the present context, to *Frobenius-like value groups*, since (in the present context) one *cannot apply Kummer theory* to these (*non-divisible*!) Frobenius-like value groups. Secondly, since G_v is (presumably!) the *profinite* absolute Galois group of k_v, "$\mathbb{Z}$" should be replaced by "$\widehat{\mathbb{Z}}$".

(C10) §2.1.6, "$(v(\underline{\underline{q}}_v))_v$": There are *two inessential technical errors* here. First of all, since $v(\underline{\underline{q}}_v)$ is an element of $\mathbb{R}$ (not of $\mathbb{R}_v$!), it seems that "$v(\underline{\underline{q}}_v)$" should be replaced by "$v(\underline{\underline{q}}_v) \cdot \gamma_v$". Secondly, in the definition of *"pilot objects"*, v should range only over the *bad primes* (i.e., in the notation of [IUTchI], the elements of $\underline{\mathbb{V}}^{\mathrm{bad}}$), *not* over all primes of the number field.

(C11) §2.1.6, second display, "$\mathbb{R}_{\circledcirc} \ni \ \ldots \ \in \mathbb{R}$": Here, it is important to note that this isomorphism "$\mathbb{R}_{\circledcirc} \cong \mathbb{R}$" that maps $\gamma_{\mathrm{can}} \mapsto 1$ does *not necessarily coincide with the "arithmetic degree" (in the usual sense)*. That is to say, it does *indeed coincide*, for instance, in the case of *q-pilot objects* (cf. (C12) below), but does *not coincide* in the case of Θ*-pilot objects* (cf. (C14) below).

(C12) §2.1.7, "one chooses the natural isomorphism $\mathbb{R}_{\circledcirc,q} \cong \mathbb{R}$": Presumably, this is the isomorphism discussed in §2.1.6 that sends $\gamma_{\mathrm{can}} \mapsto 1$ (cf. (C11)). In the case of the q-pilot object, this isomorphism corresponds to taking the *arithmetic degree* in the usual sense.

(C13) §2.1.8, "equals j^2 times": Presumably, here (as well as in the final sentence of §2.1.8) "j^2" in fact refers to the average of the j^2, for $j = 1, \ldots, l^*$.

(C14) §2.1.8, "when the identification $\mathbb{R}_{\circledcirc,\Theta} \cong \mathbb{R}$": Presumably, this is the isomorphism discussed in §2.1.6 that sends $\gamma_{\mathrm{can}} \mapsto 1$ (cf. (C11), (C12)). In the case of the Θ-pilot object, this isomorphism scaled by (the average of the) j^2 corresponds to taking the *arithmetic degree* in the usual sense.

(C15) §2.1.9, "canonical choice for the Θ-link": This "canonical choice" seems to be the "id-*version*" that was discussed in the final day of the March discussions, or, alternatively, [Rpt2018], (SSId). As is discussed in detail in [Rpt2018], §10, the **multiradial algorithms** of IUTch **cannot be applied** to this "id-version" (cf. [Rpt2018], (SSIdFs)), i.e., this "id-version" *fails to satisfy, in a very essential way, the conditions necessary to apply these algorithms.*

(C16) Footnote 7, "Mochizuki does not properly distinguish them, which is part of our main concern" (cf. also the first phrase "As ... work" in the third sentence of the second paragraph of §2.2): First of all, it should be stated clearly that this assertion is **completely false**. Indeed, the issue of distinguishing the *abstract category-theoretic versions of pilot objects* determined by the *intrinsic structure* of the $\mathcal{F}^{\Vdash\blacktriangleright\times\boldsymbol{\mu}}$-prime-strips from their *concrete (multiradial!) representations* on *tensor-packets of log-shells* is one of the **most central aspects** of IUTch (cf., e.g., [IUTchIII], Theorem 3.11; the proof of [IUTchIII], Corollary 3.12)! (In this context, it is of interest to note (cf. (C1)) that such key notions in IUTch as "tensor-packets" and "log-shells" do not appear in [SS2018-05]!) Rather, the *confusion*, on the part of SS, surrounding the proper treatment of *abstract category-theoretic versions of pilot objects* and their *concrete (multiradial!) representations* on *tensor-packets of log-shells* appears to be one of the most central aspects of the *misunderstandings of SS* discussed in (C17), (C18) below.

(C17) §2.2, third sentence of second paragraph (cf. (C16)); §2.2, third paragraph, "requires careful identifications"; §2.2, fourth paragraph, "consistently identify"; §2.2, displayed diagram; §2.2, fourth paragraph, "Mochizuki wants to introduce scalars": The discussion of §2.2 is a **complete misrepresentation** of the argument given in Step (xi) of the proof of [IUTchIII], Corollary 3.12. There is **never any issue** in the proof of [IUTchIII], Corollary 3.12,

(N1) of *"requiring careful identifications of copies of real numbers"* (§2.2, third paragraph);
(N2) of *"consistently identifying"* these copies (§2.2, fourth paragraph);
(N3) of *identifying such objects as the "*$\mathbb{R}_\Theta$*" and "*$\mathbb{R}_q$*"* in the bottom line of the displayed diagram of §2.2;
(N4) of *considering a* **"loop"** of the sort that appears in the displayed diagram of §2.2, which is *required to commute*; or
(N5) of *"wanting to introduce scalars of* j^2 *somewhere"* (§2.2, fourth paragraph).

That is to say, **none of (N1), (N2), (N3), (N4), (N5) ever occurs** in Step (xi) of the proof of [IUTchIII], Corollary 3.12. In particular,

> the **loop** of (N4), which is *central* to the discussion of §2.2, **never occurs** in the proof of [IUTchIII], Corollary 3.12.

This *loop* corresponds precisely to the **erroneous identification of labels/ring structures** discussed in [Rpt2018], (Smm), (GLR2), (AD), (SSAD), (SSADFs), (SSDLFs), (LbEx1), (LbEx4). That is to say,

> it is precisely because of the **erroneous introduction** of such a loop in various *modified versions of IUTch considered by SS* — i.e., versions obtained by **erroneously identifying** various **distinct** labels/ring structures — that problems of the sort discussed in §2.2 occur.

In the theory that is *actually developed* in the papers [IUTchI], [IUTchII], [IUTchIII], [IUTchIV], the ring structures on either side of the Θ-link are **distinguished** (cf. [Rpt2018], (IUAD)) — a situation that gives rise to the *highly nontrivial problem* of *computing the relationship* between these *distinct ring structures* (cf. [Rpt2018], (GIUT)). This relationship is computed by **embedding**, at the expense of introducing certain **indeterminacies**, objects of interest (such as various versions of Θ- and q-pilot objects) on opposite sides of the Θ-link into a **common container**, by means of the **multiradial representation** of [IUTchIII], Theorem 3.11. Since this container is a **single container**, which, in particular, admits a *single well-defined log-volume function* on a certain collection of its subsets, there is *never any issue of "requiring that a loop"* (as in the displayed diagram of §2.2) *"commute"*.

(C18) §2.2: Thus, in summary, the argument discussed in §2.2 is a **radically different** argument from the argument that is actually given in the proof of [IUTchIII], Corollary 3.12. The centerpiece of the argument that is actually given in the proof of [IUTchIII], Corollary 3.12, is the **multiradial representation** of [IUTchIII], Theorem 3.11, which allows one to *compute the relationship* between the *distinct ring structures* on opposite sides of the Θ-link by **embedding**, at the expense of introducing certain **indeterminacies**, objects of interest on opposite sides of the Θ-link into a **common container**. On the other hand, this multiradial representation is *never even discussed* in [SS2018-05]. In particular, the *argument discussed in §2.2 is most accurately described* (*not* as a description of a *flaw* in the logical structure of the theory that is actually developed in [IUTchI], [IUTchII], [IUTchIII], [IUTchIV] (!), but rather) as

> *a* **superficial internal contradiction** *in the definition of the* **modified version of IUTch** *proposed by SS obtained by* **arbitrarily identifying/confusing** *(distinct, from the point of view of IUTch)* **ring structures** *on opposite sides of the Θ-link*

— cf. the discussion of [Rpt2018], (Smm), (GLR2), (AD), (SSAD), (SSADFs), (SSDLFs), (LbEx1), (LbEx4). As discussed in [Rpt2018], (Smm), (GLR2), in general, given *any mathematical argument*, it is always very easy to obtain such *superficial contradictions* simply by *arbitrarily identifying/confusing* mathematical objects appearing in the argument that must be *distinguished*. On the other hand,

> *such* **superficial contradictions** *arising from* **arbitrarily modified versions** *of a mathematical argument constitute — both at a purely formal, procedural level and at an abstract logical level — a* **fundamentally qualitatively different phenomenon** *from the phenomenon of a gap, or flaw, in the logical structure of the argument.*

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