ON THE VERIFICATION OF INTER-UNIVERSAL TEICHMÜLLER THEORY: A PROGRESS REPORT (AS OF DECEMBER 2014)

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In August 2012, I released a series of papers on inter-universal Teichmüller theory (IUTeich). In December 2013, I posted a *progress report* concerning activitives devoted to the verification of IUTeich. In the present report, I wish to report on various new developments that have come to pass during the year subsequent to this progress report.

(1) In 2014, I gave two talks concerning IUTeich, as follows:

February 20, 2014 (2 hours + 2 hours, RIMS Number Theory Seminar);

May 24, 2014 (2 hours + 2 hours, Kumamoto University).

The lecture notes for these two talks are slightly modified/enhanced versions of the lecture notes for talks that I gave in the past on IUTeich. On the other hand, I took advantage of the somewhat lengthened time slots in the case of these two talks (i.e., by comparison to previous talks) to give more detailed explanations and to answer questions posed by members of the audience. I continued to edit these *lecture notes* on numerous instances subsequent to the two talks; the current version of the lecture notes (together with a survey ([Pano]) that discusses essentially the same material) may be found on my web site. The February talk was intended mainly for young people at RIMS, such as graduate students and post-doctoral researchers, as well as for researchers living in the Kansai area of Japan (i.e., close to Kyoto University) who have been attending the seminar for many years; the audience consisted of roughly 10 to 20 people. By contrast, the audience of the May talk, which resulted from an invitation by *Professor Fumiharu Kato* of Kumamoto University, consisted of roughly 40 to 50 people mainly from universities in the Kyushu region of Japan (i.e., close to Kumamoto University), such as Kyushu University. The invitation by Professor Kato was, to a certain extent, a natural outcome of the seminar concerning IUTeich that I conducted with Professor Kato during the period July 2005 $\sim$ March 2011,

once every 2 to 3 weeks (for roughly 3 to 4 hours),

when Professor Kato was an associate professor in the Department of Mathematics of Kyoto University. I wish to express my deep gratitude to Professor Kato for the quite substantial amount of time that he devoted to listening to me as I spoke for hours on end, despite his quite busy schedule and despite the fact that, at least in the earlier years of this seminar, IUTeich was still in an intermediate stage of development.

(2) I have been conducting a seminar concerning IUTeich with *Go Yamashita* (Project Lecturer at the Center for Research Interaction in Mathematical Sciences of RIMS, Kyoto University) since October 2012. In 2014, this seminar was conducted

once every 3 weeks (for roughly 4 to 5 hours).

Just as in previous years, we discussed various technical aspects of IUTeich; I then proceeded to revise the relevant papers in accordance with the technical comments (roughly 30 in all during 2014, a sharp drop from the several hundred comments that I received in 2013) that I received from Yamashita and posted the revised versions on my web site. Yamashita has been writing a survey since 2013 concerning IUTeich (as well as the various "preparatory papers" that are necessary for the theory). In the course of writing this survey, he read through (for the third time) the four papers that constitute the "theory proper", and we discussed the various questions and comments that resulted from this third reading in our seminar. It appears that the survey will come to a total of roughly 200 $\sim$ 300 pages in length (i.e., roughly a tenth of the size, in terms of the number of pages, of the original papers — a quite staggering compression ratio!). Yamashita also gave a series of lectures (in Japanese) on the preparatory papers (i.e., mainly [AbsTopIII]) at Kyushu University during the period

September 16 $\sim$ 19, 2014 (for roughly 5 hours a day)

at the invitation of *Associate Professor Yuichiro Taguchi* of Kyushu University. These lectures were apparently well received by the audience, which consisted mainly of young people such as graduate students, post-doctoral researchers, and assistant professors. During these lectures, Yamashita warned that

> if you attempt to study IUTeich by skimming corners and "occasionally nibbling" on various portions of the theory, then you will not be able to understand the theory even in 10 years; on the other hand, if you study the theory systematically from the beginning, then you should be able to understand it in roughly half a year.

A "sequel" to the series of lectures at Kyushu University (in which Yamashita plans to review the content of the lectures series at Kyushu University during the

first few days) will be given during the period

March 09 $\sim$ 20, 2015 for 10 (week)days (roughly 7 hours per day)

as a "RIMS Joint Research Workshop" (in Japanese). The *program* for this workshop has been posted on my web site. We hope to be able to publish Yamashita's survey (along with another related paper) as a proceedings volume for the workshop in the *"RIMS Kōkyūroku Bessatsu"* series.

(3) *Mohamed Saïdi* (University of Exeter (UK), Associate Professor) visited RIMS, as a Visiting Professor, for the period

June 25, 2014 $\sim$ September 24, 2014,

during which he read through the theoretical portion (i.e., [IUTchI], [IUTchII], [IUTchIII], but not the final computation of [IUTchIV]) of the "theory proper" once again (for the third time). We conducted a seminar

once a week (i.e., 9 meetings, for 2 to 3 hours per meeting)

during which we reconfirmed the content of the seminar that we conducted during the summer of 2013 and discussed various technical questions and comments (roughly 70 in all!) that arose in the course of Saïdi's third reading. It appeared that, as a result of the numerous revisions of technical oversights that had been made in the interim, the text was substantially easier to read than in the summer of 2013. Just as in the case of the seminar conducted in the summer of 2013, I revised the relevant papers in accordance with the numerous technical comments that I received each week in the our seminar and posted the revised versions on my web site. One aspect of these seminars with Saïdi that left an impression on me was the *serenity of his demeanor* as he emphasized on various occasions, with regard to the dissemination of IUTeich, the importance of maintaining a *patient, long-term stance*. Just as in the summer of 2013, I asked him what sort of advice he would give to someone in the initial stages of studying IUTeich; his response consisted, as before, of emphasizing that everything (necessary for the study of IUTeich) is contained in the "preparatory papers". At a more concrete level, we reviewed, in one of our seminars, the (essential) prequisites for [IUTchI], [IUTchII], i.e.,

- [SemiAnbd], §1, §2, §3, §5, §6;
- [FrdI]; [FrdII], §1, §2, §3;
- [EtTh] (*);
- [AbsTopI], §1, §4; [AbsTopII], §3; [AbsTopIII], §1, §2 (*)

(where the "(*)" is to be understood as a marker for "especially important" material); in order to proceed to [IUTchIII], [IUTchIV], it is necessary study, in

addition, the following material:

- [AbsTopIII], §3, §4, §5;
- [GenEll].

(4) I have been conducting a seminar with *Yuichiro Hoshi* (RIMS, Kyoto University, Lecturer) concerning various topics for some time. In 2014, we conducted a seminar centering around IUTeich

once every 2 weeks (for roughly 3 hours).

Hoshi had already studied (i.e., prior to the end of the calendar year 2013) the "preparatory papers" related to anabelian geometry (i.e., [SemiAnbd], [AbsTopI], [AbsTopII], [AbsTopIII]); in the course of doing so, he provided me with numerous technical comments and, moreover, obtained new results of his own concerning the *mono-anabelian geometry of number fields*, i.e., in the spirit of the theory of [AbsTopIII]. These new results gave rise to several papers, the most recent of which I hope to be able to publish in the proceedings volume for the March 2015 workshop. On the other hand, Hoshi finished reading the remaining preparatory papers (i.e., [HASurI], [HASurII], [FrdI], [FrdII], [EtTh], [GenEll], etc.) during the period January $\sim$ March 2014 and then proceeded to read through the four papers that constitute the "theory proper" during the period April $\sim$ July 2014. Although he only read the fourth paper (i.e., [IUTchIV]) concerning the final computation once, he eventually *read through the first three papers* (i.e., [IUTchI], [IUTchII], [IUTchIII]), which discuss the theoretical portion of IUTeich, a total of *at least five times*, in order to deepen his understanding of the theory. In Hoshi's case, these readings of papers related to IUTeich were conducted subsequent to his participation in the roughly 140 hour long seminar given by Yamashita during the period May $\sim$ November 2013. This led me to pose the following question to Hoshi: If one were to assign to the level of understanding of IUTeich that he achieved as a result of reading through the papers on IUTeich at least five times a score of "100", then how would he rate the level of understanding of IUTeich that he achieved as a result of attending Yamashita's seminar? The answer that he gave me was a score of roughly "10 $\sim$ 15". Of course, it is not clear that such numerical appraisals of "levels of understanding" have much meaning; moreover, Hoshi expressed his appreciation of the usefulness of Yamashita's seminar in providing a clear understanding of the overall picture of IUTeich prior to his reading of the papers. On the other hand, from my own point of view, Hoshi's answer just served to underscore once again, relative to the issue of various people's attempts to understand IUTeich,

the *quite essential importance* of *reading through the papers carefully*.

In the seminar that I conducted with Hoshi in 2014 once every 2 weeks, we discussed various diverse technical aspects of IUTeich; after each seminar, I made revisions to the relevant papers in accordance with the various technical questions and comments (roughly 40 in all) that I received from Hoshi and posted the revised versions on my web site. By the time that Hoshi started studying the papers that consitute the "theory proper", the verification activities of Yamashita and Saïdi had already been under way for some time, and, as a result, Hoshi said that it was rather difficult to find many additional technical oversights. On the other hand, Hoshi has a proven track record as a researcher in the field of anabelian geometry, and, as a result, he was able to apply his deep understanding of anabelian geometry to inspect the logical structure of the entire theoretical apparatus that constitutes IUTeich. This inspection by Hoshi of the logical structure of IUTeich led to the discovery of a certain technical oversight in the formulation of a certain portion of the theory (namely, the portion concerning the Kummer theory related to number fields). We discussed this technical oversight in substantial detail in our seminar, and, just as in the case of the other technical remarks that I received, I posted the revised versions — i.e., in which the technical oversight is repaired, and, moreover, several remarks (that expose the essential content of the various observations that we discussed in our seminar) were added — of the relevant papers on my web site. During a seminar with Hoshi conducted in the autumn of 2014, i.e., after the various adjustments that resulted from Hoshi's numerous comments had been completed, I asked Hoshi several times if he had any advice for researchers who encountered substantial difficulties in their study of IUTeich. He provided me with the following (unexpectedly generous) response:

> in his case, he has been studying various papers of mine since he was a student, and, up till now, he has not encountered any significant difficulties in studying these papers, so long as he studied them carefully; the papers concerning IUTeich did not, for him, constitute an exception to this state of affairs.

(5) *Chung Pang Mok* (Purdue University (USA), Associate Professor) has apparently been giving introductory talks concerning IUTeich at various universities and research institutes in the US during the period October $\sim$ November 2014. I have not had any interaction with Mok in the past and only became aware of his activities around October, as a result of a report from Hoshi. On the other hand, I hope that I will have an opportunity to interact with Mok some time around the time of the March 2015 workshop.

(6) In (2), (3), (4) above, I reported on the verification activities of three researchers in arithmetic geometry. I regard these three researchers as the *core* of

the "verification apparatus" (at least as it exists at the present time) for IUTeich. These three researchers exhibit various similarities, as well as various differences. First of all, their ages are somewhat diverse: Saïdi is in his mid-40's, while Yamashita is in his late 30's, and Hoshi is in his early 30's. Thus, just 10 years ago in the spring of 2004, Hoshi was still studying the elementary portion of Hartshorne's standard textbook on scheme theory. In particular, Hoshi's case is of substantial interest in that it demonstrates that, even if one starts from such a level of mathematical maturity, it is quite possible to achieve a rigorous understanding of IUTeich *within 10 years*. (Of course, during the past 10 years, Hoshi was engaged in numerous research and educational activities besides his study of IUTeich — such as writing roughly 20 solid research papers, advising students, giving week-long lecture series, and serving as a referee for mathematical journals!) Each of these three researchers has a solid track record in mathematical research, as well as ample experience and a solid track record with respect to serving as a referee for (over 10 mathematical papers submitted to) mathematical journals. On the other hand, whereas Saïdi and Hoshi are researchers with proven track records in the field of anabelian geometry, Yamashita's research is essentially unrelated to anabelian geometry and centers rather around such topics as p-adic Hodge theory and p-adic multiple zeta values. This difference in fields of research appears to be reflected in the following phenomenon: As discussed in (3), (4), Saïdi and Hoshi focused mainly, in their study of IUTeich, on the theory discussed in the first three papers (i.e., [IUTchI], [IUTchII], [IUTchIII]), which is developed essentially *in the spirit of anabelian geometry*, and were somewhat less interested in aspects of IUTeich related to *Hodge-Arakelov theory* and *analytic number theory* such as the concrete computations in the fourth paper (i.e., [IUTchIV]) that lead to the *inequality of the ABC Conjecture*; by contrast, Yamashita exhibited, on numerous occasions, a keen interest not only in the first three papers, but also in the fourth paper. Thus, in summary, each of these three researchers is marked by his own distinctive features. Nevertheless, each of these three researchers has made a *most significant* and *valuable* contribution to the various verification activities discussed in the *report* posted in December 2013, as well as in (2), (3), and (4) above. Moreover, a brief glance over the numerous comments that I received from these three researchers in the course of these activities serves to highlight quite eloquently the fact that it is by no means the case that the contribution of any one of these three researchers may, in essence, be "subsumed" in the contributions of the other two. On the other hand, one aspect of the *atmosphere* that dominated the quite substantial interaction that I had with all three researchers in 2014 up until around the summer that impressed me as being in *stark contrast* to the exhilarating sense of breaking fresh ground — i.e., arising from the satisfaction of being one of the first researchers (other than the author) to achieve a genuine understanding of IUTeich — that was shared by those involved in 2013 was the sense that

> after having read through and thoroughly verified the validity of IUTeich numerous times (i.e., a total of three times in Saïdi's case, at least three times in Yamashita's case, and at least five times in Hoshi's case), it was becoming *extremely difficult to locate any new material to inspect* or *call into question*.

Moreover, another aspect of the situation, which became increasingly difficult to ignore as time progressed, was the fact that all three researchers were (and continue to be), needless to say, very busy with numerous commitments unrelated to the verification of IUTeich. In particular, it appeared to be increasingly less feasible, as time progressed, for me to expect these three researchers to continue to make substantial time commitments to checking IUTeich, i.e., despite their other commitments, combined with their increasing difficulty in finding any new material to check. Indeed, I have been participating for over 20 years now, as author, referee, editor, and editor-in-chief, in the refereeing of countless papers for mathematical journals, and, as far as I can see, the verification activities on the part of the three researchers discussed above *already exceed, by a quite substantial margin* — i.e., in their content, thoroughness, and meticulousness — *the usual level of refereeing for a mathematical journal*. Moreover, although I have received comments not only from the "core three" researchers, but also from other researchers as well, concerning numerous superficial technical oversights that may be repaired immediately (i.e., a routine aspect of the refereeing process),

> I have yet to hear of *even a single problem* that relates to the *essential thrust* or *validity of the theory*.

Furthermore, I understand that, in a conversation between Yamashita and Hoshi, it was confirmed that, although it is quite possible that further technical oversights will be discovered in the future concerning the way in which the theory is written up in the papers on IUTeich, they are of the understanding that there is no possibility that the theory itself is essentially in error. In this context, I might also point out that I myself have frequently observed and, moreover, heard similar observations made independently by other researchers, that IUTeich is a "correct theory" not only in the sense that it does not contain any essential errors, but also in the sense of its *extraordinary canonicality*, which is reminiscent of the classical computation of the Gaussian integral, as well as of the classical functional equation of the theta function, and which suggests that

> IUTeich is *"the correct theory"* in the sense that it leads one to doubt the existence of any sort of "alternative proof", i.e., via essentially different techniques, of the ABC Conjecture.

(Of course, here I should caution, lest there be any misunderstanding, that I am

certainly not aware of any rigorous mathematical proof of the assertion that such an alternative proof does not exist!) The state of affairs discussed above may be summarized as follows: My understanding, at present, concerning the verification of IUTeich is that

> at least with regard to the *substantive mathematical aspects* of such a verification, the *verification of IUTeich* is, *for all practical purposes, complete*; nevertheless, as a precautionary measure, in light of the *importance* of the theory and the *novelty* of the techniques that underlie the theory, it seems appropriate that a *bit more time* be allowed to elapse before a *final official declaration* of the completion of the verification of IUTeich is made.

On the other hand, I should also state that, although such precautionary measures may serve a meaningful role for a limited amount of time, I am not of the opinion that such precautionary measures should be maintained for periods of, say, the order of $20 \sim 30$ years. That is to say, although there are perhaps numerous approaches to the issue of computing an appropriate length of duration for such precautionary measures, my current sense is that the length of duration of such precautionary measures should not exceed 10 years, i.e., counting from the time of the first oral presentation of the theory (i.e., in October 2010) and the posting of the series of papers on the theory (i.e., in August 2012). Put another way, my current sense is that some date during the latter half of the 2010's would be an appropriate time for the termination of such precautionary measures.

(7) The content of (6) above prompts the following question:

So what is the *next step*?

For instance, a certain segment of the mathematical community appears to harbor the expectation that *some prominent researcher* will step forward and deliver some sort of *definitive announcement with regard to the validity of IUTeich*. As far as I can see, however, it appears *highly unlikely* that this sort of chain of events will come to pass, for the following reason: Typically, when a researcher with a solid track record in mathematical research decides to read a mathematical paper,

> unlike the case with students or novices who take the time to *study step by step from the rudiments of a subject*, such a researcher will attempt to digest the content of the paper in as efficient a way as is possible, by *scanning* the paper for important terms and theorems so that the researcher may apply his/her vast store of expertise and deep understanding of the subject to determine just *which* of those topics of the subject that, from point of view of the researcher, have already been "digested" and "well

understood" *play a key role in the paper*.

Put another way, this amounts to the sort of *"occasional nibbling"* that Yamashita warned of during his lecture series at Kyushu University (cf. (2) above). Of course, in the case of IUTeich, a researcher who already possesses a deep understanding, as well as a solid track record in mathematical research, concerning such topics as absolute anabelian geometry, the rigidity properties of the étale theta function, and Hodge-Arakelov theory, may indeed find such "occasional nibbling" to be more than sufficient to attain a quite genuine understanding of IUTeich. In fact, however, for better or worse, *no such researcher exists* (other than myself) at the present time. The closest existing approximation to such a researcher consists of those researchers who have a solid track record in mathematical research that concerns closely related aspects of anabelian geometry, i.e., Saïdi and Hoshi (cf. (3), (4)), together with *Akio Tamagawa* (RIMS, Kyoto University, Professor), who attended the seminar held by Yamashita during the period May $\sim$ November 2013. (In Tamagawa's case, however, it does not appear realistic to expect, at least in the foreseeable future, that he will be able to study the papers on IUTeich in a genuine fashion, since he is much too busy with other work.) That is to say, in summary,

> with the exception of the handful of researchers already involved in the verification activities concerning IUTeich discussed in the present report, every researcher in arithmetic geometry (i.e., including Yamashita as of August 2012, when the series of papers on IUTeich was first posted!) throughout the world is a *complete novice* with respect to the mathematics surrounding IUTeich, and hence, in particular, is *simply not qualified to issue a definitive (i.e., mathematically meaningful) judgment concerning the validity of IUTeich* on the basis of a "deep understanding" arising from his/her previous research achievements.

In particular, from the point of view of the question "What is the next step?" posed at the beginning of the present (7), it appears that, in light of the present state of affairs, the only reasonable course of action lies in

> taking a *long-term appproach* to promoting the *dissemination of IUTeich* by cultivating a collection of researchers, one by one (and perhaps over many years of time), who gradually attain a *deep understanding of IUTeich* by studying the theory carefully and systematically from the beginning, i.e., in the style of Yamashita.

Unfortunately, however, there appear to exist, especially among researchers outside Japan, quite strongly negative opinions and antagonistic reactions to the idea of "studying the theory carefully and systematically from the beginning". As

discussed in (2), (3), (4), and (6) above, in the case of the "core three" researchers, the acquisition of a comprehensive understanding of IUTeich via such a careful and systematic study of the theory *proceeded quite smoothly*. Moreover,

> from my point of view, the precise cause of the quite *conspicuous gap* between the negative reactions referred to above and the experiences of the "core three" researchers continues to remain a complete mystery, which I have yet to succeed in unraveling.

Nevertheless, I have considered the following *"hypotheses"*, or possible explanations, concerning the *essential nature* and *logical structure* of the *obstructions to the dissemination of IUTeich*:

(H1) The total *number of pages* of the papers that constitute IUTeich is, if one includes the "preparatory papers", of the order of several thousand (i.e., depending on how one counts, roughly $1500 \sim 2500$ pages). As a result, researchers feel that they have neither the time nor the energy to study so many pages of mathematics.

(H2) Researchers feel unable to follow the arguments applied in IUTeich, despite numerous efforts to study IUTeich, because they find themselves unable to understand such aspects of the arguments that are often applied in IUTeich as the logical structure of *reconstruction arguments* in the style of anabelian geometry, the way in which one may detect the *essential issues* that must be resolved in such arguments, the way in which those essential issues are in fact *resolved*, and so on. Moreover, there does not appear to exist sufficient *"educational infrastructure"*, such as suitable textbooks and the like, for studying anabelian geometry.

(H3) Certain researchers believe that every essential phenomenon in number theory may in fact be reduced to some aspect of the *representation-theoretic* approach exemplified by the *Langlands program*. On the other hand, the fundamental ideas of IUTeich are not based on this sort of representation-theoretic approach.

(H4) In the case of the famous work of Wiles in 1995 (i.e., concerning elliptic curves over the field of rational numbers), numerous *extensions* and *generalizations* of the theory developed by Wiles have been made by other researchers to the case of various number fields (other than the field of rational numbers) satisfying special properties. By contrast, it is by no means clear that such extensions and generalizations will be possible in the case of IUTeich. (Here, we recall that, as is well-known, this work of Wiles is a typical example of the "representation-theoretic approach" referred to in (H3).)

(H5) Researchers feel that they do not have the time to study a theory that does not appear likely to be *useful in their own research* (i.e., that does not appear likely to give rise to an increase in their own research paper output). This is an especially serious issue in the case of young researchers in their 20's $\sim$ 30's who do not have tenured positions.

With regard to these various "hypotheses", we remark first of all that (H1) and (H5) (respectively, (H1) and (H2); (H3) and (H4); (H4) and (H5)) are closely related to one another, as may be readily verified by examining the explicit content of these "hypotheses". My thoughts with regard to these "hypotheses" are as follows:

(T1) In this context, it is perhaps of interest to recall that, although there are numerous differences in such aspects as the nature of its content and the number of authors, the total number of pages of the famous series "EGA" and "SGA" of the 1960's (i.e., which laid the foundations of scheme theory) that were applied in the proof of the Weil Conjectures was roughly of the order of a *little less than 10,000 pages* (i.e., an order of magnitude that is *substantially greater* than the total number of pages of IUTeich!).

(T2) Indeed, it appears that, at the present time, sufficient "educational infrastructure" (such as textbooks) has yet to be developed. One possible approach to initiating one's study of anabelian geometry is to begin by studying such *short* (roughly 8 pages!) *elementary* (from the point of view of a reader who is well-acquainted with the classical theory surrounding the arithmetic of local fields) *introductory* papers as [$\mathbb{Q}_p$GC]. On the other hand, my experiences over the years in educating various students such as Hoshi (cf. the final portion of (4) above, as well as the discussion at the beginning of (6)), as well as of listening to the comments of various researchers who encountered substantial difficulties in their study of IUTeich and related topics, have left the following impression on me: From the point of view of achieving an effective solution to this sort of problem, the most essential stumbling block lies not so much in the need for the *acquisition of new knowledge*, but rather in the need for researchers (i.e., who encounter substantial difficulties in their study of IUTeich and related topics)

to *deactivate the thought patterns*

that they have installed in their brains and taken for granted for so many years and then to start afresh, that is to say,

to *revert to a mindset* that relies only on *primitive logical reasoning*,

in the style of a student or a novice to a subject.

(T3) The representation-theoretic approach exemplified by the Langlands program does indeed constitute *one major current of research* in modern number theory. On the other hand, my understanding is that the idea that every essential phenomenon in number theory may in fact be incorporated into, or somehow regarded as a special case of, this representation-theoretic approach is simply not consistent with the actual content of various important phenomena in number theory.

(T4) One fundamental reason that it is difficult to find mathematical objects that appear to be suitable as possible candidates for generalizations of IUTeich is the fact that IUTeich is a theory that applies to *arbitrary number fields* (i.e., not just number fields, such as the rational number field, that satisfy certain special properties). In this context, it is of interest to observe that the techniques that underlie the famous work of Faltings in 1983, which also applies to arbitrary number fields, have not, as I far as I know, been extended or generalized to other situations. Another important aspect of IUTeich which does not appear to be amenable to extension or generalization, and, which, moreover, does not appear to have an evident analogue in the work of Faltings, is the *theory of theta functions on elliptic curves* (i.e., as developed in [EtTh]), which is closely related to the anabelian geometry of hyperbolic curves.

(T5) Of course, I do not deny that it is not within my power to guarantee to researchers in advance that studying IUTeich in detail will necessarily result in an increase in research paper output. On the other hand, if it is indeed the case that IUTeich has been shunned by many researchers in arithmetic geometry sheerly on account of their *lack of interest in topics that appear unlikely to be directly linked to practical benefits to themselves*, then one may take the point of view that the

status of IUTeich in the field of arithmetic geometry

constitutes a sort of faithful miniature model of the

status of pure mathematics in human society

and hence, in particular, that it is quite possible that the former (status) will serve as a *useful source of insights* concerning the essential nature of the latter (status).

Finally, let me discuss one more impression left upon me by numerous and diverse opinions, both positive and negative, that I have encountered in various situations since I initiated my research concerning IUTeich over 10 years ago:

> Quite frequently, the essential significance of such an opinion concerning IUTeich lies not so much in the issue of whether the appraisal of the theory constituted by the opinion is *positive* or *negative*, but rather in the issue of whether or not the opinion
>
> *reflects a rigorous and appropriate mathematical understanding*
>
> of the topic under consideration.

On the other hand, in this context, it should also be stated that, at the present time, I cannot recall encountering any negative opinions that were based on such a "rigorous and appropriate mathematical understanding of the topic under consideration". In this context, I would like to make the following proposal, perhaps somewhat overdue, to researchers who hold opinions — regardless of whether the opinion is positive or negative — that are *based on a rigorous and appropriate mathematical understanding of the topic under consideration*:

> any such opinion *concerning IUTeich*, together with the mathematical arguments, evidence, and so on that underlie the opinion, should be posted on the internet (or at least be made available to me, since I am the author of the papers on IUTeich) so as to render such opinions accessible for scrutiny by third parties (such as myself) and hence to expedite the process of
>
> *sorting out or cataloguing of*
> *the essential issues (if any) under dispute*
>
> concerning IUTeich.

(8) One way to summarize the state of affairs discussed in (6) and (7) above is as follows:

the focus of activities surrounding IUTeich

appears to be in a stage of transition from

a focus on *verification* to a focus on *dissemination*.

In the years to come, I hope to further strengthen efforts, centered around RIMS, to promote activities devoted to the *dissemination* of the ideas of IUTeich, as well as of other related information, both within Japan and abroad, by utilizing the *joint research facilities* of RIMS (which has been a leading center for activities of this sort over the past half-century). In this context, I recall that I have heard a number of anecdotes (although I am not familiar with the details) concerning attempts to hold seminars and workshops on IUTeich outside Japan that ultimately ended, in some sense, in failure. In particular, glancing over the various activities discussed

in the present report, such as, for instance, the workshop to be held in March 2015 here at RIMS, I feel all the more indebted to the generous support apparatus, together with the quite substantial *social and cultural infrastructure*, that is available here at RIMS. The March 2015 workshop (in Japanese) is mainly aimed at graduate students and young researchers in Japan. On the other hand, RIMS is also an active center for interaction between Japanese researchers and researchers from abroad. For instance, these *international interaction activities* of RIMS include visits of

roughly 300 $\sim$ 400 short-term visitors from abroad, as well as
roughly 10 $\sim$ 20 long-term visitors from abroad
(i.e., who stay for one month or longer)

every year. Moreover, each year roughly 10 long-term visitors stay at RIMS for at least 3 months as *Visiting Professors*. For instance, Saïdi has visited RIMS as a Visiting Professor a number of times in the past few years. On the other hand, whereas many individuals exhibit a strong tendency to assert the need for some sort of "coercion", especially in the case of international interaction, in my case, as a result of my own experiences in the past, I have always been a strong advocate of the need, in the case of both domestic and international interaction activities,

> *to maintain a humble stance dedicated to uncovering the ultimate truth of things*, i.e., in the style of a sort of "researcher", relative to the issue of facing the truth with regard to the *actual level of motivation of the prospective participants*, and, in particular, to refrain from actual implementation of the activities under consideration if this level of motivation is insufficient.

The reason that I insist on the need for maintaining this sort of stance is that I believe very strongly that

> only interaction activities that are buttressed by an *unmistakably high level of motivation* on the part of the participants have a chance of resulting in
>
> *the dynamism necessary to fuel a truly sound, long-term synergistic path of development.*
>
> (That is to say, this sort of path of development that can never be achieved by means of "coercion", even if quite severe punitive measures are imposed!)

Indeed, for instance:

- My interaction with *Yamashita* concerning the topic of IUTeich arose out of proposals that he made to me when he contacted me around Sep-

tember 2012 to inform me of his desire to study IUTeich. Moreover, Yamashita's various subsequent activities concerning IUTeich, such as writing the survey on IUTeich, arose entirely out of *Yamashita's proposals*, i.e., not out of any "proposals" or "requests" on my part.

· My interaction, starting from July 2013, with *Saïdi* concerning the topic of IUTeich, began as a result of a communication to me around the spring of 2013 (from Tamagawa, who has been engaged in joint research with Saïdi for many years) to the effect that Saïdi had begun, roughly half a year earlier (and, needless to say, entirely on his own, i.e., independently of any prodding on my part!), to study, presumably with the intention of ultimately reading the four papers on IUTeich, the "preparatory papers", i.e., such as [FrdI], and so on, necesary to study these four papers on IUTeich.

· In the case of *Hoshi*, the discussions that we held in our seminar concerning IUTeich occurred, in some sense, as a natural outcome of many years of such seminar discussions concerning various diverse topics related to anabelian geometry that I conducted with him, first as his master's/doctoral thesis advisor and later as a joint researcher. This interaction with him over the course of many years arose ultimately as a consequence of the fact that Hoshi chose me, when he entered the master's course of the graduate school program here at RIMS, as his master's thesis advisor — a choice that was motivated by Hoshi's study when he was an undergraduate (needless to say, entirely on his own, i.e., independently of any interaction with me!) of certain portions of one of my papers ([pGC]) and his resulting desire to study various topics in p-adic anabelian geometry, such as the p-adic section conjecture, once he entered graduate school.

Finally, in the case of these three researchers, I was most impressed, on numerous occasions (so numerous that I would find it challenging to compile a complete list of such occasions), by their tendency to express, as *"independent observations"* of their own — i.e., in the absence of any sort of prompting on my part! — various ideas that occurred to me in the course of writing papers or carrying out the research that led to those papers, but which I had yet to record in any sort of explicit form, i.e., in papers, and so on. This sort of tendency is, without doubt, a reflection of the *high level of motivation* that underlies their activities.

(9) I wish to express my deep appreciation for the exceptional zeal and teamwork exhibited by "the core three" Yamashita, Saïdi, and Hoshi, as well as by the various other parties involved, in the numerous contributions that they made to the activities discussed in the present report, often at the cost of making quite substantial time commitments to these activities.

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