Voices of the Anabelian Arithmetic Geometry Community (RIMS, Kyoto)

James Douglas Boyd

1 Introduction

1.1 Preface

This is an article on collective reminiscence among members of a mathematical community; in particular, the anabelian arithmetic geometry community at the Research Institute for Mathematical Sciences (数理解析研究所), or RIMS, at Kyoto University (京都大学).

Although this is not the first exercise in mathematical reminiscence ever to be conducted or published, nonetheless, the reminiscences communicated herein might be seen to differ, in certain respects, from precedent. Reminiscences on individuals and their "schools" have indeed been published previously by well-known mathematical institutions. For instance, the American Mathematical Society (AMS) published a transcript of a discussion held at the University of Chicago between Professor Luc Illusie, Professor Alexander Beilinson, Professor Spencer Bloch, and Professor Vladimir Drinfel'd on the mathematical legacy of Grothendieck. (Despite what one might think, Grothendieck was still alive at the time.) One also finds group-style interviews of individuals; held, for instance, on the occasion of the conferral of a prize. For instance, the Kavli Institute for the Physics and Mathematics of the Universe (カブリ数物連携宇宙研究機構; Kavli IPMU) published an interview with Professor Edward Witten on the occasion of his receipt of the 2014 Kyoto Prize, conducted by Professor OOGURI Hirosi (大栗博司), Professor TODA Yukinobu (戸田幸伸), and Professor YAMAZAKI Masahito (山崎雅人). (SciSci follows the convention of writing Japanese surnames first, in uppercase Romaji.) As for communities themselves, one can consider the example of the Fondation Hugot du Collège de France, which hosted a discussion – between Professor Jean-Pierre Serre, Professor Pierre Cartier, Professor Jacques Dixmier, and Professor Alain Connes – for purposes of reflection on the Nicolas Bourbaki collective during the 1945-1975 period.

Having observed such exercises in reminiscence, I nonetheless – perhaps by dint of blissful ignorance – had yet to see a group discussion among active mathematicians wherein reminiscences are both offered and related to the current status of their research and community. Reminiscences largely appear to concern individuals (e.g., Grothendieck) or historically bounded activity (e.g., Bourbaki: 1945-1975). I thought that it would be interesting for a community to talk among themselves in a group-style interview, reflecting on each other's work, up to the present. I was of the view that one could practice reminiscence not merely for purposes of reflection or celebration of the past, but to somehow contextualize current research. Perhaps, I thought, an invitation for a community to talk about itself can lend new forms of expression to its own structural embodiment as a professional and cultural domain through which working mathematicians relate and work together. Thus, the `purpose' of such reminiscence is to inquire into the relational and developmental nature of an active community; to examine the trajectories along which the community arrived at its current state; and discuss the manner in which prior developments, ongoing programs, or personal policies have shaped the community or suggest future directions. After Dr. Benjamin Collas suggested a round table discussion during my Autumn 2024 visit to RIMS, the glow of a possible opportunity for such reminiscence slowly emerged.

This article is the product of a discussion held in October 2024 on the past, present, and possible future of anabelian arithmetic geometry at RIMS, with the participation of TAMAGAWA Akio (玉川安騎男), Professor at RIMS; MOCHIZUKI Shinichi (望月新一), Professor at RIMS; HOSHI Yuichiro (星裕一郎), Associate Professor at RIMS; and the aforementioned Benjamin Collas, Researcher at the International Center for Research in Next-Generation Geometry at RIMS and a Coordinator of the Arithmetic and Homotopic Galois Theory (AHGT) international research network (IRN).

The remainder of this section consists of prologous commentary, `setting the stage' for the discussion. What follows, in subsequent sections, will largely consist of excerpts from the transcript of the discussion, with some brief framing remarks; it's better to allow the mathematicians to speak for themselves. In fact, those keen to delve immediately into reminiscence can proceed directly to subsequent sections (beginning with "Resolution of Non-Singularities"). Those seeking further context may find additional details in the following subsections, so long as they are willing to suffer my own remarks a tad longer.

1.2 On Method

Here, I will give some brief commentary on my attempts to play a facilitative role during the meeting; not to imbue the role with any misplaced magnitude of significance, but rather, as a gesture towards methodological openness. Exercises in collective reminiscence are inexorably delicate; moreover, I cannot boast any expertise in stewarding such delicacy. Nevertheless, even in the absence of expertise, one can nonetheless – imperfectly, to be sure – evade certain unmistakable quicksands of naïveté, and, at the very least, make an inelegant lunge towards some (necessarily unachievable) methodological ideal.

Although one might suppose a group discussion to be rather effortless – inasmuch as one need only ask mathematicians to `talk about their work' – doing so in the kind of reminiscent terms described above requires speaking about research with historical recollection so as to situate discussion of mathematical activity within a discourse on community development, collegial relations, and the broader development of ideas. The demanding nature of such an exercise is not to be underestimated. As for a methodological ideal: respecting such demands placed on the participants requires, ideally, more than temperance or restraint on one's part (e.g., abstention from speaking unnecessarily, asking too many questions, making interruptions, etc.); insofar as one does play a facilitative role, the ideal, perhaps, is one which places the discussion on a comfortable footing. In an ideal setting, one might, perhaps, avoid posing questions that are unduly technical in character, for they may elicit, perhaps too strongly, idiosyncratic mathematical persuasions, thereby individuating participants. On the other hand, by posing insufficiently technical questions, one might risk alienating participants in the absence of a common mathematical referent. Likewise, one might wish to select mathematical objects or theories that are neither too generically studied (e.g., the absolute Galois group of the rationals) nor too specific to any given participant (e.g., multiradiality).

Much to my regret, the above methodological description suffers from some degree of artificiality inasmuch as it is estranged from the actual setup and procedural conduct of the meeting. Furthermore, readers uninterested in methodology may nonetheless wish to know – given my role in transcribing and presenting the discussion – how I interpret the meeting to have proceeded. Thus, in order to convey both the preparations made on the facilitative side and the interpretations made on the transcriptive side, I invite the reader to consider the following portrait.

1.3 On Mathematical Reminiscences

It was a hot October afternoon in Kyoto. I entered the seminar room on the second floor of RIMS to find that the desks had already been kindly shuffled around for the meeting. They had been arranged rectilinearly, though still one desk short of forming an enclosure. The desk that might have completed the would-be rectangle at the edge closest to the doorway was left absent; an inviting gesture, for persons and memories alike. Closed-rectangle seating arrangements are too imposing on participants, too confining; like a room without a window. The configuration, on the other hand, positioned the desks into a kind of open container, reminding me of a butterfly net; still, one hoped that the participants didn't feel expected to catch anything with the net in particular. With a short agenda document having been circulated via email, it was evident that the meeting at hand pertained not to any pressing mathematical work, nor an urgent administrative issue, nor anything terribly concrete, for that matter – the gathering was no more than an invitation to reminisce.

One at a time, Tamagawa-sensei, Mochizuki-sensei, and Hoshi-sensei joined and took a seat for this unusual event. The unorthodox character of the meeting had been discussed with all parties in advance. Prior engagement with journalists among members of the RIMS anabelian arithmetic geometry community has been a rarity. In such a situation, with limited time at our disposal, I could not harbor the ambition of covering 'everything' which I felt the press had missed. One might not even dare to cover 'anything' in particular – the substance of the conversation depends on the interests and charity of the participants. Thus, the exercise at play was something of an experiment in joint recollection; with a little over 90 minutes available to us, might we be able to trace a few vignettes – even faintly – over the last three decades of anabelian arithmetic geometry at RIMS?

Nonetheless, I feared that no preliminary conversations could alleviate the awkwardness of taking a seat along reticularly positioned desks with such a capacious discourse domain having been – notwithstanding their willingness to participate – somewhat foisted upon the participants. Indeed, the deliberate character of the setup suggested some implicit expectation or pretension. Nonetheless, although the meeting was vested with a certain explicit purpose, it was not at all made clear, at least in the agenda document, what one was 'supposed to do'. What is one supposed to say at this meeting? How is one supposed to respond to the questions? Put differently – what does one do as a mesh in a butterfly net? Extensive discussions had provided assurance to everyone that the intention of the conversation was not to do anything particularly contrived, but rather, to speak about one's work in a more 'general' fashion than one might do in other professional settings. Nonetheless, one should not take for granted whatever naturalness one might attribute to such 'general' conversation; we were very much in new territory.

I didn't spend any time communicating any of the above as the meeting began; it all sounds rather ambient and nebulous until one begins discussing concrete mathematical developments. Thus, following some prefatory remarks, the mathematical discussion commenced quickly. The agenda already contained questions, typed out explicitly, with the addressees specified.

In fact, the discussion began with, of all things, the topic of the resolution of non-singularities (RNS) in anabelian arithmetic geometry. During planning discussions, I had received some informal (and well-taken) advice that it could be a rather abrupt gesture to begin the discussion with such a topic, which might be viewed as a somewhat arcane technical matter. I could not justify beforehand – nor even during the meeting, as I motioned towards the RNS topic – why it took the lead on the agenda. In hindsight, however, it has everything to do with gazing at butterflies and forgetting about the configuration of nets.

A concept such as RNS may be said to resemble a butterfly, inasmuch as, surveying two decades of anabelian geometry publications, one can see it hatch from its chrysalis and begin to pollinate rather widely. It follows naturally that one might begin to observe it, note the various theoretical filaments on which it lands, and partake of the algorithmic nectar which it has drawn.

From the outset, there were concrete details to discuss, meshed together with anecdotes. As the conversation ensued, quiet bouts of laughter periodically gave way. Granted, these could very well be attributed to nervousness or politeness. Yet, the laughter didn't feel like an accident irrelevant to the anecdotes. Rather, the wind of laughter blew in a certain whirling direction; it said something about where the conversation was moving. As memories began to flutter in, taken up in this whirl, they fell into orbiting patterns as though coating some kind of hazy object – perhaps a vignette. Such, perhaps, tells us something about the art of reminiscence, which I regret I cannot yet grasp. Perhaps it is not important which butterflies visit one's discussion, but the way in which they move. Perhaps one need not weigh the influx of memories, but instead, eye the contours along which they flow.

1.4 Why Write on Anabelian Geometry at RIMS?

There was much that could, in principle, be subject to conversation. The anabelian arithmetic geometry community at RIMS was not one which has historicized itself as it has developed; its industrious pace has left little room for retrospectives.

The term "anabelian" – referring to highly non-abelian (étale) fundamental groups – hearkens back to a certain 1983 letter written by Alexander Grothendieck to Gerd Faltings, one which communicated, for instance, his anabelian conjecture. This was subsequently proved, in the 1990's, in the affine case by Tamagawa, and in the general case – for proper, smooth, hyperbolic curves over number fields – by Mochizuki. With the conjecture attached to the namesake of the burgeoning field already settled, Tamagawa-sensei and Mochizuki-sensei simply proceeded forward. Thus, to the extent that anabelian geometry was a `dream of Grothendieck's', the RIMS anabelian arithmetic geometry community found a way, relatively quickly, to awaken from that dream and begin to forge new mathematics of their own. With a new chapter of anabelian geometry to write, the lack of a retrospective impulse is understandable; seeing all that has transpired since the 1990's, one might be convinced that the future is deeper than the past.

Of course, Grothendieck was not the first to encounter the enchantment of arithmetic fundamental groups, as shall be discussed. The term "anabelian" is his, as are his conjectures; but the greater instinct is neither unique to him nor unprecedented. Moreover, mathematical currents preceding his anabelian writings also succeeded him. Thus, it was all the more feasible, at RIMS, to develop a distinctive culture concerning arithmetic fundamental groups and pursue new directions for anabelian arithmetic geometry. Nonetheless, the emergence of this philosophy at RIMS has not been a topic covered by the Anglophone science press. It was Collas' assessment that the present moment was an opportune occasion on which to begin. After all, promising mathematical production eventually attains an international footing, and the global ambit of the RIMS anabelian arithmetic geometry community has widened in recent years.

Here, two developments warrant immediate mention. The first is AHGT, an international research project initiated in 2023, and now supported as a Centre national de la recherche scientifique (CNRS) Japan-France network between the University of Lille, École Normale Supérieure (ENS), and RIMS. Those familiar with the history of anabelian geometry may be familiar with the international collaborations of the 1990's organized around Grothendieck's *Esquisse d'un programme*; AHGT, on the other hand, is a new project realizing international collaboration with a distinctive philosophy (and the subject of a forthcoming SciSci piece). The second is Mochizuki-sensei's development of inter-universal Teichmüller theory (IUT), which has made the rounds across international media, though without the participation of Mochizuki-sensei, and without much attention paid to the anabelian culture in which it is situated.

Reading the publications of the RIMS community as an outsider, one can surmise that they indeed emanate from a community – a network of colleagues and a co-developed collection of ideas – one which produces a body of work but is nonetheless not an explicit subject of any text. One reads citations, one notes acknowledgments – one sees traces of a mathematical culture at work. The papers stretch around something unseen, like a colorfully wrapped box bearing an unknown gift. The appropriate question seemed not to be who bestowed the gift upon RIMS, but how its anabelian arithmetic geometry community developed independently in its own right.

Readers with peripheral familiarity with anabelian geometry may indeed have found it first presented to them as a product of 'late-Grothendieckian' thought, as documented in the 1983 letter and the *Esquisse d'un programme*, which was itself a research proposal, submitted (to no prevail!) to CNRS. Those still in the initial stages of making their acquaintance with mathematical culture may find rather incredulous the proposition that a private letter and rejected research proposal could be at all amenable to the spawning of any research whatsoever, let alone be regarded in hindsight as being of seminal import to the advent of a research field. Some might respond, in turn, that any such incredulity can be explained away via appeal to the fantastic mystique surrounding Grothendieck himself. Such an explanation, however, is not quite satisfactory; it doesn't explain, for instance, the development of the anabelian arithmetic geometry community at RIMS. Rather, the formation of anabelian-arithmetic-geometric communities has had everything to do with the attraction of collective appreciation towards the marvelous information content and algorithmic advantages of arithmetic fundamental groups. Such appreciation, which Grothendieck shared, can be seen in works of independent origin.

For instance, one need only look to figures such as Professor IHARA Yasutaka (伊原康隆) and Professor UCHIDA Kōji (内田興二), whose work precedes the 1983/1984 Grothendieckian texts. Uchida-sensei's results (with, perhaps, the most broadly known being the Neukirch–Uchida theorem) are themselves, effectively 'anabelian-geometry-before-anabelian-geometry' milestones. Ihara-sensei was of centripetal importance in the establishment of the RIMS anabelian geometry community. Thus, perhaps, rather than ask 'what the anabelian community at RIMS is', one could ask about the mathematicians and directions and have helped its development.

Perhaps, here, the reader will excuse a remark of wider scope on Grothendieck's legacy. As the temptation to ruminate on the triumphs of 20th-century mathematics retains its grip, perhaps it might be edifying to reflect not on pedestals of familiar admiration but rather on the undiscussed launchpads for contemporary flights of inquiry and mathematical work. Indeed, as Collas and I once discussed along the Philosopher's Path (哲学の道) near RIMS, the arguably overdetermined ethos of the memory of Grothendieck now tends to carry a somewhat stultifying effect, stunting the spontaneous growth of a greater community. Thus, the development of the initiatives, concepts, theories, and collaborations spawned by the anabelian arithmetic geometry community at RIMS, following the proof of Grothendieck's anabelian conjecture, is a particularly telling case study in mathematical community-building.

Thus, without further ado, the reader is invited to join SciSci in tracking the course of this hitherto largely-undocumented development in 21st-century arithmetic geometry; to trace how legacy vessels from prior generations have been steered into new waters; to see how charts of old appear in new maps as broader landscapes are built out. We will try to recall historical developments, such as the origins of mathematical concepts; discuss current developments, such as exchanges between RIMS anabelian geometers and those abroad; as well as future prospects, such as the – perhaps once-unforeseeable – Teichmüller-theoretic form into which the Section Conjecture, an outstanding matter of Grothendieckian vintage, has been recast.

2 Resolution of Non-Singularities

2.1 What is RNS?

The term "resolution of non-singularities" (RNS) was first coined – at least in the published literature – in a 2004 Publications of the Research Institute for Mathematical Sciences (PRIMS) paper, "Resolution of Nonsingularities of Families of Curves", by Tamagawa-sensei. Readers may be more familiar with the notion of the "resolution of singularities" in algebraic geometry, effectuated by de-singularization operations such as blow-ups. This topic itself coincides with the mathematical legacy of RIMS faculty: Professor HIRONAKA Heisuke (広中平祐), a former RIMS director and professor, had previously received the Fields Medal in 1970 for proving that algebraic varieties admit resolution of singularities in characteristic zero. RNS, on the other hand, refers to the technique of, in fact, *introducing singularities*, which can be useful in anabelian geometry since geometries with singularities are easier to detect/reconstruct using arithmetic fundamental groups than geometries without them. In the paper, it is noted that the technique is "first introduced" in Mochizuki-sensei's proof of Grothendieck's anabelian conjecture. Thus, RNS might be seen as a kind of early conceptual step in anabelian geometry beyond the original Grothendieckian view. Moreover, it was a new concept and technique begotten from the professional relationship between Tamagawa-sensei and Mochizuki-sensei, and has appeared recurrently throughout many new episodes in the saga of anabelian arithmetic geometry.

2.2 Origins of RNS

It was a rare privilege to hear the recollections of Tamagawa-sensei on the origins of RNS. Beyond his own large corpus of publications, in which many outstanding conjectures have been proven, Tamagawa-sensei's name appears in the acknowledgments of many anabelian geometry papers, such as those of Mochizuki-sensei. As I have heard from Collas, Tamagawa-sensei is often generous in his discussions with colleagues on their work, though not necessarily in a manner that results in co-authorship. Thus, one can infer that there exists community structure among RIMS anabelian geometers that does not necessarily appear in something like a citation network; background discussions also play a structural role in their research. The RNS paper is an intriguing example of an occasion in which behind-the-scenes collegial discussions manifest in a written paper coining a new concept.

Tamagawa-sensei:

In fact, I can explain the story, but I'm not sure if my story will be satisfactory; the history is not so interesting in this case. I'll try. After my study of the Grothendieck conjecture in anabelian geometry in the mid-90's, in the second half of the 90's I studied the anabelian phenomena of curves – say, hyperbolic curves [...] – over algebraically closed fields in positive characteristic. This is not like number fields or p-adic fields. [...] Usually, the fundamental group is a topological invariant and so, in characteristic zero, [only] a very small topological invariant is encoded in the fundamental group. But in positive characteristic, the fundamental group reflects more on the coordinates or moduli of the curve. So, I studied [it] at that time. [...] This is purely in positive characteristic, but [...] I proved [a] result on the specialization of the fundamental groups of curves in positive characteristic to curves over the algebraic closure of finite fields. Then, just from this, I [gave] a proof of a sort of resolution-of-non-singularity result. [...] At that time, my main interest was anabelian phenomena over algebraically closed fields in positive characteristic; the mixed characteristic application was not at the center of my interest. [...]

Here, perhaps we glean some insight into a comment given at the conclusion of Tamagawa-sensei's 2004 paper: "The present paper is, at least logically speaking, a mere small corollary of [a previous paper]" by the name of "Finiteness of isomorphism classes of curves in positive characteristic with prescribed fundamental groups". Tamagawa-sensei continued.

Tamagawa-sensei:

At that time, of course, as you already mentioned, I knew the proof [by] Shinichi [of Grothendieck's anabelian conjecture], which [gave] a sort of resolution of non-singularities in a very simple way. [...] At that time I [gave] the proof of a more general non-singularity result, but [...] I had no [further] applications in mind. So, I did not intend to write [it] down [...] but I must [have] mentioned it to Shinichi, and I also mentioned it to other colleagues in foreign countries, and they recommended to me [that I] write [it] down. At the time of writing, I created the terminology, "resolution of non-singularities", compared to the "resolution of singularities"; and "singularization", compared to "de-singularization"; [...] but this naming is not very philosophical – just for fun. [...]

I know that recently, Emmanuel Lepage; TSUJIMURA [Shota (辻村昇太)] and SAWADA [Koichiro (澤田晃一郎)]; and some other people [have made] more progress, compared to my very basic result. But I didn't expect such progress at the time. [...] I thought that "this is just an end result, which just follows from the positive characteristic result." I had no more interest than [that].

That's the story – yes.

Boyd:

So, it wasn't your intention to present this concept in order to push any [particular] research forward. But there was an international request that you write it down –

[*Roundtable Laughs*]

Tamagawa-sensei:

A very small community, a few mathematicians who are close to me, including him [pointing to Mochizuki-sensei].

Mochizuki-sensei:

Who, other than me –

Tamagawa-sensei:

Stefan Wewers... More people... within that group.

[...]

Collas:

This is something which happens from time to time: one mathematician has an idea that he thinks is just [an] accident. Then, when discussing with other colleagues, they themselves say "no, no, no – I need this lemma" or "I think this is interesting." And even sometimes, they give the follow-up to a PhD student to extend. Then, the researcher has no choice –

Tamagawa-sensei:

But I quite believe – [...] [although] Shinichi, Wewers, and some other guys recommended that I write [it] down, I'm quite sure that, at least at that time, they did not have any applications in mind –

[*Roundtable Laughs*]

Tamagawa-sensei:

"This statement is worth writing down": that's the only reason, I think. [...] On the other hand, the recent and related developments in the resolution of non-singularities [are] more conceptual and more motivated. For [more on] this, please ask him [pointing to Mochizuki-sensei].

[*Roundtable Laughs*]

Boyd:

Mochizuki-sensei – when I read the piece by Tamagawa sensei, [I noted that] RNS is presented, in part, as a way to conceptualize or generalize the method by which you proved Grothendieck's anabelian conjecture for smooth, proper, hyperbolic curves over number fields. I wanted to ask: based on your recollections [...] of your approach at the time, did you have something like RNS in mind, or a proto-RNS concept in mind? Or did you find that the RNS concept actually presented a [...] new framing of how you went about proving that conjecture?

Mochizuki-sensei:

So, I have, in fact, a lot to say about that. One thing I think I should say – just so that it can be recorded – is that Tamagawa and I were talking about this as we were walking down Imawadega Dori [今出川通] –

[*Roundtable Laughs*]

Mochizuki-sensei:

I remember the exact place where he mentioned this.

[*Roundtable Laughs*]

Mochizuki-sensei:

We were in front of a certain store, and Tamagawa said that "it's like you put a wound" – 「傷をつける」 – "and the wound has the effect of giving more information". I remember that moment when he was saying that very vividly. I think this was around 1996 or 1997. Anyway – so, at first, there was this relationship with my work in 1996 [i.e. the proof of Grothendieck's anabelian conjecture]. That paper [from] 1996 ["The Profinite Grothendieck Conjecture for Closed Hyperbolic Curves over Number Fields"] – I don't think it's a very interesting paper, but it had the seeds of ideas that would later develop into interesting ideas.

So, I think it's interesting that this is one sort of aspect of RNS. [...] The idea is – originally, the curve is, perhaps, of smooth reduction, so it doesn't have any wounds. And then, [the approach is] to [inflict] these wounds [on] the curve, and that allows one to get more information about it. I had this rough idea that it would be good if you could do that in a more systematic, controlled, and general way, but I didn't have the precise point of view of Tamagawa's RNS. [...]

The 1996 paper also had seeds of another fundamental development, which is combinatorial anabelian geometry. [...] Already, RNS is very much related to combinatorial anabelian geometry, and getting information out of the special fiber by looking at the combinatorial structure of the graph associated to it. [...]

At first, when I first saw this first [roundtable] question about RNS, my reaction was sort of similar to what Tamagawa was talking about: it's just some technical issue and it's not an interesting question. Then I thought about it and realized that RNS really occupies a very central position with regard to many different ideas. So, it's related to *p*-adic anabelian geometry; it's related to combinatorial anabelian geometry; it's related to IUT – in a very strategically interesting way. [...]

From here, the RNS concept, having been written down by Tamagawa-sensei, was utilized and applied by colleagues internationally. However, translating the framework in which it was implemented abroad into one that was intelligible to Mochizuki-sensei took roughly a decade, as we shall discuss next.

2.3 RNS on the International Stage

Turning to the present – another motivation for inquiring into RNS is the multifaceted and international way in which it has manifested itself in works in anabelian geometry research over the past decade. For instance, Emmanuel Lepage – Maître de conférences (roughly analogous to Assistant Professor) at the Institut de mathématiques de Jussieu, Paris Rive Gauche

(IMJ-PRG) – gave a talk , "Resolution of non-singularities and anabelian applications" during the "AHGT Days in Paris" workshop in September 2024, held at IMJ-PRG. Assistant Professor Lepage had also delivered a lecture series, "Berkovich Methods for Anabelian Reconstructions and the Resolution of Nonsingularities", at RIMS in April 2024, following several years of planning. His Berkovich-geometric approach begins with his 2013 paper, "Resolution of nonsingularities for Mumford curves", published in 2013. This paper and Tamagawa-sensei's original paper are also cited in a 2023 preprint (RIMS-1974), "Resolution of Nonsingularities, Point-theoreticity, and Metric-admissibility for p-adic Hyperbolic Curves", released by Mochizuki-sensei and Tsujimura-sensei.

The collegial relationship between Assistant Professor Lepage and the RIMS anabelian arithmetic geometry community is quite illustrative of the patient manner in which mathematicians attend to the challenges that arise as different segments of a global research community interface with one another. Even for small communities – wherein mathematicians regularly engage on an interpersonal basis via workshops or conferences – lapses in mutual comprehension can occur, and remain outstanding for years, between members as a mere consequence of heterogeneous theoretical or methodological sensibilities that manifest themselves in divergent, and mutually unfamiliar, choices of frameworks or objects. Translating the content of mathematical works between community segments can take years to complete, particularly among internationally distributed community members; understanding is built over a string of conferences and one-on-one interactions.

Mochizuki-sensei took the conversation along this gradient –

Mochizuki-sensei:

Then, we jump forward to Lepage's work: [his] paper was published in 2013. So, I read Tamagawa's [2004] RNS paper, but it didn't really impress me in any way from the point of view of further development. Lepage's [...] paper was published in 2013, I think; [but] it existed before then. The problem with the paper, from our point of view, is that it's phrased in the language of Berkovich spaces and rigid geometry. So, what happened is that Lepage gave talks on it, starting from around 2011 or 2012; and again, I think, around 2015 or so. He gave many talks, and we all listened; I think no one had any idea what he was doing. That sort of repeated itself. He kept giving these talks, and we had no idea what he was talking about. Interestingly, this includes – starting from around 2015 – Go Yamashita (山下剛), who is supposed to be an expert in p-adic rigid geometry, and he also didn't understand what was going on.

What was significant was – finally, in 2021, we had this RIMS project: Expanding Horizons of Inter-universal Teichmüller Theory. We had 4 workshops. In the first 2, Lepage gave talks, first on his RNS paper and second on applications of that to reconstructing Berkovich points. So, finally, during that lecture, I was able to understand what he was doing. It's not because he particularly explained it much better, but because I got enough hints to work out the argument for myself in a language that I could understand.

What I understood was: the key idea was this degeneration of Kummer coverings. [...] So, Kummer coverings involve the multiplicative structure of the ring; they involve extracting an n-th power root. So, if you look at just the right mod p to the right power, then you can see a transition between a Kummer covering and an Artin–Schreier covering – and this is the key observation of Lepage's proof. So, Artin–Schreier coverings involve the additive structure, so it's sort of like the derivative of multiplicative structure. [...] This relationship between additive and multiplicative structure – [which] happens right at the change, right at the gap between characteristic p and mixed characteristic – is very much reminiscent of IUT. [...]

RNS relates to, ultimately, this reconstruction of the types of points in Berkovich spaces: type I, type II, and type III. The key idea is that you're interested in the type I points, which are the usual points. So, RNS allows one to do this.

So, really, this new approach to RNS, [which] was started by Lepage, is very different, even though it has the same name as what Tamagawa did. Tamagawa's argument does not involve this crucial degeneration of Kummer coverings into Artin–Schreier coverings, which is what additive and multiplicative structure is about. So, on the one hand, Tamagawa's argument works under somewhat more general hypotheses. But, on the other hand, it gives a somewhat weaker result, because it doesn't apply to reconstructing arbitrary semistable models. So, in other words, it says that you can put certain amounts of wounds that access a certain level of depth in the body of the curve, but it's not really as deep as you would like to go. So, this approach that was pioneered by Lepage really gets as far as you would like, at least from the [perspective] of reconstructing the points of the curve, because it tells you that you can sort of see all of the mod p to arbitrary powers in the combinatorial-graph-theoretic structure of this special fiber. As you go up to further and further coverings, the mod p^n for deeper and deeper n is reflected in the graph-theoretic structure, [i.e.] the combinatorial structure. That's what RNS is about.

Lepage's [approach], because it was so deeply embedded in this Berkovich, rigid geometry framework, appeared only to work for Mumford curves. So, what I realized when listening to his talk in 2021 – which was a Zoom talk, incidentally; it was during Covid – is that it's really this scheme-theoretic, ring-theoretic, elementary principle that Kummer coverings degenerate to Artin–Schreier coverings. It really has nothing to do with the peculiarities of rigid p-adic geometry as opposed to [...] scheme theory. Scheme theory is known by a very large number of people all over the world. Berkovich-style rigid geometry is known to a far smaller number of people. But, it's really elementary algebraic geometry at the level of Hartshorne; this is what I realized.

Once I realized that, I realized it had nothing to do with Mumford curves. So, all of these artificial restrictions that occurred in Lepage's work were, in fact, irrelevant.

This resulted in this paper with Tsujimura. It's very much related to p-adic anabelian geometry. [...] The p-adic absolute Grothendieck conjecture result also plays a fundamental role in IUT; it's a special case of the general case, which doesn't require RNS – but still, there's this connection. It's very closely related to the combinatorial anabelian geometry of the third and fourth Combinatorial Topics papers [Topics Surrounding the Combinatorial Anabelian Geometry of Hyperbolic Curves]: namely, just what I said – this mod p^n structure is reflected in the combinatorial geometry, the graph-theoretic geometry, of the special fiber.

So, I think it's really a remarkable choice of topic, and I want to thank you for this. Did you think of this topic?

Boyd:

Yes.

Mochizuki-sensei:

I want to thank you for this very brilliant choice of topic.

Boyd:

Thank you, Mochizuki-sensei.

With Collas having been an organizer of the workshop at which Lepage gave his most recent RNS talk, I thought that I would solicit his contemporary observations of the international situation.

Boyd:

Just to improvise – Dr. Collas: I believe that [during] the "AHGT Days in Paris" [workshop], [Assistant] Professor Lepage spoke on RNS. One question is: since there has been some kind of synthesis, or reconciliation, or bridge between the IUT-inspired approach and the Berkovich approach, has Lepage's exposition, style, or view [...] evolved or changed in any way since [2021]?

Collas:

Yes, this is my understanding. You describe RNS as a central [theme] in anabelian geometry, which ramifies to many consequences. But following what happened at this workshop in Paris, and the talks of Lepage at RIMS and Paris, I would be tempted to put that in a more global framework, because there is a Berkovich school: in Germany; in France: we have Ducros, Loeser; Hrushovski in Oxford. These people – they may not see the arithmetic, but they certainly see a very specific kind of geometry. They are strongly interested in the homotopy type of the space, the Berkovich space, which they capture with the combinatorics of the valuation, as you described earlier. So, during this workshop, Lepage presented the proof of RNS, but in terms of his Berkovich language. Ducros also gave a talk about homotopy types of Berkovich spaces, and he mentioned the result of RNS and the whole construction via tempered étale fundamental groups. So, now, it looks like there is also a spreading of this arithmetic idea in algebraic geometry. So, I don't know where it will go, but there is interest to connect further.

Now, these are some techniques which I think [we] are quite familiar with: as soon as we talk about complex algebraic geometry, we have a very flexible space; so in a way, we can do a lot, but we can do too much. So, there is often this attempt to rigidify things: to kill automorphisms of objects, to obtain more arithmetic information in terms of stable curves – for example – or in terms of wounds – maybe – as you describe. This is the process that algebraic geometers often try, though the proper type is difficult to identify – whereas, for arithmetic geometers, it always appears quite clearly. I wonder if the arithmetic inputs of anabelian geometry, of the étale fundamental group, may give some finer direction to look for Berkovich algebraic geometers. [...]

Professor Hoshi, you attended the AHGT workshop in Paris. What was your impression of the interaction, the interface between Berkovich and [...]

Hoshi-sensei:

Ducros gave a lecture on Berkovich spaces, but this lecture is very complicated. [...]

Collas:

Didn't you have a comment during Lepage's...

Hoshi-sensei:

He prepared the Berkovich explanation for RNS. This is best for him, maybe. In my case, in order to understand his proof, I have to [...] study these spaces, which are not familiar to me. So, for me, his explanation sometimes is too complicated [...] Of course, for a "Berkovich person" – for such a person, his explanation does work. [...]

[As for] me: I'm sorry, I can't say anything. [...]

Nonetheless, after some time, Hoshi-sensei added the following.

Hoshi-sensei:

The Berkovich explanation by Lepage is difficult for me: this is a fact. RNS was first formulated by Tamagawa, but this formulation does not work for the application to the

Grothendieck conjecture over p-adic local fields; [but, as for] Emmanuel Lepage, his formulation does work – it seems to me. I think that, [for a] "scheme theory person", [...] such a person [would] give a formulation like [that of] Tamagawa; so, maybe it's difficult for such a person to [understand] Lepage's formulation. [...] So, in this sense, we are very happy that Lepage is interested in our study of RNS. After that, Mochizuki gave a proof of RNS via scheme theory; for me, this proof is easier than Lepage's proof. But, this step – the step of [...] Lepage's interest in RNS – this step is a [joyous] one for us. Maybe, this should be emphasized for a "Berkovich person".

2.4 RNS, Anabelian Geometry, and IUT

Professor Lepage was also one of the first members of the international anabelian geometry community to learn IUT. Given that it is often presumed, among international commentators incredulous as to the intelligibility of IUT, that mathematicians outside of RIMS cannot learn it, Professor Lepage's understanding of IUT is a notable development. Still, one might wonder what factors predisposed Professor Lepage to acquire an understanding of the theory, beyond effort alone. Mochizuki-sensei's assessment is that Professor Lepage's contribution to RNS and understanding of IUT are far from unrelated. Rather, the theoretical crux of IUT – ascertaining the relationship between the underlying multiplicative and additive dimensions of ring structure – is likewise apparent in the degeneration of Kummer coverings to Artin-Schreier coverings at the transition between characteristic p and mixed characteristic in Professor Lepage's work on RNS.

Mochizuki-sensei:

I think this is also interesting from the point of view of Lepage's involvement with IUT. So, I asked [him]: why were you able to understand IUT, whereas other people [have] had so much trouble? We talked about this for a while, and one of the key things he came up with is: he's really not interested in the abc inequality; he really doesn't care. We also [don't] – there's a nonzero interest, but it's not the central point of interest. The central point of interest, which I've emphasized since the early days of IUT, is: how multiplicative and additive structures [are] related; we want a theoretical understanding. This is very much what anabelian geometry is about. You start with multiplicative structures and you want to see how you can reconstruct the additive structure from the multiplicative structure; that's what we're interested in. This is very much related to Lepage's work and why he was able to understand IUT [and] study IUT. [...]

But I think the thing that is not understood is that we don't think of IUT as the abc inequality. We think about IUT in terms of anabelian geometry and the relationship between additive and multiplicative structures. This is a crucial component that was shared by Lepage, and this is reflected in RNS. This is what I think is interesting.

As a general matter, it is the view of both Mochizuki-sensei and Collas that the perspectival advantages afforded by anabelian geometry – for instance, the study of arithmetic fundamental groups, and the extraction of relations between multiplicative and additive structure – remains under-appreciated among those interested in Diophantine geometry (despite, as a historical note, the fact that Grothendieck himself stressed the Diophantine prospects of anabelian geometry from the outset). Thus, when encountering expressions of interest among young mathematicians in IUT, Collas encourages them to begin first by cultivating anabelian sensibilities.

Collas:

Often, I go to international workshops – in Europe, or even in Japan. After a few days, there are always a few young researchers who come to me, asking, "I don't want to be rude, but how about IUT? Can you talk about it?" So, there is some interest. Always, the question is: "how can I start?" [I] tell them: "you must have some interest in anabelian reconstruction, in Galois-Teichmüller, in anabelian arithmetic geometry." This is the key point. If you're motivated only by Diophantine equations, only by abc, this is not the proper way. I think it's something worth repeating. [...]

Mochizuki-sensei:

Yes.

Collas:

People who are interested in IUT: they must look at étale homotopy and anabelian geometry.

3 Approaches to Anabelian Arithmetic Geometry

3.1 Tamagawa-sensei: Collaborative Problem-Solving

One particularly fascinating aspect of holding a discussion involving both Tamagawa-sensei and Mochizuki-sensei is the manner in which their respective approaches to mathematics contrast themselves so readily. Were one to adopt the – by now, often rehearsed – trope of the dichotomy between problem-solving and theory-building mathematicians (similar to the taxonomy presented by Freeman Dyson of "bird-like" mathematicians with expansive views and "frog-like" mathematicians with keen focus on particular objects or situations) – one might classify Tamagawa-sensei as a problem-solver and Mochizuki-sensei as a theory-builder (though one who builds theories with specific problems in mind). Nonetheless, it seems rather arbitrary to individualize mathematicians according to such typological distinctions. More telling, perhaps, is the manner in which theory-building and problem-solving tendencies, rather than distinguishing individuals, dynamically forge communities and cultures as a duet. Thus, rather than merely enumeratively contrasting the respective styles of Tamagawa-sensei and Mochizuki-sensei, one might ask, instead, how they – both respectively and in concert – have shaped the anabelian geometry culture at RIMS. During the roundtable, I asked Tamagawa-sensei what open questions or problems in anabelian geometry he finds interesting or important. Alas, I had not included the term "important" in the prompt – and intentionally so – but it slipped out nonetheless: a kind of nervous glitch. Nonetheless, such a glitch led to the following –

Tamagawa-sensei:

In fact, again I have to say that I cannot give an interesting story.

[*Roundtable Laughs*]

Tamagawa-sensei:

This is related to my standpoint, or policy, or way of investigating mathematics. I'm not so interested in something conceptual, or something theoretical, or something well-motivated, or something important. I think that the word "important" is most far from my interest.

[*Roundtable Laughs*]

Tamagawa-sensei:

I have been investigating mathematics for more than 30 years, but always, my way is to look for the opportunity of [encountering] an unsolved problem which looks interesting to me, from time to time. Then, I concentrate on how to solve this problem by using experience or new methods. But, as such, I have been continuing to study mathematics. So, in some sense, even after 30 years, I have no good perspectives; in fact, I'm not so interested in perspectives.

[*Roundtable Laughs*]

Tamagawa-sensei:

I am only interested in the problem in front of me at the time.

Turning to the anabelian arithmetic geometry community at RIMS more broadly, one finds that this approach of Tamagawa-sensei does indeed influence its culture. Namely, Tamagawa-sensei – by letting the character of a given problem or object serve as the source of guidance for his labors, rather than an overarching strategy or perspective – has been highly receptive to international collaborators, who in turn – perhaps guided by their own perspectives or motivations – have approached Tamagawa-sensei with problems of interest.

Tamagawa-sensei:

For example [...] I had a collaboration on the configuration spaces of fundamental groups with Shinichi; and, as you wrote, a collaboration with Christopher Rasmussen related to Ihara's question on the pro-ℓ Galois representation coming from $\mathbb{P}^1$ minus three points; and, as you mentioned, I had joint work with Mohammed [Saïdi] on *m*-step solvability in anabelian geometry; also, some refinement of anabelian geometry in positive characteristic. And also, I have another collaborator: Anna Cadoret, from France. With her, I have already more than 10 joint papers. Basically, our main object is linear representations of arithmetic fundamental groups. [...]

I have already noticed: [...] almost all programs or motivations are imported by other researchers, not myself. For example, the configuration space [collaboration]: Shinichi already wrote a draft of the paper, but there was [an] exceptional case which was not treated, and I told him that, by a more algebraic method, I [thought] I could give a proof of these exceptional cases. The main motivation for the investigation of the anabelian-geometric study of configuration spaces came from him.

In the case of the finiteness conjecture [for] abelian varieties with Chris Rasmussen: [it] was his research plan; he came to RIMS, [we] discussed it, and I joined him; also [so] in the cases of Mohammed and Anna Cadoret. I have a few more collaborators. Always, they imported interesting questions. That's my way.

I have no future perspectives on interesting problems. Of course, I continue interesting projects with several people. Of course, there are several remaining open questions, but they may not be so important; it's a [matter of] personal interest. I can also say, of course, that there are famous classical open questions in anabelian geometry: the Section Conjecture or the $\widehat{GT} = G_{\mathbb{Q}}$ question [i.e. the equality of the Grothendieck-Teichmüller group and the absolute Galois group for $\mathbb{Q}$]. This is slightly more minor, but, for example, the congruence subgroup problem for moduli spaces [remains open]. These are interesting and difficult problems which are considered important – [nonetheless,] I don't like to use the word "important".

[*Roundtable Laughs*]

Tamagawa-sensei:

But, these have made recent progress by Shinichi and his colleagues, and I myself am interested in this sort of progress. But I myself am not considering these classically important conjectures. So, unfortunately, I have no interesting, beautiful, structural explanation. That's my way.

[*Roundtable Laughs*]

[...]

Tamagawa-sensei:

Two of my collaborators, Chris Rasmussen and Anna Cadoret, [...] they came to RIMS as JSPS postdoctoral fellows. This is because I had an acquaintance with their advisors. At that time, I hosted them at RIMS, and had regular meetings with them. It was natural to ask what interested them at the time. And then, naturally, we started a collaboration. In this case, when they applied for the JSPS fellowship, they had to write a research plan. For me, it is very natural to discuss their research plans.

Collas then offered his own perspectival interpretation on Tamagawa-sensei's work, against the backdrop of greater trends in number theory, algebraic geometry, and arithmetic geometry. Of course, one might not expect Tamagawa-sensei to hold this perspective (or speak of perspective at all, as a matter of policy). Nonetheless, one could gather that such was an exercise on Collas' part in relating, to a conceptually-, theoretically-, or perspectivally-minded reader, a framework according to which Tamagawa-sensei's mathematics might be interpreted; even if such is not the perspective that Tamagawa-sensei himself holds.

Collas:

If I may, I'd like to put a bit of balance [...] Please, [feel free to] contradict me. What you said, for me, illustrates two things. There are many kinds of mathematicians and ways of practicing mathematics. Now, I feel that there is a trend that you can find, for example, in the ∞-category approach: to have a project, to have well-structured things, categorical things. But, this way doesn't work for every mathematician. I would say that your approach, Professor Tamagawa, is to look at an object - it speak[s] to you, and then you pull a thread and produce beautiful mathematics. I suspect that Professor Mochizuki does something in the other direction.

[*Roundtable Laughs*]:

Collas:

It is good to remember: even within anabelian arithmetic geometry, there are already two different [approaches]. The other thing is: last time I checked, just using MathSciNet, your work involved maybe 7 or 8 conjectures. So, new mathematical insights are not always produced by a program or project; sometimes, just looking at the objects, from the bottom-up, also produces new insights which are not expected. In a way – so, this is my last point – I think maybe because of the Weil conjectures and motivic theory, arithmetic geometry is closely attached to cohomology, linear Galois representations, [...] and so on. There is always this idea of universality: we look at these linear Galois representations because they are universal.

But if we look – [as] a way to understand Grothendieck's [anabelian] conjecture – [we see that] the arithmetic of the étale fundamental group is universal, because the invariants which are produced give you information on the original geometric space. So, when one looks at a program, this universality property appears more clearly. But, also, when one looks at Professor Tamagawa's process of mathematics, this process carries the shadow or ghost of this universal property. Even if you look at an algebraic geometry problem, number theory problem, rational points – we can follow the list – if you have some homotopic sensitivity or expertise, then, your mathematics will capture something of value that you may not see only with number theory, only with low-dimensional topology, only with algebraic geometry. So, it's more difficult to see – but I always wonder if this is one [aspect] of the situation. Do you feel these things when you practice? No?

Tamagawa-sensei:

Mmmm. You summarize very nicely, but my first impression is that it is not like this.

[*Roundtable Laughs*]:

Tamagawa-sensei:

My investigations are more tentative.

3.2 Mochizuki-sensei: Convergent Theory-Building

From here, it would be a relatively elementary exercise to counterpose, relative to Tamagawa-sensei's mathematical approach, the conceptual and perspectival theory-building program of Mochizuki-sensei. However, doing so, alone, one would run the risk of endorsing the implication that, whereas Tamagawa-sensei's mathematics is clearly collaborative in spirit, Mochizuki-sensei's mathematics is, guided by the thrust of his own strategic imperatives, that of a program which is somehow pursued without observation of wider mathematical developments. Much to the contrary, however, when asked about the state of contemporary anabelian geometry, Mochizuki-sensei expresses his keen interest in the advancements and intersections of many theoretical pathways involving various mathematicians in the community. Thus, whereas

Tamagawa-sensei's policy predisposes him to make community observations of interesting problems that are brought to his attention, Mochizuki-sensei's community observations – which are very much conceptual and perspectival – concern inter-theoretical trends; and, notably, growing confluence along pathways of interest. I asked Mochizuki-sensei about his views on the

contemporary state of the field and the developments which have transpired over the past 3 decades, prefacing the question with my observation of the (not infrequently) held supposition among those peripheral to anabelian arithmetic geometry that its saga reached its climax with the proof of Grothendieck's anabelian conjecture in the 1990's. Of course, this belief obviously overlooks the Section Conjecture, which remains open. However, I anticipated that, in Mochizuki-sensei's view, such is not the only grievous oversight implicit in this supposition.

Mochizuki-sensei:

I think [it's] a fundamental misunderstanding of the situation that anabelian geometry is somehow over. It reminds me of this word in Japanese, an internet word: 「オワコン」 or「終わったコンテンツ」. It refers to contents which are over. So, in other words, someone is making a big deal of something, but that's already been resolved; it's already been done with. I think there's this feeling, outside the RIMS anabelian [geometry] community, that anabelian geometry is over. This is just completely wrong.

So, Grothendieck's letter to Faltings, or the *Esquisse d'un programme* – from my point of view, they are quite old now. They sort of give hints about further developments. They are filled with interesting insights, but the insights are not quite in the right direction. They are almost in the right direction, or very roughly in the right direction, but not really in the right direction. So, this topic is very closely related to [...] RNS and combinatorial anabelian geometry.

RNS is related to reconstructing points: type I Berkovich points. In particular, it's also much related to the p-adic Section Conjecture. In recent work on the p-adic Section Conjecture with Hoshi, RNS plays a very important role. This is closely related to Grothendieck's letter to Faltings. So, the point of view there is the Section Conjecture over number fields. Grothendieck never seems to really think about anabelian geometry over p-adic local fields; this really started with my work in the 1990's. He seems to regard the Section Conjecture over number fields as an approach to Diophantine geometry, [and] possibly a new proof of the Mordell Conjecture. The current point of view, first of all – in recent work with Hoshi – [is that] the Section Conjecture over number fields can be reduced to the p-adic Section Conjecture plus three conditions; and, those three conditions are expected to correspond to three new enhanced versions of IUT, which are currently under development.

The first version is the Galois Orbit Version [discussed here]. The other two are a little bit further away, but the Galois Orbit Version has already been substantially written. [...] The other two are really logically independent of the original IUT, but they use similar techniques, whereas the Galois Orbit Version is a strict generalization of IUT. It's interesting, precisely because of this letter to Faltings and the issue of the Section Conjecture, because it corresponds to the first of these three conditions. Basically, the condition is that a Galois section has finite height. If you assume the p-adic Section Conjecture, you get these local points; and so, they might intersect the point at infinity in some way. If they intersect the point at infinity modulo p^n, then the local height is n, and you add up the n's for various p, and that gives you the height. In the case of the Global Section Conjecture for number fields: *a priori*, this may not be bounded. The Galois Orbit version gives you a bound, which is precisely the abc inequality bound. So, in other words, the abc inequality appears as a special case. [...] It's [very closely related to] the Section Conjecture, but it's not exactly what Grothendieck had in mind; it's somewhat different from what Grothendieck had in mind.

Again, we can see RNS in this strategically interesting position of relating the Section Conjecture and IUT, and so on. It's really amazing – the strategic position of RNS.

Of course, RNS is also related to combinatorial anabelian geometry, and combinatorial anabelian geometry is related to the *Esquisse d'un programme*: this Teichmüller tower. The various moduli stacks of $[\widehat{T}_{g,r}]$ vary [i.e. for genus-g curves with r points removed], especially at the boundary at infinity: that is the combinatorial portion. A special case of this is [that of] configuration spaces. Configuration spaces are just collections of points on curves. As you go higher and higher, you have various loci where the points coincide with one another. In order to work with log-smooth objects, you have to blow up these loci. That gives rise to tripods – in other words, $\mathbb{P}^1$ minus three points – various configuration spaces of tripods. That's what configuration spaces are about.

Combinatorial anabelian geometry can be applied to understanding the anabelian geometry of configuration spaces, particularly the outer automorphisms of configuration space groups – so, in other words, geometric fundamental groups – precisely because it allows you to reconstruct this combinatorial structure. It's like bubbles – all sorts of bubbles coming up as you increase the dimension: each bubble is a tripod. You get these various bubbles that are connected to each other in a complicated, combinatorial fashion. Anabelian geometry allows you to reconstruct these combinatorics via anabelian geometry from the structure of the fundamental groups. This is very much related to the *Esquisse d'un programme*. It's not exactly what Grothendieck conjectured, but it's the right answer. Ultimately, one is led, when one investigates all these combinatorics of this Grothendieck-Teichmüller lego – that's precisely the combinatorics of combinatorial anabelian geometry – what you're led to, ultimately, is $\widehat{GT}$ [the Grothendieck-Teichmüller group].

So, $\widehat{GT}$ was originally defined – I think by Drinfel'd – using relations; and this is the point of view that was also taken by Ihara, and so on. But $\widehat{GT}$ appears in a completely different way, a logically independent way. So, you don't need the previous Drinfel'd approach in anabelian geometry to deal with $\widehat{GT}$. It's still work in progress, but I think we're very close to achieving fundamental results concerning $\widehat{GT}$, as Tamagawa referred to. This is joint work with Tsujimura and Mohammed Saïdi. It constitutes a culmination of combinatorial anabelian geometry. I refer to this as CAT: combinatorial algebraization theory. Basically, what it's about is using anabelian geometry and combinatorial anabelian geometry techniques to show that various purely combinatorial group actions in fact arise from scheme theory. This relates to $\widehat{GT}$ and also configuration space groups. These purely combinatorial actions are in fact actions that arise from scheme theory. We're still writing the papers right now, and I have a Zoom meeting with Mohammed Saïdi today, where we'll discuss this. It's work in progress. We hope to discuss the reduction of the global Section Conjecture to various versions of IUT, as well as this work on $\widehat{GT}$, and so on [...] in detail during the March workshop .

And again, RNS is also related to this aspect. So this is what is very interesting. So, if you look at the paper with Tsujimura – myself and Tsujimura – on RNS, we also give an application to $\widehat{GT}_p$, which is the p-adic version of the Grothendieck-Teichmüller group defined by Yves André. Interestingly, Emmanual Lepage [was] a student of Yves André, and I got involved with Yves André through Fumiharu Kato [加藤文元]; and Fumiharu Kato got involved with Yves André because [...] a long time ago, he came to Japan as a JSPS post-doc – that's how he got involved. So, we're all connected in this way. Yves André wrote this paper on $\widehat{GT}_p$, which had these various gaps. Here, I should remark that there was never any controversy regarding these gaps. I pointed them out: he said, "yes, I don't know what I was thinking." There was never any sort of unfortunate controversy. But anyway – there were fundamental gaps, and these gaps were repaired partly in the third combinatorial anabelian geometry paper and finally using RNS – so, everything is connected.

Another aspect, which is very interesting, which I wasn't aware of, is that: in recent work on the *p*-adic Section Conjecture [...] interestingly [...] many aspects are very reminiscent of techniques in IUT – so, there's that connection. Another thing, which is very interesting, is that the *p*-adic Grothendieck conjecture results that I obtained in the 1990's can be generalized very substantially, and that generalization is used in the function field aspects of the *p*-adic Section Conjecture that we're currently working on. That generalization requires a generalization of Bloch-Kato theory. Bloch [and] Kato's paper appears in the *Grothendieck Festschrift* [Volume 1]; so there, the theory of the exponential map only holds for *p*-adic local fields, [or] finite extensions of $\mathbb{Q}_p$. So, this Bloch-Kato theory of the exponential map – I didn't know about this until recently; it was pointed out to me by Go Yamashita – around 2002 was generalized by someone named [Laurent] Berger in France to arbitrary complete discrete valuation fields with perfect residue field. This generalization involves de Rham representations and crystalline representations, semi-stable representations of the absolute Galois groups of such fields. This theory of Berger is based on the theory of André, concerning *p*-adic differential equations. Everyone is just very much connected. It's just surprising how all these things are connected in all these ways.

Anyway, anabelian geometry is far from being over; it's booming. It's really in a stage of dramatic growth. All sorts of diverse things are coming together. So [...] I think it's a very exciting field. [...]

With all of this being said, as Mochizuki-sensei mentioned to me several times, his observations regarding affinities or reminiscences between mathematical projects tend largely to be made in hindsight, following the completion of his own mathematical activity. He pursues a segment of his program, only to perform historico-synthetic consolidation of comparative insights *ex post*.

Mochizuki-sensei:

I should maybe add that there are people – like [Leila] Schneps for, instance – who, literally, [were] very interested in meeting Grothendieck. She found Grothendieck and met him. She was very much interested in going through [the *Esquisse*] and going through the gaps, whereas I was not interested in [such] things at all; I went my own way. And then, after I developed these theories, I went back and noticed [all of this]. Also, in the case of *p*-adic Teichmüller theory and configuration space groups, you can see traces of those developments [...] in the work of Ihara [from] the 1970's and 80's. But, I didn't really look at it; in many cases, I wasn't really aware of it – I just developed it on my own.

Thus, one could say the following of Mochizuki-sensei and Tamagawa-sensei. When commencing work on a problem, Tamagawa-sensei does not maintain a conceptual thesis regarding how such work holds significance relative to a broader body of work. In Mochizuki-sensei's case too, it appears as though mathematical work does not commence with a kind of thesis comparing his works to historical precedent; he proceeds according to his own program. However, in his case, he does form a conceptual thesis relative to other works *a posteriori*. Nevertheless, Mochizuki-sensei even compared his own approach to that of Tamagawa-sensei. Thus, perhaps, in certain respects, the two do not differ as much as one might suspect *prima facie*. The appearance of this subtle insight was brought to the surface following a question posed by Collas.

Collas:

If I may say something – [and] this is something I hope someone may find a bit interesting – *Esquisse* [...] was a very stimulating object [for] develop[ing] many kinds of mathematics. It agglomerated many from Japan, the US, France – Ihara, Drinfel'd, Fried, Harbater, Schneps, Lochak, Dèbes, André ... But for a very long time, the Galois action was an input used to capture properties in anabelian constructions. Then, I think, your paper with Hoshi and Minamide [Arata (南出新)] in 2011 or 2012, maybe, is the first time we [went] in the reverse direction. We have some anabelian geometry, which provides some input to Galois-Teichmüller –

Mochizuki-sensei:

So, you mean 2017.

Collas:

2017, yes. [...] If we look back at the beginning, we had all this arithmetic anabelian geometry [work] that produced interactions with braid groups, low-dimensional topology, Oda's problem [see also the 2023 Oberwolfach Report], the number theory of Greenberg [see, e.g., Pries] and Ihara-Anderson... Now we have a new input that is more canonical, more absolute, from anabelian geometry that goes back to arithmetic anabelian geometry. So, one very efficient test is to compare this new method on $\widehat{GT}$ as a test object, for example.

Somehow, this is only the beginning of the problem – how it will interact with number theory, low-dimensional topology... Maybe we can expect [something], but it's not clear yet. So maybe, Professor Mochizuki: [do] you have some idea about this – how your development of anabelian geometry may interact with other fields, as in the past?

Mochizuki-sensei:

So how it might interact with... With what kind of research? The research of Leila Schneps?

Collas:

No, no, no. So, if you look at Ihara's problem, there have been some consequences for ℓ-adic zeta/beta functions and for motivic theory via Deligne-Ihara Lie algebras; or for Greenberg's number theory approach. Or, if you look at Oda's question on the universal monodromy representation, there has been application by Nakamura to Morita's conjecture on low-dimensional topology. The input is arithmetic. The input is Galois-Teichmüller. So, now you have a kind of new way of approaching anabelian arithmetic geometry. So, can we expect new insights in this direction?

Mochizuki-sensei:

So, with regard to Ihara's work on ℓ-adic zeta values: I don't see any relationship with that.

Tamagawa-sensei:

He's asking –

[*Roundtable Laughs*]:

Tamagawa-sensei:

– whether or not there is a potential application of [...] recent developments in anabelian geometry by you and your colleagues [to] topology, number theory, or other topics. This is not a solid [question]. His question is whether or not you feel [that there is] a possible application to other areas – such things. You are now intensively developing new aspects of anabelian geometry, but do you see a potential relationship with other areas?

Mochizuki-sensei:

I don't want to deny the possibility of such developments, but I don't particularly see any such developments. So, I guess my feeling is similar to Tamagawa's.

[*Roundtable Laughs*]:

Mochizuki-sensei:

I'm looking at what is in front of me.

3.3 Tamagawa-sensei: His Entrance to Anabelian Geometry

Early in the going, I had encouraged the roundtable participants to pose questions to one another; I felt that I need not centralize the conversation around my own questions. As shown already, Collas readily adopted the suggestion, asking questions to Tamagawa-sensei, Mochizuki-sensei, and Hoshi-sensei. I was glad to see, furthermore, Mochizuki-sensei ask Tamagawa-sensei about his foray into anabelian geometry, a question which I would not have asked.

Mochizuki-sensei:

So, even your involvement with anabelian geometry started from just looking at Uchida's paper. So, why did you start to look at Uchida's paper?

Tamagawa-sensei:

So, first of all, my encounter with anabelian geometry: this is when I was a master's student at the University of Tokyo [東京大学]. At the time, Professor Hiroaki Nakamura [中村博昭] was an Assistant Professor at the University of Tokyo. At that time, strictly speaking, he was a unique researcher in anabelian geometry in Japan. [...] He invited me to the interesting world of anabelian geometry. At that time, I just listened to his theory.

After moving to RIMS, there were some researchers in arithmetic fundamental groups, like Ihara, or Matsumoto [Makoto (松本眞)]. I was gradually influenced by them. Then, at around that time – so, this is related to Shinichi's question – there were some papers on so-called birational anabelian geometry; namely, anabelian geometry over finitely generated fields over prime fields, like number fields in positive characteristic. It was studied by Florian Pop [at the time]. I myself was interested in the anabelian geometry of curves [...] at that time. Birational anabelian geometry treats the full absolute Galois group, which has more information than the fundamental group, which kills the ramification. But, I wanted to extract some ideas from the paper on birational anabelian geometry. Florian Pop's paper is quite field-theoretic and quite valuation-theoretic, not so geometric.

At that time, the only point I learned from the paper is: the result was reduced to Uchida's result on the anabelian geometry of function fields in one variable over field fields. This is in the 70's, before *Esquisse* or the letter to Faltings. The best thing I learned from Florian Pop's paper was the existence of Uchida's paper.

[*Roundtable Laughs*]

Tamagawa-sensei:

Then, I started to learn Uchida's method, adding some more ideas on the Lefschetz trace formula. I realized that Uchida's method can be imported to the case of fundamental groups.

3.4 Hoshi-sensei: A New Anabelian Generation

Hoshi-sensei came to Kyoto University as a master's student in 2004, completing his master's thesis in 2006 with Mochizuki-sensei as his advisor; and his PhD thesis, in 2009, also with Mochizuki-sensei as his advisor. He was appointed as a lecturer at RIMS in 2011 and as an Associate Professor in 2017. Thus, Hoshi-sensei's graduate education began after Grothendieck's anabelian conjecture (in its original form) had been proved, and he has been very active in new directions such as mono-anabelian geometry and combinatorial anabelian geometry. Returning again to this supposition that anabelian geometry is somehow「オワコン」, I thought it appropriate enough to ask Hoshi-sensei for his views on the state of the field, as well as his entrance to the anabelian world.

Hoshi-sensei:

I was entering RIMS. I was a master's student. [Professor Mochizuki] was working on absolute anabelian geometry for hyperbolic curves over finite fields. [...] Of course, the Section Conjecture is still open. This situation told me that anabelian geometry is not over – in a natural way. I don't know everything, but, for someone [in my position], [because] these two [Professor Mochizuki and Professor Tamagawa] study anabelian geometry, it's difficult to think that anabelian geometry is over. [...]

The next question is the contemporary [situation]. Maybe I should declare that my way of doing mathematics is similar to [that of] Professor Tamagawa: I don't have any deep per–... I'm sorry, this doesn't imply that Professor Tamagawa doesn't have any deep perspective. I'm sorry.

[*Roundtable Laughs*]

Hoshi-sensei:

If I find something interesting, I try to solve the problem or formulate the problem in a natural way. But I should say something concrete...

Recently, I've been working on the Section Conjecture with Professor Mochizuki, as he already explained, by means of inter-universal Teichmüller theory. Actually, I have given talks – in Tokyo, Kyoto, and Paris. [...] After the local Section Conjecture, the global Section Conjecture leads to three conditions, which will be related to three new, enhanced versions of inter-universal Teichmüller theory. [...]

I think that, as someone [from] this generation, I should encourage young anabelian researchers to learn something from inter-universal Teichmüller theory. Maybe I should encourage that. Actually, it seems to be difficult to [give] a nontrivial result in inter-universal Teichmüller theory; this is a fact. But, at least, I think that it is not difficult to learn inter-universal Teichmüller theory and obtain something from this theory; I think so. Maybe, one concrete example is Tsujimura-san.

So recently, Tsujimura-san has established some various deep, nontrivial, interesting results in anabelian geometry. What is the reason why he can do [this]? [One] reason is that he is smart, he is great; of course. But is there any other reason? He, for instance, has a deep understanding of cyclotomic synchronization, which is a notion that is very fundamental – elementary, but important – in inter-universal Teichmüller theory. So, I think that such a notion [...] may help young anabelian researchers. [...]

It's difficult to obtain a deep result in anabelian geometry without a technique [from] inter-universal Teichmüller theory; I may be able to say so. I should encourage young anabelian researchers to learn something from inter-universal Teichmüller theory; maybe I should say so.

SciSci - Science for Scientists
SciFrontiers No. 1, Version 1.23
"Voices of the Anabelian Arithmetic Geometry Community (RIMS, Kyoto)"
DOI: 10.5281/zenodo.14860639
https://www.sci-sci.org/voices-anabelian-geometry-rims