ON PIONEERING MATHEMATICAL RESEARCH, ON THE OCCASION OF ANNOUNCEMENT OF FORTHCOMING PUBLICATION OF THE IUT PAPERS BY SHINICHI MOCHIZUKI

IVAN FESENKO

Inter-universal Teichmüller (IUT) theory of Shinichi Mochizuki[1] was made public at the end of August of 2012. Shinichi Mochizuki had been persistently working on IUT for the previous 20 years. He was supported by Research Institute for Mathematical Sciences (RIMS), part of Kyoto University. At the press-conference of Kyoto University, ran by M. Kashiwara and A. Tamagawa, it is announced that the IUT papers are now accepted for publications and will soon be published.[2] Experts, who have spent years on the study of IUT and appreciate its depth, are celebrating, and there is something which non-experts can celebrate: it has never been the case that the author of an important mathematical breakthrough has invested so much of his time and effort to explain, discuss and answer a 4-digit number of questions about his theory, via email communication, skype talks, numerous meetings and seminars at RIMS and international workshops, for such a long time before its publication. IUT is the outcome of a long-term work by its author. His mathematical vision and perseverance is most admirable and is a sample to follow.

The IUT theory studies cardinal properties of integer numbers. The simplicity of the definition of numbers and of statements of key distinguished problems about them hides an underlying immense complexity and profound depth. One can perform two standard operations with numbers: add and multiply. Prime numbers are 'atoms' with respect to multiplication. Several key problems in mathematics ask hard (and we do not know how hard until we see a solution) questions about relations between prime numbers and the second operation of addition. More generally, the issue of hidden relations between multiplication and addition for integer numbers is of most fundamental nature. Deep problems include several types of abc inequalities. One of them is proved as an application of IUT. But IUT is more than a tool to solve famous conjectures. IUT is the study of deformation of arithmetic objects by going outside conventional arithmetic geometry, using categorical monoidal geometry, working with groups of symmetries instead of commutative rings, and applying deep results of mono-anabelian geometry. It is a highly novel theory with a two-digit number of new concepts. It can profoundly influence number theory and mathematics. It restores in number theory the place and role of topological groups. In its future developments it will also restore the role of work in classical algebraic number theory and increase applications of computational number theory. IUT is not an increase of mathematical knowledge in an area in which there are many specialists able to study it. It is a rare pioneering vast development with many new concepts and ideas, and with a great potential for future developments.

The problems that IUT solves belong to the area of Diophantine geometry whose previous conceptual points of view and methods differ substantially from those in IUT. The main prerequisite for IUT is anabelian geometry, one of three fundamental generalisations of class field theory, whose methods and concepts are essentially different from those in the currently most popular generalisation of class field theory, the Langlands program.[3]

Date: April 3 2020.

[1] Sh. Mochizuki, Inter-universal Teichmüller theory I: Constructions of Hodge theaters, II: Hodge-Arakelov-theoretic evaluation; III: Canonical splittings of the log-theta-lattice; IV: Log-volume computations and set-theoretic foundations, 2012–2020, available from `http://www.kurims.kyoto-u.ac.jp/~motizuki/papers-english.html`

[2] `https://www.sankei.com/life/news/200403/lif2004030104-n1.html`, `https://www.sankei.com/life/news/200403/lif2004030075-n1.html`, `https://www.sankei.com/life/news/200403/lif2004030058-n1.html`, `http://www.asahi.com/ajw/articles/13271575`

[3] For more details see I. Fesenko, Class field theory guidance and three fundamental developments in arithmetic of elliptic curves, available from `https://www.maths.nottingham.ac.uk/plp/pmzibf/232.pdf`

Hence almost all experienced researchers could not use their previously acquired mathematical intuition and expertise in their specific area, to study IUT. In 2012 there were few experts in anabelian geometry outside Japan, for example in 2012 there was only one USA expert in arithmetic anabelian geometry. Some useful remarks in relation to the application of IUT to the conjectures came in 2012 from two number theorists working in areas far from anabelian geometry.

To help mathematicians to study IUT, a lot of effort has been invested in the dissemination of IUT, via various workshops, including large international, via seminars, lectures and study groups. To become an expert in IUT one has to apply strenuous efforts in the study of this new part of number theory during an appropriately long period of time. This cannot be done in the period of few weeks or months. The number of IUT experts, now from six countries, is two-digital, and it will keep growing. Time dedicated to the author work leading to IUT and dedicated to its study by others exceeds 50 years. This is one of the largest time investments in the history of mathematics into a single theory before its publication.

Sh. Mochizuki spent vast amount of time to explain aspects of his theory to mathematicians who contacted him, he was fully open to answering mathematical questions via email and internet communication. These researchers have sent a 4-digit number of questions and remarks to the author, all addressed. No valid math evidence of any serious fault in IUT has been found.[4] Minor oversights have been found and corrected. To this day there remains no mathematically substantive reason whatsoever to doubt the validity of IUT. 13 surveys of IUT and a book on IUT by 8 mathematicians from 5 countries individually present the theory in different ways.[5] 2020–2021 is a special RIMS year with 4 international workshops on anabelian geometry, combinatorial anabelian geometry and IUT[6], supported by the new Center for Research in Next-Generation Geometry.

IUT has attracted huge public interest. An article on IUT in Inference[7] had more than 10,000 viewings in the first 4 months. More than 170,000 viewers watched F. Kato's public lecture on IUT delivered in Tokyo in October 2017[8] and his book about IUT[9], for a general audience, published in Japanese in April 2019, entered the list of 20 bestsellers in all subject areas and was awarded the Yaesu prize.

We can review some general aspects of modern pioneering mathematical research, in light of IUT, its study and reaction to it. This review should also include certain negative aspects which affect modern researchers especially pioneers working on fundamentally important long-term projects. Of course, a genuine consensus about any mathematical theory can only come from experts in its subject area. Sh. Mochizuki's work includes fundamental pioneering contributions in numerous directions: Hodge–Arakelov theory, anabelian geometry, mono-anabelian geometry, combinatorial anabelian geometry, Grothendieck-Teichmüller group, p-adic Teichmüller theory, inter-universal Teichmüller theory, combinatorial anabelian geometry. Except for IUT, none of his work has ever been criticised — because it was read and appreciated by experts, while all except one[10] critical comments on IUT are produced by mathematicians far from having any expertise in anabelian geometry, the subject area of IUT. Unusually for mathematical developments, some mathematicians felt appropriate to publicly criticise IUT and its study without having any evidence of expertise in the subject or without having applied any serious efforts to learn IUT or at least attend numerous IUT workshops. In the first approximation, the number of negative, not based on any mathematical substance, reactions to IUT was inversely proportional to the number of home academicians capable to study the theory. Compare: no expert in IUT has made negative remarks about IUT. While experts were not interested in cheap talk, negative online criticism went always in a quite vague form without any single valid concrete mathematical evidence of any fault in IUT. Sometimes it was hostile to the author of IUT and mathematicians studying IUT. Certain media, as well as few bloggers

[4] see also links at footnote 17

[5] See https://www.maths.nottingham.ac.uk/plp/pmzibf/guidestoiut.html for guidance materials on IUT.

[6] http://www.kurims.kyoto-u.ac.jp/kyoten/en/index.html, http://www.kurims.kyoto-u.ac.jp/~motizuki/project-2020-english.html

[7] http://inference-review.com/article/fukugen

[8] Its record with English subtitles is available from https://www.youtube.com/watch?v=fNS7NO4DLAQ.

[9] 'Mathematics connecting universes, the shock of the IUT theory'by F. Kato, Kadokawa 2019

[10] see footnote 17

and researchers, void of understanding of the subject area (evidenced by peer review publications or workshop talks in anabelian geometry or IUT), posted very ignorant or absurd opinions, lacking mathematical content, about the theory and its study.[11] The most recent example of not doing justice to the work of the author and many experts in the last 7 years and a gross misrepresentation was a dilettantish article in Nature, a journal not publishing professional math papers, which failed to include opinions of numerous experts in IUT or anabelian geometry. What is worrying is the intrusion of partisan politics of non-experts into serious fundamental pioneering research. The intervention of ignorant opinions, beliefs and prejudices of non-experts is invariably going to be malign. Misinformation and disinformation in science has become a serious issue.[12]

We need to take pioneering research more seriously, try to understand it and our own reactions to it. The key questions include how can we support future pioneers of fundamentally new theories in their long-term work, and help mathematicians to improve their reaction to novel theories which are far ahead of the mainstream mathematics of their time.

Aspects of groundbreaking pioneering mathematical research

Hard mathematical problems can sometimes be solved as nontrivial applications of previously created theories involving appropriate advances in technical arguments, methods, mathematical tricks. Since the development goes inside an established area, there are usually experts who can check and confirm the proof reasonably fast. There is another way: develop a new theory or a program, over a long period of time, that views the problem from a novel perspective. Finding and choosing new perspectives on a complex problem plays a fundamental role in mathematics. Often such perspectives emerge gradually as the result of the work of generations of mathematicians, such as class field theory. Rarely, however, as in the case of IUT, a theory is developed by a single mathematician. Because this is a new theory, there are no or few experts in it; its study will require considerable effort and perhaps some 'elastic' thinking[13]. The history of mathematics demonstrates that innovative theories can be hard to understand for their contemporaries, and the challenges of novelty can be large enough to produce subjective reactions of rejection and non-acceptance.

An important aspect, in the case of pioneering developments in mathematics, is the potential lack of mathematical infrastructure and language to communicate novel concepts and methods. A substantial part of the IUT papers is a development of appropriate infrastructure and language. The process of refining such new infrastructure, to make it function in an optimal way, can be long, going far beyond the time of the original work.

As the author of IUT remarked, when the language (in this case English) used in a text written to describe a complex theory arises from a substantially different cultural and historical background from that of the author of the theory, his text may be perceived by mathematicians at a substantial cultural distance from the author in the following way: the text may appear rather foreign and psychologically impenetrable, even if it is free of flaws. Moreover, somewhat paradoxically, the lack of technical linguistic flaws may even make the text feel all the more foreign to such mathematicians.[14]

The author can write as clearly as possible for him, and still his presentation of his theory can be difficult to follow for others. There is no obligation for the author of a breakthrough theory to write at the level of a well polished textbook. Sometimes, the author is led to present certain things in a way which is natural from the point of view of how the theory has been developing in his head, but remains unknown to the readers of his papers. There is much more about the theory, which is known to the author, but cannot be included in the author's paper by various reasons. Good learners should reach this stage of knowing. In the case of IUT, this

[11] for more facts see `https://www.maths.nottingham.ac.uk/plp/pmzibf/rapg.pdf`

[12] Epidemiologist Bergstrom recently wrote 'we are also fighting on a second front that we did not anticipate, fighting a battle against misinformation and disinformation in a hyper-partisan environment ... the world has changed in profound ways since even 2010. Social media, hyper-partisanship, the broad populist distrust of experts, plummeting standards of factfulness', `https://twitter.com/CT_Bergstrom/status/1243252341756669953`

[13] e.g. in the sense of L. Mlodinow's book 'Elastic'

[14] e-mail communication, March 23 2016

stage is not achievable without a solid knowledge of anabelian geometry. Pusillanimous efforts to study cannot lead to success.

Following details of a theory at the conscious level does not necessarily imply that one has reached the stage when one is happy to acknowledge and accept the theory. Subconscious mathematical imprints and cultural conditioning play a fundamental role in accepting things. Mathematical arguments can make sense only against an appropriate contextual background which requires appropriate time to digest. When a theory is entirely novel, no conditioning and imprints yet exist at the subconscious level of most of its readers. It needs time to develop. Conceptually new visions of IUT naturally meet with the resistance of researchers working in their own areas, far away from IUT.

Rigidity in anabelian geometry is its central feature. Value rigidity, in the terminology of R. Pirsig, is the inability to reevaluate what one sees because of commitment to previous values. By obvious reasons it may stronger affect people who have been longer in the field. Value rigidity may also include a certain intolerance of new ways of seeing things. One's immersion into the conventional wisdom of one's area may impede one from accepting new ideas.

Younger researchers generally need less time to adjust to a new theory, in particular, since their vision is not obstructed by years of work in a narrow area. Therefore they can communicate a more objective picture of how difficult a new theory is. All younger experts in IUT have not found the study of the theory more difficult than their study of other theories.

IUT and number theory

New discoveries and academic theories that never existed in earlier studies always appear on stage in the form of a minority view. When a solution of a famous hard problem by a leading mathematician becomes public, researchers typically do not delay to study it on their own, arranging seminars and workshops to understand its new concepts, ideas and methods. However, the challenge to study of IUT has attracted smaller numbers of mathematicians than one could have expected. IUT is an extraordinary novel and complex theory, but still. There are certain reasons for this reaction to IUT. This text does not aim to list all of them. Some of them are indicated above, some are due to ossification of parts of number theory, some are of ethical nature, and some reflect more general problems with long-term fundamental research in modern science.

Four most important mathematical reasons explaining the difficulty to study IUT are related to the overall poor digestion of the Grothendieck heritage by number theorists, to a relatively large distance between anabelian geometry and IUT and the mainstream directions, to a large number of new concepts in IUT and to its relatively large volume. A relatively small number of number theorists have an experience of working with the étale fundamental group, one of first key objects of anabelian geometry. The Grothendieck heritage, so essential for IUT, has not been properly digested by significant numbers of number theorists. Strikingly naive questions at the Oxford IUT workshop about why does one need to use categories in number theory is a reminder of that issue. Other areas of number theory, e.g. analytic number theory or research on Galois representations, or aspects of the Langlands program, are far away from key substantial methods of IUT.

The Japanese tradition of highly original pioneering research in mathematics originated from T. Takagi, the fundamental contributor to algebraic number theory. His contribution to class field theory, the key development in algebraic number theory, included his existence theorem in class field theory of general type, which was a great conceptual breakthrough from the preceding class field theory of special type. 50 years after its start, the Langlands program still does not have developments parallel to general class field theory. Recent analysis shows that we are still in the 'pre-Takagi' stage in the Langlands program, in the sense that similar conceptual breakthroughs to class field theory of general type have not yet happened there.[15] The main prerequisite for IUT is anabelian geometry, one of key generalisations of class field theory. Among hundreds of researchers working in the Langlands program or in Diophantine geometry, experts in anabelian geometry in 2012 could be counted on the fingers of one hand.

[15] see the text cited in footnote 3

It does take a lot of time for many fundamental theories to fully develop, mature and be simplified.[16] Certain developments in general class field theory were understood by small numbers of mathematicians at the time of their publication, smaller than the number of IUT experts now.

Exams

Together with the verification of a new fundamental theory, there is always another parallel exam going on: how responsibly take the task of its study contemporary researchers and how they react to the new theory. The ethical responsibility of mathematicians includes a certain duty, never precisely stated in any formal way, but felt by and known to serious researchers: to dedicate an appropriate amount of time to study each new groundbreaking theory or proof in one's general area. Truly groundbreaking theories are rare, so this duty is not too cumbersome. This duty is especially applicable to researchers who are in the most active research period of their mathematical life and who have already senior academic positions so that they can afford to dedicate their time to the study of a new fundamental development. The real life is more complicated, but it is natural to expect that a reasonable number of mathematicians in each major math country appropriately studies new groundbreaking theories. With respect to the study of IUT, it is fair to say that this second exam has so far been generally failed.

One can meet with 'believing' or 'non-believing' in a theory or a proof. It is one situation when this is based on solid mathematical knowledge of the subject area or a theory. More commonly, 'believing' or 'non-believing' is not based on concrete mathematical knowledge but either on one's own imagined and often partially or substantially incorrect picture of what the theory is about, or, even worse, it is influenced by herd mentality. For example, many number theorists 'believe' in the Deligne proof of GRH, but few have throughly studied it; all 'non-believing' in IUT mathematicians the author of this text has talked with have not been able to indicate any concrete mathematical evidence justifying their attitude, and first inquiries into their knowledge of the theory revealed huge gaps. Do we need to increase the number of 'believers' in IUT, and do we give priority to increasing the number of experts in IUT? The answer is obvious.

To become an expert in IUT one has to study the subject area seriously and for a long time, not for one week and not for a couple of months. We have contrasting examples of PhD students patiently and diligently studying IUT and eventually contributing to its further extensions, and full professors in the prime of their mathematical productivity taking the convenient niche of sceptical attitude or referring to the difficulty to study IUT despite the increasing number of experts in it and the body of its surveys. Of course, mathematicians working on their own fundamental breakthroughs, should be let concentrate on their work.

It is reasonable to be sceptical about a new fundamental development but only if one has or has acquired an expertise in the relevant area, which in the case of IUT is anabelian geometry and IUT itself. To declare oneself a sceptic in relation to a theory, whose subject area one does not know and does not apply efforts to study, is shoddy.

The use of internet facilities can play a positive role or a negative one. In the case of IUT the internet has been used by small groups of people to deliberately disinform and confuse instead of to assist and inform. Reading online texts about IUT may induce opinions totally opposite to the real situations. Some of negative remarks and misrepresentations on the internet may have misled some mathematicians who trust what they read on the internet or who could not distinguish an expert in or a serious learner of the subject area from an irresponsible non-expert. It is regrettable that it has become acceptable for some researchers to produce repugnant public statements about the work of other mathematicians without any valid mathematical justification.

Several researchers, who could have become learners of IUT, declined invitations to participate in the IUT workshops, some among them have broken professional rules of conduct and made public their ignorant and sometimes even intolerant opinions about IUT, without providing any evidence of concrete math problems with IUT. It is irresponsible to make one's general negative opinion about math work public when it is not based on

[16] examples include Galois theory and Lobachevsky's hyperbolic geometry

its good knowledge. Still, it was possible to contribute useful questions/comments/remarks in relation to more conventional parts of the theory.

In 2013–2017 no concrete mathematical remarks originated from mathematicians making negative public remarks about IUT. The only written comment, just on ten pages, arrived in 2018 after years of requests to its non-expert author to reveal any problem, was fundamentally incorrect, based on some careless incorrect oversimplification of the theory, see various materials at this page[17].

Some may wrongly imagine two expert sides in their take on IUT, but in truth there is one side only: the side of experts in IUT, who have worked for years to learn the subject area and the theory. They, together with the referees and the group of editors processing the IUT papers, have all concluded that the IUT papers have no mathematical flaws. Part of this process was an unprecedented event when the author of IUT has answered more than 1000 of questions for more than 7 years. Two year long seminars on IUT in 2018–2019 for new learners of the theory did not detect mistakes.

Increasing obstructions to fundamental breakthroughs

Some roots of the decline of support to long-term fundamental work, such as the shortsighted race to higher number of publications and higher citation index, which often results in pressure to produce short-term work that consists essentially of minor improvements to known results, originate from causes external to the mathematical community. This race was initiated and stimulated by bureaucrats who need quantity instead of quality, in order to be useful in measuring something, to justify their own jobs.

Young mathematicians bear the brunt of this short-sighted race and other related aspects. Their potentially great mathematical life is unlived. They are forced to lose the enthusiasm for research lasting more than a couple of weeks, they fail to escape the common rut, and they exhibit unremarkable attitude to what and when to study in mathematics. They have to specialise very narrowly, thus the emphasis on technical perfection as opposite to innovation and on presentation rather than substance of work. Following this path eventually makes it more arduous to think in broader terms, to learn new areas or concepts, to study new groundbreaking theories, to develop in new directions. Associated issues are lack of inventiveness, fear to look too far away or think non-linearly, more widely spread imitation, fear to stand alone in scientific endeavour and the implied need to belong to some group and hence to be too dependent on other people opinions.

A research grant proposal in a narrow technical area can often attract higher level of support from peers working in the same secondary development who are keen to help to sustain it. A research grant proposal in a primary development and, especially a long-term program of fundamental investigations in a new emerging fundamental area, can easily receive short-sighted referee comments and not get funded. Getting research grants support from existing grant providers may come at the cost of undesired intervention into groundbreaking innovative programs of work and may prevent their realisation.

Modern pure mathematics can be approximately compared with a large tree where most of the growth happens at the periphery.[18] Being involved in the growth at the periphery often but not always means to concentrate and reach a high level of technical expertise in a narrow area, knowing little or nothing about what is going on other main branches (and, from recent times, on other neighbouring branches of the tree), i.e. in other areas of mathematics. While there are sources to support short-term and sometimes mid-term research work, long-term fundamental work is generally lacking any substantial support. Larger volumes of intra-disciplinary work and long-term fundamental work in mathematics may broaden links between branches of the tree of mathematics and stimulate its robust growth.

---

[17] `http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTch-discussions-2018-03.html`, also see `https://www.maths.nottingham.ac.uk/plp/pmzibf/rapg.pdf`.

[18] A different view was stated in I.R. Schafarevitsch, Über einige Tendenzen in der Enwicklung der Mathematik, Jahrb. Akad. Wiss. Göttingen, 31–36 (1973): 'so kann man sagen, daß die Entwicklung der Mathematik sich von dem Anwachsen eines Lebewesens unterscheidet, welches nämlich dabei die durch seine Begrenzung geegebene Form bewahrt. Jene Entwicklung ähnelt vielmehr dem Wachstum eines Kristalls oder der Diffusion eines Gases, das sich solange frei ausdehnt, bis ein äeußeres Hindernis entgegentritt'.

Mathematical work can involve the highest freedom of thinking. Working in mathematics is an opportunity to establish new truth independently of anyone's authority or opinion. To produce fundamental achievements, one has to be to a certain degree free from group or community influences, and go one's own way for some years. Many mathematicians are generally susceptible to the influence of herd mentality, these days distributed online by people far away from active leading experts. Being affected by the herd mentality can prevent successful work on genuine advances in mathematics, and too many mathematicians prefer to wait for somebody else to study radically new theories.

It is natural to expect new pioneering fundamental theories which will take a long time for others to study and confirm.The question is how can we help mathematicians to increase their support to pioneers and their breakthrough research and how to raise the level of responsible attitude towards new fundamental theories which may differ so much from previous theories already viewed as conventional? Senior researchers can do more to encourage younger ones to be pioneers or to learn new groundbreaking work and help to develop it further. Members of international research institutes or leading math departments, as well as researchers in the prime of their mathematical age can do more to study new groundbreaking work in their general areas. Young researchers can trust themselves more and in not wait for senior people to tell them what and when to study.

Pure mathematical achievements

IUT hints that other extraordinary novel fundamental mathematical concepts and theories are awaiting to be discovered. To reach to such theories one cannot proceed via incremental and technical improvements of existing results. One cannot proceed to new fundamental discoveries in a linear way. One has to engage in a courageous and highly risky exploration needed for genuine breakthroughs.

Pure mathematics has been indispensable for science and engineering, and for the development of an abundance of technologies. It is remarkable that mathematics for these applications resulted from merely curiosity driven fundamental research, with the actual applications (as well as enormous economic and societal impact) emerging only many years later. However, all those future applications were not original motivations behind important mathematical developments.

It is fascinating which heights pure mathematics research can reach. On the other hand, unlike space exploration or artistic achievements whose significance can be recognised and appreciated by millions, top achievements in mathematics can be comprehended by smaller numbers even if they are made public by talented presenters.

How many stunning revolutionary discoveries are we missing due to lack of appropriate funding of long-term research work? Even though there are still some (rare) fascinating fundamental developments, mathematics and natural science are overall slowing down in terms of the quality of its outputs, despite existing research institutes activities and relatively large existing funding available from state and private sources.

Can we restore genuine interest and enthusiasm, revitalise the mathematical environment to stimulate spectacular mathematical achievements and activities? Can we support and encourage new explorers and pioneers in mathematics? Can we increase the number of researchers able to work for a long time on fundamental problems successfully? — We can, if an appropriate scale of investment and financial support is available and appropriate people are engaged to conduct the required activities in a novel and flexible way compatible with the modern challenges not only inside mathematics but more generally at the societal level in different countries. In the UK, the recent new additional funding of mathematics will address some of these issues.[19]

Acknowledgement. Versions of this text and various related aspects have been discussed with many mathematicians. The author is especially grateful to A. Beilinson, C. Birkar, F. Bogomolov, H. Iwaniec, M. Kapranov, K. Kremnitzer, L. Lafforgue, S. Mochizuki, A. Stasinski.

[19] `https://epsrc.ukri.org/newsevents/news/major-funding-boost-for-mathematical-sciences/`