

REMARKS ON ASPECTS OF MODERN PIONEERING MATHEMATICAL RESEARCH

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Inter-universal Teichmüller (IUT) theory of Shinichi Mochizuki¹ 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.²

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. The problems include the abc conjecture, the Szpiro conjecture for elliptic curves over number fields, the Frey conjecture for elliptic curves over number fields and the Vojta conjecture for hyperbolic curves over number fields, all proved as an application of IUT. But IUT is more than a tool to solve famous conjectures. It is a new fundamental theory that might profoundly influence number theory and mathematics. It restores in number theory the place, role and value of topological groups, as opposite to the use of their linear representations only. IUT is the study of deformation of arithmetic objects by going outside conventional arithmetic geometry, working with their groups of symmetries, using categorical geometry structures and applying deep results of mono-anabelian geometry. It is a highly novel exotic theory with a two-digit number of new concepts.

Since the work on IUT had been supported by RIMS for many years, the IUT papers were submitted to its journal provided that all the standard rules to referee and process submitted papers are strictly followed.³ The referees of the IUT papers worked on the IUT papers since September 2012. The author dedicated 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. Every mathematician had online access to the papers and an opportunity for six years to send questions/comments/suggestions to the author. Their number was several hundreds, and all have been addressed by the author of IUT.

While giving one or several hours talks on IUT does not make much sense, since the theory is so large and with so many new concepts, workshops and seminars have been organised, surveys have been produced and are

Date: July 16 2018.

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*, available from <http://www.kurims.kyoto-u.ac.jp/~mochizuki/papers-english.html>

² Scientists at RIMS are normally not engaged in undergraduate teaching, thus having more time to conduct research and to work with young researchers.

³ This was not the first time when fundamental papers are submitted to journals of their home institutions.

being written, and numerous general talks about the theory have been delivered.⁴ The two international workshops, the Oxford IUT workshop⁵ and the RIMS IUT Summit workshop⁶, were organised to assist researchers worldwide in their study of the theory. These workshops were attended by more than 100 mathematicians. Several of its participants well progressed in their further study of the theory. Interestingly, participants of the IUT workshops included geometers and logicians who contributed new valuable insights.

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 very different from those in the currently most popular generalisation of class field theory, the Langlands program. Hence almost all experienced researchers could not use their previously acquired mathematical intuition and expertise in their specific area, to study IUT. Still, 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 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. Reflecting its pioneering nature, the only expert in IUT in September 2012 was its author. The number of IUT experts, now from six countries, is two-digit, and it will keep growing.⁷ Time dedicated to the 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.

IUT has attracted huge public interest. An article on IUT in *Inference*⁸ had more than 10,000 viewings in the first 4 months. More than 170,000 viewers watched Fumiharu Kato's public lecture on IUT delivered in Tokyo in October 2017⁹.

We should review some general issues affecting modern pioneering mathematical research, in light of IUT, its study and reaction to it. This should also include a discussion of the following negative aspects. Much smaller numbers of mathematicians than expected are known to have applied appropriate efforts to study IUT. Various candidates to study the theory chose to do essentially nothing for six years or to adopt the stance of sceptical attitude not based on expert knowledge of the subject area.¹⁰ Unusually for mathematical developments, some mathematicians felt appropriate to publicly criticise IUT and its study without having applied any serious efforts to learn IUT. In the first approximation, the number of negative reactions to IUT inversely correlated with the number of home academicians capable to study the theory. Tellingly, negative online criticism went always in a very 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. Some of negative posts were apparently coordinated in order to achieve goals having nothing to do with IUT. Certain media, as well as few US bloggers

⁴ see <https://www.maths.nottingham.ac.uk/personal/ibf/guidestoiut.html> for various guidance materials on IUT

⁵ I. Fesenko (ed.), Extended online proceedings of the Oxford workshop on IUT theory of Sh. Mochizuki, available from <https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-sch1.html>

⁶ I. Fesenko, Sh. Mochizuki, Yu. Taguchi (orgs.), Inter-universal Teichmüller Theory Summit 2016, available from <https://www.maths.nottingham.ac.uk/personal/ibf/files/iut-kyoto-pr.html>

⁷ There are at least 8 mathematicians satisfying each of the following conditions: they have studied the IUT theory for two years or more, have communicated to the author their questions, comments and suggestions on previous versions of the papers, have given talks about the theory, have written surveys/papers/books about or related to the theory or are preparing such texts.

⁸ <http://inference-review.com/article/fukugen>

⁹ Its record with English subtitles is available from <https://www.youtube.com/watch?v=fNS7N04DLAQ> and a forthcoming book is to be published soon.

¹⁰ see also Sh. Mochizuki's 2014 report <http://www.kurims.kyoto-u.ac.jp/~motizuki/IUTeich%20Verification%20Report%202014-12.pdf>

void of understanding and working in areas far from number theory or the subject area of IUT, felt suitable to attract attention to themselves by publishing ignorant or absurd articles and posts about the theory and its study.

We must take pioneering research more seriously, try to understand it and try to rumble into our own reactions to it. The key question is 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. This text aims to contribute answers to this key question.

Acknowledgement. Versions of this text and various related aspects have been discussed with more than 100 mathematicians, of different age groups. The author is especially grateful to A. Beilinson, C. Birkar, F. Bogomolov, H. Iwaniec, M. Kapranov, K. Kremnitzer, L. Lafforgue, S. Mochizuki. In June 2018 this text was distributed among members of the executive committee of IMU and the program committee of ICM 2018.

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 some ‘elastic thinking’¹¹. 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. Studying the IUT papers one cannot help marvelling at their interesting features reflecting the substance of underlying mathematics: statements of theorems are often their proofs, and there are numerous discussions aiming to help the reader to get used to its new infrastructure. One reason why some learners of IUT failed to study it can be related to their rushed push through its texts, without mastering its new language first.¹² There is a fundamental aspect of ‘speaking in tongues’¹³ in the IUT, this aspect reflects the nature of the theory and the work on it.

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

¹¹ in the sense of L. Mlodinow’s book ‘Elastic’

¹² see also the text of the footnote 10

¹³ as described at <https://en.wikipedia.org/wiki/Glossolalia>

¹⁴ e-mail communication, March 23 2016

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 stage is not achievable without a solid knowledge of anabelian geometry. Some researchers may need several resolute attempts to proceed with the IUT papers.

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. 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 Robert 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.

Diversity in number theory and associated problems

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 in Western countries than one could have expected. Of course, IUT is an extraordinary novel theory, but still. There are various essential reasons for this reaction to IUT. This text does not aim to list all of them. Some of them are indicated above, some are of number theoretical nature, 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 very 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 very significant part 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. Galois representations or L -functions, or aspects of the Langlands program, used by many number theorists, are very far away from key substantial methods of IUT.

The Japanese tradition of highly original pioneering research in mathematics originated from Teiji Takagi, a 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. The main prerequisite for IUT is anabelian geometry, one of key generalisations of class field theory. The number of experts in anabelian geometry was probably lower than 10 in 2012. Among hundreds of researchers working in the Langlands

program¹⁵ or in Diophantine geometry, experts in anabelian geometry in 2012 could be counted on the fingers of one hand. One possible way to connect the Langlands program with anabelian geometry and IUT is by going back to class field theory and then to higher class field theory and higher adelic analysis and geometry, but this requires a good knowledge of class field theory.

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

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. With respect to the study of IUT, this second exam is obviously failed in several countries. Moreover, rather unusually for pure mathematics, we have witnessed cases of unethical and unprofessional behaviour with respect to IUT. In the first approximation, one could say that the number of such instances was somehow inversely proportional to the level of expertise in the subject area.

The ethical responsibility of mathematicians includes a certain duty, never precisely stated in any formal way, but of course 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, and this duty is not too cumbersome. For example, in number theory of the 21st century such theory is IUT. This duty is especially applicable to researchers who are in the most active research period of their mathematical life and have already senior academic positions. In real life this informal duty can be taken to mean that a reasonable number of mathematicians in each major mathematical country studies such groundbreaking theories.

It was clear that to become an expert in IUT one has to study seriously and for a long time. 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 body of its surveys.

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. It should be stated clearly that 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 unethical.

One can also 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 on one’s own imagined incorrect picture, due to rushed study, of what the theory is about or it is influenced by herd mentality. Many number theorists ‘believe’ in the Deligne proof of GRH, but very few have thoroughly 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.

When one does not hunker down to study the area of a fundamentally new theory, one does not become an expert in it, even though it is still possible to contribute useful questions/comments/remarks in relation to more

¹⁵ see diagrams of I. Fesenko, Class field theory guidance and three fundamental developments in arithmetic of elliptic curves, available from <https://www.maths.nottingham.ac.uk/personal/ibf/232.pdf>

¹⁶ see the text cited in footnote 15

conventional parts of the theory. In this case it is irresponsible to make one's general negative opinion about the theory public, since such an opinion is not based on mathematical knowledge.

In 2013–2017 no concrete mathematical remarks originated from any mathematician who made negative public remarks about IUT. Several researchers, who could have become potential learners of IUT and then progressed to become experts, declined invitations to participate in the IUT workshops. Some, affected by negative emotions, broke professional rules of conduct and made public their ignorant and sometimes even intolerant opinions. Tellingly, the only questions were communicated only after several years of requests to do so and they were disappointingly shallow and quite misplaced.¹⁷¹⁸

Some of those negative public remarks may have misled some mathematicians who could not distinguish an expert in or a serious learner of the subject area from a non-expert. It should be further analysed why has it become acceptable for some mathematicians to produce public negative statements about the work of other mathematicians without any concrete mathematical justification and what fundamental harm to pioneering research this unethical behaviour makes. Several journalists, in 'their mad rush for a scoop'¹⁹, as well as some US bloggers, none working anywhere close to the subject area of IUT, eagerly cited ignorant opinions or contributed to spreading false rumours and disinformation.

It is natural to expect new pioneering fundamental theories which will take a long time for others to study and confirm.²⁰ 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 not to wait for senior people to tell them what and when to study.

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. People are losing the enthusiasm and passion for long-term research and exhibit most pragmatic attitude to what and when to study in mathematics. They specialise quite narrowly, which leads to 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

¹⁷ Two July 2018 texts by Sh. Mochizuki, 'Report on discussions, held during the period March 15–20, 2018, concerning inter-universal Teichmüller theory (IUTch)' and 'Comments on the manuscript by Scholze–Stix concerning inter-universal Teichmüller theory (IUTch)' are ready to be made public. These texts and a text of Scholze–Stix were originally supposed to be made public at the beginning of July.

¹⁸ Compare with physics: 'In my years in science, I have heard many colleagues complain that the experts who referee papers sometimes approach them from a fixed point of view and proceed to misunderstand what they read because they approach the material hurriedly, thinking that they already know what the authors are trying to say.' – L. Mlodinow, p. 159 of the text in footnote 11

¹⁹ Message of Pope Francis for world communications day, 24 January 2018, http://w2.vatican.va/content/francesco/en/messages/communications/documents/papa-francesco_20180124_messaggio-comunicazioni-sociali.html.

²⁰ answering the question of Yu. Milner at 2015 Breakthrough Prize Symposium, 26:00- of <https://www.youtube.com/watch?v=eNgUQ1pc1m0>

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.

An opinion of Robert Langlands on current trends in supporting long-term fundamental research work can be heard during the 52nd minute of his video lecture at one of our conferences.²¹

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. Being affected by the herd mentality may prevent successful work on genuine advances in mathematics. In particular, mathematicians prefer to wait for somebody else to study radically new theories. An editorial article in Asahi Shimbun²² emphasises that 'new discoveries and academic theories that never existed in earlier studies always appear on stage in the form of a minority view'.

Pure mathematical achievements

The emergence of 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.

Mathematics is more than 5000 years old. It is a key feature of the civilised world. Without mathematical achievement, the technology revolutions in software, internet and AI would have been impossible. Mathematical mindfulness plays increasingly more important role in fundamental aspects of modern society. Mathematical achievements are among the longest to be kept in the memory of civilisations. While the range of activities where humans are better than AI continues to narrow, pure mathematics is most likely to be one of the last to be completely passed over to AI.

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.

²¹ R. Langlands, Problems in the theory of automorphic forms: 45 years later, video lectures at Nottingham–Oxford conference on symmetries and correspondences, July 2014, <https://www.maths.nottingham.ac.uk/personal/ibf/files/S&C-schedule.html>

²² <http://www.asahi.com/ajw/articles/AJ201705290021.html>

How can we help

The UK invested on average \$1M per year in each of its Rio Olympic medals. This investment has dramatically improved its rating in the count of Olympic medals in the last 20 years. Now several other countries are keen to follow. However, this is a short term investment, unlike fundamental mathematics. How many new great mathematicians are in need of support of their long-term fundamental research, support not as grandiose as the support of the olympic sport folks? 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.²³

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? — Yes, we can, even though no existing research institute has apparently ever applied efforts to do that directly. 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.

It is time to discuss the current state of mathematics and its future on a larger scale. It is time to start to properly support modern long-term fundamental research in mathematics. Other areas of fundamental science experience somehow similar problems with the support of long-term fundamental research. Nobel Prize winner Yoshinori Ohsumi used his prize money to establish a foundation to promote fundamental biological research with long-term goals.²⁴ The good news for mathematics is that no vast funding is required to substantially change the situation and to improve the future of mathematics research. Pure mathematics research does not need laboratories and equipment and the cost of supporting a pure mathematics institute is the minimal one in comparison to all other areas of natural sciences and technology.

One solution could be a new international mathematical institute which can inspire, flexibly support, disseminate and promote long-term fundamental work of its fellows on new groundbreaking achievements. The new institute will have the task of carefully and diligently searching for most promising researchers, select candidates and support and encourage them in several aspects of their work. While some reasonable financial support of highly pioneering work is important, social, cultural and psychological support of these researchers during their long-term work is crucial. The institute can systematically stimulate long-term work in key directions and on key problems, help to overcome communication and other barriers, rapidly respond to innovative fundamental theories and proofs by organising groups of mathematicians to study them, help larger groups of mathematicians to cope with the challenges of fundamental novelty in its fellows' work and not to go down the wrong path of intolerant or hostile reactions towards them, as well as conduct high quality public presentations and engagements activities for the general public. The fellows can work in one of national branches of the international institute and have the option to move from one branch to another, to individually choose the most stimulating environment conditions for their long-term pioneering endeavour.

²³ 'Current funding models are broken and favor political skill over scientific genius', S. Altman, <http://blog.samaltman.com/>, <https://blog.ycombinator.com/yc-research/>

²⁴ <http://www.asahi.com/ajw/articles/AJ201709130039.html>