MOTIVATED MATHEMATICS

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September 1983
WP-83-84

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FOREWORD

Mathematics seems to be treated at IIASA as if it were some sort of shameful disease, carefully hidden away in departments labelled with trendy but unrevealing names. Any debate about the role of mathematics and mathematicians at IIASA appears fated to be confused by the introduction of many seemingly unrelated issues, by the fact that many of the protagonists have their own vested interests to protect, and by the fact – why not admit it – that there are many dark fears of the more "esoteric" side of mathematical activity.

This paper is an attempt to clarify some issues regarding the role of mathematics. It is not a plea to increase the proportion of IIASA's budget (8%) allocated to mathematical research; it is rather an analysis (which attempts to be as honest as possible) of the uses, abuses and limitations of mathematical metaphors, particularly in the so-called soft sciences.

If there is a plea, however, it is to support the development of *motivated mathematics*. This is one activity in which IIASA could truly claim to be a pioneer, since it is an aspect of mathematical development that has been sadly neglected over the past decades.

It is time for mathematicians to come out into the open!

You have undoubtedly been asked over and over again what purpose mathematics serves, and whether these delicate structures that we summon forth fully fledged from our minds are not artificial and born of caprice. I must define a special category among the people who ask this question: those practical people who demand of us only the means of making money. They are not worthy of an answer.

Henri Poincaré
(La valeur de la science, Chapter 5)

MOTIVATED MATHEMATICS

The term *applied mathematics* often taken as the opposite of *pure mathematics* is misleading, for it encourages the belief that there is just a simple distinction between the fundamental development of mathematical techniques and their use in solving problems encountered in other scientific fields. Indeed, these expressions actually conceal one of the most important elements in the progress of mathematics: the *motivation* that mathematicians can derive from the study of other sciences. Only those who choose to forget the lessons taught by the history of science could deny the fact that man's strong urge to explore his environment has had a consistently beneficial influence on the progress of mathematics, an influence much greater than that exerted by the eventual applicability or short-range usefulness of the resulting theories or techniques. A simple inspection of the facts shows that without this natural curiosity there would have been neither technical advances nor technological progress. However, what we cannot do is to predict which areas of mathematics will turn out to have practical applications, or which path future generations will choose to pick through this convoluted labyrinth of theories on which so many scientists have laboured for so long.

It is in an attempt to shed some new light on this old debate that I propose the term *motivated mathematics*; and new light is something that is urgently needed at a time when excessive formalization of mathematics on the one hand and the huge amounts of money now associated with possible applications on the other makes the debate more obscure than ever.

Like other means of communication (languages, painting, music, etc.), mathematics provides *metaphors* that can be used to explain a given phenomenon by associating it with some other phenomenon that is more familiar, or at least is felt to be more familiar. This feeling of familiarity, individual or collective, inborn or acquired, is responsible for the inner conviction that this phenomenon is understood.

In the last analysis we come down to man's desire to "explain reality". We are brains which *perceive* and *interpret* the environment and which *communicate* these interpretations by a variety of means*. This leads to a definition of a *degree of reality* (for a given social group at a given time) in terms of the consensus interpretations of the group members' perceptions of their physical, biological, social and cultural environment.

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*Contrary to what has often been said, mathematics is not just another language, however much richer or more precise. Mathematical reasoning is one of the faculties shared by all human beings, in the same way that the ability to speak and write, to listen to or compose music, to look at or paint pictures, to believe in and obey cultural and moral codes, is common to all. These faculties are not shared equally and can be developed to a greater or lesser degree by different individuals. It is clearly impossible to arrange these different faculties in any sort of hierarchy, and least of all to claim the superiority of mathematical reasoning!

Since our brains are built according to the same biological blueprint, and since the general acceptance of local cultural codes seems to be an innate and universal phenomenon, it is highly probable that the individuals comprising a social group arrive at a consensus wide enough for a reasonably believable concept of reality to emerge. However, the prophets and scholars of each group continually question the validity of the metaphors on which this consensus is based, while the high priests and other guardians of ideological purity ultimately try to transform it into dogma and impose it on the other members of the group*. It is through this permanent struggle that knowledge evolves. But there is an important difference between the metaphors of science and those of, say, religion or ideology: a metaphor that claims scientific validity must be limited, even narrow, in scope. The more "applied" a scientific study, the narrower it must necessarily be.

Scientific theories - scientific metaphors - must be capable of logical refutation (as in mathematics) or of experimental falsification (which of course requires that theories be falsifiable). Ideologies escape these requirements: the "broader" they are, the more seductive they appear.

Nevertheless, there have been many cases where scientific metaphors have been extended beyond their natural limits: the misuse of catastrophe theory, information theory and thermodynamics by "broad thinkers" to "explain" (by playing with words) phenomena lying outside their original terms of reference provides an obvious contemporary example.

The construction of mathematical metaphors naturally requires independent development in the field which provides the theories eventually to be linked with observed phenomena: this is the domain of pure mathematics. The development of the art of mathematics follows its own logic, as do literature, music, painting, etc. In all of these areas, *aesthetic satisfaction* is both an aim to be achieved and a signal by which successful work can be recogised. In all of these domains, too, *fashion* - or social consensus - influences the aesthetic criteria by which the work is judged. How fashion itself is created and evolves is a question that still remains to be answered.

We have already described a mathematical metaphor as a means of associating a particular mathematical theory with a certain observed phenomenon. This association can arise in two different ways. The first possibility is to look for an existing mathematical theory which seems to provide a good explanation of the phenomenon under consideration. This is usually regarded as the domain of applied mathematics.

However, it is also possible to approach the problem from the opposite direction. Other fields provide mathematicians with metaphors, by suggesting new concepts and lines of argument, by giving some inkling of possible solutions, or by developing new modes of intuition: and this is the domain of what can be called "motivated mathematics".

The history of mathematics is full of instances in which mathematical techniques motivated by problems encountered in one scientific field have found applications in many others. *It is this "universality" which renders mathematics so fascinating.*

*It often happens that the prophets and scholars themselves eventually become high priests; movement in the reverse direction is much less common.

A mathematician's job should not be seen simply as providing formulas and models, together with more or less sophisticated mathematical programming software packages. An institution which confines most of its mathematicians to this type of activity will very quickly find that it is getting the opposite of what it intended: mediocre work which is very soon superceded.

It is impossible to draw precise boundaries between these three main types of mathematical activity, so numerous are the interactions between them. The distinction is more concerned wth the intellectual and creative behaviour of individual mathematicians than with the nature of the various problems, which should be tackled at every level, using every available approach. The most that can be said is that a mathematician collaborating with engineers, managers or physicians is probably more "applied" than a colleague working with physicists, economists or biologists: the latter could be seen as more of a "motivated" mathematician. It should also be emphasised that this classification is by no means absolute: it will always be possible for any individual mathematician to be regarded as "too applied" by some of his colleagues and as "too pure" by others*. In any case, who can ultimately say whether a given piece of work is applied or not, or if a particular mathematical field will eventually turn out to be useful? It should be recalled that Gauss, now recognised as one of the greatest mathematicians that ever lived, actually spent most of his working life in tedious computations of the trajectories of the planets ("useful research"); he probably "relaxed" by looking at some seemingly "useless" problems in arithmetics, which eventually made him famous, and which, by a quirk of fate, has since found important applications in sophisticated software technology!

At the root of the confusion we can detect the workings of the laws of psychology and sociology (to which mathematicians, as mere human beings, are still subject), which organise these different intellectual approaches into an implicit (or more often, alas, an explicit) hierarchy: pure mathematics is good, applied mathematics is bad, and motivated mathematics is generally ignored. This classification is not only absurd, it is also dangerous because it is self-perpetuating, new talent tending to distribute itself according to this perceived hierarchy.

One reason why motivated mathematics is often ignored may be found in the fact that the work of motivated mathematicians involves a lot of risk, especially when their problems are derived from "soft" disciplines such as the social sciences, or, to a lesser degree, the biological sciences. Very many hours of deep thought can lead to mathematical trivialities, or to problems that cannot be solved in the short term – the same effort applied to a well-structured problem in pure or applied mathematics would normally yield some visible results.

We often find well-meaning pure mathematicians asking specialists in other fields to present their problems to a mathematical audience. This is generally impossible, however, since these specialists would require some *a priori* knowledge of the appropriate mathematical techniques simply to *state* their problems.

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*Criticism of work as "too abstract" generally comes from people who either despise pure mathematics *per se* or who fear the difficulty of this type of work and wish to conceal it.

This type of work requires *motivated mathematicians*, scientists who are both familiar with the "other" discipline and have access to a sizable arsenal of mathematical techniques as well as the ability to extend this arsenal. They must, throughout a difficult and frustrating dialogue, keep a check on whether the problem at hand can be solved by existing mathematical techniques, and, if not, try to restructure the problem - this may lead them to apparently forget the original problem - and to construct an *ad hoc* theory that they feel intuitively will be of use (much) later in the process. They must convince their colleagues that a very long "learning time" is required "just" to grasp the language of a mathematical theory (its "abstract" side) and to derive the basic results; that to prove even the simplest, the most naive or the most attractive of statements may require new mathematical techniques which take years and many books to develop; and that understanding of a mathematical theory is not a static process - it is something that is growing all the time. At a time when we cannot afford to spend a century building a sky-scraper, as they did with cathedrals in the Middle Ages, it is easy to see why vocations (and positions) for motivated mathematicians are so scarce!

It could be said that one of the main differences between mathematicians and other scientists is that in some senses their work is governed by different time constants. Not surprisingly, the slowness and esoteric nature of mathematical work can very soon exhaust the patience of those collaborators (or employers) who wish for quick and concrete answers to their problems. This is aggravated by the fact that potential users are not always convinced of the relevance of mathematics to their particular problem; even if they are, their interest is usually limited to what mathematics can achieve in the way of *immediate impacts*. They do not seem to realise that results are inextricably linked with the mathematical construction as a whole, and that it is not possible to have isolated "great leaps forward" in mathematics as in other domains of human activity. The artificial division between pure and applied work introduced as a result of the huge increase in the number of mathematicians after World War II only emphasised that no long-term progress can be made in an "applied group" cut off from the mainstream of mathematical development, and that isolation in such an "ecological niche" condemns the group to ossification and decay.

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Having demonstrated the need for motivated mathematics, we now have to consider how such work could best be encouraged and developed.

The first possibility is to encourage individual mathematicians to make use of all three approaches in their work, i.e., to look for new problems in different fields (motivated mathematics), to develop new mathematical techniques (pure mathematics) and to use these techniques to find explicit or numerical solutions to "concrete" problems (applied mathematics). However, it is no longer easy to find individual mathematicians capable of such "eclecticism" - Von Neumann may well have been the last. The *competition* among the growing number of mathematicians and the recent emphasis on relatively short-term *productivity* has naturally led to increased specialization. It is becoming more and more difficult to take one's time and keep one's perspective. Nevertheless, it should still be possible to build up small teams of between three and ten mathematicians who between them could cover the three aspects of mathematics described above *without putting them into any sort of hierarchy.*

One should not underestimate the psychological difficulties of this type of team work, which requires mutual compatability, individual unpretentiousness and the repression of the paranoid tendencies to which scientists owe their eternal youth!

Another possibility would be to exploit an important feature of the collective psychology of the scientific community, namely to encourage well-known "pure" mathematicians to shift their interests towards motivated (or applied) mathematics, and to use their prestige to attract talented young mathematicians to this innovative type of research. This approach (if it worked) would also have the advantage of bringing some (friendly) pressure to bear on mathematicians to look outside for their sources of inspiration.

Thus, mathematicians must look to the managers of research establishments and scientific programs to devise some institutional means of bringing together mathematicians and scientists with different interests and skills; this should be done not according to some rigid procedural rule, but rather with a touch of anarchy. This is necessary to widen the network of information exchange in which scientific discoveries are mysteriously conceived. "Ideas" seem to shun open places, short-cuts and straight lines; they thrive underground in dark and complex mazes, appearing briefly (and all too rarely) when least expected.

What is needed from the scientific management is regular evaluation of research, and quick dissemination of results, not continual research "planning", which would require impossible predictions of when and where the next breakthrough will occur. It should be realised that a community of mathematicians (which requires investment only in computing equipment and every means of communication) cannot be treated in the same way as an administrative or industrial group, and indeed it would be dangerous to do so.

The plea for more motivated mathematics goes hand-in-hand with the need for increased emphasis on the history of mathematics. It is sometimes of the utmost importance to trace the meandering flow of ideas back to its source, to see where it branches, and to identify (possible) dead ends. Many delays and "mistakes" have been caused simply through ignorance of long-discarded "ideas". A mathematical "idea" is not a perfectly fashioned artifact which can be contemplated like a painting in a gallery. It is a living and evolving mythological monster, a hydra with hundreds of heads and as many tails. And, as the blind men discovered with the elephant, familiarity with one or even several heads is not sufficient to identify the beast; it is also necessary to study the tails, and everything that lies in between.

There is a tacit conspiracy among mathematicians to hide these monsters from the public, allowing them only partial glimpses of their nicer parts; to look for proofs which are as rigorous and direct as possible, concealing the real motivation and genesis of the work; and to share the true background only with friends in the intimacy of one of the close encounters (of a mathematical kind) so essential to scientific progress. The result of all this concealment is that the actual evolution of ideas is lost, and can be brought to light again only through research.

There may be some connection here with the paradox which states that abstract knowledge is more transmissable than concrete knowledge (even though the former is more inaccessible and harder to acquire), because it can be shared by more persons; concrete knowledge may be regarded in some senses as unique and therefore of interest to only a limited group of people. The breadth of the "mathematical market" for which a given piece of work is destined may therefore be seen as determining its degree of abstraction. This touches upon some sensitive issues related to the didactics of mathematics, where the trend is to teach purer and purer mathematics, because of its universal character, with the underlying assumption that there will be time enough to apply it in the future, time which more often than not never comes...

In addition, the requirement that mathematicians be "productive" leads to the teaching of the most "direct" and "simple" proofs of results which were originally obtained in a way that the students are not yet capable of understanding. All that this type of presentation succeeds in doing is to destroy any ability to learn through "play" and any intuitive (as distinct from logical) faculty the students may originally have had. Perhaps the teaching of a given theory should reflect its historical evolution, not lingering too much over any of the individual stages, but not ignoring them either. Only after the historical background has been presented should more recently discovered short-cuts be introduced.

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The history of science is full of examples of the use of mathematical metaphors in physics and mechanics: consider, for example, the concept of the derivative of a function introduced by Fermat, Leibnitz and Newton in the 17th century. It should be noted in passing that if Fermat and Leibnitz were principally motivated by their mathematical interests (the search for optimum points and the first variational principle), Newton based his intuition on mechanics. Some time later, the Bernouilli brothers began to study the problems which lie at the heart of field known as the "calculus of variations"; this subject was later taken up by such eminent mathematicians as Euler (18th century), Lagrange and Jacobi (19th century), and, more recently, Poincaré and Hilbert. The calculus of variations - which is connected to the theory of partial differential equations through Euler-Lagrange equations - has provided physics with many of its fundamental models. The solution of these models was taken as a challenge by mathematicians, forcing them to look once again at what was meant by a *derivative*. The rigorous definition given by Cauchy was obviously fundamental, but, by freezing it, fossilizing it in this form, mathematicians essentially cut themselves off from the means to solve many of their problems. It must have taken a lot of audacity for Laurent Schwartz to introduce the concept of *distributions*, which are more general than ordinary functions and can be "differentiated" indefinitely. What was preserved was the "idea" of differentiation; what was changed was its formal definition. It then became possible to solve many partial differential equations by finding solutions among distributions rather than among functions. But this was not achieved overnight. Dirac, a physicist, had already proposed some "formal" results and Leray and Sobolev had had a feeling that something of this sort was needed. But even these advances were not enough: the calculus of variations, and one of its contemporary branches, optimal control theory, continually pushes mathematicians into developing new definitions of derivatives (such as subdifferentials of convex functions and other generalized gradients) in an attempt to give some meaning to the old Fermat rule* for increasingly complex problems. These techniques, derived from the old calculus of variations, are now used in many economic models, as far as they are relevant...

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Physics and mechanics are not the only scientific fields that have provided mathematicians with some motivation. Economists have also looked for strong mathematical metaphors to help them to understand the workings of complex economic systems. Two centuries ago, Adam Smith made the bold and counter-intuitive suggestion that it was possible to solve the problem of decentralized allocation of scarce resources among economic agents, ignoring the state of the market and the decisions of other individual agents. For want of a better explanation of how this was possible (and no good explanation has yet been forthcoming), he poetically invoked an *invisible hand*. We then had to wait a century for Léon Walras, a former engineer, to propose that this invisible hand "operates" on economic agents through prices, gaining enough information to guarantee that their decisions will be consistent and therefore that the scarcity constraints will be satisfied. The concept of *economic equilibrium* that we owe to him is not his only claim to our gratitude: Léon Walras was the first person to suggest that mathematics could be useful in economic theory. Originality is often more a question of finding a new way of looking at the world than of making discoveries that attract the attention of one's peers. Walras introduced mathematical rigour into a domain which had never before been subjected to detailed analysis. He did it with disregard for - even in opposition to - the prevailing economic thinking of the times, despite tremendous difficulties, alone and without help, without the encouragement and moral support of his colleagues. He did it because, deep within him, he realised the far-reaching consequences of his bold vision.

Walras proposed to define an economic equilibrium as a solution of a system of nonlinear equations. At that time, when only linear systems were understood, the fact that the number of equations was equal to the number of unknowns led him and his followers to make the optimistic assumption that a solution should necessarily exist.

*This states that the derivative of a function vanishes at any point which minimizes the function.

But it took another century, until 1954, for Arrow and Debreu to find a mathematical solution to this problem. This solution, however, could not have been obtained without the fundamental fixed point theorem discovered in 1910 by Brouwer, which in turn required much modification to tailor it to this specific case - in particular the development of certain theorems whose assumptions could bear the same degree of economic interpretation as the conclusions. Recent advances now allow us to construct elegant and simple (elegant because simple) proofs of these results.

Since then, many other mathematical techniques (differential geometry, non-standard analysis,...) have been proposed for use in economic theory and tested in various ways - this is still going on. For instance, the static character of the "general equilibrium model" is clearly inconsistent with observed economic behaviour, which shows that prices actually vary. Smart - but superficial - minds have tried to use these shortcomings to claim that any decentralized mechanism using prices as a basis for allocating scarce resources among economic agents is merely a fantasy dreamed up by scientists from their ivory towers, and even to reject the relevance of mathematical metaphors in economics.

This is a typical instance of impatience and the totalitarian desire for a monist explanation. It is in fact now possible to construct another mathematical metaphor, valid in an evolutionary context, in which prices are permitted to change.

Mathematicians are a long way from having said all that there is to say about this problem. They have actually said very little, especially in view of the massive efforts that they have made in this area. However, they know how modest and patient they must be, how frustrating it is to attain such a narrow and limited understanding after so much work, and how much still remains to be done...

Nevertheless, at each stage of this long process, the simply formulated questions of economic theory have spawned many new problems of interest to mathematicians, and have proved to be the cradle of several new mathematical theories (convex analysis, non-smooth analysis, non-linear analysis, etc.). One of the crucial steps in this field was taken by J. Von Neumann. The fact that this eminent mathematician was interested in these economic questions lent them some credibility and attracted other well-known mathematicians to this area. The efforts put into proving the resulting theorems were not wasted, however, for they eventually turned out to be useful not only in solving economic problems, but also in many other fields.

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The mathematical techniques motivated by, and used in, economics, the social sciences and several areas of biology are now undergoing a fundamental change. A careful and thorough investigation of the *static* framework was the mandatory first step in these as in many other fields, despite its obvious shortcomings. The tools necessary for this work - mathematical programming and equilibrium theory - have now been developed and are in common use, although they can obviously still be improved and modified for specific problems.

However, the systematic use of static models - and in particular mathematical programming models - in the analysis of macrosystems, though quite reasonable when no alternative techniques were available, can no longer be justified. Even the concept of evolution borrowed from Newtonian mechanics has severe limitations. It has led to the misleading identification of mathematics with a *deterministic* paradigm, which implies that *the evolution of macrosystems can be predicted.* Even if we were to accept the existence of deterministic mechanisms underlying the evolution of biological, economic and social macrosystems, we know that such systems can be inherently unstable - and this places the actual computation of their trajectories beyond the capabilities of even the most sophisticated of present-day computers. To "run" models which have some inbuilt structural instability can serve no useful purpose.

The applicability of such mathematical techniques to systems analysis and the resulting models, if not to be questioned on purely logical grounds, should at least be reassessed in the light of our experience in the study of mechanical systems. Newton needed Kepler, and Poincaré had to devise *qualitative analysis* to provide us with consistent models for the motion of the planets.... and now we discover that some of our "perfectly deterministic" models can exhibit all sorts of different trajectories. These are "chaotic" systems, systems in which the state space is divided into a number of cells, each of which can be "visited" in any given way by at least one trajectory, making prediction virtually impossible. At the same time, the search for mathematical metaphors in such fields as mechanics (turbulence), physics, meteorology, economics and biology is yielding new concepts such as "chaos" and "strange attractors".

It is now clear that the study of macrosystems requires a non-deterministic approach which takes into account:

- our ignorance of the future environment of the system (or the impossibility of duplicating experiments)
- the absence of determinism (including the impossibility of a comprehensive description of the dynamics of the system)
- our ignorance of the laws relating the various controls to the states of the system
- the variety of dynamics available to the system
- the lack of well-defined decision makers acting as controllers
- the continuous flow of decisions and adaptive system responses

It is now even possible to find mathematical metaphors for what is known as "Darwinian" evolution, to provide a mathematical interpretation of the quotation attributed to Democritus: "Everything that exists in the Universe is due to chance and necessity". Evolution of this type can be described by means of "differential inclusions" (with or without memory); these are dynamical systems such that, at each instant, the velocity depends in a multivalued way upon the state (or history) of the system, and possibly also upon various regulatory controls. Such systems also consume scarce resources and/or conserve them for future use.

A recent approach called *viability theory* proposes a class of selection methods in which we choose only the trajectories that, at each instant, obey given restrictions known as *viability constraints*. These constraints determine a region of state space, called the *viability domain*; *viable trajectories* are those lying entirely within the viability domain. The viability domain can depend on time, the present state or history of the system, the regulatory controls, and so on.

This approach makes explicit the necessary and sufficient conditions for the existence of at least one viable trajectory starting from any viable initial state. It also provides the *feedbacks* (concealed in both the dynamics and the viability constraints) which relate the state of the system to the controls. These feedbacks are not necessarily deterministic: they are set-valued maps associating a *subset* of controls with each state of the system. We observe that the larger these subsets of controls are, the more flexible - and thus the more robust - the regulation of the system will be.

Systems of this type may also exhibit "heavy trajectories", which are associated with controls which remain constant for as long as possible, and which evolve only with minimal velocity once they are forced to do so.

It is perhaps worth touching on another aspect of viability theory - that concerned with complexity and robustness. According to the theory, the state of the system becomes increasingly robust the further it is from the boundary of the viability domain. Therefore, after some time has elapsed, only the parts of the trajectories furthest away from the viability boundary will remain. This fact may explain the apparent discontinuities ("missing links", "punctuated equilibria") and hierarchical organization observed in certain evolving systems.

In summary, the *main purpose of viability theory is to explain the evolution of a system, given feasible dynamics and constraints, and to reveal the concealed feedbacks which allow it to be regulated.* This involves the use of a policy, *opportunism*, which enables the system to conserve viable trajectories that its *lack of determinism* - the availability of several feasible velocities - makes possible.

For the time being at least, this type of theory lies within the domain of motivated mathematics: and it still may not provide an ideal description of the evolution of macrosystems. Some potential users (economists, biologists, etc.) may have been disappointed or discouraged by the results obtained so far - this is not surprising as it is still too early for such a theory to be "applied" in the engineering sense. However, whatever the ultimate outcome, the motivation provided by the study of macrosystems will have benefited mathematics by reviving and enriching the theory of dynamical systems and differential equations.

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FOREWORD

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It is time for mathematicians to come out into the open!