

# NEW TECHNOLOGIES ON MATHEMATICAL RESEARCH

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## 1 INTRODUCTION BY RAFAEL DE LA LLAVE

Presentation of the Round Table by the Moderator.

### 1.1 Introduction

The needs of technology affect the direction of research in Mathematics. Even if this effect is smaller than the effect technology has in other sciences, it is nevertheless important.

Of course, Mathematics has some internal coherence that allows it to raise its own problems and work them out without any reference to the external world. Among the sciences, Mathematics is the only one with such ability – no need to experiment.

Nevertheless, the fact that this isolation is possible, does not mean that it happens very often. Much less, it tells us that it is desirable or healthy.

These are the questions we would like to discuss in this Round Table. Needless

to say, the question is so deep that we can only hope to raise the questions for the audience and to call attention to some (well known) examples which can be relevant for an answer.

## 1.2 The role of technology in research

There are several reasons why technology affects research in Mathematics.

- **The needs of technology suggest new questions to mathematical researchers.**

Even if Mathematics can raise its own questions to a larger extent than other sciences, the fact is that many of the deep mathematical disciplines that are later pursued by their own internal beauty, were originally developed to address questions of practical importance that were suggested by external reasons.

Suffice it to mention the example of geometry. It started (presumably) as a practical tool for surveyors. Nevertheless, once its internal beauty was discovered – it led to the axiomatic method – it started to develop on its own. After years of development, it developed new branches, some of which were developed because of their own internal beauty – which later found applications – but others were suggested by applications. For example, differential geometry was started by the illustrious surveyor Gauss to serve surveying needs. Others such as Riemannian geometry, later found unexpected applications in relativity.

We could also mention many other fields of Mathematics. Certainly Calculus and Partial Differential Equations, Fourier Analysis.

Of course, there are fields that do not seem to have started motivated by technology. The most notorious is Number Theory, which even in this century, Hardy – an illustrious practitioner – claimed it was completely useless. Closer to my specialty, I remember reading in the books of Thue, which started symbolic dynamics, gave as motivation only "to think about a hard problem".

Even if I have only used historical examples, it seems clear that the same is happening nowadays. There are many problems coming from technology that give rise to quite interesting and deep mathematical problems. For example, numerical analysis which studies the ranges of validity of numerical methods commonly employed has received a great impetus.

The theory of algorithms is a new and exciting mathematical discipline, stochastic modeling, linear programming, operation research, game theory are all disciplines that appeared due to very practical needs very recently. I see cryptology, signal and image processing, tomography, emerging as important sources of problems (the list should be longer) and it is clear that there are others in the wings.

It is humbling to realize that we still do not have decent mathematical understanding of things such as the working of the jet engines in rarefied atmosphere (in spite of the fact many of us climbed in one without thinking to come to this conference) or of the traffic in an Ethernet or the economy of the information.

Of course, if besides serving technology, we added serving to other sciences (notably Physics) the list of recent fields would be even longer including for example such beautiful theories as rigorous quantum field theory and statistical mechanics, transport theory.

We should, however not believe that the finding of good problems is easy or that going to the service of technology would make us better mathematicians: Surveying in the 18 century raised many problems, some of which were quite dull – e.g. tricks on how to multiply certain numbers with some particular patterns of digits faster – others were moderately interesting – e.g. finding ways of compiling tables more precisely –.

It took Gauss to find two first class problems in the field of surveying: The basis of intrinsic differential geometry mentioned above and the technique of least squares – based on the central limit theorem – to reduce errors which, of course, lead to mathematical statistics.

Of course, Gauss, had found great mathematics in the problem of saving the pension fund of Prussia. It is interesting to remark that Gauss got his spurs working in the – then useless – field of number theory or in issues such as constructions with ruler and compass. I think that Gauss, alternating between number theory, ruler and compass, surveying, electromagnetism, etc. is a perfect example of the unity of the pure and applied mathematics.

One could find more examples – perhaps not united in the same person – of very disparate fields (pure and applied) that could not have been developed one without the other or of very pure fields that arose from practical problems that were considered dull.

I would also like to mention that usefulness is not always related to understanding and that not all the things that we want to do are useful. Sometimes understanding and beauty are goals for their own sake. These deep questions, are however, not on the agenda of this round table.

- **Serving the needs of other fields prevents lowering of standards.**

Mathematicians working on their own are often tempted to just consider the generic case, where a systematic theory is easier to obtain or is more complete.

When the problems come from the real world one is not allowed to lower the standards in this way. Sometimes one is forced to go for the case that appears which requires new mathematics.

Let me give you a few examples from PDE's. (I could have also mentioned the construction of non-trivial quantum field theories or many other examples) As it turns out, many of the equations from mathematical physics happen precisely at the critical values of the Sobolev embedding theorem. It was very tempting to just do the sub-critical and the super-critical case. Nevertheless, working out the critical case of some equations has led to beautiful results such as Donaldson theory. Pseudo-differential calculus was originally developed for functions with smooth coefficients. Many of the applications in Physics – e.g. quantum mechanics with

Coulomb potentials – required the extension of these techniques to very singular coefficients.

I think that, unless these problems had appeared in applications anybody would have had the stamina or the vision to embark in their study and the big payoff that appeared when they were finally analyzed would have been lost.

In summary, the external world forces us to study these critical cases, which often lead to very important discoveries.

• **Technology gives the means of advancing certain areas of research.**

The most obvious example is the computer, which has made it possible to discover new phenomena or even helped with the proof of results.

This is certainly so well known that it would be boring to repeat it.

I would, however like to emphasize that the aspect of research in Mathematics that has benefited more from the computer is the experimentation. One can generate conjectures and check them. This aspect, even if it was quite important to make the research progress and was used by all the researchers, was for some reasons not well documented and not subject to scrutiny and to standards.

An experimental verification of a result does not slow the search for a proof – on the contrary, the added confidence provides an stimulus –.

Indeed, the area in which I work most – dynamical systems – has benefited enormously from this experimental approach. Many of the most important theorems of the last two decades were suggested first in experiments.

Computers may help also in proofs that are combinatorial and which require just the examination of a finite number of cases (the classification of finite groups could not have been accomplished without this). Many algebra theorems include already computational parts.

Somewhat more surprising – but nevertheless true – is the fact that one can use computers to help with computer assisted proofs in analysis.

The fact that a computer could verify and even generate proofs of all the theorems is called the Church-Turing thesis (basically it is a definition of what one would call a theorem.) According to these thesis that a proof is – should be – just a finite number of manipulations of a finite number symbols according to a finite set of rules. Hence, a computer could find an arbitrarily large number of theorems if one lets it running long enough. Of course this Borgesian dream is not (yet) very practical. Chess – significantly simpler than even the most elementary Mathematics – has required quite a technological effort to be brought to the reach of computers.

Still computers can be invaluable tools in the verification of proofs conceived by humans or even in the carrying out of easy – but tedious – lemmas. By now, one can verify all the theorems in Euclid as well as many other classical ones automatically. (Note that the theorems in classical geometry of circles and triangles are just identities among polynomials – subject to some inequalities – ) Amusingly, about 20 years ago, one could also verify a proof of the Gödel theorem based in the halting problem (It is impossible to write a program that decides whether another program

will run forever or not.)

It should be mentioned that this check for consistency has important technological applications, turns out that the problem of verifying theorems is logically the same as the problem of verifying programs. The state of the art of the technology seems that one can verify some relatively structured programs such as compilers. Given the fact that the famous Pentium bug costed Intel 400 million dollars – according to some reports that I read in the press – and that it could have been prevented by this mathematical tool, one can see that even something seemingly as remote as logic can have clear applications. Even if this press report could have been exaggerated – I cannot vouch for it, and presumably nobody really knows the real costs – it seems clear that it was enormous and that even a very long shot that could have had an 1 in a 100 odds of preventing it would have been extremely valuable.

- **Technology may affect the sociology of the mathematical profession.**

One of the most complicated aspects the relation of Mathematical research and technology is the sociological one.

One complicated side of this relation is that technology has a direct relation to economy and power. Even more, our society, sometimes seems to worship technology for its own sake.

The effect of this is rather complicated and I cannot even hope to deal with it in a systematic way. Let me just mention a few examples and jot some unsystematic reflections, that I hope will stimulate the thoughts of the participants.

One distortion is that areas that have direct commercial applications receive more funding and attract more people. On the other hand, often the solutions come from unexpected places and from fundamental advantages.

One example – very close to technology – that comes to my mind is the development of Unix which was accomplished by a team of people who wanted to hook up a disused computer to a printer. By layering an structure of programs – incredibly close to the way a mathematician layers lemmas and propositions for a brilliant theorem – they could accomplish something that has been, from the technical point of view unsurpassed even twenty five years later. This is one case – very close to technology – which technological brilliance is negatively correlated with commercial success.

The technology worship can be seen in the fact that, very often the solutions that are proposed are the ones which involve more complicated technology.

If one speaks with funding agencies, one never runs into trouble by recommending as a first step that more money is spent on faster computers (and slower software). Of course, a bit of thought would show that if some indicator has decreased in the last 10 years, when computer usage has increased dramatically, there has to be other factors at play.

Another side of this relation is that technology is affecting the lives of everybody. In particular of Mathematicians and their research. Nobody can deny the effect of technology in the communication of Mathematics. The Internet has fostered a new

style of communication, relatively inexpensive publication technology and Xeroxing has allowed people even in remote places to carry out research, the possibility of travel has multiplied the contacts.

It is not clear what will happen. One should not read much into the sociological implications of technology. The limiting factor continues to be human nature. Technology will change the situation, but not as much as naive quantitative extrapolation will predict. Human nature reacts to limit technology. For example: The time for a paper to reach people will decrease, but the time it will take to be understood will remain the same (it will perhaps increase if they are written with less care.) Communications will become more abundant, but they will carry less information. One can see it happening. e-mail is more convenient than letters, but to organize a seminar or to accept an invitation, one has to send many more e-mails than letters.

The thing that I believe will be a permanent effect is that this technology will equalize the geographical distribution of research centers. The model of a central clearinghouse which selects and gives value to the different accomplishments that has worked from the times of the schools in Alexandria will have to be revised.

### 1.3 The role of Mathematical research in technology

#### •Sometimes the applications come in unexpected ways.

We have already examined the case in which technological applications suggest problems and the mathematicians can solve them.

A somewhat surprising fact is that often the mathematical tools predate the applications – sometimes by centuries –.

In the words of Puig-Adam, *The only mathematical tools that remain without application are those that you do not master.*

Perhaps the most striking fact is the development of the computer. The crucial idea that made computers possible is the idea that programs can be treated as data. This idea appeared first in the Gödel theorem. Turing developed the *Turing machines* to give a simple and conceptual proof of the Gödel theorem. The application to the computer appeared when Von Neuman – the same Von Neuman who worked in logic and hosted Turing as a post-doc while Turing developed the Turing machines – took over the development of the ENIAC.

It seems clear that there is an intellectual lineage between the investigations in something so abstruse as theoretical logic and something as technological as the development of the computer which indeed has modified modern life.

Another example is Number Theory. Even if the subject is centuries old, well entered this century a distinguished practitioner such as Hardy was boasting in his autobiographical notes that it was utterly useless. Now, parts of number theory are crucial to the development of cryptography, digital communication and the electronic information commerce. (Incidentally, there are still no good conceptual mathematical models of the information economy.)

The examples from Physics are even more striking and better known. Many fields

found the tools they needed already developed. General relativity found Riemannian Geometry, Quantum Mechanics found the Hilbert space almost well developed, Gauge theories found the theory of connections.

•**Applications require developments in a broad front.**

Very often the applications that one needs are not so easy to predict.

One of the emerging industries is computer animation. In the USA, it moves a year several thousands of millions of dollars – compared with about two or three hundred which is the total support for Mathematics research.

The fact that nowadays a serious application is having shadows of imaginary dinosaurs jumping in a screen may give a moment of thought to the people who think that proving theorems is a frivolous activity.

It is very interesting to note that this application of computer graphics requires a broad array of mathematics. Certainly, the development of the computer was a necessary step, but also, the classical differential geometry of surfaces. If one wants to get good speeds and obtain interesting effects, one needs rather sophisticated mathematics. For example, the detection of intersections of surfaces requires algebraic geometry – the Gröbner basis of prof. Buchberger are indispensable –. The detection of edges uses wavelets, which were developed by the confluence of applications in Physics and Geophysics and the harmonic analysis of the 60's and 70's.

Let me draw two lessons from this example. The first one is to note that indeed to get something working it is necessary to draw from a large variety of sources. Some of those were never anticipated. There are many more examples of the same phenomenon.

The second lesson is that one needs to rethink what is frivolous and what is not. Nowadays a serious application is creating shadows of imaginary dinosaurs jumping in a screen. This may give a moment of thought to the people who think that proving theorems is a frivolous activity. Technology is related to economy and, specially now, this is a very paradoxical field.

Even if a mathematician working on chaos at University of Texas, may be accused of being working in a self-serving activity. Another mathematician working in having shadows of dinosaurs in a film eating a fictional chaotician from University of Texas is doing a very practical thing.

#### 1.4 Final reflections

The relations between Mathematics and technology are complex and go both ways. They could be mutually beneficial.

The relations are changing in time and have surprises. Therefore, to realize this potential requires an open mind and a continuous dialogue as well as a critical examination of the problems and the results. One should leave the door open to the unexpected and keep an open mind.

## 2 CONSIDERATIONS BY BRUNO BUCHBERGER

### 2.1 The Emphasis of My Consideration

In this round table we discuss the impact of the new technologies on mathematical research. We agreed that what we mean by “the new technologies” is computer technology (i.e. hardware, software, communication technologies). The title of the round table suggests that we only discuss the impact of these technologies on mathematical research. It should be clear that the impact of these technologies on mathematical education and the application of mathematics is at least as important as the impact on mathematical research. However, since the relationship between computer technology and mathematical research is much less popular and actively pursued by the international mathematical community than the use of computers in education and application, in this discussion, I want to focus on computers and mathematical research.

My contribution to the discussion will emphasize, and hopefully clarify, the following items:

- Computer technology is not just “a” technology that, occasionally, may have impact on mathematics but I consider this technology as “the” mathematical technology, i.e. it is inseparably connected with mathematics.
- The interaction of computer technology with mathematics is not a one-way relation of the type “impact on” but a relation of mutual advancement.
- Many mathematicians (in particular many of those who call themselves “pure”) have a very limited view on the essence of the computational aspect of mathematics. This is not only in sharp contrast to the comprehensive view of mathematicians in earlier centuries but is also a serious obstacle to having mathematics accepted as the core technology for the present information and communication society.

### 2.2 Computer Technology and Mathematics

In my view, mathematics is the technology of gaining knowledge and solving problems by reasoning. The aspect of gaining knowledge and the aspect of solving problems are just the two sides of one coin: Solving mathematical problems needs mathematical knowledge and proving mathematical knowledge often amounts to solving mathematical problems. (*Example:* An efficient solution of the problem of computing the greatest common divisors of two numbers needs Euclid’s theorem. *Example:* The proof of the fact that the limit of the sum of two sequences is the sum of the limits needs the solution of the problem of finding the appropriate index bound for the sum sequence from the index bounds of the given sequences.)

Ideally, the solution of a mathematical problem should result in a procedure that explains as systematically and explicitly as possible how, for any given instance

of the problem, a suitable answer can be constructed. (*Example:* The problem of integration asks for a procedure that, given a function in some representation, finds the representation of a function whose derivative is the given one.)

The question now is what we consider to be a “systematic and explicit procedure”. Over the centuries, the notion of “systematic and explicit procedure” has evolved from a vague (but quite concrete) understanding to a more and more refined understanding. Only in the early decades of last centuries, by the work of Gödel, Church, Turing, and others, a rigorous definition of the notion of “systematic and explicit procedure” (in the form of the notion of “algorithm”) has been given and turned out to be a quite stable and fundamental notion. Also, it is clear that, for many mathematical problems, we have to content ourselves with quite “unsystematic and implicit” solution procedures:

- Sometimes, the problem at hand has *uncountably many instances*. Thus, by a simple cardinality argument, we never can expect to obtain an algorithmic solution since algorithms can have only countably many possible inputs. (*Example:* We cannot have an algorithm for the integration problem for the class of “all” integrable functions, say, from the reals to the reals because there are more than countably many such functions.)
- Sometimes, although the problem at hand has only countably many instances, one can prove that the problem is “*algorithmically unsolvable*”, i.e. one can prove that the existence of an algorithm for the problem would lead to a contradiction. (*Example:* The problem of deciding whether or not a given system of multivariate polynomials over the integers has an integer solution cannot be solved by an algorithm. This was proved in the famous paper by Matyasevich in 1970.)
- Sometimes, the current state of *mathematical knowledge is just not yet sufficiently developed* for being able to come up with an algorithmic solution and, thus, one must be satisfied with having a few unsystematic, heuristic or partial solutions for a couple of special cases. Later, after developing deeper mathematical theory, it may turn out that, in fact, a general algorithm can be given. (*Example:* The question of computing canonical representatives of multivariate polynomials modulo a given polynomial ideal, in a paper in 1974, was still conjectured to be algorithmically unsolvable but it turned out to be algorithmically solvable with Gröbner bases theory.)
- For some problems, we succeed in giving an *algorithmic solution only relative* to a couple of basic functions that are assumed as “black boxes” but, in many cases, are not algorithmic themselves. (*Example:* The proof of Bernstein’s theorem that tells us that two sets that can be injected into each other have equal cardinality establishes a procedure that constructs the bijection between

the two sets relative to the possibility that we can decide membership in the two sets.)

- Sometimes, we know an algorithmic solution for a problem that, however, is *too complex to be practically useful* except for some particularly simple problem instances. In this case, the mathematical challenge remains to come up with less complex algorithms. (*Example:* The problem of deciding whether or not any formula of the theory of real closed fields is valid was first proved to be algorithmically solvable by Tarski in 1948 but became feasibly solvable only by the method of Collins in 1975.)

Whatever the notion of a “systematic and explicit procedure” is and even if we know that, for certain problems, no systematic and explicit procedure in the sense of ‘algorithm’ exists, it has always been the ultimate goal of mathematics to come up with better and better procedures, based on deeper and deeper theory, for solving problems. In fact, even pure “existence theorems” in all but the trivial cases convey lots of procedural problem solving information. (*Example:* The main theorem of integration gives us a procedure that reduces the problem of finding antiderivates to the problem of finding definite integrals, i.e. to the problem of computing certain suprema of infinite sets of finite sums - and vice versa. Even if the problem of computing suprema, in general, may be equally hard as the original problem of finding antiderivatives, in some cases, the reduction “procedure” may be very helpful.)

Thus, summarizing, the goal of finding *procedures* for solving problems or, in other words, the goal of making mathematical problem solving a “technology”, is as old as mathematics and, in fact, the essence of mathematics. Hence, the advent of the “new computer technologies” is nothing else than the systematic fulfillment of the age-old aspiration of mathematics and the most explicit form of making mathematics a technology.

### 2.3 The Interaction Spiral Between Computer Technology and Mathematics

On a trivial level, these days, even “pure” mathematicians acknowledge that *computer technology makes the life of mathematicians more comfortable*: We use Latex, e-mail, web surfing, e-conferencing, etc. for organizing our scientific life and we feel that, thereby, our output in terms of research, publications, and global interaction with colleagues tends to increase. However, this type of impact of computer technology on mathematics is not significantly different from the impact of this technology on other fields of science and engineering and I do not comment on this any further.

On the next level, the availability of mathematical software systems like Mathematica, Maple, Derive, etc., in the past two decades starts to *change the way how mathematics can be taught*. Mathematical software systems have made enormous

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progress both in terms of mathematical sophistication and user-friendliness. Summarizing, basically all the mathematics taught in high-schools and undergraduate mathematics and engineering curricula is now available on computers and it both a tremendous opportunity and a non-trivial didactic challenge to use this potential in a pedagogically sound way. More and more math teachers embark on using these systems for re-organizing math teaching but, still, the majority of math curricula is not really taking full advantage of the new technology available. Since quite some literature is already available on the impact of the new technology on math education, I do not expand on this subject either. For what I consider to be a balanced view on this subject, see B. Buchberger, Should Students Learn Integration Rules? *SIGSAM Bulletin* Vol.24/1, pp. 10-17, 1990.

On a third level, current mathematical software systems, of course, made *sophisticated mathematical methods readily available for applications* in all areas of science and engineering. I guess that the power of mathematics as represented in current math software systems is much more recognized by scientists and engineers in fields outside mathematics than by the mathematicians themselves. The literature on application problem solving supported by current math systems is impressive and is the subject of quite some specialized conferences and special sessions at conferences. Since the impact of the combined math and computer technology on application is, hence, well recognized I do not further dwell on this aspect either.

Rather, as announced above, I would now like to put my emphasis on a fourth level, the level of *interaction between mathematical research and computer technology*, which I find to be the most fundamental, natural, and exciting interaction but also the level of interaction that so far, unfortunately, has obtained the least attention by the mathematical research community.

I think that the relationship between computer technology and mathematical research is not only a case of mutual interaction but can only be understood as an open-ended spiral where, in each cycle of the spiral, known technology is *used* and, at the same time, new technology is *produced*:

In a typical situation in mathematical research, for the mathematical area under study, we want to derive new knowledge or, dually, new methods for problem solving. For doing so, we can *use all known technology* for studying examples, generating counter-examples, representing typical mathematical objects in the area in various formats, e.g. graphical format, translating between different formats, producing entire spectra of examples of increasing complexity, tracing typical computations, formulating and testing conjectures etc. Using all known technology may help in generating insight, coming up with new conjectures and directing our research for proving or disproving conjectures. Thus, we finally may arrive at a situation where the problems at hand can now be solved by some systematic method. In the ideal case, this is an algorithm or, in case algorithms for the problems at hand were already known, an algorithm of lower complexity than the known ones. This means that, while we are *using known technology* for supporting our research, the result of

our research should ultimately *produce some new technology*.

*Example:* Let's assume we want to find a method for solving systems of multivariate nonlinear equations. In our research we may well resort to studying examples of such systems using known technology like evaluation algorithms for functions, graphical representation algorithms for plotting 3D-pictures, calculating partial derivatives and plotting tangent planes, finding intersection lines of the tangent planes with a coordinate plane by linear algebra algorithms etc. From our experiments with known technology, we may obtain the conjecture that the iteration of linearization, under certain conditions (which ones?), establishes a convergent solution procedure for nonlinear systems. If we succeed to prove the conjecture we now have *a new technology* (Newton's method) at our disposition which can be used - as a "black box" - in future, higher-level, research phases, for example, when doing research on the construction of systems of basic wavelets.

*Example:* When studying how the proofs of geometry theorems like Pappus' theorem, Desargues' theorem etc. can be obtained in a systematic way, one may want to consider the formulations of these theorems after appropriate coordinatization. It then turns out that the formalizations of many of these theorems are universally quantified implications with all premises and also the conclusion being nonlinear multivariate polynomials. If the conclusion polynomial is in the ideal generated by the premises then, of course, the conclusion polynomial vanishes for all values for which the premise polynomials vanish. Thus, one may *use the known technology* of Gröbner bases for deciding ideal membership for mechanizing the check of validity of certain geometry theorems. After some further experimentation with this idea one may obtain the conjecture for *a general new algorithmic method* for deciding arbitrary geometry theorems whose formalization, after coordinatization, are universally quantified boolean combinations of nonlinear polynomial equalities and, in a further step, one may even come up with a general method that automatically generates "side conditions" in the case where a geometrical conjecture is not true in general but only in "non-degenerate" cases. Again, this new method (which, in a slightly different form was first introduced by W.T. Wu in 1978) can now be used - as a "black box" - in future, higher-level, research phases, for example, when answering questions about the stability of constructions in computer-aided design.

By iterating mathematical research phases that use known mathematical technology and produce new one, the spiral of mathematical creation leads to more and more sophisticated, powerful, and general methods backed by more and more sophisticated and deep mathematical theory.

I would also like to point out an important distinction in the nature of how mathematical technology produced in one phase is used in some later phase:

- In many cases, like in the above example of Newton's method, we just *use the problem solving technology produced in some area A of mathematics (e.g. linear algebra) for constituting new problem solving technology in some other area B of mathematics (e.g. nonlinear systems)*. The relation "technology A is

used as a constituent of technology  $B$ ” forms the exciting web of mathematical technology. (A systematic study of the logical structure of this web would be an interesting research project in itself.)

- In some cases, maybe the most exciting cases, what actually happens is that a technology worked out in some area  $A$  of mathematics is used for automating *reasoning* in some area  $B$ . The above example of geometry theorem proving falls into this category: The technology of Gröbner bases, which is a general problem solving technology for the area of polynomial ideal theory, in the above context, is used as the crucial constituent for automating theorem proving in the area of geometry theorem proving. The distinctive feature of this category of “technology transfer” from one area of mathematics to some other one is that, here, knowledge and methods from one area are *lifted* to the meta-level (proof theory) of some other area.

It turns out that, if one analyzes the exploration strategies of many areas of mathematics carefully, the transfer of knowledge and methods of one area to the meta-level of establishing new, condensed and efficient, proof technology in some other area happens many more times than one is usually aware of. I tend to believe that this strategy of “self-application” of mathematics to itself is one of the most important ingredients to the creative power of mathematics. Self-application, therefore, should also be made one of the crucial design features of future computer-supported mathematics systems. (In fact, it is one of the main design features made in my Theorema system, see B. Buchberger et al., Theorema: A Progress Report, *Proceedings of the Calulemus 2000 Conference*, St. Andrew, UK, August 5-9, to appear). Understanding this self-application strategy also answers the question, which puzzles quite a few mathematician, about the value of computer-generated proofs: Checking computer-generated proofs by humans is *not* the real issue. Rather, checking the proof of the correctness of the proving algorithm is the handle to having control over computer-generated proofs! In other words, in the same way as it does not make sense to check the result of every computer-generated multiplication of long integers, it does not make sense to check the result of every computer-generated proof. Rather, what makes sense is to check the mathematical correctness of the multiplication algorithm and, analogously, to check the mathematical correctness of a proving algorithm (e.g. the correctness of the Groebner bases method.)

## 2.4 Implications

My main point is that the impact of the “new technologies” to mathematics should not so much - or not only - be seen in the application of existing algorithms and their implementation (in mathematical software systems) to the exploration phase of inventing mathematics but, rather, the really exciting impact is the other way round: By having the possibility, these days, to finally implement our methods on fast and big (though physically small) computer systems, all areas of mathematics receive

a tremendous new impulse for coming up with new and deep theory that enables us to make our methods effective and efficient. This is particularly challenging and demanding in areas that so far has been deemed to be abstract and “pure” (as, for example, ideal theory, category theory, topology, group theory, functional analysis, partial differential equations etc.).

Summarizing, I believe that the existence of the “new technologies” will and should have the following impact on creating a new age of mathematical research:

- We should be strongly motivated to come up with *new mathematical theories* with more subtle notions, deeper and stronger theorems, and more difficult proofs that aim at algorithmic solutions of problems in mathematics for which so far only existential results or rough algorithmic solutions or algorithmically complex solutions were known.
- Also, a huge number of new and challenging *mathematical research problems stem from the necessity of building more and more powerful machines, languages, and software*. In other words, the impact physics and natural sciences had for the development of math research in the past three centuries in certain areas like analysis, functions theory, group theory etc., computer technology could have / has on the development of math research today.
- The *invention process in mathematical research* (in which typical mathematicians exclusively still use ‘pencil and paper’) could / will be (?) more and more supported by mathematical software whose foundation and development is a deeply mathematical research topic.
- The global organization of accumulating mathematical research results in a meaningful way that allows *active* work with these results (instead of the *passive* use through traditional libraries) is, again, a highly demanding mathematical research topic that goes far beyond the “collection and presentation of information” in other scientific or economic areas (as, for example, the collection of data in a tourist information system).
- We should aim at gaining new insight about the fundamental interplay between logic (as meta-theory of math), pure (structural, existential) mathematics, algorithmic mathematics and computer science. In particular, mathematics should not let computer science drift away from core mathematics. It think it was one of the big political mistakes of mathematicians to let this drift happen in the past five decades and a drastic re-direction is of utmost importance for maintaining the position of mathematics as the core “thinking technology” of the current age.

Although all this appears completely trivial and natural to me, I know that these ideas are not main-stream in mathematics and it makes me nervous to observe

that, by not seeing the driving impact of the “new technologies” on mathematics, mathematics these days tends to lose its central role in the current science and technology oriented society.

### 3 CONSIDERATIONS BY SERGEI MATVEEV

It is widely accepted that computer-related technologies made a significant impact on the development of mathematics in a broad sense, including research, education, and publication aspects. Many positive sides of the impact are evident, and it would be boring to list them. I would like to attract attention to some negative sides as well as to some positive aspects which are probably not so evident.

**1. Education.** What would your reaction be, if a student, asked to multiply 2 by 2, takes a calculator out and, after a few seconds of button pushing, gives the correct answer? It may happen that the student will have later difficulties with learning, say, group theory, since for him the right perception of an abstract binary operation as a function of two variables might be fogged by reminiscent of buttons.

Similar example: would you consider a rigorous computer proof of a theorem from Euclid geometry as a satisfactory one? Mathematicians want usually to know not only the correctness of a statement, but also WHY the statement is true. This desire helps them to reveal hidden interaction of mathematical objects, which determines after all the internal beauty of mathematics. Without this, the study of mathematics would resemble the study of English literature through learning to write business letters.

**2. Publication.** The number of mathematical publications is rapidly increasing, as well as the number of new mathematical results. The problem is that the ratio of these two numbers seems also to be increasing. This means, that essentially the same result is published in many papers, possibly in different forms. Electronic journals/archives are especially dangerous from this point of view. A mistake in a paper published in a classical journal would live as long as libraries do. This and the natural limitation of the volume and the number of publications force the author to exact writing and careful checking, hence to exact thinking, which makes the paper more valuable.

The matter is quite different with respect to electronic archives: you may always put in an improved version, or even several successive versions, and nobody will remember the old ones. This stimulates actually appearing of sloppily written and superficial publications of a competition nature, which is harmful for mathematics.

**3. Communication.** While the speed and the ease of new electronic communication means (electronic mail in particular) do enhance information and ideas sharing, they also lead to overloading mailboxes. Some of the messages might be of particular interest or importance and thus scream to be paid attention, and it is simply impolite to overlook the rest. As the flow of the information grows, attending to the content of one's mailbox takes increasingly more time and effort. Useful as that work is, its effect is greatly dispersed, taking the form of adding a cup to a sea. Undoubtful use and difficulty of this activity might also create a feeling of working productively and hard. The receiver's actual productivity may, however, decrease, as no space is left for large problems requiring a lot of time and concentration.

From another side, the amount of information available through communication

means such as e-mail or Internet is intimidating and causes helpless feelings of one not being able to learn what is going on even in a narrow field of study. Indeed, the human ability of apprehending new knowledge is limited, and that limit is far exceeded by the new communication technologies. Therefore, even when the overflow of information is not detrimental, it still can be useless.

**4. Research.** Here are a few challenging questions that maybe concretize some aspects of new technology impact on pure mathematics.

(4.1) Can a computer "proof" of a theorem be considered as a strong one? To be more specific, what should we think on a proof that consists of the following two steps:

- (a) A theoretical reduction of the statement to verifying a finite number of possibilities;
- (b) The claim that the statement is true for all of them with a reference to a computer checking program.

(4.2) Should the mathematical visualization be considered as a part of theoretical mathematics? Certainly, computer graphical methods and visualization techniques require fairly elaborate theoretical concepts and mathematical algorithms. In a sense, mathematical visualization is a marriage of mathematics and computer sciences. But can the final result (for example, a computer program that shows colored surfaces swimming and bifurcating on the screen) be considered as a mathematical result?

(4.3) Why algorithms are of interest? The possible answers are:

1. Because there exist algorithmically unsolvable problems, and it would be very good to know at advance whether the problem is algorithmically solvable. It does not mean that in case of the negative answer one should stop the attempts to find a solution. One may try to find either a reduction to problems from neighboring branches of mathematics, or a nonalgorithmic solution.
2. One may use their computer realizations as tools for answering practical (in math sense) questions and collecting experimental facts;
3. A more significant aspect of this question is that the problem of creating an algorithm solving a mass problem is a well-stated global problem that shows the right direction of investigation.

By a well-stated problem I mean a problem that

- admits a simple formulation;
- has a general nature;
- seems not to be hopeless, at least, admits potentially solvable approximations;
- is nontrivial, i.e., it cannot be solved by known methods (the main property).

Trying to solve such a problem, we inevitably run across new deep structural properties of the real world, the discovery of which is the final goal of mathematics.

For example, the recognition problem of 3-manifolds had stimulated the discovery of normal surface theory, characteristic submanifold theory, and hierarchies of 3-manifolds.

**(4.4)** The same as (4.3) with respect to efficient algorithms, whatever it means. I do not mean here decreasing the runtime of an algorithm by various artificial tricks. The question is to find a better algorithm based on new principles. In this sense the problem of creating a partial efficient algorithm can be considered as a pure mathematical problem.

There is a subtle but very important difference between the problems stated in (4.3) and (4.4); the latter seems to reflect the NEW TECHNOLOGY problem more adequate.

## 4 CONSIDERATIONS BY MIKA SEPPÄLÄ

### Advanced mathematics has changed technology, and now advanced technology changes mathematics.

I suppose that even non-mathematicians acknowledge that advanced technology would not be possible without advanced mathematics. Analytic geometry has made the advanced CAD programs possible, and these programs have contributed, for example, to the fact that engines in modern cars take much less space than in older models – a very practical consequence of being able to compute the intersection of surfaces.

Computers and networking relies on advanced mathematics for encryption, data packing etc. We all use brilliant mathematical results without which on-line banking, WebTV, internet radio etc would simply not be possible. The role of mathematics has been and continues to be crucial.

New technology will have a profound affect on how next generations of mathematicians find and publish their mathematical results, share ideas and teach mathematics. It will also change the way students learn mathematics and the way mathematics is being used in other disciplines.

Previous contributions to this Round Table have discussed almost all relevant aspects of the matter. I would like to add to the discussion an example of ways in which technology allows us to *experiment* with deep mathematical results.

#### 4.1 Example of analytic and algebraic geometry

In the end of the 19<sup>th</sup> century Riemann surfaces were simply a different view of algebraic curves. They arose from the desire to be able to solve  $y$  in terms of  $x$  from a polynomial equation

$$P(x, y) = 0.$$

The set  $C = \{(x, y) \mid P(x, y) = 0\}$  is an algebraic curve (provided that  $P$  satisfies certain regularity conditions) which was also known as a Riemann surface.

*Uniformization theorem*, which was discovered late 19<sup>th</sup> century but rigorously proved only much later, states that the algebraic curve  $C$  can be uniformized, i.e., that it is possible to find a uniformizing domain  $D \subset \mathbb{C}$  and a uniformizing mapping

$$\pi : D \rightarrow C, z \mapsto (\pi_x(z), \pi_y(z)).$$

In most cases  $D$  can be taken to be the unit disk in the complex plane.

Uniformization really made Riemann surfaces an independent field of study. In modern expositions, Riemann surfaces are defined as topological surfaces equipped with a conformal structure. Excluding certain special cases, these surfaces can be represented as the unit disk  $D$  mod the action of a properly discontinuous group  $G$  acting in  $D$ . Hence the relationship to algebraic curves was lost, and Riemann surfaces became an interesting and important object of study in analytic geometry

and in complex analysis. Consequently the fact that compact Riemann surfaces are really algebraic curves has become an important theorem.

In the meanwhile algebraic curves lead their own life and became an important object of study in algebraic geometry.

Jacobian varieties are a corner stone in the theory of algebraic curves and Riemann surfaces. They are defined in terms of periods of holomorphic differentials. The Jacobian variety of a genus  $g$  Riemann surface is  $\mathbb{C}^g$  mod the action of a lattice determined by computing integrals of differentials over closed curves on the Riemann surface. By the Torelli theorem, a compact Riemann surface is determined uniquely by its Jacobian variety. The same applies, of course, to algebraic curves.

The methods used by complex analysis to study Riemann surfaces are quite different from the methods of algebraic geometry, leading to the creation of two different mathematical cultures that study the same objects. Few mathematicians can appreciate the methods and results of both of these cultures.

This is where technology steps in and helps us to develop and to implement computational methods to:

1. pass from the category of compact Riemann surfaces to that of projective algebraic curves (and vice versa)
2. compute a period matrix for a given Riemann surface or algebraic curve (and vice versa)

If the above can be done, then one would be able to experiment with Riemann surfaces, algebraic curves and with their Jacobians thus possibly being able to find new facts about these objects, or to make new conjectures. Good computational tools here would be bridges allowing one to pass from one mathematical culture to another.

The difficulty here is that *Uniformization has successfully stood up against efforts to do it algorithmically*. Leading mathematicians starting with Poincaré, Burnside, Myrberg and Whittaker have invested serious efforts to study this problem already since late 19<sup>th</sup> century. Their work lead to algorithms that were too complicated to be useful for computations without computers.

*But with new technology we can now*

1. find a representation for a given compact Riemann surface as an algebraic plane curve
2. uniformize large class of algebraic curves numerically
3. compute period matrices of algebraic curves (and hence of Riemann surfaces)

Part of the above is still at an experimental level, but some components, like the program computing an approximation for a period matrix of an algebraic curve, are already distributed commercially (as a part of the current release of Maple).

The mathematical community is building bridges connecting different mathematical cultures, and some of the bridges are already open for traffic. Big part of this would not be possible without new technology.

The above example may sound, to many persons as, one of the most useless parts of the bastion of pure mathematics. But that is not so: *a better understanding of periods of differentials of hyperelliptic Riemann surfaces is needed for being able to model hard disk read heads better, and hence this example may lead to hard disks in which the data can be more densely packed.* Current models for the magnetic field around a hard disk read head use elliptic Riemann surfaces instead of hyperelliptic ones.

## 4.2 Publishing mathematics — MathML and OpenMath

I predict the following:

**Conjecture 1** *By the end of 2005, most of the traditional journals have become electronic. The publishers will remain – they are needed to guarantee the quality of the material – but the way they publish is undergoing a profound change.*

The main consequence of this is that journals become cheaper, since the cost of printing and mailing disappears, and they become easily available everywhere. Also searching for new results on a given topic becomes easier.

The fact that even the current versions of popular web browsers cannot display mathematical formulae natively has held back the revolution that electronic journals are going to make in the publishing of mathematics. This is now changing. Thanks to the efforts of several international projects, **MathML** and **OpenMath** provide ways to include mathematical formulae on web pages so that the meaning of the formula in question can be automatically understood.

The work towards establishing a standard protocol for mathematics was started in 1993 by Gaston Gonnet, who organized the first OpenMath workshop that took place at ETH, and by Mika Seppälä, who was the coordinator of the first OpenMath project funded by the European Union. Now the center of activities is in Oxford. The current extensive OpenMath project is coordinated by Mike Dewar at NAG in Oxford. The project has further refined the protocol and developed tools that allow one to translate LaTeX documents into OpenMath documents.

MathML sprung off from the original OpenMath project and provides, under the auspice of the WWW Consortium, a way to place K12 mathematics in the web. OpenMath extends MathML and provides a mechanism to cover all of mathematics.

Thanks to the work of these projects, it will soon be possible to cut a formula from a mathematical web page and paste it to Maple or Mathematica and then compute with the formula. This is not yet the case. Most current mathematical web pages represent mathematical formulae as gif-pictures. You cannot compute with a picture, and you cannot use a picture as a key word in a search. But by the

introduction of next generations of web browsers all this will change. That makes the presentation of real live mathematics in the web possible in a reasonable way. And once it is there and a set of appropriate tools is available, mathematics will move into the web in a big way.

*This is also a challenge to mathematicians: How to present their work in the web in the best possible way?*

Currently the so called electronic journals are mostly presentations of the traditional printed journals in the web. Usually this is done by using pdf which makes it possible to place documents in the web so that, when printed, the result matches the quality of the best traditional printed documents.

But this is not what electronic journals should be. Lots of experimenting is needed to find the best ways to publish mathematics in the web. This development is really only starting now with the adoption of MathML and OpenMath.

An important consequence of this development is that xml documents using MathML or OpenMath to represent mathematics, will eventually replace LaTeX as the main language for mathematical documents. TeX (and LaTeX) will remain as the typesetting engine when documents need to be printed, but the documents themselves will start living in the web. They will not anymore be linear presentations of a topic, and hence cannot be presented in the traditional book form. Parts of the documents will still be printed, and then TeX will be used to typeset whatever we want to print.

This is a revolution in the publishing of mathematics. It has barely started, and it will, very quickly, affect the work of each one of us.

### **4.3 Teaching mathematics**

Almost all universities in the US and in Europe are developing ways to offer their courses in the web. This will change the way we teach mathematics, and soon a small number of sophisticated virtual universities will attract large numbers of students. Basic education can be in this way offered cost-effectively to motivated students. This will be suitable for students who cannot attend usual classes for work or for some other reasons. This will also open possibilities to financially challenged students to get education. In this sense technology will be an equalizer offering educational possibilities to people who otherwise could not study.

To find appropriate ways to use technology in education is also a big challenge to professors not only in mathematics. A lot of effort is needed and initial results will, in many cases, be disappointing. Here we all are students needing to learn new things.

#### 4.4 Conclusion

I summarize my considerations in the following statements:

1. New technology will offer tools that allow mathematicians to experiment with statements and results thus helping to gain deeper understanding of the facts. Making of mathematics is moving from a "pen and paper and eraser" business to a business in which the basic ingredient is an advanced mathematics system like Maple or Mathematica. This change is gradual and slow.
2. Publishing of mathematics will change quickly, and journals become electronic. This is largely due to the fact that it is soon possible to deal with mathematics in the web in a proper way. This change is rapid and will affect all mathematicians.
3. Mathematics education will change and virtual web universities will offer cost-effective alternatives to traditional universities (but will not replace them). This change is also rapid. For basic courses like Calculus new learning systems will be developed. These learning systems keep track on the material that the student has already mastered and offer him or her problems in the areas in which he or she needs more practice. This will enhance traditional classroom teaching and will make it possible to take mathematics courses on-line. These on-line courses are not for everybody. They are suitable for motivated students.
4. The mathematical community needs to revise its way of evaluating the activities of individuals working towards applications of new technology in mathematics. This work does not always result into publications and hence the persons performing the work will not always get the appropriate credit. Often the work is in the implementations even though some aspects of this work may belong to the realm of classical mathematics. The traditional evaluation mechanism leads sometimes to situations where persons, who have done important mathematical work to improve and implement a specific algorithm or to develop a new one, will not be acknowledged and do not get funding to continue their work, or, in the worst case, do not get a proper job. This slows down the development.

*These are great times filled with new opportunities and challenges. The way we do, use, publish and teach mathematics is going to change.*

## **5 CONTRIBUTIONS TO THE DISCUSSION**

Editing of the contributions to the discussion.