The Future of Mathematics: 1965 to 2065.

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Abstract. William Gibson, the science fiction writer, who coined the term *cyberspace* well before he purchased his first personal computer has commented that the future is already here but that it is very poorly distributed. Mindful of the dangers of futurology, I shall look forward and back fifty years while where possible eschewing the unknowable.

1 The bigger picture

> It's generally the way with progress that it looks much greater than it really is. (Ludwig Wittgenstein, 1889-1951, "whereof one cannot speak, thereof one must be silent")

> The world will change. It will probably change for the better. It won't seem better to me. ... There was no respect for youth when I was young, and now that I am old, there is no respect for age. I missed it coming and going. (J. B. Priestley, 1894-1984)

I was asked to take on a daunting and futile task—that of talking about the future of our discipline. I negotiated myself back to the current title. At least that way, I can be demonstrably wrong about past events even as I fail miserably when looking at the future.

Or I could take the coward's way out as Ken Davidson and I did when we agreed to write *Mathematics in Canada. The Future of Mathematics in Canada 50 years later.* [10]. We fairly accurately reprised the present (1995) and then made milquetoast predictions about the very proximate future. These were so conservative that I shuddered when I reread the article—just fifteen years later—in preparation for my current remit.

'Futurology' is the domain of fools and flim-flam artists. It can be very profitable. I prefer science fiction like Daniel H. Wilson *Robocalypse* (2001) to fiction masquerading as science—my view of Ray Kurzweil's *The Singularity is Near* (2005). The further out one looks the less one can say, and the loonier the results are likely to be in retrospect. An astonishing example is a recent attempt by

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Scientific American to look at computing in 2165; that's right 150 years away! [1] [2] A fine reprise of the errors of my fore-runners is Leon Kappelman's 2001 article T*he Future is Ours* [28]. He wrote

> Predicting the future is an activity fraught with error. Wilbur Wright, co-inventor of the motorized airplane that successfully completed the first manned flight in 1903, seems to have learned this lesson when he noted: "In 1901, I said to my brother Orville that man would not fly for 50 years. Ever since I have ... avoided predictions." Despite the admonition of Wright, faulty future forecasting seems a favored human pastime, especially among those who would presumably avoid opportunities to so easily put their feet in their mouths.

Some of the quotes in [28] were somewhat or very plausible when uttered:

> "This 'telephone' has too many shortcomings to be seriously considered as a means of communication. The device is inherently of no value to us." — Western Union internal memo, 1876.
>
> "Folks, the Mac platform is through – totally." — John C. Dvorak, *PC Magazine*, 1998.
>
> "By the turn of this century, we will live in a paperless society." — Roger Smith, chair of General Motors, 1986.

Some were folly. As defined by Barbara Tuchman [41, p. 5], folly is error "perceived as counterproductive in its own time".

> "The abdomen, the chest, and the brain will forever be shut from the intrusion of the wise and humane surgeon." — Sir John Eric Erickren, British surgeon to Queen Victoria, 1873.
>
> "Radio has no future. Heavier than air flying machines are impossible. X-rays will prove to be a hoax." — William Thomson (Lord Kelvin), English physicist and inventor, 1899.
>
> "Credit reports are particularly vulnerable ... [as] are billing, payroll, accounting, pension and profit-sharing programs." — Leon A. Kappelman on likely Y2K problems, 1999.

And some were (unintentionally) correct in part or in whole:

> "The problem with television is that people must sit and keep their eyes glued on a screen; the average American family hasn't time for it." — *New York Times*, 1949.
>
> "Where ... the ENIAC is equipped with 18,000 vacuum tubes and weights 30 tons, computers in the future may have only 1,000 vacuum tubes and weigh only 1.5 tons." — *Popular Mechanics*, 1949.

That said some things are clear. Yet unimagined advances in information and communication technology will have radically reworked much of what we do. This, of course, assumes a certain level of optimism—that the ravages of global warming, and concomitant competition for increasingly scarce resources, or horrifying as-yet-unknown pandemics (or a wandering meteorite) have not overwhelmed our race.

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[1] See `http://www.scientificamerican.com/article.cfm?id=computing-in-2165`.

[2] A more useful Scientific American article discusses assessments made in 1913 of the ten most important inventions of the prior quarter century, see `http://www.scientificamerican.com/article.cfm?id=inventions-what-are-the-10-greatest-of-our-time&WT.mc_id=SA_DD_20131021`.

1.1 Some probable verities

We mathematicians are not entirely free agents We benefit from societal advances (Moore's law[3] and fiber-optics, the abolition of mandatory retirement in most of the English speaking world) and are constrained by societal missteps (current intellectual property and copyright law). We swim in the sea of the other sciences [30].

> All professions look bad in the movies ... why should scientists expect to be treated differently?
> —Michael Crichton[4]

Further professionalisation of University adminstration will continue and we will be bombarded with new initiatives. We may look at Khan Academy and MOOCs as new phenomena[5]. But for many decades—in the USA and elsewhere—spending on private-sector in-house tertiary education has been much greater than the total university budget. Yet, it would be wrong to count the University out. As Giamatti [22] and others have noted it is, with all respect to the Vatican, the last surviving successful medieval institution.

We are not the least popular academic profession Roughly twenty-five years ago, my brother and frequent collaborator Peter surveyed other academic disciplines. He discovered that students who complain mightily about calculus professors still prefer the relative certainty of what we teach and assess to the subjectivity of a creative writing course or the rigors of a physics or chemistry laboratory course.

Mathematics will continue to be important Unlike Geography or Latin departments, as long as there are universities [22] in some form or another there will be mathematics departments. What is less clear is whether there will be funds for research in mathematics for its own sake.[6] Mathematics as the language of high technology will thrive (at least in the medium term), but the fate of curiosity-driven research is less clear. One can argue that only for thirty-five years (1945-1980) was mathematical research funded as a good in its own right. It started with Vannevar Bush[7] and ended with the Reagan revolution. .

1.2 Some things will not change much

Certainly there are areas being deeply studied that were not in 1965 but in broad brush—despite Gödel and Turing—the nature of mathematical research has changed surprisingly little over the last half-century [24].[8] We do collaborate more, but most researchers take stunningly little advantage even of tools like *Skype*.

[3] See `http://www.scientificamerican.com/article.cfm?id=moores-law-found-to-apply-beyond-transistors`

[4] Addressing the 1999 AAAS Meetings, as quoted in *Science* of Feb. 19, 1999, p.1111.

[5] See `http://www.newrepublic.com/article/112731/moocs-will-online-education-ruin-university-experience`

[6] See Keith Devlin's gloomy assessment regarding the death of mathematics in `http://edge.org/response-detail/23783`.

[7] Bush who directed the war-time Office of Scientific Research and Development is inarguably the father of NSF, see `http://en.wikipedia.org/wiki/Vannevar_Bush#National_Science_Foundation`.

[8] Much of this subsection is taken from [30].

> ... Like Ol' Man River, mathematics just keeps rolling along and produces at an accelerating rate "*200,000 mathematical theorems of the traditional handcrafted variety ... annually.*" Although sometimes proofs can be mistaken—sometimes spectacularly—and it is a matter of contention as to what exactly a "proof" is—there is absolutely no doubt that the bulk of this output is correct (though probably uninteresting) mathematics.— Richard C. Brown [15, p. 239]

Pressure to publish This is unlikely to abate, and qualitative/quantitative measurements of performance[9] are for the most part fairer than leaving everything to the whim of one's Head of Department. Thirty years ago my career review consisted of a two-line mimeo "*your salary for next year will be ...*" with the relevant number written in by hand. At the same time, it is a great shame that mathematicians have a hard time finding funds to go to conferences just to listen and interact. Csïkszentmihályi [18] writes:

> [C]reativity results from the interaction of a system composed of three elements: a culture that contains symbolic rules, a person who brings novelty into the symbolic domain, and a field of experts who recognize and validate the innovation. All three are necessary for a creative idea, product, or discovery to take place.—Mihály Csíkszentmihályi

We have not paid enough attention to what creativity is and how it is nurtured. Conferences need audiences, and researchers need feedback other than the mandatory "*nice talk*" at the end of a special session. We have all heard distinguished colleagues mutter a stream of criticism during a plenary lecture only to proffer "*I really enjoyed that*" as they pass the lecturer on the way out. A communal view of creativity requires more of the audience. The technologies now available—from *Skype* to *MOOCS*[10]—can both improve and exacerbate the situation. Most of this relies on organization and motivation not machinery and cash, see [12]. Though typically, a lot more cash is also needed than senior academic administrators are wont to assert. None of it entirely removes the need for physical removal to conferences and retreats.

I began University in 1967. My academic life started in the short but wonderful period of a vast infusion of resources for science and mathematics 'after sputnik', see [15]. It now includes the *iPad Kindle* (on which I am listening[11] to a fascinating recent biography of the *Defense Advanced Research Projects Agency*, DARPA). The tyranny of a Bourbaki-dominated curriculum has been largely replaced by the scary grey-literature world of *Wikipedia* and *Google scholar*. And pure research has already been kicked pretty far out of the playing field.

The robustness of who does what and where The impact of taste and bias will not diminish:

> Some subjects can be roughly associated with geographic locations: graph theory is a Canadian subject, singular integrals is an Argentine subject, class field theory an Austrian subject, algebraic topology an American subject, algebraic geometry an Italian subject, special functions a Wisconsin subject, point-set topology a Southern subject, probability a Russian subject.— Gian-Carlo Rota [32, p, 216].

[9] For an incisive analysis of citation metrics in mathematics I thoroughly recommend the relatively recent IMU report and responses at: `http://openaccess.eprints.org/index.php?/archives/417-Citation-Statistics-International-Mathematical-Union-Report.html`.

[10] See `http://en.wikipedia.org/wiki/Massive_open_online_course`.

[11] They will read to you in a friendly if unnatural voice.

This is quite akin to the salt water-fresh water divide in economics[12] in which a University of Chicago centred supply-side prejudice has overwhelmed reasonable Keynesianism for decades. It also shows in the selection of fields represented at Ivy league schools and in those in which one can win Fields medals (most strikingly in 2002 when only two were awarded).

The need to make our subject accessible This is more not less pressing than 50 years ago [15]. Rota [32, p. 216] captures some of the problems:

> It takes an effort that is likely to go unrewarded and unappreciated to write an interesting exposition for the lay public at the cutting edge of mathematics. Most mathematicians (self-destructive and ungrateful wretches that they are, always ready to bite the hand that feeds them) turn their noses at the very thought. Little do they realize that in our science-eat-science world such expositions are the lifeline of mathematics.—Gian-Carlo Rota

The Matthew effect or principle This is sociologist of science Robert Merton's apt dictum that "to those who have shall be given"[13]. And yet the rise of the blogo-sphere makes it all the easier to focus on a few star names. Thus, *Polymath* quickly becomes synonymous with 'Gowers and Tao' (who are two of the really smart and really good guys—those two qualities are not tightly coupled, something our discipline often neglects).

Dieudonné-like shortsightedness A 1970 survey of the Bourbaki project includes [19, p.140]:

> Let us now see what is excluded [from the corpus]. The theory of ordinals and cardinals, universal algebra (you know very well what that is), lattices, non-associative algebra, most general topology, most of topological vector spaces, most of the group theory (finite groups), most of number theory (analytical number theory, among others). The processes of summation and everything that can be called hard analysis-trigonometrical series, interpolation, series of polynomials, etc.; there are many things here; and finally, of course, all applied mathematics.—Jean Dieudonné.

Rota again is spot-on about this sort of attitude in his review of Paul Halmos' mathematical autobiography [33, p.701]:

> Take, for example, the turning point of the author's career, the incident of his leaving the University of Chicago.
>
> ...
>
> Whatever his other merits, Halmos is now regarded as the best expositor of mathematics of his time. His textbooks have had an immense influence on the development of mathematics since the fifties, especially by their influence on mathematicians in their formative years. Halmos's glamor would have been a far sounder asset to the University of Chicago than the deep but dull results of an array of skillful artisans. What triumphed at the time is an idea that still holds sway in mathematics departments today, namely, the simplistic view of mathematics as a linear progression of problems solved and theorems proved, in which any other function that may contribute to the well-being of the field (most significantly, that of exposition) is to be valued roughly on a par with that of a janitor.

[12] See http://en.wikipedia.org/wiki/Saltwater_and_freshwater_economics.

[13] See http://en.wikipedia.org/wiki/Matthew_effect_(sociology)

I have a first-rate younger colleague at a major state University who is a driving force in the intelligent use of computers in mathematics. His software alone should be enough to have him tenured and celebrated. But he has been told that it will count for (less than) nothing in all academic assessments. For his local academic judges he is most assuredly a janitor.

1.3 Some things have changed

From the perspective of pure intellectual progress, I offer eight results we could only have dreamt of having achieved in 1960.

A few good theorems I suggest that the following are fine candidates:

1. Independence of Continuum Hypothesis (Cohen 1963)
2. Luzin Conjecture: a.e. Fourier convergence in L^1 (Carleson 1966)
3. Four Colour Theorem (Appel-Haken 1976, Robertson-Seymour-Thomas 1997)
4. Classification of Finite Simple Groups (Feit-Thompson, Gorenstein, et al. 1955-1995? [39])
5. Fermat's Last Theorem (Wiles-Taylor 1993-94)
6. Poincaré Conjecture (Hamilton-Perelman 2004, ...)
7. Primes in long arithmetic progressions (Green-Tao 2008)
8. The Most Striking Result in Your Own Area

At the other end of the spectrum, I list a sampler of 'digital assistance' tools [3] that have radically changed the prospects for how we will do mathematics in the next fifty years.

A few tools: good, bad and ugly The existence of the current tools, starting with TeX, and platforms (including tablets, smart phones, Google glasses) is the start not the end of a changing praxis (see Section 2.2). Here are some of the many tools I use or personally avoid—like Facebook and Twitter.[14]

1. *Use of Modern Mathematical Computer Packages*: Symbolic, Numeric, Geometric, Graphical, Statistical, ...
2. *Use of More Specialist Packages or General Purpose Languages*: Fortran, C++, Python, CPLEX, GAP, PARI, MAGMA, Cinderella, Blender, ...
3. *Use of Web Applications*: Sloane's Encyclopedia, Inverse Symbolic Calculator, Fractal Explorer, Euclid in Java, Weeks' Topological Games, MathOverflow, Polymath, ...
4. *Use of Web Databases* Google, MathSciNet, ArXiv, JSTOR, Wikipedia, MathWorld, PlanetMath, DLMF, MacTutor, Amazon, Kindle Reader, Nook, Wolfram Alpha, ...

[14] URLS for the bulk of these are most easily found by a web search.

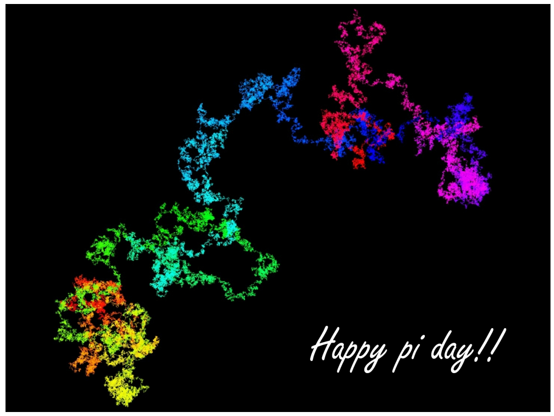
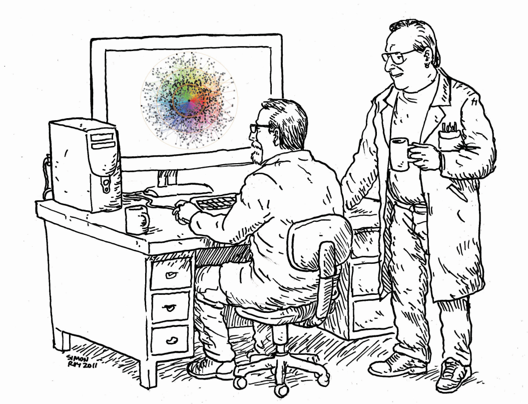


Figure 1: A walk on 200 billion bits of π. Pi day 2015 is special: 3.1415.

All entail data-mining—"exploratory experimentation" and "widening technology" [3]—as is occurring in a big way in pharmacology, astrophysics, biotechnology,.... An example of large-scale data mining in mathematics [1] is given in Figure 1. Clearly the boundaries are vague and getting even vaguer, while judgments of a given tool's quality vary and are context dependent.

For instance, NIST's wonderful *Digital Library of Mathematical Functions*, http://dlmf.nist.gov, is fundamentally still a 19th century handbook in 21st century garb. By contrast INRIA's prototype *Dynamic Dictionary of Mathematical Functions*, http://ddmf.msr-inria.inria.fr/, points to a future in which mathematical knowledge is generated algorithmically and extensibly, rather than from lookup tables.



"Sometimes it is easier to see than to say."

A few things are certain First, I am willing to predict that—as both Felix Browder (in his final address as AMS President) and Tim Gowers (at a meeting of Fields medalists) have said—*the future of mathematics is intimately coupled to computing.* (But then, so is everything else [34].) We should proudly consider ourselves one of *The Sciences of the Artificial* [36].

As a reminder of the youth of computer science as a discipline[15], I recall that the University of

[15] If it is one rather than an amalgam of mathematics, engineering, library science, business and other subjects.

Toronto undergraduate computer science (CS) programme[16] was founded as late as 1971.

In the case of mathematical research, this tight coupling with computational—as opposed to computer—-science presages more emphasis on algorithms and constructive methods [8], visualisation [23], aesthetics [37] and the like; and less focus on abstraction for its own sake. These are trends we can already observe.

The increasing role of collaborative research [9, 12], which can be exaggerated (mathematicians have always needed to talk to each other [29]) may help make the subject more attractive as a career for women, by keeping them in the STEM stream long enough to develop *A passion for science* [42]. As it stands why would bright and articulate young women (including my own three adult daughters and my brother's three) with many options be likely to opt for a STEM Career?

Second, we shall assuredly know vastly more about the working of the brain and in particular neurology [17] of mathematical creativity [25, 35] but whether that will resolve the *mind-body problem* to the satisfaction of philosophers like Thomas Nagel [30] only time will tell. In consequence, one may also hope that 'evidence-based' mathematical education will become the rule not the exception.

Finally, as the economic transformation of South America, Asia, and Africa accelerates, the geographic dispersal of mathematical research is certain to grow; we are a pretty cheap science. As any editor can attest the sheer quantity of submissions originating in Asia is now stunning. One may hope a corresponding increase in quality is not far behind. The rapid development of first-rate universities in places such Hong Kong, Singapore, Korea and China is a positive sign. But building robust cultural traditions of academic enquiry is a long and difficult job.

2 The Proximate Future

2.1 Watson for mathematics: intelligent assistance

Since 'academic plans' and 'technology road-maps' are good for at most about five years, let me reprise one I was asked to write in 2011 under the title "If I had a blank cheque for mathematics" in a series in *The Conversation* on what one could do with infinite resources in the sciences.[17]

Project: Retool IBM Watson for mathematics. **Cost:** $500 million. **Time frame**: 5 years.

Mathematics has many grand challenge problems, but none that can potentially be settled by pouring in more money—unlike the case of the Large Hadron Collider*, the* Square Kilometre Array *or other such projects. Maths is a different beast. But, of course, you're offering me unlimited, free dosh, so I should really think of something.*

Grand challenges in mathematics In his famous 1900 speech *The Problems of Mathematics* David Hilbert listed 23 problems that set the stage for 20th century mathematics. It was a speech full of optimism for mathematics in the coming century and Hilbert felt open (or unsolved) problems were a sign of vitality:

[16] Computer Science as an academic entity dates from 1964 at Toronto, 1965 at Stanford and Carnegie-Mellon, while EE became EECS in 1975 at MIT.

[17] See https://theconversation.edu.au/if-i-had-a-blank-cheque-id-turn-ibms-watson-into-a-maths-genius-1213.

> "The great importance of definite problems for the progress of mathematical science in general ... is undeniable ... [for] as long as a branch of knowledge supplies a surplus of such problems, it maintains its vitality ... every mathematician certainly shares ... the conviction that every mathematical problem is necessarily capable of strict resolution ... we hear within ourselves the constant cry: There is the problem, seek the solution. You can find it through pure thought ..."

Hilbert's problems included the continuum hypothesis, the "well-ordering" of the reals, Goldbach's conjecture, the transcendence of powers of algebraic numbers, the Riemann hypothesis, the extension of Dirichlet's principle and many more.

Many were solved in subsequent decades, and each time it was a major event for mathematics. The *Riemann hypothesis* (which deals with the distribution of prime numbers) remains on a list of seven "third millennium" problems. For the solution of each millennium problem, the *Clay Mathematics Institute* offers—in the spirit of the times—a one million dollar prize.

This prize has already been awarded and refused by Perelman for resolving the Poincaré conjecture. The solution also merited *Science*'s "Breakthrough of the Year" in 2009, the first time mathematics had been so honoured.

And their limitations Certainly, given unlimited moolah, learned groups could be gathered to attack each problem and assisted in various material ways. But targeted research in mathematics has even less history of success than in the other sciences ... which is saying something.

Doron Zeilberger famously said that the Riemann hypothesis is the only piece of mathematics whose proof (i.e., certainty of knowledge) merits $10 billion being spent.

As John McCarthy wrote in *Science* in 1997:

> In 1965 the Russian mathematician Alexander Konrod said 'Chess is the Drosophila [a type of fruit fly] of artificial intelligence.' But computer chess has developed as genetics might have if the geneticists had concentrated their efforts, starting in 1910, on breeding racing Drosophila. We would have some science, but mainly we would have very fast fruit flies.

What a fine illustration of the likely unintended consequences of certain, perhaps most, scientific grand challenges.

Unfortunately, the so-called "curse of exponentiality"—whereby the more difficult a mathematical problem becomes, the challenge of solving it increases exponentially—pervades all computing, and especially mathematics. As a result, many problems—such as most cases of Ramsey's Theorem—will likely be impossible to solve by computer brute force, regardless of advances in technology. Except, just possibly by an unconventional mode of computing such as quantum computing.

Money for nothing But, of course, I must get to the point. You're offering me a blank cheque, so what would I do? A holiday in Greece for two? No, not this time. Here's my manifesto:

Google has transformed mathematical life (as it has with all aspects of life) but is not very good at answering mathematical questions, even if one knows precisely the question to ask and it involves no symbols. In February 2011, *IBM's Watson* computer walloped the best human *Jeopardy!* players in one of the most impressive displays of natural language competence by a machine. I would pour money into developing an enhanced Watson for mathematics and would acquire the whole corpus of maths for its database.[18] Maths ages very well and I am certain we would discover a treasure trove.

Since it's hard to tell where mathematics ends and physics, computer science and other subjects begin, I would be catholic in my acquisitions. Since I am as rich as Croesus, claim no rights, and can buy my way out of trouble, I will not suffer the same court challenges *Google Books* has faced in the past few years.

[18] Sadly, IBM has so far only shown interest in more mundane if lucrative things like 'Watson for oncology'.

I should also pay to develop a comprehensive computation and publishing system with features that allow one to manipulate mathematics while reading it and which ensures published mathematics is rich and multi-textured, allowing for reading at a variety of levels, see [11].

Since I am still in a spending mood, I would endow a mathematical research institute with great collaboration tools [9] for roughly each ten million people on Earth. Such institutes have greatly enhanced research in the countries that can afford and chose to fund them.

Content with my work, I would then rest.

But soon I would realize how much I had left uncovered. For instance, finding the quote by Rota about special functions and Wisconsin. One wants to ask who wrote "Special functions [is] a Wisconsin subject"? With quotes and without the 'is' it was easy. Without quotes or with the 'is' I laboured for many hours. I asked a knowledgeable friend who assured me it was probably not a Rota quote. We are a long way from being able to get satisfactory answers from "What is known about something like the following ... ?" questions. In a 2013 paper [5] the authors describe a variety of promising if limited attempts to build "fingerprints" for extracting knowledge about theorems in a given area.

2.2 Reliability and reproducibility

Conventional wisdom sees computing as the third leg of science, complementing theory and experiment. That metaphor is out-dated. Computing now pervades all of science. Massive computation is often required to reduce and analyse data; simulations are employed in fields as diverse as climate modeling and astrophysics. Unfortunately, scientific computing culture has not kept pace.

Experimental researchers are taught early to keep notebooks or computer logs of every work detail-design, procedures, equipment, raw results, processing techniques, statistical methods of analysis, etc. In contrast, few computational experiments are performed with such care. Typically, there is no record of work-flow, computer hardware and software configuration, or parameter settings. Often source code is lost. While crippling reproducibility of results, these practices ultimately impede the researchers' own productivity.

The state of experimental and computational mathematics Experimental mathematics—application of high-performance computing technology to research questions in pure and applied mathematics, including automatic theorem proving—raises numerous issues of computational reproducibility [3, 8]. It often pushes the bounds in very high precision computation (hundreds or thousands of digits), symbolic computation, graphics and parallel computation. As with all computational science, one should carefully document algorithms, implementation, computer environments, experiments and results.

Mathematics is special Even more emphasis needs to be placed on unique aspects of the discipline:

a) Are precision levels (hundreds or thousands of digits) adequate?

b) What independent consistency checks were employed to validate results?

c) If symbolic manipulation software was employed (e.g., Mathematica or Maple), which version was used? What precise functions were called, what parameter values and environmental settings?

d) Have numeric spot-checks been performed for derived identities etc.?

e) Have symbolic manipulations been validated, say using two different packages?

Such checks are crucial, because even the best symbolic and numeric computation packages have bugs and limitations—often exhibited only during hard computations. It is worth emphasising that it is impossible to be expert with all of the algorithms now embedded in a computer algebra system. Hence, most of us are prone to oscillating between expert and ingenue as we move through different parts of our computationally–assisted research, see [2].

Automatic theorem proving has now achieved some truly impressive results such as fully formalized proofs of the Four color theorem and the Prime number theorem. While such tools currently require great effort, one can anticipate a time in the distant future (by 2065?) when all truly consequential results are so validated.

Interactive theorem proving for mathematics and computation "Interactive theorem proving", a method of formal verification, uses computational proof assistants to construct formal axiomatic proofs of mathematical claims. Examples as of now include coq, Mizar, HOL4, HOL Light, ProofPower-HOL, Isabelle, ACL2, Nuprl, Veritas, and PVS.

Notable theorems such as the Four Color Theorem have been verified in this way, and Thomas Hales's Flyspeck project, using HOL Light and Isabelle, aims to obtain a formal proof of the Kepler conjecture. Each one of these projects produces machine-readable and exchangeable code that can be integrated into other programs. For instance, each formula in the web version of NIST's authoritative *Digital Library of Mathematical Functions* may be downloaded in TeX or MathML (or indeed as a PNG image) and the fragment directly embedded in an article or other code. This dramatically reduces chances of transcription error and other infelicities being introduced.

Reproducibility in computational and experimental mathematics Motivated by such concerns, a workshop on the topic was held in late 2012 at the Institute for Computational and Experimental Research in Mathematics at Brown University. Participants included computer scientists, mathematicians, computational physicists, legal scholars, journal editors and funding agency officials, representing academia, government labs, industry research, and all points in between.

While different types and degrees of reproducible research were discussed, an overwhelming majority agreed the community must move to open research: research using accessible software tools to permit (a) auditing computational procedures, (b) replication and independent verification of results, and (c) extending results or applying methods to new problems Of course, the level of validation should be proportional to the importance of the research and strength of claims made.

Three principal recommendations Coming out of the ICERM workshop, these were:

1. *First, researchers need persuasion that efforts to ensure reproducibility are worthwhile*, leading to increased productivity, less time wasted recovering data or code, and more reliable conversion of results from data files to published papers.

2. *Second, the research system must offer institutional rewards* at every level from departmental decisions to grant funding and journal publication. The current academic and industrial research system places primary emphasis on publication and project results and little on reproducibility. It penalizes those devoting time to developing or just following community standards.

   It is regrettable that software development is often discounted and becomes one of Rota's janitorial tasks. It is typically compared, say, to constructing a telescope, rather than doing real science. Thus, scientists are discouraged from spending time writing or testing code. Sadly, NSF-funded web-projects remain accessible only about a year after funding stops. Researchers are busy running new projects without time or money to preserve the old. Given the ever-increasing importance of computation and software, such attitudes and practices must change.

3. *Finally, standards for peer review must be strengthened.* Editors and reviewers must insist on rigorous verification and validity testing, along with full disclosure of computational details [21]. Some details might be relegated to a website, with assurances this information will persist and remain accessible.

Exceptions exist, such as where proprietary, medical, or other confidentiality issues arise, but authors need to state this upon submission, and reviewers and editors must agree such exceptions are reasonable.

Help is already here Many extant tools help in replicating past results (by the researcher or others). Some ease literate programming and publishing computer code, either as commented code or notebooks. Others capture provenance of a computation or the complete software environment. Version control systems are not new, but current tools facilitate use for collaboration and archiving complete project histories.

Numerical reproducibility itself is a major issue, as is hardware reliability. For some applications, even rare interactions of circuitry with stray subatomic particles matter. The enormous scale, however, of state-of-the-art scientific computations, using tens or hundreds of thousands of processors, presents unprecedented challenges. Not to mention what the future will bring. Forty years ago, concern about proof by appeal to authority, 'von Neumann says', was a common-place philosophical response to the four colour theorem proof's reliance on computation. Without adoption of the sort of recommendations above we will have to settle for '*Mathematica* says we have a proof'.

Open source By 2013, the US had followed the UK, Australia and others in mandating some form of public release of publicly funded research, including data [31]. I hope this brings a cultural change in favour of consistently reproducible computational research. This is further discussed in the ICERM workshop report [40] and Wiki [27] and in a 2013 summary of articles on the "[T]ectonic shifts [that] are happening in the way scientific research is done" commissioned by *Nature*.[19]

Yet, I remain unconvinced that mathematics, with its 'long-tail' use of the literature and very large unfunded publication base, will flourish in a regime driven by the needs only of the 'big' sciences.

3 The Far Future

By this I count things more than fifteen years out. For fifteen years it is fair to extrapolate. We will still have to fill out too many pointless forms. We will have good verbal and gestural control of our computers. The computers will look a lot different. We shall certainly have natural computer voices by then—they already exist in the research lab. If you want your Mom or Tom Cruise to read to you that will be fine. Computer algebra systems will be much better and way faster than now. The 3D *Maple* graphics will look pretty damn good in my *Google* glasses. And so on.

Further out is guess-work. As David Bailey trenchantly wrote to me while I was drafting this piece:

> How could anyone, even ten years ago, have predicted that by now almost everyone above the age of eight would now be on Facebook for minutes (or hours!) of every day?[20] How could anyone have predicted twenty years ago that almost every person above the age of eight would have their own personal supercomputer, systems far more powerful (and useful!) than the Cray supercomputers of the time?
>
> Who could have foreseen, even five years ago, that toddlers would take to smart-phones and tablets with such aplomb that concerned parents would find it necessary to ration the time kids

[19] See https://theconversation.com/open-publishing-is-happening-the-only-question-is-how-13100.

[20] Care to predict if Facebook will be a powerhouse or a footnote in 2025?

spend on these devices? For that matter, how could anyone have anticipated that CO2-induced global warming would loom as an existential threat to civilization?

What will Universities look like? My own guess is: much like now only different. I recently read some science fiction by Connie Willis about time-travelling historians based in Oxford of the 2060s. The history I think was pretty good, but the future had pre-mobile phones, and Oxford seemed entirely unchanged from my stint there over forty years ago.

By 2030, will the immanence of global warming[21] lead to a Nixonian 'war on warming' with a corresponding gutting of pure research and the demise of mathematics as a *ding-an-sich*, or will it spur another sputnik-like golden period? Make both predictions and one will likely be right. As my brother has observed, 50% is a great average in the prognostication business.

Will there be general-purpose quantum computers? I have no basis for well-founded insights, but my gut feeling is 'no'. Largely, because there are very few free lunches. In this—and this alone—I agree with Milton Friedman. As Cris Calude has pointed out, my gut feeling is in accord with the more reasoned analysis (some of it mathematical) of the quantum computing community. But it would be great to be wrong, probably. Working out *what are the implications of quantum computing for mathematics* is an exercise I propose to undertake, probably.[22]

I suggest, perhaps rather hope, that by 2065, the working philosophy of mathematics will have evolved from its current inchoate Platonism [8] to something a little more nuanced. This is not entirely an issue for philosophers. As Edwards [20] has pointed out the distinction between inventions (patentable) and discoveries (not protectable) is basic to current intellectual property law.

One of the epochal events of my childhood as a faculty brat in St. Andrews, Scotland was when C. P. Snow (1905–1980) delivered an immediately controversial 1959 Rede Lecture in Cambridge entitled "The Two Cultures".[23] Snow argued that the breakdown of communication between the "two cultures" of modern society—the sciences and the humanities—was a major obstacle to solving the world's problems—and he had never heard of global warming. In particular, he noted the quality of education was everywhere on the decline. Instancing that many scientists had never read Dickens, while those in the humanities were equally non-conversant with science, he wrote:

> A good many times I have been present at gatherings of people who, by the standards of the traditional culture, are thought highly educated and who have with considerable gusto been expressing their incredulity at the illiteracy of scientists. Once or twice I have been provoked and have asked the company how many of them could describe the Second Law of Thermodynamics, the law of entropy. The response was cold: it was also negative. Yet I was asking something which is about the scientific equivalent of: *'Have you read a work of Shakespeare's?'*

I hope that by 2065, scientific literacy will be ubiquitous and that C. P. Snow's two cultures will both still be thriving, and will be well-aware and appreciative of each other; though I honestly do not expect that to happen.

[21] *Storms of our grandchildren* [26] are too distant to have any political impact.

[22] Interesting work in this direction by Calude and Tadaki can be found at http://www.cs.auckland.ac.nz/research/groups/CDMTCS//researchreports/434CT.pdf.

[23] Subsequently republished in [38].

4 Conclusion

After 60 years with really only two input modalities: first via keyboard and command line computing; and then thirty years later with Apple's adoption of Douglas Engelbart's mouse[24] along with iconic graphical user interfaces (GUI), we are now in a period of rapid change. Speech, touch, gesture, and direct mental control are all either realized or in prospect. As noted, the neurology of the brain has developed in twenty-five years from ignorance to a substantial corpus.

It is barely twenty years since the emergence of the World Wide Web[25] and it would be futile to imagine what interfaces will look like in another twenty.[26] We are still exploring the possibilities suggested by Vannevar Bush in his seminal 1945 essay "As We May Think"[27] and some parts of Leibniz' dream [16] still seem very distant.

In any event, in most of the futures, mathematics will remain important and useful, but those of us who love the subject for its own sake will have to be nimble. We cannot risk leaving the task of looking after the health of our beautiful discipline to others.

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[24] See http://sloan.stanford.edu/mousesite/1968Demo.html. Note that William Gibson was right—the future was already there for Steve Jobs to distribute.

[25] On a slow news day in 2013, *The Washington Post* reposted a 1995 CNN report http://www.washingtonpost.com/blogs/wonkblog/wp/2013/03/29/what-the-internet-looked-like-in-1995/?tid=pm_business_pop.

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