News from New Zealand

BY

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This column didn't appear in the October issue due to a clerical problem in the change of editorship.

1 Scientific and Community News

The latest CDMTCS research reports are (http://www.cs.auckland.ac.nz/staff-cgi-bin/mjd/secondcgi.pl):

438. Y.I. Manin My Life Is not a Conveyor Belt. 05/2013
439. M. Stay and J. Vicary. Bicategorical Semantics for Nondeterministic Computation. 05/2013
440. J.C. Baez and M. Stay. Algorithmic Thermodynamics.05/2013

2 A Dialogue with Jon Borwein about Experimental Mathematics

Professor Jon Borwein, http://www.carma.newcastle.edu.au/jon, is Laureate Professor of Mathematics at the University of Newcastle, Australia.

His interests span pure mathematics (analysis), applied mathematics (optimisation), computational mathematics (numerical and computational analysis), and high performance computing. He has authored many books (several on Experimental Mathematics, the latest with the late Alf van der Poorten, Jeff Shallit, and Wadim Zudilin, Neverending Fractions*, to be published by Cambridge University Press in 2014) and over 400 refereed articles. He is a well-read blogger:* `http://www.carma.newcastle.edu.au/jon/blogs.html`.

Professor Borwein has received many awards including the Chauvenet Prize (1993), Fellowship in the Royal Society of Canada (1994), the American Association for the Advancement of Science (2002) and the Australian Academy of Science (2010), an honorary degree from Limoges (1999), and foreign membership in the Bulgarian Academy of Sciences (2003). He is an ISI highly cited mathematician for the period 1981–1999.

CC: Born in St. Andrews, Scotland, you received the D.Phil. from Oxford University as a Rhodes Scholar at Jesus College. Where is the mathematical gene coming from?

JB: I suppose it comes from a mix of inheritance and environment. My father David is an ex President of the Canadian Mathematical Society and my younger brother Peter is a distinguished analyst and number theorist. David and my mother Bessie were the first in their families to go to University but their fathers were both remarkable men. They both got their families from Lithuania to South Africa before 1930. My maternal grandfather had studied to become an Hasidic rabbi in Lvov before losing his faith. He is still remembered in Hasidic Yeshivas as an example of the danger of free thinking. My paternal grandfather was an engineering student in Glasgow in 1914. He dropped out to join the Royal Flying Corp and the armistice found him in a German POW camp. It was a better war for a Jew to be in a German camp than the next one. My sister is a medical doctor with an undergraduate mathematics degree and her son has just finished a CalTech mathematics degree and is embarked on a mathematics PhD. One of my sons-in-law is a research mathematician. David did not hot house any of us but he did offer a wonderful role model for an academic but engaged life.

Here and below I take the liberty of quoting from my own writings when I feel that I have already produced a good answer to part of the question.

> Both my brother and I ultimately became academic mathematicians and not surprisingly have from time to time mulled over what factors led us to take up the same vocation. I started University determined to be a historian. Neither of us was in any sense “hot-housed". In my undergraduate career I had precisely one lecture from my father,

otherwise he assiduously scheduled classes so as to avoid our meeting. The only exception being a 5 pound bet with his colleagues in St Andrews—also for a large quantity of cheese—that he could teach his six year old son to solve two-by-two simultaneous linear equations by making it into a game. In still recently post-war Britain I was so taught and, while conning neither reason nor rationale, I loved playing this mysterious game and taught my best friend also to play.

From then until I was a third year undergraduate David's (Dad's) role in my education was restrained. I was offered very little overt enrichment Nor in the politically heated and drug laden late sixties would I have brooked much intrusion. But what I did infuse in confrontational discussions at the dinner table over Johnson and later Nixon, and more quietly, as we began to jointly solve problems posed in the MAA Monthly was the timbre of a to-the-manor-born academic a man who nonetheless cared deeply about the external world; a man with a subtle and inexhaustible sense of humor; a man who would happily stay up all night polishing a proof or hunting for the resolution to an obdurately untameable mistake. Above all a man who demonstrated with every fibre that he was doing just what he wanted to be doing, that fads were fads but that scientific knowledge would not ever be entirely deconstructed. And so by 1971 when I graduated from UWO, and went somewhat uncertainly as a Rhodes scholar to Oxford, he had helped me become inescapably a mathematician despite James Sinclair's (Pierre Trudeau's father in law) offer that if I studied PPE (Politics, Philosophy and Economics) in Oxford he would give me a cement factory to manage on my return![1]

CC: You worked in various universities around the world: Dalhousie, Carnegie-Mellon, Waterloo, Simon Fraser and recently, Newcastle, Australia. Please describe the motivation in following this path.

JB: My partner of 43 years and wife of 40 and I met as undergraduate students at Western in London Ontario. We promised each other we would never turn down interesting opportunities. This has left us experience rich—if somewhat cash poor as compared to a stay-in-one place strategy. My trajectory was Dalhousie Math (1974), CMU in OR and in part GSIA (1980), Dalhousie Math and CS (1982), Waterloo C&O (1991), SFU Shrum Chair of Science and then Canada Research Chair in Collaborative Technology (1993), Dalhousie Faculty of CS and CRC in

[1]Excerpted from J. M. Borwein, "The best teacher I ever had: personal reports from highly productive scholars", *Royal Society of Canada Volume*, A. Michalos, ed., Althouse Press (2003). See also `http:///www.carma.newcastle.edu.au/jon/CMS/programme.htm`.

Distributed Environments, then ‘finally’ Newcastle as a Laureate Professor in Pure and Applied Mathematics.

The first job was the only one I applied for in a standard way. The others followed from either unsolicited or solicited invitations. So I can with a straight face claim to have been employed as most kinds of mathematical scientist other than as a statistician and have made only voluntary moves.

CC: You are a mathematician with expertise, interest and many achievements in computer science, especially in high performance computing. Is computation relevant for mathematics?

JB: I think it is crucial to the future of mathematics. Much of what can be discovered without *digital assistance*[2] has been. This is why I have invested so much time in advancing technology-mediated Experimental Mathematics. I have recently written “The Future of Mathematics: 1965 to 2065" for the *MAA Centenary Volume*, 2015.[3] This was a daunting project as one wishes to be stimulating without seeming foolish in ten years time. I concluded that article as follows:

> 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[4] 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.
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> It is barely twenty years since the emergence of the World Wide Web[5] and it would be futile to imagine what interfaces will look like in another twenty.[6] We are still exploring the possibilities suggested by Vannevar Bush in his seminal 1945 essay “As We May Think"[7] and some parts of Leibniz’ dreams till seem very distant.
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> 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

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[2] See my article on Exploratory Experimentation `http://www.ams.org/notices/201110/rtx111001410p.pdf`.

[3] See `http://carma.newcastle.edu.au/jon/future.pdf`.

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

[5] 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`.

[6] A 2013 summary of applets useful in taming scientific literature can be read at `http://blogs.scientificamerican.com/information-culture/2013/03/26/mobile-apps-for-searching-the-scientific-literature/?WT_mc_id=SA_DD_20130326`.

[7] See `http://en.wikipedia.org/wiki/As_We_May_Think`.

will have to be nimble. We cannot risk leaving the task of looking after the health of our beautiful discipline to others.

CC: What is Experimental Mathematics, its objectives, tools and uses?

JB: Let me quote from my 2008 book with Keith Devlin (also the 'math guy' on National Public Radio), *The Computer as Crucible: an Introduction to Experimental Mathematics*, AK Peters, 2008. ISBN-13: 978-1568813431. Japanese edition: (O'Reilly) 2009. This was our best shot at explaining modern experimental mathematics non-technically.

> **What is experimental mathematics?** United States Supreme Court justice Potter Stewart (1915-1985) famously observed in 1964 that, although he was unable to provide a precise definition of pornography, "I know it when I see it." We would say the same is true for experimental mathematics. Nevertheless, we realize that we owe our readers at least an approximate initial definition (of experimental mathematics, that is; on your own for pornography) to get started with, and here it is.
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> Experimental mathematics is the use of a computer to run computationsï£¡ sometimes no more than trial-and-error testsï£¡ to look for patterns, to identify particular numbers and sequences, to gather evidence in support of specific mathematical assertions, assertions that may themselves arise by computational means, including search. Like contemporary chemists and before them the alchemists of old who mix various substances together in a crucible and heat them to a high temperature to see what would happen, today experimental mathematician puts a hopefully potent mix of numbers, formulas, and algorithms into a computer in the hope that something of interest emerges.
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> Had the ancient Greeks (and the other early civilizations who started the mathematics bandwagon) had access to computers, it is likely that the word *experimental* in the phrase "experimental mathematics" would be superfluous; the kinds of activities or processes that make a particular mathematical activity experimental would be viewed simply as mathematics. We say this with some confidence because, if you remove from our initial definition the requirement that a computer be used, what would be left accurately describes what most if not all professional mathematicians spend much of their time doing, and always have done!
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> Many readers, who studied mathematics at high school or university but did not go on to be professional mathematicians, will find that

> last remark surprising. For that is not the (carefully crafted) image of mathematics they were presented with. But take a look at the private notebooks of practically any of the mathematical greats and you will find page after page of trial-and-error experimentation (symbolic or numeric), exploratory calculations, guesses formulated, hypotheses examined (in mathematics, a hypothesis is a guess that doesn't immediately fall flat on its face), etc.
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> The reason this view of mathematics is not common is that you have to look at the private, unpublished (during their career) work of the greats in order to find this stuff (by the bucketful). What you will discover in their published work are precise statements of true facts, established by logical proofs, based upon axioms (which may be, but more often are not, stated in the work).

CC: Please tell us about the software company *MathResources* you co-founded (1994) and the role of interactive software in school and university mathematics.

JB: In the mid-eighties I coauthored a Dictionary of Mathematics. The publisher, Collins, out of ignorance left us "the musical and electronic rights." As my coauthor was in Glasgow and I was in Halifax we worked largely by sending floppy discs across the Atlantic and became the first book set from disc in Europe. This was an ugly experience but left us with a 7,000 word database being used only as a recipe to print. I have written about this at some length in my review of *The Oxford Users' Guide to Mathematics* as a featured *SIAM REVIEW* 48 (3) (2006), 585–594.

When hypertext arrived in the late 80s my friend and computer science/library science colleague Carolyn Waters (now vice President Academic at Dalhousie) started exploring its use. This lead to a prototype of my dictionary with Maple and Mathematica embedded inside it. It was very satisfactory but by 1994 we had given up on persuading mathematics publishes to go down the same road for calculus texts and incorporated as a company run by the third partner Ron Fitzgerald who had worked for 15 years in publishing. The company, *MathResources*, is about to turn twenty. We struggled to find investors before two wonderful angel capitalists took us on.

At that point we wrote a letter to Harper Collins (which had bought Collins) on the instruction of our lawyers. It ran "It is our understanding that we own the electronic rights to the Harper-Collins Dictionary of Mathematics and we intend to exercise those rights." A month later a letter came back from Harper's head office "Dear Sirs, much as we hate to agree with you, you do indeed own the rights ..."

MathResources has paid many salaries and has built many good products and taught me a lot about the issues in building an educational software company, the differences between commercial and research software, large regional development grants, IP issues, taking contract work to stay alive, and much else. While I made no money out of this, and since a falling out with Ron Fitzgerald in 2009 have not had any connection with the company, it was a great experience. I still believe profoundly that good technology and computing need to be an integral part of mathematics education. I find it frustrating that there have been few real successes in so integrating the process.

CC: You have been dubbed "Dr. Pi" after developing, together with brother Peter, extremely fast algorithms that enabled extremely large calculations of π. Why is π so interesting?

JB: For three reasons. (i) It is arguably the only object from the first stratum of mathematical research still being seriously studied today and the underlying mathematics is deep and beautiful; (ii) fast computation of elementary functions relies on being able to fast compute π and this has become a benchmark for various types of computation; (iii) π has some resonance with the general public and so makes for great outreach. I keep an uptodate 'beamer talk' on these matters at `http://carma.newcastle.edu.au/jon/piday.pdf` and Dave Bailey and I write about these matters for the March 2014 (3.14) edition of the MAA Monthly with the title "Pi Day is upon us again and we still do not know if Pi is normal".[8] I find the concrete complexity reduction required for pi computation—reducing the time to multiply trillion digits numbers from eons to hours is key—still really exhilarating.

CC: Quite unusual for a prolific mathematician, you have a vast scientific administration experience: Governor at large of the Mathematical Association of America (2004-07), past president of the Canadian Mathematical Society (2000-02), chair of (the Canadian National Science Library) NRC-CISTI Advisory Board (2000-2003), chair of various of NATO's scientific programs, chair of the International Mathematical Union's Committee on Electronic Information and Communications (2002-08), currently Chair of the Scientific Advisory Committee of the Australian Mathematical Sciences Institute (AMSI). What is the motivation and the reward?

JB: Since High School I always been politically engaged (25 years ago I was briefly treasurer of the Nova Scotia New Democratic Party, which now governs the province) and find such activities both stimulating and a good antidote from the somewhat autistic life-of-the-mind that many good research mathematicians lead. I think I am a pretty good administrator in part because I actually want the meetings to end so I can go back to my real profession reasonably quickly. Quite often I agree to take on jobs after I ask "Who precisely will do this if I do not" and

[8] See `http://carma.newcastle.edu.au/jon/pi-monthly.pdf`.

the answer is unpalatable. The consequence of these experiences is that I have an unusually varied and, I think, nuanced world view. For example, I was at NATO headquarters the night before Kosovo was bombed.

I have been fortunate to occupy research chairs for more than 20 years and view scientific administration and expository writing as part of my obligation that comes with the job. I am now actively blogging often with Dave Bailey for the Conversation and the Huffington Post.[9] I see that as a small attempt to be a public intellectual.

CC: Can you explain one of your favourite mathematical results?

JB: Since I work in several fields I can not pick one. In applied functional analysis my favorite is Ekeland's variational principle (1972) which says that a lower semicontinuous function on a complete metric space can be slightly perturbed to attain its infimum at a point near to any approximate minimum. I was able to produce a smooth version in 1987 which has proven almost as useful as the original and view this as one of my best contributions to nonlinear analysis.[10] In number theory my favourite results are those in Jacobi's amazing *Fundamenta Nova* in which, like Athena from the head of Zeus, the subject of elliptic and theta functions emerges almost fully complete. In 1991 my brother Peter and I were able to find "A cubic counterpart of Jacobi's identity and the AGM," [11] and it was wonderful to be treading so closely in the footsteps of Jacobi and Gauss.

CC: Please tell us about your long collaboration with David Bailey.

JB: David contacted us early in 1986 after reading an article Peter and I wrote on fast computation[12] in SIAM Review. He wanted to implement our algorithms for π and those for elementary functions as part of commissioning the first CRAY 2 at NASA's Ames Lab. This led to a record computation of π, a lot of press, and a 1993 shared Chauvenet prize[13]. It also uncovered subtle hardware and software errors on the CRAY that lead to the algorithms being run as part of CRAY's in-house test suite for many years[14]

We have now written more than 30 joint papers, and several books on experimental mathematics, and since 2009 have blogged together. This is really an

[9] See `http://www.carma.newcastle.edu.au/jon/blogs.html`.

[10] J.M. Borwein and D. Preiss, "A smooth variational principle with applications to subdifferentiability and to differentiability of convex functions," *Trans. Amer. Math. Soc.* 303 (1987), 517–527.

[11] *Trans. Amer. Math. Soc.* 323 (1991), 691–701.

[12] J.M. Borwein and P.B. Borwein, "The arithmetic-geometric mean and fast computation of elementary functions," *SIAM Rev.* 26 (3) (1984), 351–366.

[13] For J.M. Borwein, P.B. Borwein, and D.H. Bailey, "Ramanujan, modular equations and pi or how to compute a billion digits of π," *MAA Monthly* 96 (1989), 201–219

[14] This is one of many cases where 'extreme mathematical computation' has laid bare problems with chips, storage or other computer issues that intensive but more routine tests did not.

exemplary story—when we wrote our SIAM article we were new to the fields involved and did so with some trepidation. It was the first of many times that I have been rewarded for taking some risks. In July 2013 I attended an after dinner talk by Australia's chief Defence Scientist. He listed the mathematical science areas that his portfolio needed people working in. He started with statistics and ended with experimental mathematics. When David and I started working together 'experimental mathematics' was viewed as an oxymoron. I am pleased to think we have help change its status.

CC: Can you comment on today's fraud in science from the historical perspective, for example given by the book *Free Radicals* book `http://www.freeradicalsbook.com` by Michael Brooks?

JB: I was not aware of the book but from the preface I largely agree. My *experimental methodology* is also a call for honesty in how we describe what we do—to our colleagues, our students and the public. We are humans with all the foibles, vices and impulses that that implies. Science is one of the most successful human ventures. But as Richard Feynman neatly put it, somewhat less sensationally, in his Nobel acceptance lecture:

> We have a habit in writing articles published in scientific journals to make the work as finished as possible, to cover up all the tracks, to not worry about the blind alleys or describe how you had the wrong idea first, and so on. So there isn't any place to publish, in a dignified manner, what you actually did in order to get to do the work.

Having subsequently read Brooks' book, I think he exaggerates for effect. And—as some reviewers have noted—he indulges in (really engaging) anecdotal cherry-picking to knock down a straw-man that no scientist truly believes exists. There may be more malarkey among 'paradigm' shifter's, in Kuhn's sense. But, for every free radical like Einstein, Maxwell or Kary Mullis, there is a Darwin spending eight years on barnacles or worms ("its dogged that does it") or an Andrew Wiles refusing relentlessly to give up on 'normal science' in his pursuit of Fermat's last theorem. Let me add that I think Frans de Waal's fine 2013 book *The Bonobo and the Atheist: In Search of Humanism among the Primates*[15] offers *inter alia* a better accounting of scientific misbehaviour though it is far from his central theme.

I am engaged in current related attempts for greater openness and reproducibility in computational science. This is quite urgent as is described in two recent

[15] See `http://www.scientificamerican.com/article.cfm?id=mind-reviews-bonobo-and-atheist`.

articles.[16]

CC: Brooks' list may look cherry-picked, but it contains cold details about fraud committed by a long list of iconic scientists, a list which starts with Galileo, Newton, Maxwell, Einstein and ends with more Nobel laureates. Mathematicians don't get into this list as fraud comes mostly in experimental science...

JB: Well those folks are described as 'frauds' but that is not entirely fair. They are by Brooks's own description largely guilty of selectively interpreting their data, bashing their critics, or sweeping away unpleasant artefacts. Were they not ultimately proven right the story would look different. Barry Marshall (the Australian MD and 2010 Nobelist who discovered the bacterial basis of most stomach ulcers) is beautifully described. He ended up experimenting on himself (which is actually not taboo) and has now cured millions of diseases. Here is a committed free radical but no fraud.

On the 50th Anniversary of Keynes' death. Sir Alec Cairncross (in *The Economist*, April 20, 1996) wrote "Keynes distrusted intellectual rigour of the Ricardian type as likely to get in the way of original thinking and saw that it was not uncommon to hit on a valid conclusion before finding a logical path to it". Many of Brooks's best descriptions discuss the consequence (from electro-magnetism to prions) of such conviction. There are very few frauds described in his book. In our recent article, "Scientific fraud, sloppy science—yes, they happen"[17], Bailey and I wrote:

> Fraud. It's an ugly word, an arresting word. As with "cheating" it comes loaded with negative connotations, but can potentially lead to far greater penalties and consequences. And yet fraud in science is not unheard of. The world of economics was shaken two weeks ago[18] by the revelation that a hugely influential paper [by Reinhart and Rogoff] and accompanying book in the field of macroeconomics is in error, the result of a faulty Excel spreadsheet and other mistakes—all of which could have been found had the authors simply been more open with their data.
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> Yet experimental error and lack of reproducibility have dogged scientific research for decades. Recall the case of N-rays (supposedly

[16]"'Setting the Default to Reproducible' in Computational Science Research," *SIAM News*, vol. 46, no. 5 (June 2013), 4–6. Also D.H. Bailey, J.M. Borwein and Victoria Stodden, "Set the default to open," *Notices of the AMS* 60 (6) (2013), 679–680.

[17]`https://theconversation.com/scientific-fraud-sloppy-science-yes-they-happen-13948`.

[18]See `https://theconversation.com/the-reinhart-rogoff-error-or-how-not-to-excel-at-economics-13646`.

a new form of radiation) in 1903; clever Hans, the horse who seemingly could perform arithmetic until exposed in 1907; and the claims of cold fusion in 1989.[19]

> Medicine and the social sciences are particularly prone to bias, because the observer (presumably a white-coated scientist) cannot so easily be completely removed from his or her subject.

We went on to mention two other famous examples ignored by Brooks:

> Of even greater concern are proliferating cases of outright fraud. The “discovery" of the Piltdown man in 1912, celebrated as the most important early human remain ever found in England, was only exposed as a deliberate fraud in 1953. An equally famous though more ambiguous case is that of psychologist and statistician Sir Cyril Burt (1883-1971). Burt’s highly influential early work on the heritability of IQ was called into question after his death.
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> After it was discovered that all his records had been burnt, inspection of his later papers left little doubt that much of his data was fraudulent—even though the results may well not have been.

Perhaps the most egregious case in the past few years is the fraud perpetrated by Diederik Stapel.

> Stapel is/was a prominent social psychologist in the Netherlands who, as a November 2012 report has confirmed, committed fraud in at least 55 of his papers, as well as in ten PhD dissertations written primarily by his students.
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> (Those students have largely been exonerated; though it is odd they did not find it curious that they were not allowed to handle their own data, as was apparently the case.)
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> A 2012 analysis by a committee at Tilburg University found the problems illustrated by the Stapel case go far beyond a single “bad apple" in the field.
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> Instead, the committee found a “a general culture of careless, selective and uncritical handling of research and data" in the field of social psychology:
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>> [F]rom the bottom to the top there was a general neglect of fundamental scientific standards and methodological requirements.

[19] None of these were mentioned by Brooks.

> The Tilburg committee faulted not only Stapel's peers, but also "editors and reviewers of international journals".
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> In a private letter now making the rounds, which we have seen, the 2002 Nobel-winning behavioural economist Daniel Kahneman has implored social psychologists to clean up their act to avoid a potential "train wreck".

We need mechanisms to catch folks like Stapel and behaviour like that of Reinhard and Rogoff, not to excoriate Maxwell's and Einstein's.

CC: Increasingly many offenders—from politicians and top ranking military to businessmen—not only show no remorse, but try to keep former positions and even to capitalise on their experiences in the wrong. High-ranked politicians in EU—including a head of state, a prime-minister and a few ministers—have been recently exposed to have plagiarised their PhD Theses. Some resigned, but not all (e.g. a prime-minister). It seems that "dying of shame" is morphing into "the death of shame". Do we see this trend also in research and academia? If, yes, to what extent?

JB: My impression is that in academia one can survive almost anything except unambiguously forging academic credentials or research—if one has a thick enough skin. Amusingly, a mathematician acquaintance of mine is married to the new (and presumably scandal free) German education minister. In Germany, almost uniquely, there is great prestige in public life to being called 'Doktor'. This explains some of the recent European scandals. By contrast, at a leadership competition twenty five years ago for leadership of the Canadian NDP (currently the official Federal opposition) six of the seven or eight candidates had doctorates and they all actively avoided mentioning the fact. I have written recently that plagiarism is a symptom not a disease:

> Plagiarism is a bit like the weather. Everybody talks about the topic but nobody does anything much about it. Sure students are admonished not to and punished when caught; but that is about it, other than out-sourcing much of the issue to money-making outfits like turnitin.com. There are many reasons for this and I intend to discuss a few of them.[20]

CC: "If enough eminent people stand together to condemn a controversial practice, will that make it stop?" This is the first sentence of Nature News Blog `http://blogs.nature.com/news/2013/05/`

[20] See `http://experimentalmath.info/blog/2013/05/plagiarism-is-a-symptom-not-a-disease/`.

scientists-join-journal-editors-to-fight-impact-factor-abuse.html from 13 May 2013 regarding the San Francisco Declaration on Research Assessment http://am.ascb.org/dora. What is your opinion?

JB: I am well aware of the background to this. It can not hurt and the goal is admirable. That said, after twenty years editing book series for Wiley and Springer and a great deal of diverse journal editing experience (I am currently co-editor in chief of the Journal of the Australian Mathematical Society), I am convinced that most mathematicians are deeply uninterested in most matters to do with publishing. I spent ten years on the International Mathematical Union's electronic communications committee. We talked to everyone we could about copyright, open access, abuse of metrics and much else. But the truth is most authors have no idea who published their most recent article unless — like the American Mathematical Society — it is in the name of the journal. In the decade after the AMS changed its rules to allow authors to keep copyright and just give adequate permission to publish, only a handful exercised that right.

Likewise. Tim Gower's recent crusade against Elsevier struck me as somewhat ill-conceived. The 'big E' is by no means the worst of the commercial publishers. Finally, it is my impression that the impact factor is not used as substantially in mathematics and computing as in the hard sciences. Mathematics is often forced into a model that works for neuroscience or astrophysics, where funding models, journal culture and much else are very different and poorly suited to our needs.

CC: Many thanks.