Three year plan for my future research

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Abstract — **Cites of the form [1] refer to the references at the end; of the form [#34] refer to my online list of NECI tech reports** http://www.neci.nj.nec.com/homepages/wds/works.html.

1 Non-plan

My first reaction to some people's demand for a "3 year plan," was "see my separate 'retrospective sketch of my research' and expect more of the same, only different!"

My experience has been that in mathematics it is not desirable or often feasible to make detailed multiyear plans. New developments (both results and failures, and also the appearance of collaborators) are too unpredictable. The Andrew Wiles approach of working on a fixed plan for 7 years, is, I think, not often realistic, although it sure is nice on the rare occasions when somebody manages it. In this respect mathematics differs from some other areas of science (e.g. where one has to buy, and justify buying, expensive equipment).

Furthermore, I want to be a generalist, a cross-disciplinarian, rather than being a specialist like Wiles. I think a lot of the very best mathematicians had that attitude (Poincare, Von Neumann, Weyl, Hilbert), and I think it is inherent in the nature of mathematics as a unifying theme or toolset for all science, a connecting force between various areas, that the generalist approach is inherently a valuable one. I believe that due to socio-psychological forces, the balance is currently too tilted in favor of the specialists.

2 Plan

Nevertheless, here is a possible plan.

I've been working for some time in the area of "physics meets computer science." That is, what do the laws of physics say about fundamental limits on computer power? What do computer science ideas have to say about physics? I have quite a few papers or tech reports in this area. For example, my fundamental limits on information flux and bit density [#10, #43, and a paper on info flux in progress]. I also have quite a few more manuscripts not finished yet. For example, here are some results which you shouldn't bank on until I've finished nailing all the details and writing them up, but I'm pretty sure I've got all the ingredients:

1. A new class of quantum error correcting codes.
2. A class of computational problems unsolvable by, and/or provably hard to solve by, computers which are physical.
3. New derivation of quantum "master equations" governing decoherence.
4. A new study/survey of "mechanisms" made of rigid objects.
5. A formulation and proof of "Church's weak thesis" in quantum mechanics, i.e. loosely put, "quantum mechanics is decidable."

Of these, item 5 is the hardest and most interesting result, and I think also a bit of a blockbuster (surely one of the most interesting and basic possible questions one can ask about quantum mechanics, or any physical theory, is "is it decidable?" and it is high time such questions were answered).

Re this and also item 2, let me say this. Before the advent of *computer science* it was hard to compare the quality of mathematical results, and afterward it became easier. For example, I have a theorem and you have a theorem. Which is better? Well, many feel that if your theorem is a nonconstructive existence proof, but I have an algorithmic existence proof, mine is better. But if you have an algorithm with better computational complexity than my algorithm, yours is better[1]. So one of the great virtues of computer science is the fact that it can serve as an *organizing principle* in mathematics: it's a way to compare results, and a way to point the direction to look in to get better results, and a way to spot which results may be weak and susceptible to improvement. Now, the same role can and should be played by computer and information science in *physics*. The whole goal of physics is to predict Nature. But this is a computational problem. Physical theories that lead to algorithms of lesser computational complexity, are better. Physicists have essentially ignored this and buried their heads in the sand, so far.

So a good multi-year plan would be: (1) to write a book about this area, (2) to finish off the stuff I just listed, (3) whatever else comes up as a spinoff.

[1] Also there is the notion that "simpler" proofs and ideas are better, and information theory can sometimes give a quantitative meaning to "simpler."

I happen to have a feeler from a publisher about writing a book, and I have been trying to write lecture notes (see my web subpage about the course I taught at Princeton for a lot of pages of my lecture notes) with the idea that they will form the basis for the book. This is a hard book to write because of a high fraction of original work and also the fact that other people are advancing this field so much recently too (e.g. quantum computing [3][4]). If you check out those of my lecture notes on line, you'll see I have quite a bit there, but also quite a bit more needs to be done to turn them into a book, and I suspect that if I were to teach this course again, I that would provide enough driving force to get me most of the way there.

3 Other possible plans lurking in the background

Check out my short technical note [#33] "Cruel and Unusual Behavior of the Riemann Zeta Function." I'm not sure whether to be annoyed or gratified that some similar ideas were generated recently by some other people and appeared in the last year in Journal of Number Theory. This is an apparently new, and simple, approach to trying to prove the Riemann Hypothesis (RH).

I think this approach seems to have a lot of promise, and in fact I know a good deal more than I said in that report. (For example, I know how to write recurrences among the coefficients c_n, and know various ways to express them in various more-or-less closed forms. And there are surely more ways.) Even apparently very weak asymptotic statements about the coefficients c_n in that report, would suffice to prove RH.

I doubt I'm going to prove RH, but this is worth looking into, and *any* asymptotic result about the c_n is a partial result toward RH, even if it is not a strong enough asymptotic result to prove RH.

Also consider [#34] "Molecular Tinkertoys and How to Assemble Them." This cries out for a collaboration with computational and synthetic chemists. But for socio-financial-political reasons it is hard for me to get one. That by itself could be another multiyear plan.

References

[1] Tom M. Apostol: *Introduction to analytic number theory*, (Corr. 4th print) Springer 1995.

[2] A.Ivic: *The Riemann zeta-function*, Wiley 1985.

[3] John Preskill: *Reliable quantum computers*, Proc. Roy. Soc. Lond. A 452, 1969 (8 Jan 1998) 385-410. (Special issue of PRS A on "Quantum Coherence and Decoherence.")

[4] Peter W. Shor: *Polynomial time algorithms for prime factorization and discrete logarithms on a quantum computer*, SIAM J. Computing 26 (1997) 1484-1509.