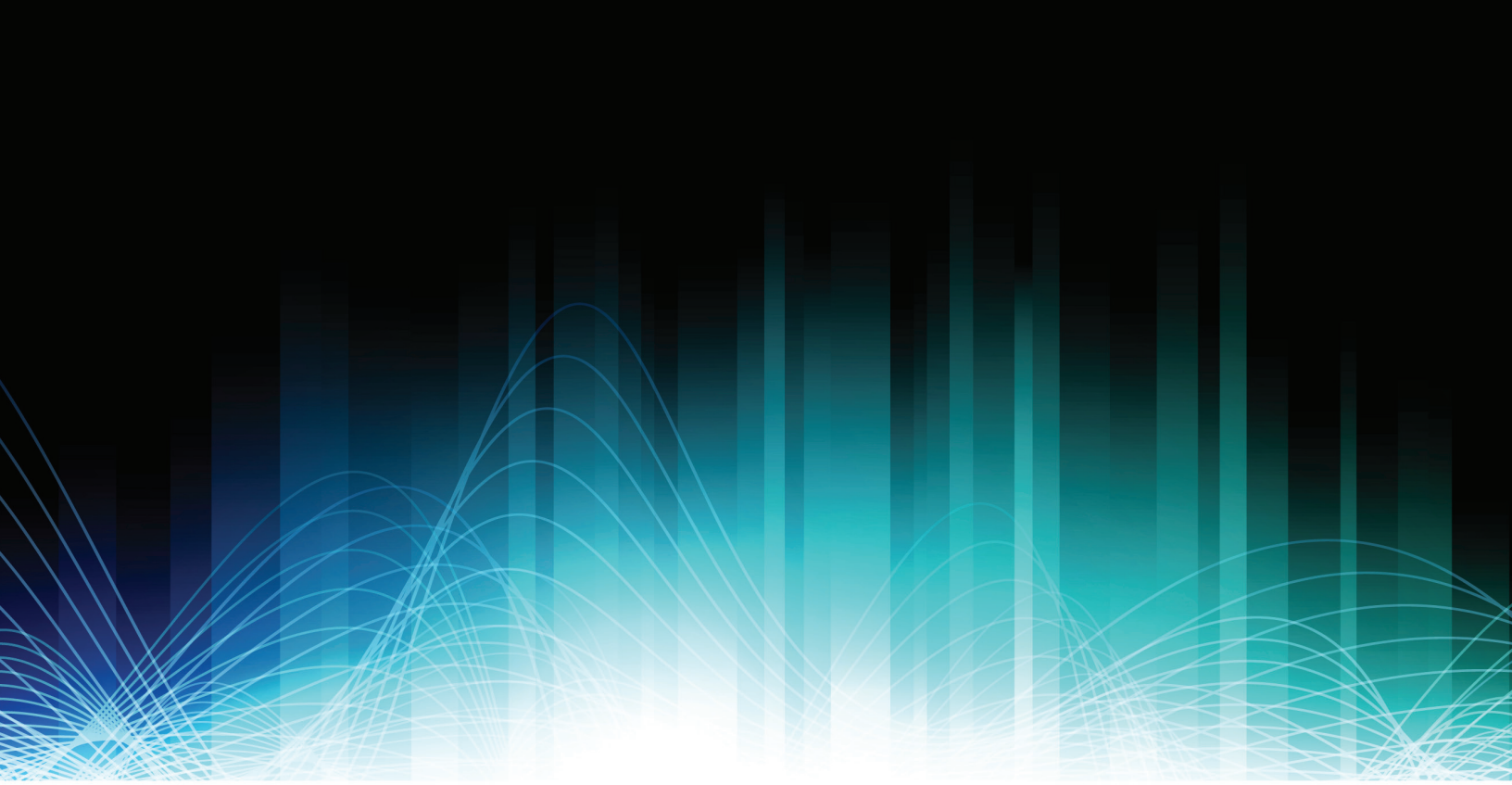
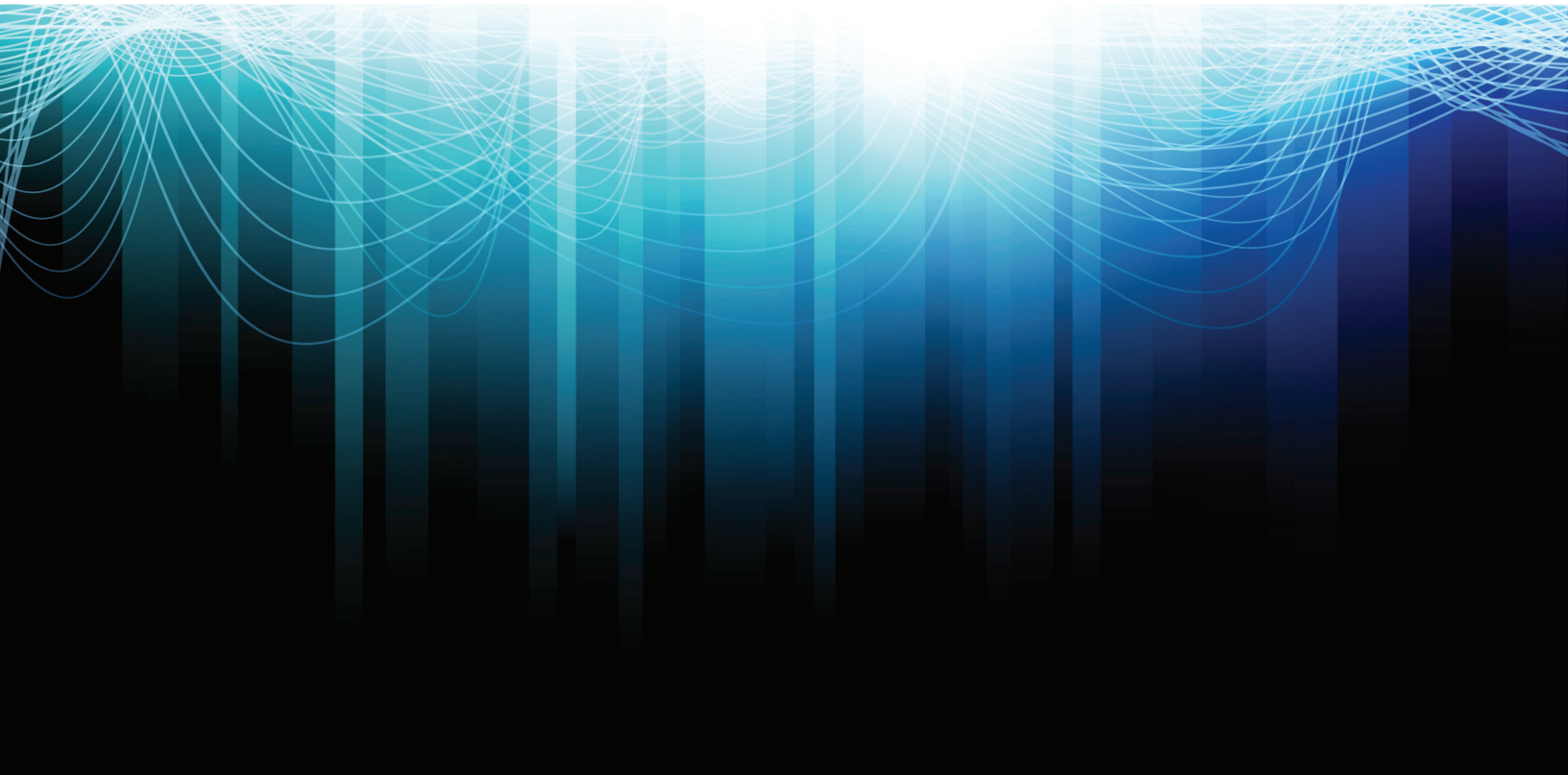


From Monopoles to Fault Tolerant Quantum Computation:

A mentor's guide to the universe



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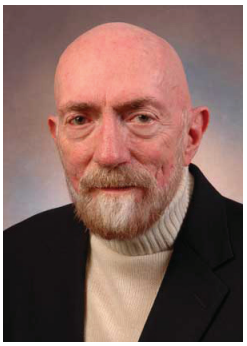
Dear John,

Two years ago, during one of our lunches outside Chandler cafeteria at Caltech, I asked you to name your greatest accomplishment in science. You looked taken aback by my question, while others jumped in to mention your work on everything from monopoles and black-hole complementarity to fault-tolerant quantum computation and topological entanglement entropy. Any one of these ideas would have been enough to answer my question, but none of them seemed quite right. In my mind, you have always been a figure that is larger than life, somehow combing a graceful and unassuming self with a fiery treasure trove of knowledge. Like a great tree with many branches, you have chosen to give of yourself freely, so that others will grow to be strong like you one day.

I hope that, in this book, you find a small token of our appreciation as your colleagues, collaborators and students throughout your incredible career. Still, since you are only turning 60 this year, you should prepare to put up with us for a long time to come.

Sincerely yours,

Spiros



Kip Thorne

California Institute of Technology

The Feynman Professor of Theoretical Physics

Dear John,

What an intellectually stimulating and enjoyable colleague you have been for me, over the three decades since your arrival at Caltech!

It was fortunate for me that you arrived when you did. Just four and a half years later, in 1988, we lost Richard Feynman. Richard had been the intellect against whom I measured all others; the person to whom I turned for vetting and advice on those rare occasions when I thought I had a good physics idea outside classical general relativity. By 1988, I knew that among all my Caltech colleagues, you, for me, were the only viable replacement.

(And so naturally, when I stepped down from the Feynman Professorship 20 years after that, it seemed obvious that this professorship should be yours.)

As I struggled in the 1980s to understand the entropy of black holes, and in the early '90s, whether vacuum fluctuations can always destroy time machines, and in the late '90s, issues in quantum measurement connected to LIGO, it was to you that I came to try out ideas.

I never really knew you well, personally, until our memorable trip together in 1991, to London and Cambridge, to try to recruit Stephen Hawking for the Caltech faculty. Our conversations (as we walked the streets of London and as we waited in Cambridge for Stephen to meet with us, and to make sentences) led me to appreciate you as a person and a friend — moving beyond my view of you as an intellectual giant.

We failed in our wooing, but we managed to catalyze Stephen's subsequent, near-annual visits to Caltech. And those visits triggered our bets with him, on cosmic censorship (two) and information loss. I took great pleasure in emcee-ing the press conference in Dublin, where Stephen conceded the information loss bet to you. I've still not conceded, myself. You are probably right, but until all the experts (I'm not one!) whom I respect throw in the towel, I'll hang back. Maybe that will require waiting for Bill Unruh and Bob Wald to die.

Throughout my Caltech career, I took great pride in my teaching and mentoring. It was obvious to me that you were one of the very few who could match or exceed me in this. I was especially impressed the year you had five grad students completing their PhDs, each working in a different subfield of physics! When I stepped down from the Feynman Professorship, your intellectual, research, mentoring, and teaching prowess made you the obvious one to succeed me—obvious not just to me but to everyone around us.

Attached to this letter is our one joint publication — quite fittingly: our Foreword to The Feynman Lectures on Gravitation (1995). Writing that Foreword with you was fun, and you taught me a bunch. Also great fun have been our joint performances — at Hawking dinners and lectures, and above all at TedxCaltech, with Stephen and Rives. I am re-watching that now, on YouTube, as I write; it still brings grins to my face. Your script was great, and I even learned to ham it up — of course not as well as you, but I didn't do bad.

In a more serious vein, our decades together on the Physics Faculty Staffing Committee were—well, not pleasurable, but certainly satisfying. I think you carried more weight than anyone else on the committee; you commanded the greatest respect. On rare occasion you and I disagreed strongly, but we and the other committee members always worked toward the same goal: securing the greatness of Physics at Caltech for decades to come, and I think we succeeded.

As I moved methodically toward retirement, so as to ramp up a new career, I unloaded things on you. I know you were already overburdened, but you didn't complain. Oversight of the project to produce electronic editions of The Feynman Lectures on Physics (and the curious and temperamental characters that entails); the care and feeding of Mike Scott: I'm so glad that those are now yours, and not mine. Thank you!

And more importantly, I thank you for your intellectual leadership in pushing Caltech Physics forward into the twenty-first century. That is a major factor in allowing me to let go, and leave Caltech to younger generations. You may be turning sixty, but you will have many more years of leadership, and of joy in research, mentoring and teaching. I'm sure you will continue to enjoy them and will continue keeping Caltech great.

Your friend,

Jeff Kimble

California Institute of Technology

Dear John,

Please accept my best wishes as you complete your 60th lap around El Sol. Although our friendship is relatively young compared to your ties with wonderful colleagues from theory land, it is an association that has been and remains most important to me. Quite apart from your heroic and historic achievements in science, I have long considered you to be a shining example of the very best of the proudest traditions of science. Our community is all the better because of the standards for excellence and integrity that you have set over your career. Somehow you have managed all this with the most generous of spirits while amassing a staggering set of achievements that are by now pervasive across physics. I certainly do not understand most of your work in detail, but do greatly value the depth and breadth of your understanding. Your willingness to work graciously to fill the vast voids of my knowledge inspires me that there is hope even for Texans.

So what publications have we authored together? Sadly, not a quantum theory of gravity nor an experiment to detect a new essence for atoms and photons. But together we have been a daunting force of nature when it comes to the grand challenge of writing proposals, as the attached documents for the IQI and IQIM will remind you. Your leadership of IQI has changed the world for QIS and, in particular, for my research. IQI has enabled a new future for scientists worldwide. Without you and your IQI, there would be no IQIM. Thank you, John.

Certainly six decades is a significant milestone, and as always, I continue to follow your lead towards the future. But clearly as master of time ticking and black holes, two billion seconds will be cause for yet another celebration of your life, and maybe by then, measured by an optical clock with squeezed atoms!

Please accept best wishes from Midge and me to you and your family on this happy day for all of us.

With highest regards,

H. Jeff Kimble
William L. Valentine Professor
and Professor of Physics

Jim Cline

McGill University

Happy birthday John!

I'm glad this old paper is finally getting put to one more good use, the previous one having been to launch me into publishing, following a year of going nowhere with the first research problem you had given to me: the lattice fermion doubling problem. (Maybe I should try assigning more challenging projects to my new students!) It was great that you suggested this second, less important problem, of trying to pinpoint the flaw in Patrascioiu's argument against asymptotic freedom of the $O(N)$ nonlinear σ model in $d = 2$, since it enabled me to use my familiarity with lattice computations to actually obtain a concrete result.

Perhaps you recall there was a little drama connected with this forgotten paper. David Politzer didn't seem to have much regard for me at first (I will always remember the time I asked him how he preferred to be addressed and he said, "you can call me shithead"), and it was very stressful for me when I discovered that he and George Siopsis were working together on the same project as me and weren't looking for another collaborator. I appreciate that you wagered in my favor (so I later heard) when bets started going around as to who would get to the bottom of it first. My paper got published, perhaps thanks to Howard Georgi's cavalier refereeing policy in those days. Then Patrascioiu mailed letters to departments around the country condemning my work and I was worried because he might have had a valid criticism. You comforted me by taking a copy of my paper and tossing it in the trash, symbolizing that it wasn't very important in the big picture.

Even though we never managed to produce any collaborative work, you also influenced my later development, by getting us all excited about Sidney Coleman's wormhole mechanism for the cosmological constant problem in a seminar you gave in advance of his pre-print, thanks to your Harvard connections. That was my ticket out of string theory and into phenomenology (Λ is a measured quantity after all) during the summer following my thesis defense. I'm still rather proud of my PRL "Can $\theta_{QCD} = \pi$? which was inspired by your finding (with Sandip Trivedi and Mark Wise) that $\theta_{QCD} = \pi$ was the other prediction of Coleman's theory. It just shows that the "benign neglect" supervision philosophy can be quite effective, in the hands of a master.

Best wishes,

Jim Cline

Elias Kiritsis

Crete Center for Theoretical Physics

I take the opportunity, on the occasion of the celebration of John Preskill's 60th birthday to recall my contact with him as my PhD advisor and his impact on me.

My first contact with John coming to Caltech was that I (together with many of my fellow students) took his advanced quantum field theory course. John's knowledge, pedagogical skills and high standards were sufficient to enchant most of the graduate students taking the course, including myself.

Having accepted me as a graduate student John was never pressing me to choose a subject of his choice. He offered to do this if I wanted to and indeed I asked him for a suggestion although I had at the same time some ideas of my own for research.

The project I tried eventually did not work out as I was not very motivated to pursue it, and John did not press me.

I eventually worked on my own problems during my PhD and in particular I have not published a paper with John, as happened with many of John's students at the time. John however was always willing to discuss with me when I needed it, and on whatever subject I would bring up to him. He did this to all his many other students, a fact that I find remarkable and self-effacing.

He influenced importantly my thinking during some of the work I have done as a graduate student. I especially mention the first research paper I have written, A Topological Investigation Of The Quantum Adiabatic Phase. published in Commun.Math.Phys. 111 (1987) 417 which owes a lot to indirect and direct influence of his thinking on me.

As I realized more recently, this paper contains the starting point of the topological classifications of free-fermion Hamiltonians (in this case real and complex ones) that was eventually extended to the complete current topological classification of condensed matter hamiltonians with various symmetries.

I have interacted very little scientifically with John after I left Caltech. His influence on me, during these formative years was however essential and I think I will carry his mark as long as I can function.

Happy birthday John!

Elias Kiritsis

c. 1987

Alexios P. Polychronakos, PhD

Remembering John's early "heroic" days at Caltech

In the fall of 1983, Caltech was a hard place for budding high energy theorists. The particle theory group consisted of the old legends, still exciting but reluctant to take students, and one brilliantly productive but not yet canonized senior research associate (the other John). So it was a turning point when an infusion of new blood suddenly came in: John Preskill and Mark Wise joined the department. The excitement and relief among many of us were palpable. It was no surprise that all aspiring high energy theorists flocked to the courses John and Mark taught and clamored to become their students.

Pretty soon, John had an impressive number of graduate students. How this happened is an interesting story. Mark was more phenomenological, while John was more theoretical, and therefore 'sexier' (nobody said physics graduate students in the 80s were particularly mature or realistic). Moreover, John was a nice guy and didn't know how to say 'No'. If a student looked reasonably good and was eager to work with him, how could John refuse? In the next couple of years more joined and John kept amassing us, until he had a harem of two full houses of students! Names that I remember were Ian Angus, Milan Mijic, Brian Warr, Jim Cline, Sandip Trivedi, Jiyu Feng, Olivier Espinoza, and the Greek gang, George Siopsis, Elias Kiritsis and myself.

Now we all know that unless you run a sizable lab, and have a whip handy, it is virtually impossible to supervise 10 students. How did John manage? With some difficulty, I imagine. But here is where John's exceptional intellectual and moral qualities really played out.

First, John gave us free reign in the topic of our research. He would suggest a general area or problem, even a few papers to read, but the rest was up to us. No 'assigned' projects with timetables, day-to-day monitoring of progress, or specific calculations to perform and results to work out. That can be hard for a student. It is easier to be taken by the hand, given a concrete problem to work on and guided every step of the way until things pan out. But it can also be liberating, and was certainly character-building: if you managed, you knew you could swim on your own.

Second, John was always there to help us with advice and ideas. His knowledge of physics was deep and broad, almost encyclopedic; his intuitions were nontrivial and useful; and his advice was more aspirational than categorical: a suggestion of a direction or an indication of a possibility, followed by his famous W-smile, and you were good to go. So everybody wanted a piece of his mind and, with so many students, it became almost a logistics problem. It was amusing how there was always somebody stalking him in the corridor ready to catch him right after the previous guy left his office. Thinking back, that must have been exasperating for John. I can imagine how someone else would have become curt, dismissive or imperious in such a situation. Yet somehow John managed to remain calm and receptive.

Finally, John was fair and honest. Rather than accumulating publications through his students, he would refuse to add his name to our papers if he didn't feel that he had done enough of the work, even though he often fully deserved the credit. And that was before he had tenure, when a fat publication list could be a crucial asset. I probably speak for all his students at the time that John set for us standards of ethics and personal pride that were well above the grab-all-you-can instincts of our field and gave us moral ballast in dealing with the cut-throat competition of our academic marketplace.

As a result, John was a cherished and popular mentor, a liked and beloved figure even as we had to share him. His moral and emotional influence on us was remarkable. A few words of encouragement and optimism from him were enough to put anyone of us demoralized from a setback back into good spirits. And just a hint of a (smiling) reproach was powerful enough to kick anyone of us stagnating because of lack of motivation or sheer laziness back into working mode. John occupied in us the narrow space between respect and familiarity and had the quality to keep his position there without drifting either way. He was thoughtful, but could be outspoken.

He was gentle and nurturing, but would squarely award praise and preference based on performance and quality. He would maintain an effortless dignity, but could take a joke.

I remember the time that we decided to spice up one of his final lectures in the Elementary Particle Theory class with a prank joke. We all sat in the first row, with legs crossed in the same direction, and the understanding that when John would say any word starting with "p" we would all switch and cross legs in the other direction, in a kind of sitting synchronized dance. John lectured on lattice gauge theory and, for a while, no p-word was uttered. Whether he noticed the strangeness of us sitting all in front with aligned crossed legs was not clear. And then, "plaquette" came up, and we switched. The look in John's face was priceless! He kept lecturing and the next p-switch surely gave him the message that something seriously demented was going on with his class. Yet we kept going, until he gave us the sentence "...we perform the path integration plaquette by plaquette..." which set us in a wild leg-tossing sequence that quickly devolved into chaos. After that we dropped the game. At the end of the lecture, when we confessed our little trick and told him that he really got us with his deliberate string of p's, John claimed that he actually never figured it out. I wouldn't be surprised if, indeed, John's power of intuition had subconsciously led him to string phrases that maximized and unhinged our weird behavior. That, or he was literally pulling our leg.

After we graduated we all drifted in various directions, gradually diffusing into the phase space of geography and careers. But none made a more dramatic shift than John himself. The field of quantum computation was just budding at the time and could have been easily dismissed as a fad or a meaningless curiosity. Yet, with characteristic foresight and boldness, John embraced it, studied it, learned it, worked on it and established himself as one of the leading gurus in the field. To some of us this transformation may have been disconcerting: our John, the magician of quantum fields and particles, now frolicking with Alice and Bob in quantum courtship? It was a bit like watching your father move to Las Vegas and marry a young dancer after you leave for college! And yet, the fact that he did it, that he saw it was worth the effort, and that he was so eminently successful, somehow gave us the message and hope that such reinventions are within the spectrum of possibilities for physicists who still have a pulse. After all, if we managed to learn anything from John during his early heroic years at Caltech, maybe we could do it too some day!

I'm sure that each of John's students remembers him in his own way, colored by the special events and situations that shaped their time together, even with a special moniker. The three of us in the Greek gang had the advantage of an inscrutable language that we could use to communicate among ourselves and keep it confidential. For each member of the theory group we had a Greek nickname so we could talk about them incognito. For us, depending on the context or mood, John was either Αρχηγός or Δάσκαλος (look them up!). And so he will always be.

An excerpt from my PhD thesis, with John's blessing

I actually have no joint publications with John. This is forever regrettable, but understandable given the situation during John's early days at Caltech that I described before. Still, it leaves me with a want to identify something in my work that bears John's flavor strongest and reminds me of him clearest.

My PhD thesis contains an acknowledgment where John is the only person mentioned specifically and by name, thus giving him in my mind a notion of ownership of the work. (It also includes a quote in Greek by Thucydides and my own controversial English translation, which was prudishly left out by the digitizer!) At the end, an Epilogue contains some highly opinionated personal comments on the path that physics seemed to be taking at the time, and especially the role of string theory. After writing it, I had second thoughts. So I gave my thesis to John and said: "Please read the Epilogue. It is a little over the top, I think I should revise it." John took it, gave me his broad smile and said "Well, I am taking it in the bathroom, so you *know* I am going to read it!" Indeed, I did know that. I started pacing and waiting for John's return from the inner sanctum with his verdict. Here is a part of what John read:

"It is the author's personal opinion that part of the reason why strings enjoyed such immediate popularity after their internal consistency was established is due to the fact that their mathematical structure is exceedingly rich and one has the feeling that one gets out in terms of results more than one puts in in terms of effort. This is, simultaneously, the great peril of the subject. Young physicists who get immediately involved in the field, and after having indulged in the mathematical sybaritism of string theory, run the risk of never quite appreciating the challenges and importance of ordinary field theory. There are, however, a lot of open and important questions there and effort on them should by no means be abandoned. Will people have the courage and will to go back to a "lower standards of living" physics if reality makes it clear that this is the only way to progress?

The author's involvement in the subject, so far, of insignificant contribution as it may have been, was one resembling Ulysses' mythical trip past the isle of Seirenes: Tightly bound to his mast with the ropes of physical reality, he enjoyed their songs of incomparable mathematical lyricism without running the risk of being terminally seduced and captured into their surreal world. One cannot be too sure about oneself, however. If the pleasures of working on the subject grow uncontrolled and the challenge of remaining in business while doing "classical" physics keeps increasing, his involvement may become total. And, after all, one should not underestimate the romanticism that any physicist carries in himself to a lesser or greater degree: the prospect of making progress towards the ultimate unification of

all interactions, however utopic, is enough to make anyone abandon any set of deeply-rooted principles, and the author cannot hope to be an exception.

May all four forces be with us soon!"

As time went by, my tension mounted. Would John think of me as an arrogant fool? Would he think that I was on drugs? At long last, the noise of Niagara hydraulics signaled the end of the deliberation. John emerged wielding the thesis as a baton, and, wearing an even broader smile, said: "I think it's adorable. I wouldn't change a word!"

I felt liberated. Now I could still be proven wrong, foolish and arrogant by the course of future events. But at least I had John's blessing as an amulet. For me, the adventure of graduate studies at Caltech had come to a closure.

Milan Mijic

Shahram Hamidi, who was a student of John Schwartz, and I shared an office on the 4th floor of Lauritsen lab, right across the seminar room. The doors were always open, of course. All day long, or so it seemed to us, there were muffled voices and "tac, tac, tac"sounds of the chalk on the chalkboard coming from across the corridor, third door to the right. That was John, with one of grad students. The list of topics the mumbles were about was seemingly endless: monopoles, cosmic strings, sphalerons, inflation, giant void, non-Gaussian perturbations, supersymmetric regularization, Berry phase, anomalies...

We would often marvel: first, obviously, how come he knows so much about so much? But more than that, how come he has a stamina and patience to deal with so many people, to shift his attention with every request?

Sometimes, after hearing, "*Thank you, I will think about it,*" Shahram and I would just smile and look at our watches. Sure enough, barely 30 seconds later there would be another, "*Excuse me, do you have a minute?*", and John's cheerful, "*Sure, come in!*" Then again, mumble and "tac, tac, tac". We would go back to playing hack.

A few years later John visited the University of British Columbia and delivered an excellent, comprehensive colloquium on cosmic strings. Later on, at the airport or waiting for a car to the airport, we were chatting alone, and John mentioned a cosmology conference he attended earlier that year. As told by John, a very senior physicist asked rhetorically the experts present : "*OK, tell me, which wave function of the universe is right?*" I said nothing at the time and soon we parted, but somehow that comment disturbed me. A day or two later I knew that I have something to say. The presentation at the 1988 Yamada Conference came in a few months, and it was elaborated later on.

These little benefits aside, the strongest memory from those years around John was feeling that the physics does exist as a whole, and that there is someone who can tell us about it, with a style. That was both comforting and inspirational. Happy birthday boss!

Martin Bucher

I owe a lot to John Preskill, who had a major impact in helping me develop into a mature scientist. As I try to be a good thesis advisor for my PhD students, I often think back to John's example. I know that other of John's students with whom I have spoken over the years share this same sentiment.

When I arrived at Caltech as a graduate student in 1987, I was probably quite well prepared in terms of mathematical or formal training, but I was obsessed with detail and subtleties, and held the conviction that I had to read every paper on a subject before daring to try something new. I learned a lot from John, but probably the most important lesson, which cannot be learned from books and papers, was how to think about things simply and grasp the main idea of what is going on.

John was a generous man and spent a lot of time with me. Typically we met once a week, and he would answer my questions. One point that stands out was that he would never read anything I wrote but instead insisted on sketching things out on the blackboard, and I was always astounded with how he could get to the crux of a problem with simple order of magnitude estimates and the like. I also remember the binders on his bookshelf that he would consult from time to time with notes from talks that he attended, papers read, etc., where everything seemed to have been worked out in the simple crystal clear way that characterizes John Preskill, probably in most cases more clearly than in the talks on which these notes were based.

Sometimes talking with John could be discouraging because he knew too much. You thought you had a great new idea and he would immediately point out all the weak points and give a historical summary of all the clever people who thought along similar lines and either failed or did not get very far. One might have wondered whether it would have been better simply to plow ahead with naive optimism, but in the end it seemed that John was almost invariably accurate in his assessments. John set a high standard for his students, mainly through his example.

I am particularly grateful to John for `rehabilitating' me, as he put it at the time, from my unfortunate one-year stint at law school after finishing my PhD. His support and faith in me allowed me to continue in physics as a postdoc at the IAS.

The enclosed paper was my first paper, which was on non-Abelian statistics. This paper was strongly influenced by John's work on `quantum hair' and fascinating discussions with him on discrete local charge and the Aharonov-Bohm effect, which at the time seemed somewhat like magic to me.

It is a pleasure to congratulate John Preskill on his 60th birthday, and I am extremely grateful to him to have served as my former thesis advisor. If I were to travel backward in time and chose a thesis advisor again, I would chose none other than John Preskill.

Martin Bucher

Hoi-Kwong Lo

Dear John,

Congratulations on your 60th birthday! Thank you very much for everything. You have helped me a lot throughout the years. In particular, thank you very much for being so patient with me. I did not realize how patient you were until I became a Ph.D. supervisor myself. It is my pleasure to co-organize this conference for you. I hope you will enjoy it.

With Best Regards,

Hoi-Kwong

Daniel Gottesman

Dear John,

It always seemed a bit peculiar that we never wrote a paper together until a number of years after I graduated from Caltech. That doesn't mean, however, that you didn't have an influence on what I was doing while I was there.

I've included my first paper on stabilizer codes in this book. Obviously, I never would have written it if you hadn't gotten me involved in quantum computing, but beyond that, your influence was shaping the types of questions I looked at in the field. When the original QECC papers by Shor and Steane came out, you began working with some of the other students to try to find more efficient codes.

This got me interested in figuring out upper bounds on codes so that you would know what to aim for. The first thing I thought of was the quantum Hamming bound – each error should give a linearly independent state on orthogonal codewords – but of course degenerate codes did not need to obey it. I began to study degenerate codes, which led me to the notion of stabilizer codes. I was disappointed, of course, that I hadn't been able to solve the problem I set out to work on. (It's still open today whether degenerate codes can violate the quantum Hamming bound.) I did think that the stabilizer approach was something worthwhile, and discussing it with you convinced me of that. I had the idea, perhaps incorrect, that you were more interested in the family of codes I made up to illustrate the power of stabilizers, so that became the title of the paper.

Anyway, thank you for exposing me to a variety of new physics ideas, not just in quantum computing, which have certainly served me in good stead in the years since I've left Caltech. Thank you also for giving me the freedom as a graduate student to explore for myself until I stumbled over something useful.

Daniel

Debbie Leung

In Fall 1993, I took Ph106a (classical mechanics) from "Professor Preskill" (how I addressed John for 10 years until I returned to IQI Caltech as a postdoc). What made the lectures so interesting was that, the fundamental princples and obviously reasonable has-to-be-true conditions often lead to extraordinary conclusions that I could not stop thinking for days to come (for example, deriving Poincare recurrence under very moderate ergodicity conditions).

I also have vivid memories of John's Physics colloquium on Chromatic aberrations, and one on the black hole information paradox. They were so accessible that even an undergraduate would get a brief illusion of understanding (those words were borrowed from Daniel Lidar after John's talk at IBM Almaden) and a high dose of intellectual stimulus. The "removal of all jokes" in the April 15th colloquium due to the rescheduling from April 1st was unforgettable.

These certainly added to my determination to pursue a PhD in Physics, against the advice of most of my older friends at Caltech. One time, I stopped by John's office, hoping he remembered who I was (I didn't do that well in his class) and could give me a word or two concerning graduate schools. I will not forget his generosity in given many helpful advice.

I was initially hoping to study cosmology (the blackhole stuff was pretty tempting) but John turned out instrumental in my switch to quantum information. I was exposed to the subject of information theory from Hoi-Kwong Lo, whom I believe got interested in the subject because of John. Soon, Seth Lloyd was giving yet another colloquium I found irresistable. (I think John invited Seth.) Before long, I was using the links from John's homepage (called "words from the mouth of truth") and was browsing interesting papers like the universal gate paper by Barenco et al, and subscribing to quant-ph. Eventually, a couple of papers by Ike and Professor Yamamoto on quant-ph gave the final push to a move that would be one of the two most important decisions I've made in my life (the first was to pursue my study at Caltech).

When I made my first attendance to a quantum information meeting (that was August 1996 in ITP UCSB, called that way back then), I was very glad to see John and his group. Ike, Mike, Hoi-Kwong, and I then stopped at Caltech for a short visit. I was giving my first talk in quantum information at Caltech, on bosonic codes for amplitude damping only, such as encoding $|0\rangle$ and $|1\rangle$ in $|04\rangle+|40\rangle$ and $|22\rangle$ to correct for one de-excitation and saving a few dimensions compared to the 5-bit code. It was not an overstatement to say that, the talk was – a bit of a disaster – I simply didn't know how to prepare for one. (I should have taken the SURF talks more seriously.) I was grateful John was still "asking me some questions" after the talk, and by spelling things out in clear exposition, we worked out a remaining question on the limitation of the code family, and the optimal fidelity achievable. Years later, I read from Chris Fuchs that he was told before going to Caltech some Professor X has a reputation of talking to others about their papers understanding it better than the authors. I surely know what they was about!

Since then, I have many enjoyable scientific exchanges with John, and eventually, I was back at IQI Caltech (called that way then) for a very fruitful three-year postdoc where John's continuing guidance and inspiration had been another blessing.

Back in 1993, I would not have dreamed to be writing this letter 20 years later, and I feel extremely fortunate and honoured to be able to do so.

Dear John: Thank you! Debbie

Andrew Childs

Dear John,

It's hard to believe that it was almost fifteen years ago, in the pre-IQI summer of 1998, that we worked together on our SURF project on quantum information and precision measurement. I feel lucky to have been at the right place at the right time to learn quantum information from one of its masters while the field was still young. I can't thank you enough for the opportunity.

Just as important as being introduced to quantum information in particular, that experience showed me what it meant to do research. You gave us enough structure that we had something concrete to work on, while leaving room to go in new directions and think creatively, and you were surprisingly tolerant of my showing up at your office with a misunderstanding or a crazy idea. Now that I know how hard it is to supervise successful student projects, I appreciate your guidance all the more, and I struggle to achieve the same balance with my own students.

It was a pleasure to have the chance to come back to Caltech after grad school to be a postdoc at the Institute for Quantum Information. The interminable Wednesday group meetings were legendary. Your randomized strategy for choosing a subset of group members to give "short" presentations helped keep the length in check somewhat, but not much. Considering the example you set of thinking deeply about carefully selected problems, we all wanted to show that we had some- thing worthy to contribute. Of course, that was a very high standard to maintain—for someone who once claimed in one of those group meetings that he hadn't had a good idea in n years, you've had an awful lot of good ideas. (I can't recall the precise value of n, but it was unreasonably large.)

Thanks for all your teaching and mentorship over the years. Have a very happy 60th birthday!

Andrew M. Childs

Andrew Childs

Joe Renes

Dear John,

First of all, thanks for getting me started in the field! The SURF project that led to this paper was a very enjoyable experience, an excellent mix of easy-to-tackle problems suitable for undergraduates as well as the freedom and encouragement to explore our own angles on the project.

Being a part of your research group was as much fun as it was intellectually rewarding. I recall a particularly hilarious talk by Chris Fuchs. He started off slowly, in a ponderous tone, saying something like "you know what I think about when I'm up late at night?" Steven van Enk didn't miss a beat, immediately interjecting: "skip this bit." I still laugh at that one.

While the SURF project was my formal start in the field, an earlier spark was your last Ph12c lecture on Maxwell's demon. I distinctly remember sitting in the last row of the lecture hall in the basement of Downs, hearing about trapdoors, sorting molecules, and the role of information in resolving the paradox. Sitting next to me was my housemate Alan Jones, who remarked at the end that if only physics were as fun and interesting as that lecture! It wasn't just the interesting topic, but the "typically Preskill" manner of very logical organization and thorough presentation, to the point that I often left the lecture that quarter (and Ph12b before it) thinking it odd I didn't already know that day's material, since everything followed so effortlessly from what came before. I'm not the only one who regards that as "typically Preskill"—Kip Thorne warned us at the start of Ph236 in my senior year (1999) that we might be disappointed if we were expecting something very nicely structured, with all the details introduced at just the right moment, like Preskill would do it. I opted for Ph229.

Happy 60th birthday!

Joe Renes

James Harrington

Dear John,

What a privilege it is to help celebrate your many explorations of and contributions towards all things quantum. I chose to highlight our paper involving the numerical simulations of toric codes, which has certainly played a significant role in my career for the past decade. It's hard to know where I would be now without this background, and I very much appreciate the opportunity to join in with you and Chenyang. It's a pleasure to be able to interact with you again since I've moved back to southern California, and I do hope to keep visiting Caltech on a regular basis. I'm thankful for your mentorship in the past and your continued sharing of insights, but I'm especially grateful for your constant willingness to ask questions, in any setting.

To many more years, filled with questions and explorations!

Jim Harrington

Bose Sougato

Dear John,

I fondly remember my time as a postdoc in your group at the IQI in 2002-2003. Indeed, the best part of it were the group meetings in the evenings over pizza in which we became aware of the varied research directions of the group. That year, postdocs Hayden, Vidal, Leung, Duan, Bacon, Doherty, visitors such as Nielsen, Schumacher, Childs, Raussendorf, and of course, yours and Kitaev's presence, made the environment really stimulating for my own research. I was able to write my most successful paper during my stay and the high quality of this paper was largely due to the great feedback I received during the group meetings. I have decided to present this old paper (on the transferring of quantum states through spin chains) in this volume as it owes a lot to comments by yourself and others at IQI. For example, you pointed out the connection to quantum random walks on interesting graphs being studied by Childs et. al at MIT. I looked up their paper, and found some useful approximations to Bessel functions so that I could make some analytic estimates (Eq.(12) of my paper). More important were perhaps your comments on the focus of the paper -- initially the motivational aspect of the paper was veering a bit and in the group meetings I presented varied motivations. I remember that you stressed very strongly that the use of a spin chain as a "vquantum channel" was perhaps the most interesting aspect of the work. Indeed, when I wrote the paper, I made that the primary motivation, and I think that made a big difference in terms of where it got published and the impact it had. On a broader note, at the very start, I used to be embarrassed that my work bordered on impractical -- however, I remember your remark to me at a group meeting that "nothing is considered science fiction here" (by "here" I think you meant your group at IQI). Indeed that gave me the confidence to proceed full steam ahead.

I heartily thank you for another help much later in 2006. For a professorship outside the UK, your reference was sought by the prospective employers as being the important one, and they spoke to you on the phone. Whatever you said, must have worked because I got that job. That offer, in turn, helped me to negotiate not only my current professorship at UCL, but also the support for a larger quantum information group at UCL.

Have a fabulous 60th birthday,

Sougato

Gil Refael

This birthday is a good opportunity to say how glad I am to have you as a colleague, a mentor and a friend. Navigating the starting years of my job at Caltech, as you can imagine, was not easy. Even before starting, I was fretting that sometime in the spring of 2004 Caltech would realize what mistake it made, and send me a small letter apologizing for the mix up. And then we ran into each other at the KITP around April and chatted a bit. I was – well – concerned, that you'll find our discussion disappointing, since I didn't quite do much that winter. But then, as a last resort, I told you about this little thing that Joel and I did on entanglement entropy of random spin chains. I couldn't tell what you were thinking throughout. When I ended, however, you finally gave an encouraging smile. It made my spring.

Waiting for the smile continued while we had our annual 'tracking meetings'. I always tried to peek through the poker face, to guess what you were thinking: is it all nonsense? Was I doing okay? I couldn't tell until the end. You usually gave out a smile, and encouragement that would keep me going for another year.

I think that succeeding at Caltech would have been impossible without the help of two or three people. You are chief in that group, and I'm very grateful to you for it. By the same token, you continue to be a crucial role model. Achieving such a wide spectrum of feats - establishing and maintaining an immensely successful group, while also making constant and deep intellectual contributions: I know that this is far from trivial. In short, you are our academic superhero! It is just fantastic to be able to come along for the ride.

Ignacio Cirac

Dear John

I never worked with you, but my research has been largely influenced by your long-term vision of the field of quantum information theory. Several of my closest collaborators (Guifre, Frank, Norbert) joined or will join (Fernando) your group at CALTECH, benefited from the wonderful atmosphere you have created there, and have created a nice connection between CALTECH and MPQ. I also had the pleasure of joining your group as a Moore scholar last year, and I really loved the time and discussions I had with you and the other group members. Thank you, John, for all this.

It is a great privilege for me to contribute to this book. I am enclosing one of the papers I am most proud of, and which exploits your visionary idea of using the tools developed in the context of quantum information theory to describe many-body quantum systems. This paper, which Frank and I wrote back in 2004, introduced what we called projected entangled-pair states (PEPS). It has never been published, since it was repeatedly rejected in Phys. Rev. Lett., but it started a line of research which we have been developing since then.

Happy Birthday!

Ignacio

OP-ED CONTRIBUTOR

The Truth Is Still Out There

By PAUL GINSPARG
Published: August 3, 2004

Ithaca, N.Y.

Stephen Hawking's recent concession that black holes do not irretrievably eradicate information after all has garnered much attention. It is refreshing to see the public focused, if just for a moment, on an important conundrum that has fascinated theoretical physicists for three decades, and prompted much conceptual progress. The scientific issues, however, remain much less settled than Dr. Hawking's celebrated wager on the question. He most recently pronounced: "If you jump into a black hole, your mass energy will be returned to our universe, but in a mangled form, which contains information about what you were like but in an unrecognizable state."

To appreciate why this is different from awakening after any night's sleep requires a brief reprise of 20th century physics. Einstein's theory of general relativity in 1915 was the culmination of centuries of classical physics, and Einstein and others soon found solutions to his gravitational field equations. One of these solutions was later termed a "black hole" in the 1960's, since it describes the gravitational field produced by an object so dense that nothing can escape, not even light. Indeed, the existence of black holes is inferred only through their gravitational effects on other astronomical bodies. Stars recently detected orbiting very close to the center of our own Milky Way galaxy, for example, suggest the existence of a supermassive black hole at the center, almost three million times the mass of our sun and about five million miles in radius. If the mass of the entire Earth were compressed into a black hole, it would be a little ball only a third of an inch in radius. Fortunately, the Earth is in no imminent danger of collapse in part because of the electrostatic repulsion of its constituent atoms.

Quantum mechanics, which describes the behavior of very small objects like atoms, blossomed a decade after general relativity, and the two are notoriously difficult to reconcile. Thirty years ago, Dr. Hawking published a calculation incorporating some quantum mechanical effects into black hole physics, and showed that matter or energy could leak from a black hole. While surprising, this was not paradoxical since there are examples of processes forbidden by classical physics but allowed by quantum mechanics. Shortly afterward, however, Dr. Hawking articulated a more shocking consequence of his calculation.

One of the central tenets of relativity theory, termed causality, is that nothing, not even information, can travel faster than the speed of light. This means that as long as a black hole exists, no information about objects that had fallen into it can ever emerge. Moreover, according to Dr. Hawking's original calculation, the radiation emitted quantum mechanically from a black hole is generic, in the sense that it conveys no information. Therefore, if a black hole were permitted to evaporate entirely, then the information content of any objects previously ingested by it would vanish from the universe, without a trace. By contrast, if you throw your diary into a fireplace, then the information contained therein could be reconstructed, at least in principle, from subtle properties of the resulting

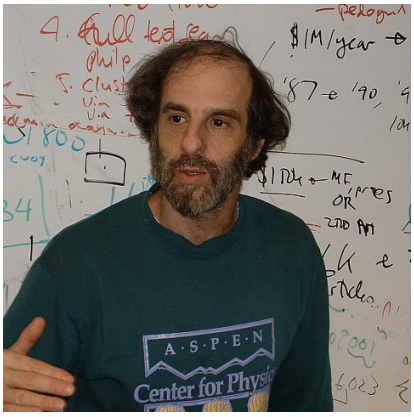
Dear John,

This OP-ED would not have been written had you not stood on principle and committed to the wager, and also saved that critical vote count from 1993. This is the most profound effect you have had on my career.

(Of course a more principled physicist would have refused to accept the proceeds of the wager, since the concession was more publicity stunt than scientifically dictated. But happily, inspirational personages are permitted to deviate from principle when it suits the occasion.)

I do not, however, understand how you could be 60 when I am still under 40.

Happy Birthday, PG



smoke and flames. A permanent loss of information because of black hole evaporation, on the other hand, is in contradiction with one of the central tenets of quantum mechanics, termed unitarity, which permits tracking information flow in all such processes and forbids its disappearance.

In the early 1980's, I was fortunate to attend some of Dr. Hawking's lectures in which he speculated on ways to modify quantum mechanics to accommodate this potential loss of information. He stimulated much debate among quantum field theorists, who in turn enjoyed working to rebut his arguments. The black hole information paradox thereby emerged as an important catalyst toward further theoretical progress in reconciling gravitational and quantum effects. Despite many new ideas and progress on other fronts, no definitive resolution emerged.

Near the end of a small meeting I attended in 1993, the question of "What happens to information that falls into a black hole?" arose, and a democratic method was chosen to address it. The vote proceeded more or less along party lines, with the general relativists firm in their adherence to causality, and the quantum field theorists equally adamant in their faith in unitarity. Of the 77 participants, 25 voted for the category "It's lost;" and 39, a slight majority, voted for "It comes out." Seven voted that the black hole would not evaporate entirely, and the remaining six voted for an unspecified "Something else." I voted with the majority, anticipating progress and hoping that one of us would soon perform a calculation to help Dr. Hawking and the relativists see the light. But with the question still unresolved four years later, three of the protagonists eschewed the old political duel-to-the-death methodology for a variety of practical reasons, settling instead on a simple wager whose unsatisfying outcome was announced last month.

It once was that important scientific results were presented to the general public only after they were subjected to peer review and accepted for publication in an edited journal. Some professional journals, particularly in medicine, still refuse to publish results that have already been announced via press release. Since the early 1990's, articles in many fields have nonetheless been publicly available in "prepublication" form through organized Internet repositories, a type of instant communication frequently concurrent with peer review.

The recent "resolution" of the information puzzle, however, has neither supporting publication nor calculation, peer-reviewed or otherwise. While a press release may be sufficient in some realms of human endeavor, one of the joys of scientific research is that it is subject to more objective measures of progress. It is possible that some new revolutionary mechanism to avoid information loss will yet emerge from the latest spectacle. But without even a hint yet as to what might have been missing from Dr. Hawking's original calculation, it is more likely that theoretical physicists will continue to view the information paradox as a profound puzzle whose resolution will provide clues to understanding the basic laws of physics.

String theory, a parallel quantum gravitational effort over the past 30 years, offers many tantalizing hints toward possible resolution of the puzzle. Perhaps some of its ideas have subconsciously persuaded Dr. Hawking to join the quantum conservation of information camp. But should we ultimately be more inclined to trust Dr. Hawking's past youthful intuition? Physicists, particularly eminent British ones, have a historical tendency to stray in excessively speculative directions in their later years. A bigger surprise may yet await us, and those who voted for "Something else" may prove the most prescient. Perhaps the real winners of this bet will be some middle-school students who, inspired by the current hoopla, will help provide a more substantive answer a decade from now.

Paul Ginsparg, professor of physics and information science at Cornell University, was named a MacArthur fellow in 2002.

Frank Verstraete

As a student in a remote university in Belgium, I was in awe of John Preskill. Everything I knew about quantum information theory, I had learned it from his fantastic lecture notes on the theory of quantum information and quantum computation. How was it possible for somebody to be so generous as to put those notes freely available online? How was it possible for somebody to gather all those famous postdocs around him? Would I ever be able to understand his amazing notes on quantum field theory?

I sobered up however during my first encounter with John when he offered me a postdoc at Caltech; when asking for a larger salary (I had to sustain a wife and 3 small kids), he answered: "Look, here in the US we are not a communist country like where you come from in Europe; you work the same, you earn the same." This was the first glimpse into the wonderfully complex and intriguing character of John; of course he arranged for a salary raise.

After that, I spent 2 wonderful years at Caltech. The weekly highlight was certainly the group meeting orchestrated by John. No subject was esoteric enough, no area law mundane enough, no fault tolerance threshold large enough, for John to lift the whole discussion to another level by his innocuous sounding but very deep questions.

As far as I can witness, John had a huge impact on all his postdocs, including myself. He was the ultimate role model of a multidisciplinary theoretical physicist: he is modest, extremely jovial, has a fantastic physics intuition, and understood more clearly than anybody else how the theory of quantum entanglement allowed for constructing new inroads into the vast plain of quantum many-body physics. Topological quantum order, area laws and the black hole information paradox: these are but a few of the hugely popular topics that John foresaw.

The enclosed paper, which was written during my time at Caltech, is a direct product of this exotic interdisciplinary atmosphere that John advocated at the IQI, and involves area laws, critical systems, topological quantum order, and computational complexity.

Congratulation John with your 60th birthday, and many, many thanks for the wonderful ideas and souvenirs that you have given us!

Yi-Kai Liu

Dear John,

Happy birthday! So, it is hard to know what to give you on this occasion. Probably the best gift would be a r*eally interesting paper*. But since I don't have one right now, let me instead express my appreciation for some of the excellent things about you, that have made a strong impression on me over the years: your extensive knowledge of quantum information and theoretical physics; your efforts to bring talented and interesting people to IQI, and to encourage scientific interaction at lunches and group meetings; and your conviction that, if one is sufficiently smart and hard-working and lucky, one can make some real progress in understanding our universe.

Being a postdoc at IQI was a great experience for me, and it set high expectations that I am still striving to meet. As for you, best wishes for future birthdays, and discoveries of all kinds.

Sincerely,

Yi-Kai Liu

Ben Toner

The University of Melbourne, Australia

School of Physics

Dear John,

I chose Caltech for grad school because I wanted to be your student. Once I arrived in September 2001, the challenge was to work out how to persuade you to accept me!

I first tried to make it a fait accompli by asking you to sponsor my membership to the Athenaeum. You didn't mind, of course, but you did reply in mock indignation, asking if I thought you were my adviser. Chagrined, I mumbled something noncommittal and disappeared to plot another attempt. I eventually resolved to just hang around your group and, happily, you tolerated my presence at the weekly meetings.

It wasn't until a year later that you finally got on board with my plans! I gave a talk at a group meeting about work with Dave Bacon [Phys. Rev. Lett. 91, 187904, 2003]. You said "Wow," and the next day arranged an office and computer for me. I was probably more pleased about your reaction than the result itself: it was a great boost of confidence.

Thank you for the wonderful environment that you created during my time at Caltech. It felt like every important quantum person passed through as a visitor or postdoc at some point. I appreciate how lucky I was to do my PhD in these surroundings.

Thank you also for teaching me that physics is supposed to be great fun.

I appreciate the free rein you gave me to follow my own interests. You invited me to collaborate with you once, but I had my own direction by then, and I'm not sure how well it would have worked: even by the end of my PhD I was intimidated by the power of your intellect and my brain would always turned to mush whenever I was in the same room as you. Nonetheless, all my collaborations owe a lot to the environment you created.

The following paper (joint with Frank Verstraete) is one of my favourites. It is actually the direct result of one of your group meetings. I shared a numerical result and Frank immediately saw how to prove the corresponding theorem.

Congratulations on your 60th birthday. I hope that you have a great conference and I'm sorry that I'm unable to attend the celebrations.

Yours sincerely,

Ben Toner

Patrick Hayden

The Man Who Understood Everything

I first arrived in John Preskill's group as a postdoc in 2001. At that point, I only knew John through his lecture notes on quantum computation, which had been my introduction to the subject, and through some of his influential papers on fault tolerance. I respected John and was grateful that he had hired me into my dream job, but I soon discovered that even my genuine enthusiasm for working with him paled next to the reverential attitude of most of the other people in the group. Other postdocs who had been at Caltech longer, or who had been undergraduates there, saw John as The Man Who Understood Everything.

While I was inclined to be skeptical, the evidence soon started piling in that the characterization was true. Regardless of which subject came up, whether it was phase transitions in frustrated spin systems, polynomial bounds on quantum query complexity or Chern-Simons theory, John always seemed to understand the details of the relevant arguments and to be able to explain them to those of us less who were less polymathic. Over time, I came to believe that John just sat, listened and immediately understood everything uttered in his presence.

There were clues, however, that the story was more complicated. Namely, I never once saw John attend a seminar without taking notes. All that note-taking seemed like an awful lot of work for someone endowed with such superhuman abilities of effortless understanding.

The true story only became clear to me a few years ago when John posted scanned copies of his old lecture notes online. Beautifully written in full sentences, complete with tables of contents, his notes were more carefully organized, complete and clear than most published books. His quantum computation notes weren't the exception, they were his rule. There were notes for basic quantum field theory, general relativity, topological aspects of field theory, quantum chromodynamics, and scattering theory among other topics. No wonder John understood everything – he had devoted untold hours of effort to developing that understanding!

This new revelation about John only increased my respect for him. His broad and deep knowledge and his penetrating intuition are the products of a career's worth of joyful and diligent development. He is an inspiring leader, a generous supporter of young scientists, a brilliant researcher, an excellent co-author and, I'm lucky to be able to say, a good friend.

Patrick Hayden

Quantum-Bayesian Coherence: The John Preskill Version

Christopher A. Fuchs[†,‡] and Rüdiger Schack[♯,‡]

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In the Quantum-Bayesian interpretation of quantum theory (or QBism), the Born Rule cannot be interpreted as a rule for setting measurement-outcome probabilities from an *objective* quantum state. But if not, what is the role of the rule? In this paper, we argue that it should be seen as an empirical addition to Bayesian reasoning itself. Particularly, we show how to view the Born Rule as a normative rule in addition to usual Dutch-book coherence. It is a rule that takes into account how one should assign probabilities to the consequences of various intended measurements on a physical system, but explicitly in terms of prior probabilities for and conditional probabilities consequent upon the imagined outcomes of a special *counterfactual* reference measurement. This interpretation is seen particularly clearly by representing quantum states in terms of probabilities for the outcomes of a fixed, fiducial symmetric informationally complete (SIC) measurement. We further explore the extent to which the general form of the new normative rule implies the full state-space structure of quantum mechanics.

Contents

Prologue

John Preskill from time to time likes to tell an anecdote about Richard Feynman. One of the authors (CAF) from time to time likes to tell an anecdote about John Preskill. Here's one from 2007.

In August of that year, I was preparing a talk for the "Many Worlds at 50" conference at Perimeter Institute in honor of Hugh Everett's 1957 paper. My talk was to be titled "13 Quotes from Everettian Papers and Why They Unsettle Me," and I wanted to take a negative stand on the whole development. One of my devious ideas was to send out a questionnaire beforehand to many of the founders of quantum information and computing asking them to what extent Everett-style thinking played a role in their finding of this algorithm or that protocol. I tailored the question to each correspondent's particular contribution to the field. [Of course it was delightful to see Deutsch and Jozsa give opposite answers for the Deutsch-Jozsa algorithm—as I had suspected—but probably the funniest answer came from Dan Simon who said, "Who's Everett, and what's his interpretation?"]

John answered the questionnaire right away but couldn't help letting me know the circumstances. His note ended, "Yes, I don't usually answer you so promptly, but I'm waiting to board an airplane and I'm kind of bored."

Anyway, here's to your sixtieth birthday John! In everything I've composed since 1996, you've never been far from my mind. I think of your calm, honest "Why is that?" and I always strive to do better. One day I keep hoping for that "Aha!" moment. Below is a fragment of a paper slated to go elsewhere—this is the "John Preskill Version." It was not directly inspired by your influence,

but we hope you will enjoy it and that maybe it will provoke you to recount a new Feynman story. At the end are some questions for an airport in case you get bored.

I. INTRODUCTION: UNPERFORMED MEASUREMENTS HAVE NO OUTCOMES

> We choose to examine a phenomenon which is impossible, *absolutely* impossible, to explain in any classical way, and which has in it the heart of quantum mechanics. In reality, it contains the *only* mystery. We cannot make the mystery go away by "explaining" how it works. We will just *tell* you how it works. In telling you how it works we will have told you about the basic peculiarities of all quantum mechanics.
>
> — R. P. Feynman, 1964

These words come from the opening chapter on quantum mechanics in Richard Feynman's famous *Feynman Lectures on Physics* (Feynman, 1964). With them he plunged into a discussion of the double-slit experiment using *individual* electrons. Imagine if you will, however, someone well-versed in the quantum foundations debates of the last 30 years—since the Aspect experiment say (Aspect, 1982)—yet naively unaware of when Feynman wrote this. What might he conclude that Feynman was talking about? Would it be the double-slit experiment? Probably not. To the modern mindset, a good guess would be that Feynman was talking about something to do with quantum entanglement or Bell-inequality violations. In the history of foundational thinking, the double-slit experiment has fallen by the wayside.

So, what is it that quantum entanglement teaches us—via EPR-type considerations and Bell-inequality violations—that the double-slit experiment does not? A common answer is that quantum mechanics does not admit a "local hidden variable" formulation. By this one usually means the conjunction of two statements (Bell, 1981): 1) that experiments in one region of spacetime cannot instantaneously affect matters of fact at far away regions of spacetime, and 2) that there exist "hidden variables" that in some way "explain" measured values or their probabilities. Bell-inequality violations imply that one or the other or some combination of both these statements fails. This, many would say, is the deepest "mystery" of quantum mechanics.

This mystery has two sides. It seems the majority of physicists who care about these things think it is locality (condition 1 above) that has to be abandoned through the force of the experimentally observed Bell-inequality violations—i.e., they think there really are "spooky actions at a distance." Yet, there is a minority that thinks the abandonment of condition 2 is the more warranted conclusion. Among these are the *Quantum Bayesians* (Appleby, 2005a,b; Caves, 2002, 2007; Fuchs, 2002, 2003, 2004, 2010,b, 2012; Mermin, 2012; Schack, 2001; Timpson, 2008). Giving up on hidden variables implies in particular that measured values do not *pre-exist* the act of measurement. A measurement does not merely "read off" the values, but enacts or creates them by the process itself. In a slogan inspired by Asher Peres (Peres, 1978), "unperformed measurements have no outcomes."

Among the various arguments the Quantum Bayesians use to come to this conclusion, not least in importance is a thoroughgoing personalist account of *all* probabilities (Bernardo, 1994; de Finetti, 1931, 1990; Ramsey, 1931; Savage, 1954)—where the "all" in this sentence includes probabilities for quantum measurement outcomes and even the probability-1 assignments among these (Caves, 2007). From the Quantum-Bayesian point of view, this is the only sound interpretation of probability. Moreover, this move for quantum probabilities frees up the quantum state from any objectivist obligations. In so doing it wipes out the mystery of quantum-state-change at a distance (Einstein, 1951; Fuchs, 2000; Timpson, 2008) and much of the mystery of wave function collapse as well (Fuchs, 2002, 2010b, 2013).

But what does all this have to do with Feynman? Apparently Feynman too saw something of a truth in the idea that "unperformed measurements have no outcomes." Yet, he did so because of considerations to do with the double-slit experiment. Later in the lecture he wrote,

> Is it true, or is it *not* true that the electron either goes through hole 1 or it goes through hole 2? The only answer that can be given is that we have found from experiment that there is a certain special way that we have to think in order that we do not get into inconsistencies. What we must say (to avoid making wrong predictions) is the following. If one looks at the holes or, more accurately, if one has a piece of apparatus which is capable of determining whether the electrons go through hole 1 or hole 2, then one *can* say that it goes either through hole 1 or hole 2. *But*, when one does *not* try to tell which way the electron goes, when there is nothing in the experiment to disturb the electrons, then one may *not* say that an electron goes either through hole 1 or hole 2. If one does say that, and starts to make any deductions from the statement, he will make errors in the analysis. This is the logical tightrope on which we must walk if we wish to describe nature successfully.

Returning to the original quote, we are left with the feeling that this is the very thing Feynman saw to be the "basic peculiarity of all quantum mechanics."

One should ask though, is his conclusion really compelled by so simple a phenomenon as the double slit? How could simple "interference" be so far-reaching in its metaphysical implications? Water waves interfere and there is no great mystery there. Most importantly, the double-slit experiment is a story of measurement on a sin-

Sandy Irani

Dear John,

I chose this particular paper to include in this compilation because the research began during a visit that Daniel made to Caltech the year I was on sabbatical. For this reason, I see this work as a direct product of the research environment that you have created at IQI. I should also note that you had a key insight that our results in this paper applied to N-Representability which we did not realize initially.

I consider myself very lucky to have been able to spend my sabbatical year in such an active and engaging place as IQI. Being a newcomer to the field of Quantum Information and Computation, I learned a tremendous amount that year, both from my interactions with colleagues as well as attending your wonderfully clear lectures in class.

I have always been impressed with how much you have contributed to this research community. I realize that it would have been very easy for you to just focus on your own students and research but instead you chose to expend the considerable effort involved in creating and sustaining a vibrant research center in IQI. I have certainly benefitted personally from your efforts. I also appreciate that you have, directly and indirectly, fostered so much influential work and have helped cultivate the careers of so many people in our field.

Thank you very much!

Best Regards,

Sandy Irani

Robert Koenig

Dear John,

I've been unsuccessfully trying to think of a single thing that could be described on a page and that would give an accurate impression of my time at Caltech and your part in it as my employer, mentor and friend. Instead of trying to condense my impression, I'll try to recall some of my experiences there – perhaps this is more appropriate for the occasion.

I recall first meeting you at lunch near the Red Door Cafe. I remember being somewhat nervous; I had the naïive expectation of being led to your office for a more formal discussion of scientific matters, among other things. At the time, I was not aware that this was not how things worked at IQI. I did not know that these lunchtime conversations would become somewhat of a daily highlight for me and that I'd become the 'organizer' thereof, calling you every day in Lauritsen to see whether you were available.

The third week of my postdoc, I gave my first group meeting talk. It must have been a pretty awkward talk to endure. I'm sure I did not explain the motivation very well. Towards the end of the talk, when I realized I had lost everyone trying to explain some technical detail, I asked whether my explanation had been completely unclear. Your answer was a clear and brief 'yes'. This made me wonder whether you were already regretting giving me a job. I tried to take comfort in the idea that things could only improve (I could hardly have made a worse first impression on my boss!). Indeed, they did (from my perspective) – soon after, I was really enjoying these weekly meetings for their educational and entertainment value. I also began to realize how effective and useful your feedback and questions were to the speakers. I still sometimes ask myself whether you'd be happy with a certain explanation, or agree with the claimed importance of a result. Regrettably, I cannot ask you as easily now for feedback as I used to be able to at IQI, or at least try to read the expression on your face in reaction to someone's remarks. I do miss our discussions.

Three months later, I had my first and only meeting with you in your Lauritsen office. This was triggered by two things: firstly, I was wondering what I should work on next; and secondly, I thought it was about time I asked you what exactly you wanted me to work on. Again, my expectations were off. Instead of telling me what to do, you essentially told me to do what I like. This was definitely a nice surprise. I was especially happy that you did not express any objections to my desire to spend a significant amount of time learning about topological computing. You even encouraged me to do so, telling me it was a good time for me to learn something new. Finally, while not telling me to work on either, you suggested looking at two broad research topics: fast scramblers, and renormalization. The latter intrigued me because I had no clue what it was.

Three years thereafter, I wrote a paper with Ersen Bilgin on entanglement renormalization for topologically ordered systems. This paper perhaps best represents my time at Caltech: its content is quite different from anything I had worked on before moving there, and in many ways, it relied on the regular spontaneous lectures you gave us either on the way to lunch or afterwards. And like almost everything else I was involved in at Caltech (with the exception of having to think about and make plans for life after leaving IQI), it was great fun pursuing this project. It was especially nice to know that you seemed to care about our endeavors. In this particular instance, you had even initially suggested the topic. Perhaps you had not expected it would take three years though.

Several senior researchers have told me that their time as a postdoc had been the best in their academic career. It is rather meaningless to say something like this at my stage. But I believe I can say with great confidence that my time as a postdoc in your group will likely remain at the very top for the rest of my life. I honestly don't know what could beat it – but then that may just be my lack of imagination!

Happy Birthday!

Roberts

Jeongwan Haah

To John,

It was a warm winter evening when I drew cubes on the whiteboard at the group meeting. I was explaining a code on three dimensional lattices that might lack any string logical operator, but I was nervous worrying I might not make any sense. You asked me a few critical questions and were still calm and modest as usual, but said softly "interesting." That made me excited. Your interest was good enough for me to push it forward anyway, although that code turned out to be one of those that were completely useless.

I tried to be rigorous and systematic to convince you thoroughly, and become finally able to distinguish what was true and what was false. The falsified model was very disappointing, for it was the first problem that I worked for the longest period of time ever then. I just wanted to find another problem.

One day you dropped by my office, and asked me a question on the two-dimensional partial self-correction. Preskill had a question for me! It refueled my enthusiasm and self-confidence to attack the problem once again. It didn't take too long to reprogram the old computing source code and apply established arguments.

I met you in front of the water dispenser and said
"I think I've found a model. Do you want to talk about it after lunch?,"
"That's good news. I want to hear about that."
I confidently knocked the door and said that I had checked the computation twice.
You smiled and said "That's how you get fault-tolerance."

Were it not for your encouragement and support, this paper would certainly not have existed. Also my life as a graduate student at Caltech might have been quite different if I did not ask you to be my advisor. Many worries have been lifted as you have listened to me. Your answer or comment often was simple which made problems of any kind easy. I can surely say that the best choice I have made during my graduate school is to become your student. Congratulations on your sixtieth birthday and thank you.

With the deepest gratitude,

Jeongwan

Liang Jiang

Yale University

Assistant Professor of Applied Physics

Dear John,

Happy Birthday! It occurs to me that ten years have passed since we first met. Do you still remember that in early 2003 a nerdy Caltech junior, asked if you could be his advisor for summer undergraduate research (SURF)? You took him and patiently led him into the exciting field of quantum information science. That has greatly changed his life and career.

As a SURF student, I was curious about quantum information, but I did not even know where to begin. You recommended me many insightful papers that exposed to me the exciting frontier of the field. It turns out that some of them have had significant long-term influence on my later research. In particular, the elegant paper on "Methodology for quantum logic gate construction" (by Zhou, et. al PRA 2000) seeded my future research on distributed quantum computation. The other paper on "Topological quantum memory" by you and your collaborators introduced me to the field of topological quantum computing and stimulated my interest for topological quantum information processing.

In 2009, after five years of graduate study at Harvard, I had the opportunity to come back to Caltech to continue my research in quantum information, as a postdoctoral fellow in the Institute for Quantum Information (IQI). As the director of the IQI, you encouraged and supported many young researchers like me to explore novel ideas and new topics in quantum information. In my first year of postdoc, I had the idea of bridging topological and conventional quantum systems coherently, but I did not know what kind of quantum system I should look into. When I came to your office for advice, you suggested me to look into superconducting quantum systems, and even shared with me your detailed notes on superconducting qubits. Your great insight really guided our projects towards the right direction, which led to the publication with Prof. Charlie Kane on "Interface between Topological and Superconducting Qubits."

It has been my greatest honor to have the opportunity to learn from and work with you on quantum information. I really admire your profound understanding of physics in both breadth and depth. I am sincerely grateful to all your guidance and support. You not only lead me and my fellows to the exciting research of quantum information, but also showed us the role model of a great scientist.

Happy Sixtieth Birthday, John!

Sincerely,

Liang Jiang

Daniel Lidar

I first met John at one of the early ISI-Torino workshops, the summer of 1997 (or was it 1998?). He already loomed large in quantum information, and I was an intimidated graduate student (or an impressionable postdoc?). We ended up sitting across from each other at one of the lunches, and I mustered enough courage to ask a naïve question about hairy black holes. I got a mumble for a reply, and Charlie Bennett chided John to give more details, which he did with the great clarity that I later learned is so typical of his expositions. Our next Torino encounter was mediated by Ike Chuang, which again included Charlie Bennett. The four of us went out to dinner, following a nice evening walk. We were all learning about decoherence at the time, and I tried to explain, quite unsuccessfully, the relationship between the T1 and T2 times in NMR. Still nervous, I asked John about his watch, quite unusual in its simplicity and sportiveness, half expecting a reply that he was a triathlete, but instead he told me it was a gift from his wife, and he just liked to wear it. I think this is still the case, and suspect he must get asked about it often. The next scene involved the four of us around the dinner table, eating communal pizzas. This reminded me of the following riddle: two players sit at a round table and take turns placing identical circular disks on the table. Disks are not allowed to overlap in any way and must fully fit on the table. The winner is the one to place the last disk. What is the winning strategy? Ike stared in the distance. Charlie grumbled. John looked down at his plate and a 30sec silence followed. He then proceeded to give the solution, which of course starts with the player that should go first, and a nice mirror symmetry argument for the remaining moves. I was impressed, especially given how much time I'd spent figuring this out myself. The bar for quantum information thinking speed just got raised a few notches!

When I moved from Toronto to USC, I was excited by the prospect of collaborating with the IQI group. John graciously agreed to my request to spend my first sabbatical (2008-9) at IQI. At the time I was working on dynamical decoupling, and had made several attempts to merge it with quantum error correction and fault tolerance. Trying to work out too many details I lost track of time and arrived 15 minutes late and out of breath at our first meeting at the Red Door, and ran after John who had just about given up on me. The meeting went well nevertheless, and John introduced me to Hui-Khoon Ng, his graduate student at the time. Hui was a terrific collaborator, and after a few weeks we had a credible idea for combining dynamical decoupling and fault tolerance, which we felt comfortable presenting to John. The idea was a rough one, and John used some awesome powers to grow it into the paper that I'd like to contribute to this collection. Working together was one of the most memorable collaborations I've experienced. I learned lessons about rigor, depth, perseverance, and writing, which I've vowed to try and emulate in all my future scientific endeavors.

Thank you John, for being a role model for all of us in the quantum information community.

Stephen Jordan

I arrived at the IQI in the Fall of 2008 and by the Fall of 2009 decided to investigate quantum algorithms for simulating quantum field theories. This topic had been talked about on a casual basis in the quantum information community for many years, but nobody had published serious research on the quantum complexity of simulating continuum quantum field theories. I suspect that one reason for this is that people could see that a large time investment would be needed in order to produce a journal article on the topic. Luckily, John had granted me a three year postdoc position without serious publish-or-perish pressure, and this made the project possible. Knowing that I was getting in over my head, I began trying to recruit actual quantum field theory experts to join me on this quest. To my good fortune, John was interested in direct involvement, as was Keith Lee, an expert in Effective Field Theory (EFT), who at the time was a fellow Caltech postdoc across campus in the physics building.

The final result of this project was a paper published in Science in June 2012, nearly three years later. By this time, I had taken a job at NIST in Maryland, and Keith had taken a postdoc at the University of Pittsburgh. For me, the result was well worth the investment of time. John's influence is visible throughout. In particular, my initial attitude was to approach the problem from a complexity theory perspective and ask only whether the simulation problem could be solved on a quantum computer in polynomial-time, ignoring detailed questions of efficiency. John, however, maintained higher standards and insisted on quantitative complexity estimates and optimization of the algorithm. It turned out that optimizing the algorithm forced us to pay much more attention to the underlying physics, particularly the issue of locality, which is central to relativistic quantum field theories.

One of the keys to completing this project was patience. I can recall numerous epic telephone conversations with Keith, several of which exceeded three hours. John was also remarkably generous with his time, and even requested greater involvement with the day-by-day nitty gritty of the research. I can recall in particular, listening with maximum concentration to John explaining a subtle point about quantum field theory. Looking at my facial expression, John commented that I looked concerned, as though I were expecting him to say something wrong. What he didn't know was that I was actually concerned that I would squander my opportunity by failing to understand or remember what he said!

Yaoyun Shi

University of Michigan

Department of Electrical Engineering and Computer Science

Dear John,

Your wearing a gorilla mask to the IQI seminar on Halloween, 2001 was among the most memorable moments of my postdoctoral year at Caltech. I almost fell of my chair, especially when intelligent questions spit out from that giant, ferocious head.

I have since then been a vampire, a phantom, V for Vendetta, and the latest, a butcher threatening to cut off the ears of my students with a bloody machete if they were not listening, in my classes taking place on Halloweens. Your inspiration contributed to the seasonal boost of my teaching evaluations in fall semesters. Thank you!

Deep not just intellectually but also in the sense of humor, John you are an American Idol to me, the academic edition. On this occasion of your 60th birthday, it is my great honor and privilege to dedicate my paper "Quantum Simpson's Paradox" to you. I hope that you and other readers find the paper in the same spirit of being not just thought provoking but also entertaining.

Sincerely,

Yaoyun Shi

Quantum Simpson's Paradox

Dedicated to Professor John Preskill on the occasion of his 60th birthday

Yaoyun Shi*

Department of Electrical Engineering and Computer Science,

University of Michigan, 2260 Hayward Street, Ann Arbor, MI 48109-2121, USA

(Dated: January 14, 2013)

Abstract

The well-known Simpson's Paradox, or Yule-Simpson Effect, in statistics is often illustrated by the following thought experiment: A drug may be found in a trial to increase the survival rate for both men and women, but decrease the rate for all the subjects as a whole. This paradoxical reversal effect has been found in numerous datasets across many disciplines, and is now included in most introductory statistics textbooks. In the language of the drug trial, the effect is impossible, however, if *both* treatment groups' survival rates are higher than *both* control groups'. Here we show that for quantum probabilities, such a reversal remains possible. In particular, a "quantum drug", so to speak, could be *life-saving* for both men and women yet *deadly* for the whole population. We further identify a simple inequality on conditional probabilities that must hold classically but is violated by our quantum scenarios, and completely characterize the maximum quantum violation.

*Electronic address: shiyy@umich.edu

David Poulin

Dear John,

For your 60th birthday booklet, I'm presenting a paper entitled "Local topological order inhibits thermal stability in 2D", written with my student Olivier Landon-Cardinal. Where can I begin to explain the many ways in which you have influenced this research project? Most of the concepts used in the paper are IQI branded: topological quantum computing, self-correcting quantum memories, local topological order, etc. There is also the fact that you and Jeongwan have worked on the very same topic, giving me one more "I've been scooped" shiver. But more importantly, your idea that unitary quantum physics as we know it could emerge as a low-energy approximation to a theory that is fundamentally irreversible, and that fault-tolerant quantum computing may hint at the underlying mechanism for this phenomenon, truly fascinates me. This unique ability that you have to connect practical matters to fundamental questions has had a great impact on me. So of all the many ways in which you have influenced this research, the most important was teaching me to focus on what I believe are the important questions and to explore the consequences of my own findings in other areas of physics. Perhaps it doesn't show so much in this particular work, but these are the principles that you've imprinted on me and that I strive to develop, to a modest degree.

Best wishes on your 60th birthday,

David

Ben Grinstein

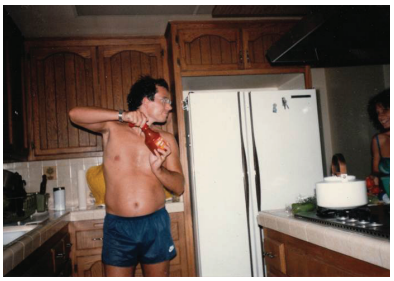
Dear John,

I have a couple of personal stories (more like notes). I am attaching the pictures.

The first one, with you in swimming trunks is special to my wife and I. Rebeca and I were (I am pretty sure) the first guests to stay with you and Roberta after you purchased your home near the mountains near Pasadena (we were also first to babysit for you). We were nervous about staying with The Professor, I had just graduated and was coming to Caltech for my first postdoc. We dressed well and rang the bell. You answered the door looking just like in the picture (but wet), looked at us and said (I will never forget) "Welcome, we'll show you what decadent California lifestyle is all about!"

Happy 60th birthday!

Ben



Todd Brun

Dear John,

It is my honor to contribute this small sketch to the compilation in honor of your 60th birthday. My obligation to you goes back to graduate school, but it continues up to the present day--and, I hope, will continue on for a long time to come.

As a Ph.D. student in Physics at Caltech in the early 1990s, I worked largely on my own. My advisor provided me with scientific inspiration and guidance, but he was both very busy and frequently absent on travels. You were one of a small number of other professors who kept a gentle eye on me from time to time, helped maintain my connection to the rest of the department, and to whom I could go for practical help when I needed it. Though you were not my advisor, you let me have the benefit of your advice, and I might well not have graduated without it.

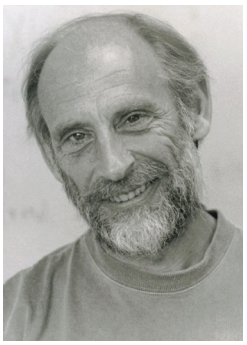
After graduation, my work drifted from the foundations of quantum mechanics to the newly emerging area of quantum information science. It was a happy coincidence when I discovered, a couple of years later, that you had started working on quantum computation as well, and that you and your students were laying the groundwork for much of our current understanding of how practical quantum computation might be achieved. You encouraged me to visit Caltech whenever I could, and my visits always left me with renewed interest and determination.

Then, when I became a faculty member at USC, you invited me to participate in the Institute for Quantum Information as a visiting faculty member. Thanks to you, Caltech remains my home away from home, and I feel very much a part of the vibrant research community that you have built around you. For your mentorship and many kindnesses over the years, I owe you a debt of gratitude--and one that I hope I will have many opportunities to repay.

This small paper on fault-tolerant quantum computation is inspired by your work, and builds on the foundations that you helped to lay. I hope that you find it of interest. My best regards, and congratulations on this milestone in your life and work.

Very sincerely yours,

Todd Brun



Leonard Susskind

Stanford University

Stanford Institute for Theoretical Physics

Dear John,

It's an honor for me to be invited to speak at your festschrift. You, of course, are one of the great leaders of the quantum-information revolution, and I am really a nobody in the subject.

We have been friends for a very long time, and I'm sure that we have influenced each other about many things, from monopoles in the early universe, to fundamental questions about quantum chromodynamics. But the deepest things I learned from you were about black holes. I especially remember the year 1993 when we met in Santa Barbara, during the dark ages of black hole quantum mechanics. I came to talk about something I called Black Hole Complementarity, a subject I was pretty sure no one else (with the possible exception of 't Hooft) was thinking about. I found out differently; John Preskill had been thinking about the same things, and in many ways more deeply than I had. It was enormously encouraging to me that you had come to the same conclusions as I had.

Black Hole Complementarity, more or less as we had conceived it in 1993, became "received wisdom" over the intervening years. For me it seemed to culminate with the remarkable (amazing seems a better word) paper that you wrote with Patrick: "Black Holes as Mirrors". The insight that you brought to the subject seemed to seal the deal and put a pretty bow around it.

But theoretical physics is full of surprises. For the last several months everything we said about Black Hole Complementarity has been challenged by the Santa Barbara firewall enthusiasts. The threat is serious. I'm uncertain about the answer, and it worries me even more that you uncertain. But still, I bet in the end we will come out of it ok.

Happy Birthday and my best wishes for many more,

Lenny

Black Hole Complementarity and the Harlow-Hayden Conjecture[†]

Leonard Susskind[b]

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[†] **On the occasion of John Preskill's 60th birthday.**

Abstract

I'm not sure whether John Preskill wrote any paper using the term *black hole complementarity* before 2007, but he was certainly one of the founders of the subject.

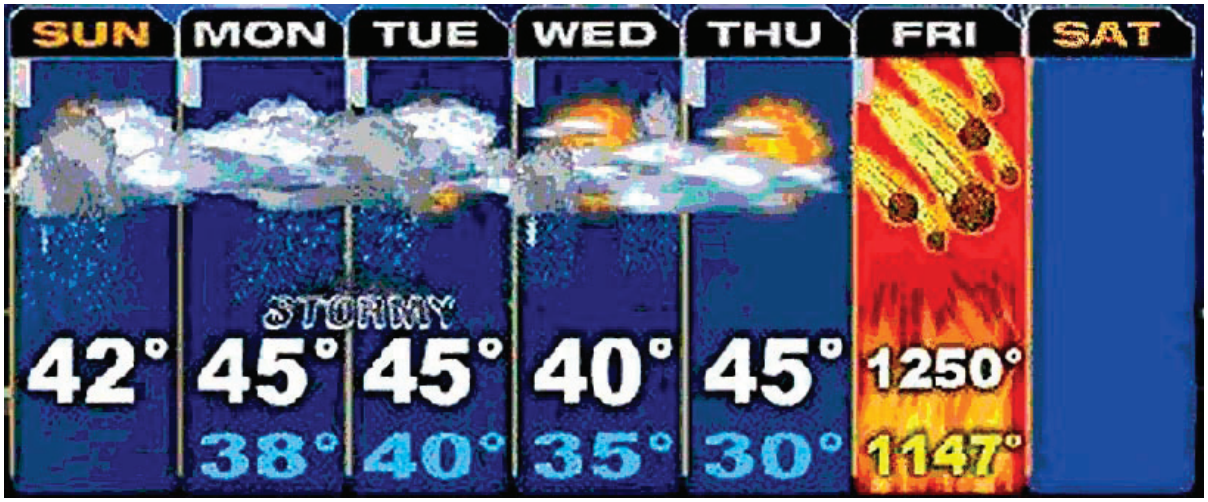
But right now black hole complementarity is under fierce attack by the firewall advocates, who argue that the postulates of BHC are internally inconsistent. The firewallers may be right, and for a time I was pretty sure they were, but I'm not so sure any more. The key to the paradox may lie in the validity of a conjecture—due to Daniel Harlow and Patrick Hayden—about the limits of quantum computation. If the conjecture is correct then black hole complementarity may be alive and well.

Charles Bennett

Quantum Information, Algorithmic Information, and Cosmology

Charles H. Bennett (IBM Research Yorktown)

I got to know John Preskill while visiting Caltech 20 years ago. At the time, quantum information was more a collection of intriguing phenomena than a coherent field of inquiry. Some of the peculiar effects I told him about then, such as Wiesner's superdense coding, and Schumacher's whimsical remark about it that one of the bits "travels backward in time through the entangled pair", may have piqued his interest in the field which he later did so much to build into a mature discipline, both personally and through his students. Recently, John has piqued my interest in cosmology, and its relation to the deceptively simple question “Why is the world complicated?” This question is hard to escape in modern cosmology, because so many possible worlds are boringly simple, but it is also hard to pin down, both because we don’t know how to correct for anthropic bias (our world—the part of the universe accessible to us—may not be typical) and because we need a good definition of complexity, of that which increases when a self-organizing system organizes itself. The following cosmological musing, based on a December 29 blog post, was provoked by my reading John’s blog post about the firewall question amid the flurry of silly warnings that the world would end at the wraparound of the Mayan Calendar. I am grateful to John for inspiring this, and Alejandro Jenkins for some helpful criticism, but I remain fully responsible for the remaining inaccuracies and misconceptions.



Joe Polchinski

Dear John,

You have had a big influence on two of my favorite and most original papers: the renormalization proof written 30 years ago at Harvard, and the firewall paper written last year. I have contributed the latter to this volume, because it is closely connected to much of your work — your paper with Patrick Hayden, of course, but also your many other contributions to black hole quantum mechanics over the years. Also, you helped us to frame the argument, your questions at the KITP conference forced us to sharpen it. But I'd also like to fill in some details on the genesis of the other paper, which you described in your recent blog post.

When I sat down in your class that day, I had studied all the classic proofs of renormalization, from Bjorken and Drell, BPHZ, Callan, and probably others, and was interested to see your approach. I was dissatisfied that they all seemed to depend, to greater and lesser extents, on graphical arguments, whereas the criterion for renormalizability was simply dimensional analysis. From applications of effective field theory I had a mental picture of a flow between renormalizable and nonrenormalizable couplings. But I had never put the two together. So when you said, very clearly, that you found none of the existing proofs satisfactory, the entire argument leapt into my mind. Your post mentioned, correctly, how quickly how quickly the paper was written. This was the first really original thing I had done, the first time I felt that I could change how we thought about something fundamental, and I was in a hurry to get it out there. I wish now that I had taken two extra weeks, to write it more cleanly.

You may feel that your part in this was small, but a perceptive remark at the right time can have a big effect. It is entirely possible that without it my paper might never have been written, it is not something that I was actively thinking about. It also helped that I had a good coffee buzz that day, so thanks for inspiration also went to C. Arabica (the coffee plant) and also to Dan Friedan, who taught me what relevant and irrelevant meant.

Beyond this, I'd like to thank you for being such a calm and thoughtful colleague through the years, for the well-written notes that you generously post on your site, and for lowering my expectations.

With fond memories and wishes for the future,

Joe Polchinski

Aram Harrow

On birthdays and clocks

As we celebrate John Preskill's contributions to the field of quantum computing, I want to say a little about how he influenced me personally, and to discuss one of my favorite examples of his work.

In fact, a talk by John Preskill on quantum error correction was my first introduction to the area of quantum computing. It was the Nov 4, 1999 MIT Physics department colloquium; I was a junior majoring in math and physics; and I remember seeing boxes containing colored balls being attacked by snakes. I may not have fully understood what was going on, but I knew I had stumbled across something fascinating and deep. In hindsight, I am very glad I happened to go to that colloquium!

In grad school, I was lucky to be able to make several visits to John's group at Caltech, usually during the month of January. On one such trip, discussions with the IQI postdocs led me to come up with the concept of coherent classical communication [4]. The idea of coherent classical communication is that if a classical message can be sent in a way where the environment learns nothing about it (as in super-dense coding, entanglement-assisted channel coding, or many other protocols), then in fact something stronger than classical communication, but weaker than full quantum communication, can be achieved instead. What made this work possible was the connection, which at the time most of us had only slowly begun to digest, between entanglement and privacy that was established in the famous Shor-Preskill QKD-security proof [9]. Only recently has this extremely useful perspective come to fully take over quantum information [1, 3].

Birthdays are a good time to think about clocks and the processes by which we all agree on a common time. Around the same year as [9], John also wrote another beautiful paper that has received much less attention, on the difficulty in applying error correction to clock synchronization [7]. This paper discussed the idea of making clocks out of qubits evolving according to the Hamiltonian $H = \omega\sigma_z$. Preparing the product state $|+\rangle^{\otimes n}$ and measuring each qubit after time t will yield an estimate that has error $O(1/\omega\sqrt{n})$, while starting with the cat state $\frac{|0^n\rangle+|1^n\rangle}{\sqrt{2}}$ can achieve error $O(1/\omega n)$. The catch of course is that the cat state is extremely vulnerable to noise. Might quantum error correction be able to protect it? Here [7] enters and shows that various attempts all fail. For example, an error correcting code that would prevent errors would also *stop the clock*, thereby protecting the state while doing nothing to achieve clock synchronization.

I believe that this paper makes a general point about the difficulties of applying the principles of fault-tolerant quantum computing to the problems of *input/output* that we would eventually like a quantum information processing device to help with. Classical computers protect their information with large repetition codes (by using the state of many electrons to encode each bit), but via analog-to-digital and digital-to-analog converters, can also be useful tools in manipulating unprotected classical information. Similarly we hope to use entanglement and techniques from quantum computing for tasks such as precision measurement, but will have to consider no-go theorems based on ideas from Preskill's [7]. Recently steps towards such a universal no-go theorem have been taken by a number of researchers. In an increasing number of settings, it has now been proved that any constant rate of noise turns the $O(1/n)$ error scaling achievable with entanglement into a $O(1/\sqrt{n})$ scaling [2, 5, 6]. A related phenomenon occurs in the oracle problem of unstructured search, where replacing the oracle with one with any nonzero rate of noise destroys the quadratic advantage of Grover's algorithm [8].

Although John is known to all of us as one of the greatest contributors to, and proponents of, the field of fault-tolerant quantum computing, I can't help but point out that he was also the first to identify a large range of areas in which quantum error correction appears *not* to be able to help us. We should all aspire to be as open-minded in our pursuit of the truth! Happy birthday, John.

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[8] O. Regev and L. Schiff. Impossibility of a quantum speed-up with a faulty oracle. In *Proceedings of the 35th international colloquium on Automata, Languages and Programming, Part I*, pages 773–781, Berlin, Heidelberg, 2008. Springer-Verlag.

[9] P. W. Shor and J. Preskill. Simple proof of security of the bb84 quantum key distribution protocol. *Phys. Rev. Lett.*, 85(2):441–444, Jul 2000. arXiv:quant-ph/0003004.

Sergio Boixo

John,

You are of course best known for your personal scientific contributions, but I can best speak of you as an incomparable mentor, for me, and for the field of quantum computation in general. Your research group is an incredibly nurturing environment. I consider myself very fortunate that I could be part of it for two years. I was very intimidated to join a group with so many extremely talented people and with predecessors which I admire without reserves. Right of wrong, I felt that the main rule was this: I had complete freedom to work on anything, but it did help if I found the choice important for some reason, and even better if I could articulate that reason. I remember that Carl Caves told me some time ago that Caltech is the marine corps of science, always ready to be the first to address difficult problems with uncertain payoff. Whether I was in a gridlock with a question that I could not resolve, or there was a new topic to learn about, all doors, yours and anyone else, were always open. I could always find an expert close by. In several occasions I have clearly benefit from this free flow of ideas when I was stuck with a tough mathematical problem, and it just turned out that somebody else in your group had developed the exact technique needed to solve it. Your generosity and integrity is a model for everyone around you.

Happy 60th birthday!

Sincerely,

Sergio

Nate Linder

California Institute of Technology

Division of Physics, Mathematics and Astronomy

Dear John,

There are many things I would like to thank you for. On this occasion, I would like to put some in writing.

As a beginning graduate student, I perceived your name and the Institute for Quantum Information you were running in a sort of legendary aura. I did not know what to expect when I arrived here for my first postdoc interview on US soil. And justly so, because what I discovered upon arriving would have probably been beyond my imagination. I was deeply impressed by the wide spectrum and high level of scientific activity, and even more so when I realized, at the group meeting, that you closely kept track of everything. I still carefully keep the memory of our conversations during that week; when it was over, I could not tell whether I had caught your attention or made a complete fool out of myself.

And so I would like to thank you for giving me the opportunity to become a member of your group (can I finally conclude that the interview wasn't such a disaster?). Professionally, I could not expect a better postdoc experience. Under the professional conditions in your group and more generally, in Caltech, I have been able to transform my research focus and style. Being constantly exposed to cutting edge ideas at the forefront of the field, allowed me to select a research direction I was truly happy with. Without doubt, the opportunity you gave me will have a profound effect on the rest of my research career.

The professional aspects of a postdoc experience are inarguably important; more personal aspects are, in my opinion, of no less importance. Soon after I arrived, I was off to a rough start. During that time, I experienced full support from you, from our group and from the rest of the

Caltech community I was in touch with. This support was crucial in putting me on a fast track to recovery, and I am truly thankful for a remarkable personal experience at Caltech.

I have learned a great deal from our interactions, both direct and indirect. In fact, just by observing you running your group at Caltech, I have learned how a scientist can be at the same time very broad, as well as dig into fine details when he feels it necessary. I enjoyed learning from you; I felt free to ask even the most basic questions, and have always received insightful answers. In many cases, discussing the current problem I was working on was greatly helpful in putting me on the right track. This was all the more true during my work on topological quantum computing, and helped put me in a position to write a review on this topic. I am attaching it to this letter. I would like to thank you for being able to interact with a truly great physicist.

To an outside observer, the differences between the American and Israeli cultures might seem to be subtle. In fact, they are not; to my surprise, I very quickly came to feel at home as one of your group members, and I give you a great deal of the credit for this smooth transition. Your door was always open when I needed advice, on both professional and more personal matters. After I return to Israel, I hope to keep in touch, and hopefully even to be able to repay you with a similarly warm hospitality.

Happy 60th Birthday, and thanks again for everything,

With friendship, Netanel

Physicist John Preskill turns 60

Quantum information theorist at Caltech is widely regarded for his own contributions and his support of young researchers in the field. *Nature Relocations* has asked his former students, postdocs, and colleagues to provide their thoughts to mark this special occasion.

Dear John,

I get asked a lot about what it was like to be at Caltech. I guess the name "Caltech" transcends the world of science and research. People have a curiosity about the home of Richard Feynman, of JPL, of the characters in the Big Bang Theory, and of the most losing basketball team in the world. But the question that people ask has an easy answer, and one that I never tire of telling. Caltech is truly a special place, and the three years I spent as a postdoc in the IQI have been by far the best experience of my professional career.

I've actually never asked you what your vision was when you set up the IQI, but whatever it was, it works beautifully. I can honestly say that I never had a bad day in my three years there. To me, the IQI was a place that I was excited to go to each day, to learn, talk about, or try and figure out something new. To throw around crazy ideas and see what sticks. Heck, even to go and dabble in experiments, to see if your idea might actually work. And best of all, to be around a bunch of people who are completely gung-ho about doing science. I think that everyone fed off of your curiosity about all things unknown – it showed up in the lunchtime conversations and group meetings and everything in between. To be in a place where scientific curiosity was the first, last, and only thing that mattered was truly special.

And beyond any of the actual research that I did, that environment at the IQI is the most lasting memory of my time there. At the risk of sounding cheesy, every day it serves as inspiration of why and how I want to do science. So, John, thank you – I am eternally grateful for that. Happy 60th birthday!

With best regards,
Darrick

Dear John,

It is very difficult to express in writing how much I enjoyed my time at IQI or how grateful I am to you for making it all possible. Your great influence on me personally, however, is not limited to IQI itself even though I don't think I ever mentioned this to you. Almost ten years ago in 2003, you gave a university colloquium in Leiden in the Netherlands. At that time I was still an undergrad in Amsterdam, but as I was very curious about quantum information I had gone to Leiden to see your talk. I am sure you don't remember me from Leiden, as I was much too terrified to talk to anyone there at all.

Nevertheless, your talk itself had a great effect on me as it was my first encounter with quantum error correction, and showed me that quantum computing was not just about algorithms - but rather that my prior notions about classical error correction and information theory were completely inadequate and quantum information was fundamentally different. If it hadn't been for your inspiring talk, I might have missed this beautiful subject and gone on to do something rather different.

Some time after my first visit to IQI, you offered me a place in your group. I remember I was actually at my computer just when your email arrived and I almost replied to you within the span of about a minute. Almost, because I had this strange idea - quite bizarre in retrospect - that I should maybe control my excitement and pretend to think about this, and so I waited nearly three days to tell you that I would be most happy to come. However, it would be a stretch to say that it took me even one minute to decide. In fact, I should confess that I was so much hoping to join IQI that I had not even applied anywhere else at that point, simply because I was still trying, and failing, to identify a place that would even remotely compare.

Needless to say Caltech itself was a very special place, and as a whole offered me a glimpse of very diverse and intriguing research. I have never felt so at ease - so comfortable and relaxed anywhere else. Caltech itself, however, is not what made IQI special. That was entirely your doing, and I'm sure IQI would have been equally fantastic anywhere else. You created a great place for us to discuss, do research, travel, host our own visitors and even hold workshops. You allowed us to be entirely free in pursuing our own passions, but at the same time showed keen interest and concern in whatever we were doing and always had an open mind. You devoted your time to keep up with all the diverse strands of research at IQI, where you could switch from one topic to the next with seemingly no effort at all. For all of this, and more, I am deeply grateful.

I am attaching two snippets from my time at IQI. On a personal level, my stay was great fun, and one of the things you may remember is the retrieval of your lounge chair after our move to Annenberg. In case you're wondering how it actually made its way to Annenberg, Darrick and I are now prepared to reveal the true story. I also very much enjoyed my time scientifically. I am sure somewhere in your records about the group meeting you will find vague claims by myself that one could use quantum techniques to prove convergence of SDP hierarchies for polynomial optimization. It took some time, but Andrew and I have now finally completed our article! We are attaching it because it draws on ideas starting with Andrew's work on testing separability done during his own stay at IQI nearly a decade ago, all the way up to ideas developed during my own stay and afterwards. For me personally, it represents one of the things that I enjoyed most about IQI namely that ideas from very different areas, be it physics, computer science - in fact any strand of quantum information - have the opportunity to interact at IQI to find a very new approach. Again, many thanks for making this all possible.

With this, I wish you a very happy 60th birthday!

Warm regards,
Stephanie

Dear John,

It is amazing to reflect on my time at Caltech and to realise that it is nearly ten years since I left. The intellectual environment there is something that I have missed ever since, and in particular the group you led which became the IQI. The impact that the IQI and your scientific and intellectual leadership has had on quantum information is truly impressive. Importantly for me though, my entire research program now has arisen very directly from the things I learnt during my time at Caltech and as part of the IQI. I was a very ignorant student from a distant province when I first arrived and I am incredibly grateful for the opportunities you and others at Caltech gave me.

For this reason I am particularly happy that Stephanie and I can include our recent paper in this booklet. Although it has no physics really, it grows very directly from work I did with Federico Spedalieri and Pablo Parrilo when I was at Caltech and it reflects the unexpected interactions and genuinely interdisciplinary work that goes on there.

Your example as a group leader is one that many of us who are alumni of the IQI have tried to emulate in our later careers. I think many of us particularly remember the Preskill group meetings and the weekly ritual of reporting what we had been doing that week, the subsequent whiteboard talk, and your searching questions from the floor throughout. Although the components are familiar, your meetings have a special flavour that seems to have influenced many groups in our field where alumni of the IQI regard the Preskill group meeting as a gold standard. I know at Sydney that when Stephen Bartlett and I discuss what is working and not working in our group, we constantly refer to how things are done at the IQI.

However, one aspect of the meetings that I do recall as causing occasional grumbling was the fact that after the group increased in size meetings had been known to exceed three hours! On one occasion when the meeting room in Jorgenson was particularly packed matters came to a head and it was decided to draw lots to divide the group in two so only half would report on their week's work. A Canadian coin was at hand and the group was to be divided into "Queens" and "Bears" to match the two sides of the coin. As it happened the natural dividing line down the middle of the room ran straight through me and you needed to adjudicate which side I should be deemed to be on. You looked thoughtful for a minute and remarked, quite seriously but perhaps with a gleam in your eye, that in seeing me around campus you had always thought that I seemed more a queen than a bear. And so that was that!

It is this sort of careful and considered judgment that has led to your great success in research, in building a group and in inspiring others in your research field!

Have a very happy 60th birthday!

Warm regards,
Andrew