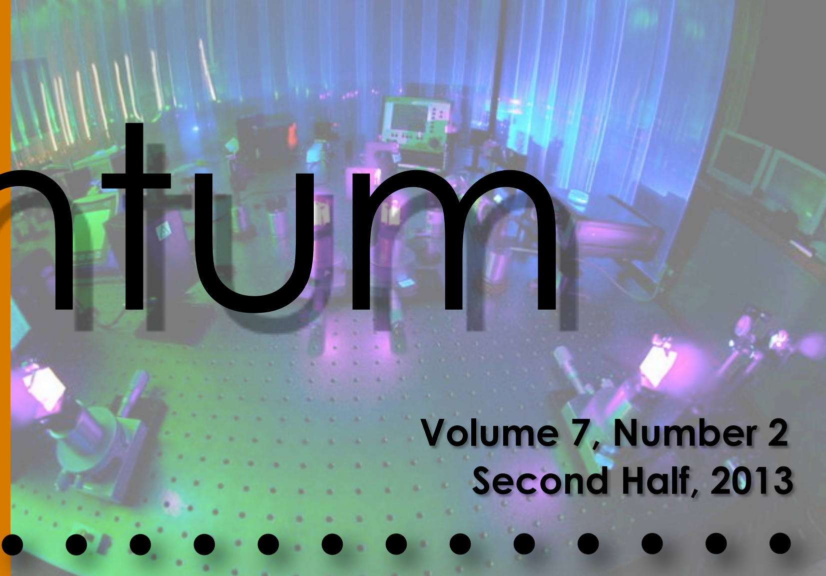
The Quantum Times



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Inside this issue

How I came to know Jeff Kimble

Jun Ye

Editor's note: *The following first appeared on quantumfrontiers.com on the occasion of Jeff Kimble's receiving the Herbert Walther Award from the Optical Society of America and the Deutsche Physikalische Gesellschaft (German Physical Society) in December of 2012. It is reprinted here in its entirety with the consent of the author. GQI offers its heartfelt congratulations to Jeff on this historic award and its thanks to the author and to John Preskill for facilitating the inclusion of this article in The Quantum Times.*

I heard of Jeff Kimble long before I met him in person. Legend had it that he was extremely rigorous with research and very tough on nonsense. So when I decided to approach him in October of 1996, at the annual OSA meeting in Rochester for a possible postdoc position, I was as nervous as I was excited. To be sure, I had learned a few experimental tricks from Jan Hall; and yes, I had remembered a bit of quantum optics theory from Marlan Scully. But, here was a guy who dealt with the annihilation operator as deftly in the lab as on paper; so I was hesitant. Then I listened to his lecture on flying qubits and single-photon quantum logic gates — his speech for the Max Born Award. Armed with courage after surviving my own very first invited talk at OSA, I decided to give it a try.

I still remember most of our discussions from that first meeting, but none is as clear as my recollection of the pain from Jeff's handshake. His grip was more than just firm; it actually squeezed the bones of my hand. So naturally, I took the handshake as a sign that he really wanted me to join his group. When an offer of a Caltech fellowship arrived three months later, I accepted it without hesitation. In 1997, I had no way of knowing that Jeff's way of doing science would leave a profound mark on my career and that his deep friendship would continue to enrich my life and that of my family for many years.

After I arrived at Caltech, Jeff asked me to work on a project involving single-atom cavity quantum electrodynamics (cavity-QED). As a newcomer to the field, I studied the literature to educate myself on the relevant scientific context and learn latest tools of the trade. In its simplest form, cavity-QED describes the coupling of a single atom to a single mode of a radiation field. However, this deceptively simple system is rich for fundamental physics.

The development of the field of cavity-QED began in the 1970s and early 1980s in the disciplines of atomic physics and optical physics. The atomic physics community focused on solving the "boundary" problem for quantum optics, first identified by Casimir and Purcell. This effort led to the demonstration of suppressed

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spontaneous emission in a microwave cavity by Daniel Kleppner and electron radiation in an ion trap by Hans Dehmelt. Meanwhile, the optical physics community was studying optical bistability and other emerging nonlinear optical phenomena. Both communities focused on the dissipation dynamics of cavity-QED systems.

Herbert Walther soon realized the importance of coherent interactions for cavity-QED and revived the community's interest in the Jaynes-Cummings model. This then turned into the main theme of modern cavity-QED: the study of the single quanta-dominated strong coupling regime, where quantum dynamics dominate over dissipation. For his part, Jeff pioneered the development of strongly coupled cavity-QED in the optical regime, in an effort parallel to the historical achievement of Serge Haroche, this year's Nobel Laureate, on the microwave cavity with Rydberg atoms.

Jeff's contribution has been profound. He established explicit connections between the boundary problem and optical bistability research in cavity-QED. More importantly, he pushed for an increasingly large role for coherent coupling between atoms and optical fields from the very beginning; in 1992, he was the first to demonstrate strong coupling in cavity-QED with the observation of "vacuum-Rabi" splitting for a single atom in an optical cavity.

Exploring new physics in the strong coupling regime has been one of the mainstays of Jeff's scientific career. Quantum dynamics can no longer be treated in the perturbative limit. Researchers are now forced to explore genuine quantum dynamics. Such explorations have led to a revolution in the study of open quantum systems and quantum measurement. Jeff himself later went on to expand optical cavity-QED to touch on many areas of modern quantum physics: quantum measurement, open quantum system dynamics, quantum entanglement, and quantum information science in general.

It was with this scientific background that Hideo Mabuchi (then a graduate student of Jeff's) and I started working together on an experiment to extract the full susceptibility of a single atom as it transits through a small optical cavity in the strong coupling regime. Our work was motivated by the goal of monitoring real-time quantum trajectories to describe an open quantum system in which evolution dynamics are conditioned upon measurement. We were initially bothered by a persistent birefringence inside the optical cavity. This birefringence was imprinted on the mirror reflection phases by the mechanical stress exerted on the mirror substrates. The uncertainty of the polarization eigenaxes and the differential phase shift introduced imperfection in the stabilization of the cavity resonance and introduced an error into the full amplitude-phase analysis of the transmitted optical field.

I took it upon myself to study this birefringence problem. I made a careful set of measurements, performed a detailed analysis, and determined the property of the cavity birefringence to high precision. I was asked by Jeff to give a presentation to the group. I remember that I did not take the talk very seriously, figuring that it was just a small technical step in the grand scheme of things that would be all quantum mechanical. After the talk when the dust had settled on questions and answers, Jeff said to the group, "Well, this is how serious measurements are made. Never mind big scientific pictures or theory framework, a serious experimental measurement is what we can contribute to science in the most meaningful way." Somehow this statement was grilled deeply into my memory, perhaps because it came from a first-rate experimental physicist who was also an original thinker responsible for many of the theoretical concepts in quantum optics. I have certainly tried to pass down this perspective to my own students.

About six months into my postdoc tenure, Hideo and I finished our project. Hideo graduated and left for Princeton. Another graduate student, David Vernooy, teamed up with me, and our task was to create the first deterministic single-atom cavity-QED system. We needed to trap a single atom inside a high finesse cavity over an extended period of time and then use this atom over and over for whatever quantum tasks we could imagine to pile up on the poor fellow. Dave was so full of energy that by the time I got into the lab each morning, he had already ridden his bike up 2000 vertical feet and back from Mount Wilson behind Caltech. Later in life I would directly experience Jeff's intensity performing this very same exercise. In any case, we emptied an entire lab and started building our apparatus from scratch. This was a terrific time in which I learned all kinds of experimental tricks by simply trying various schemes in the lab. Jeff trusted whatever Dave and I cooked up! It was the golden time in my life — I could play hard, but I had minimal responsibility!

There was a particularly memorable incident. I designed a vacuum chamber that would allow us to collect cold atoms in one location and then shuffle them into another place where the scientific cavity was located. However, I made a serious mistake in the design. The glass cell where we would do our initial laser cooling and trapping was connected at both ends to the main vacuum chamber. I knew that the stress on the cell could be a problem, so we added two bellows, one at each connection. We pumped it down and baked it out over the weekend. I went to the lab on Sunday night to turn off the heat, thinking that by Monday we could start aligning optics around the apparatus. When we returned to the lab the next day, we found a crack in the glass.

Thinking back on this incident, I always feel chagrined that I did not learn my lesson right away. What I actually did was to immediately drive to a local glass blower in Pasadena and ask him to make me a new glass cell. A week later, the new cell was in, pumped down, and baked. This time, we even cooled it down and arranged the

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optics for laser cooling around the cell. Two days later, we found the cell had imploded. I had a sense of despair that morning, along with a guilty conscience for taking a short cut. Jeff's response upon seeing the pile of glass was "Oh Jun, Dave and you have worked so hard on this … Let's move on." Dave and I ordered a mini-vacuum octagon, with overnight delivery, placed it where the cell used to be, and because we already had so much experience in setting up pump, bake, and cool down, we got it running within a couple of days. The system would go on to become a workhorse in Jeff's lab for the next 10 years.

Fast forward a year. I was then in my last few months as a postdoc in Jeff's group. Dave and I labored hard, and we began to observe atomic trajectories from the cavity's transmitted field. We used the atomic signals to trigger a classical trapping field to capture the atom while it was traveling through the cavity. We met with a small success: we observed that the selected atoms started hanging out a bit longer inside the cavity because of our control field. Jeff started to come in to the lab at night and over the weekends to work with us. He made pages of Mathematica calculations on the classical and quantum field distributions inside the cavity.

On the very last night of my Caltech postdoc career, with my wife and daughter sleeping in the on-campus club house of the Athenaeum and our furniture already on the way to Boulder, Colorado, Jeff, Dave, and I pulled off an all-night adventure in the lab. That night, we witnessed individual atoms remaining for about 1 second in the cavity when we asked them to. It was an incredible feeling when I walked out of the East Bridge Laboratory of Physics into a cool morning breeze in Pasadena. My ears echoed with Jeff's excitement, "Jun, we have climbed on top of a hill now, and I can see so many flowers lying in the valleys ahead." I boarded a plane to Denver that morning.

It turns out that the flowers would not blossom for awhile. People had a hard time repeating the experiment with long trapping times. Jeff never wavered in his belief that what we saw that night was real. However, our experimental controls had not been robust, nor optimized. Careful investigations were launched into the thermal excitation modes of the cavity, parametric heating of the trap, polarization impurity of the intracavity light, and residual magnetic fields, and so forth. After a year or so of hard work, things did work again, this time systematically. Jason McKeever and colleagues in Jeff's lab observed atoms trapped for multiple seconds inside the cavity, at any time they wanted. Finally, the flood gates opened. Accomplishments included the realization of a one-atom laser in a regime of strong coupling, the generation of single photons on demand, and the observation of photon blockade.

Jeff is a unique and distinguished scholar in the atomic, molecular, optical, and quantum physics community. His scientific vision and rigor, dual competence in experiment and theory, as well as the quality and impact of his published work all set very high bars for our field. He embodies many outstanding characteristics as a scientist. He is a daring pioneer, an original thinker, a groundbreaking technologist, and a relentless seeker of ultimate truth and knowledge. In the early days, I often wondered how he achieved these qualities in life. Finally, I started to peek into his secret sauce once I had a chance to develop a profound friendship with him. I have been invited back to Caltech many times, and Jeff has come to visit me and my family in Colorado a few times. We have ridden mountain bikes together, drunk Texas beer, and camped out in remote areas of Utah.

When our second daughter, Selene, was about 4 years old, Jeff came to our house for dinner. Selene had never seen a guy as tall as Jeff so she could not stop playing with him. One of their games was a two-person thumb war, where Jeff's enormous palm would completely absorb Selene's tiny one (and I hoped the squeeze was gentle). And yet, Selene's thumb remained visible as she tried really hard to capture Jeff's thumb. Watching from a distance, I saw Jeff's complete absorbance in the game. I wished Selene had been old enough to realize how seriously Jeff was playing the game with her. He was playing with her with the same concentration that has characterized most things Jeff has wanted to pursue in life.

On this memorable occasion of Jeff's being awarded the Herbert Walther Award from OSA and the German Physical Society, I could not help but write down these words to help celebrate his legacy and to thank him for the many things he's given to us.

Jun Ye *is a Fellow at JILA and NIST in Boulder, Colorado. He received his PhD in 1997 from the University of Colorado at Boulder under Jan Hall and worked as a Milliken postdoctoral fellow at Caltech under Jeff Kimble from 1997 to 1999 when he returned to JILA.*

POINT–COUNTERPOINT

Can anything be learned from surveys on the interpretations of quantum mechanics?

In what follows, **Matt Leifer** and **Nathan Harshman** present opposing views on the value of surveys on foundational attitudes towards quantum mechanics. Three such surveys were recently published and their results are summarized in Table 1. Matt takes the `point,' arguing that such surveys are *not* useful, while Nathan takes the `counterpoint.' A complete set of references for both is given at the end.

POINT MATT LEIFER

Q1. Which of the following questions is best resolved by taking a straw poll of physicists attending a conference?

A. How long ago did the big bang happen?
B. What is the correct approach to quantum gravity?
C. Is nature supersymmetric?
D. What is the correct way to understand quantum theory?
E. None of the above.

By definition, a scientific question is one that is best resolved by rational argument and appeal to empirical evidence. It does not matter if definitive evidence is lacking, so long as it is conceivable that evidence may become available in the future, possibly via experiments that we have not conceived of yet. A poll is not a valid method of resolving a scientific question. If you answered anything other than E to the above question then you must think that at least one of A-D is not a scientific question, and the most likely culprit is D. If so, I disagree with you.

It is possible to legitimately disagree on whether a question is scientific. Our imaginations cannot conceive of all possible ways, however indirect, that a question might get resolved. The lesson from history is that we are often wrong in declaring questions beyond the reach of science. For example, when big bang cosmology was first introduced, many viewed it as unscientific because it was difficult to conceive of how its predictions might be verified from our lowly position here on Earth. We have since gone from a situation in which many people thought that the steady state model could not be definitively refuted, to a big bang consensus with wildly fluctuating estimates of the age of the universe, and finally to a precision value of 13.77 +/- 0.059 billion years from the WMAP data.

Traditionally many physicists separated quantum theory into its "practical part" and its "interpretation," with the latter viewed as more a matter of philosophy than physics. John Bell refuted this by showing that conceptual issues have experimental consequences. The more recent development of quantum information and computation also shows the practical value of foundational thinking. Despite these developments, the view that "interpretation" is a separate unscientific subject persists. Partly this is because we have a tendency to redraw the boundaries. "Interpretation" is then a catch-all term for the issues we cannot resolve, such as whether Copenhagen, Bohmian mechanics, many-worlds, or something else is the best way of looking at quantum theory. However, the lesson of big bang cosmology cautions against labeling these issues as unscientific. Although interpretations of quantum theory are constructed to yield the same or similar enough predictions to standard quantum theory, this need not be the case when we move beyond the experimental regime that is now accessible. Each interpretation is based on a different explanatory framework, and each suggests different ways of modifying or generalizing the theory. If we think that quantum theory is not our final theory then interpretations are relevant in constructing its successor. This may happen in quantum gravity, but it may equally happen at lower energies, since we do not yet have an experimentally confirmed theory that unifies the other three forces. The need to change quantum theory may happen sooner than you expect, and whichever explanatory framework yields the next theory will then be proven correct. It is for this reason that I think question D is scientific.

Regardless of the status of question D, straw polls, such as the three that recently appeared on the arXiv [1-3], cannot help us to resolve it, and I find it puzzling that we choose to conduct them for this question, but not for other controversial issues in physics. Even during the decades in which the status of big bang cosmology was controversial, I know of no attempts to poll cosmologists' views on it. Such a poll would have been viewed as meaningless by those who thought cosmology was unscientific, and as the wrong way to resolve the question by

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those who did think it was scientific. The same is true of question D, and the fact that we do nevertheless conduct polls suggests that the question is not being treated with the same respect as the others on the list.

Admittedly, polls about controversial scientific questions are relevant to the sociology of science, and they might be useful to the beginning graduate student who is more concerned with their career prospects than following their own rational instincts. From this perspective, it would be just as interesting to know what percentage of physicists think that supersymmetry is on the right track as it is to know about their views on quantum theory. However, to answer such questions, polls need careful design and statistical analysis. None of the three polls claims to be scientific and none of them contain any error analysis. What then is the point of them?

The three recent polls are based on a set of questions designed by Schlosshauer, Kofler and Zeilinger (SKZ), who conducted the first poll at a conference organized by Zeilinger [1]. The questions go beyond just asking for a preferred interpretation of quantum theory, but in the interests of brevity I will focus on this aspect alone. In the Schlosshauer et al. poll, Copenhagen comes out on top, closely followed by "information-based/information-theoretical" interpretations. The second poll comes from a conference called "The Philosophy of Quantum Mechanics" [2]. There was a larger proportion of self-identified philosophers amongst those surveyed and "I have no preferred interpretation" came out as the clear winner, not-so-closely followed by de Broglie-Bohm theory, which had obtained zero votes in the SKZ poll. Copenhagen is jointly in third place along with objective collapse theories. The third poll comes from "Quantum theory without observers III" [3] at which de Broglie-Bohm got a whopping 63% of the votes, not-so-closely followed by objective collapse.

Table 1. The collected responses to the three published sets of survey results [1-3] for the question "What is your favorite interpretation of quantum mechanics?" is shown below. Note that respondents were allowed to check more than one box and that Sommer [2] included "Shut up and calculate" as a separate interpretation, whereas Schlosshauer, Kofler, and Zeilinger [1] and Norsen and Nelson [3] do not.

Survey	**SKZ [1]**	**Som. [2]**	**NN [3]**
Number of Respondents	33	18	67
A. Consistent histories	0%	0%	1%
B. Copenhagen	42%	11%	4%
C. De Broglie-Bohm	0%	17%	63%
D. Everett (many worlds and/or minds)	18%	0%	0%
E. Information based/information theoretical	24%	6%	5%
F. Modal Interpretation	0%	0%	0%
G. Objective collapse (e.g., GRW Penrose)	9%	11%	16%
H. Quantum Bayesianism	6%	6%	3%
I. Relational quantum mechanics	6%	6%	0%
J. Statistical (ensemble) interpretation	0%	6%	4%
K. Transactional interpretation	0%	0%	0%
L. Other	12%	0%	8%
M. I have no preferred interpretation	12%	44%	11%
N. "Shut up and calculate"	NA	17%	NA

What we can conclude from this is that people who went to a meeting organized by Zeilinger are likely to have views similar to Zeilinger. People who went to a philosophy conference are less likely to be committed, but are much more likely to pick a realist interpretation than those who hang out with Zeilinger. Finally, people who went to a meeting that is mainly about de Broglie-Bohm theory, organized by the world's most prominent Bohmians, are likely to be Bohmians. What have we learned from this that we did not know already?

One thing I find especially amusing about these polls is how easy it would have been to obtain a more representative sample of physicists' views. It is straightforward to post a survey on the internet for free. Then all you have to do is write a letter to *Physics Today* asking people to complete the survey and send the URL to a bunch of mailing lists. The sample so obtained would still be self-selecting to some degree, but much less so than at a conference dedicated to some particular approach to quantum theory. The sample would also be larger by at least an

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order of magnitude. The ease with which this could be done only illustrates the extent to which these surveys should not even be taken semi-seriously.

I could go on about the bad design of the survey questions and about how the error bars would be huge if you actually bothered to calculate them. It is amusing how willing scientists are to abandon the scientific method when they address questions outside their own field. However, I think I have taken up enough of your time already. It is time we recognized these surveys for the nonsense that they are.

NATHAN HARSHMAN COUNTER-POINT

Let us first dispense with three easy criticisms of the Schlosshauer, Kofler, Zeilinger (SKZ) survey [1] and its other applications [2,3]:

1) **Quantum interpretation is a waste of time.** Even in the "Point" to my "Counterpoint" Matt Leifer grants that recent developments (i.e. over the last fifty years) have made quantum interpretation a subject for polite scientific company. Questioning quantum interpretation probes the murky border between physics and metaphysics, but that has been demonstrably productive in all the traditional senses of scientific progress: description, prediction, explanation and control. We now can, with a straight face, say "quantum teleportation" and "cat-like entanglement" even to funding agencies and even in the United States.
2) **This survey cannot reveal the Truth About Quantum Mechanics.** This criticism is entirely valid. I have colleagues that self-identify as social scientists and even they know that the primary purpose of surveys is to reveal information about the respondents. I remember watching the television game show The Family Feud as a child. The survey question was "Name a big fish" and a randomized sample voted "whale" by a landslide. That uncovers a gap in the science education of the respondents but should not be taken as taxonomical truth. The SKZ survey, and those who have repeated it, never imagined that it would reveal "Yes, in fact, Quantum Bayesianism is the Truth About Quantum Mechanics." Abusing terminology slightly, the intention of this survey is not to determine the ontic state of quantum mechanics as a theory, but to explore the epistemic state of quantum mechanics as a people.
3) **This survey is imperfect.** This criticism is also valid. And, if the survey were a scientific instrument, it would be our solemn duty to observe, hypothesize, experiment, and revise it until we have sharpened its resolution to the quantum limit, so to speak. However, since this survey is not a scientific instrument, we should feel no such compulsion. Optimizing the quantification of a potentially metaphysical stance is, to my mind, missing the point. I concede that larger sample sizes, more representative samples and research-based question revision could provide more meaningful results, even if the survey is acknowledged as a qualitative, subjective tool. But such seemingly scientific steps will not lead us to a quantitative, objective Truth Discovering Instrument. But again, that is not what this survey is attempting to do.

So then, is using an imperfect, non-scientific, possibly metaphysical tool a waste of time and nonsense, as claimed by the "Point"? I believe not, for at least the following reasons.

1) **The survey is an active-learning experience for quantum physicists.** Depending on the version of the survey, there are thirteen or fourteen item choices for "Question 12: What is your favorite interpretation of quantum mechanics?" The mind reels at the multiplicity, and puzzling through the implied comparisons is a satisfying mental work-out. I hereby assign the survey as homework for every member of the Topical Group, and it should be taken open-book, with trusty Google close at hand. For example, after a little sleuthing [4] in the on-line Stanford Encyclopedia of Philosophy, I have discovered a new, and according to the survey results uncommon, pleasure: the Modal Interpretation. (However, I admit it may only be my favorite interpretation until the next interpretation sweeps me off my feet. As an undergrad, I had a torrid affair with Everett, and although in grad school I pledged troth to Copenhagen, I secretly dallied with the Ensemble Interpretation.)

   Like all good active-learning educational tools, the survey authors intentionally built ambiguity into the survey. The survey authors seem to delight in the discomfort elicited by the vagueness of the survey items. For example, they begin the commentary after "Question 9: What interpretation of quantum states do you prefer?" with the statement "This is a perfect example of a question where the options are not well

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defined." Again, if this were intended to be a scientific instrument this would be an admission of gross misconduct, but here it signals that this survey is an attempt to activate the survey-taker and to encourage discussion. Since, as the "Point" admits and history has shown that discussing quantum interpretations is scientifically productive, perhaps we should be encouraging other subdisciplines like string theory and cosmology to engage in similar ambiguous and vague exercises. The process of surveying opinions on our field can enable positive change through conversation, consideration and possible conversion and convergence.

2) **The survey reminds quantum researchers that science is a social endeavor.** Please don't mistake this for my support of some controversial hypothesis like "scientific truth is socially constructed" or some other intellectually-flabby pseudo-relative appropriation from a misunderstanding of modern physics. I merely mean that science is an activity done by people, spread through space and time. As an intellectual community, scientists in general and physicists in particular consider themselves on-guard against group-think, personal bias, dogma and other Baconian "Idols of the Mind." Yet, when some report from a funding agency or other peer review undervalues a colleague's work (this happened to my friend once), one often hears the complaint that the work isn't being evaluated on merit, but instead on fashion, reputation or some other social influence. Acknowledging this, our community should welcome any activity that throws light on personal and social processes that can cloud logic and observation. As Norsen and Nelson [3] elucidate, and the "Point" also notes, it is not surprising that at a conference organized by Anton Zeilinger that 76% of respondents averred "Quantum information is a breath of fresh air for quantum foundations," while this selection was preferred by only 15% of respondents at the Bohmian-heavy conference Quantum Theory Without Observers. And I can imagine that if anyone had shown up at these conferences with a predilection for the Transactional Interpretation, she or he may have become so dispirited as to not even complete the survey, explaining that particular null result. Perhaps the biggest concern is that this survey and its sectional analyses could, like one's choice of cable news channel, encourage intellectual tribalism and partisan sniping.

3) **The survey is a vehicle for the celebration and popularization of quantum physics.** The proof is in the raisin pudding. The media found the story charming and gave it attention (see references in [3], also Google). It was discussed in academic corridors, laboratory cafeterias, blog posts, and even APS Topical Group newsletters. Instead of shaking our heads that this is a waste of time, we should rejoice. A few more people heard about quantum physics! Our cultural impact grew! Headlines like "Experts still split on what quantum theory means" and "Why quantum mechanics is an 'embarrassment' to science" may cause a few physicists to roll their eyes, especially among those most confident in their own interpretations, but I still believe the old saw: any press is good press. So let's put this survey in the same category as operas about Oppenheimer and Einstein, plays about Bohr and Heisenberg, sitcoms about Sheldon and Leonard, and lamps made to look like atomic orbitals. Category: Good Things. Subcategory: Non-Science but Pro-Quantum.

Matt Leifer *is a long-term visitor at the Perimeter Institute whose research interests are focused on quantum information and quantum foundations.* ***Nathan Harshman*** *is Chair of the Department of Physics at American University. His research interests center on the intersection of quantum information with particle physics, notably entanglement in composite particle systems.*

References

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[3] T. Norsen and S. Nelson, Yet Another Snapshot of Foundational Attitudes Toward Quantum Mechanics, arXiv:1306.4646 (2013).

[4] O. Lombardi and D. Dieks, Modal Interpretations of Quantum Mechanics, *The Stanford Encyclopedia of Philosophy* (Fall 2013 Edition), Edward N. Zalta (ed.), forthcoming URL = <http://plato.stanford.edu/archives/fall2013/entries/qm-modal/>.

BOOK REVIEW

Computing With Quantum Cats: From Colossus To Qubits
by John Gribbin
Bantam Press, 2013, **$28.95**
ISBN-13: 9780593071151 (hardcover)

Schrödinger's Killer App: Race To Build The World's First Quantum Computer
by Jonathan Dowling
CRC Press, 2013, **$39.95**
ISBN-13: 9781439896730 (paperback)

The task of writing a popular book on quantum computing is a daunting one. In order to get it right, you need to explain the subtleties of theoretical computer science, at least to the point of understanding what makes some problems hard and some easy to tackle on a classical computer. You then need to explain the subtle distinctions between classical and quantum physics. Both of these topics could, and indeed have, filled entire popular books on their own. Gribbin's strategy is to divide his book into three sections of roughly equal length, one on the history of classical computing, one on quantum theory, and one on quantum computing. The advantage of this is that it makes the book well paced, as the reader is not introduced to too many new ideas at the same time. The disadvantage is that there is relatively little space dedicated to the main topic of the book.

In order to weave the book together into a narrative, Gribbin dedicates each chapter except the last to an individual prominent scientist, specifically: Turing, von Neumann, Feynman, Bell and Deutsch. This works well as it allows him to interleave the science with biography, making the book more accessible. The first two sections on classical computing and quantum theory display Gribbin's usual adeptness at popular writing. In the quantum section, my usual pet peeves about things being described as "in two states at the same time" and undue prominence being given to the many-worlds interpretation apply, but no more than to any other popular treatment of quantum theory. The explanations are otherwise very good. I would, however, quibble with some of the choice of material for the classical computing section. It seems to me that the story of how we got from abstract Turing machines to modern day classical computers, which is the main topic of the von Neumann chapter, is tangential to the main topic of the book, and Gribbin fails to discuss more relevant topics such as the circuit model and computational complexity in this section. Instead these topics are squeezed very briefly into the quantum computing section, and Gribbin flubs the description of computational complexity. For example, see if you can spot the problems with the following three quotes:

"...problems that can be solved by efficient algorithms belong to a category that mathematicians call `complexity class P'..."

"Another class of problem, known as NP, are very difficult to solve..."

"All problems in P are, of course, also in NP."

The last chapter of Gribbin's book is a tour of the proposed experimental implementations of quantum computing and the success achieved so far. This chapter tries to cover too much material too quickly and is rather credulous about the prospects of each technology. Gribbin also persists with the device of including potted biographies of the main scientists involved. The total effect is like running at high speed through an unfamiliar woods, while someone slaps you in the face rapidly with CVs and scientific papers. I think the inclusion of such a detailed chapter was a mistake, especially since it will seem badly out of date in just a year or two. Finally, Gribbin includes an epilogue about the controversial issue of discord in non-universal models of quantum computing. This is a bold inclusion, which will either seem prescient or silly after the debate has died down. My own preference would have been to focus on well-established theory.

In summary, Gribbin has written a good popular book on quantum computing, perhaps the best so far, but it is not yet a great one. It is not quite the book you should give to your grandmother to explain what you do. I fear she will unjustly come out of it thinking she is not smart enough to understand, whereas in fact the failure is one of unclear explanation in a few areas on the author's part.

Dowling's book is a different kettle of fish from Gribbin's. He claims to be aiming for the same audience of scientifically curious lay readers, but I am afraid they will struggle. Dowling covers more or less everything he is interested in and I think the rapid fire topic changes would leave the lay reader confused. However, we all know that popular science books written by physicists are really meant to be read by other physicists rather than by the lay reader. From this perspective, there is much valuable material in Dowling's book.

Dowling is really on form when he is discussing his personal experience. This mainly occurs in chapters 4 and 5, which are about the experimental implementation of quantum computing and other quantum technologies. There is also a lot of material about the internal machinations of military and intelligence funding agencies, which Dowling has

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copious experience with on both sides of the fence. Much of this material is amusing and will be of value to those interested in applying for such funding. As you might expect, Dowling's assessment of the prospects of the various proposed technologies is much more accurate and conservative than Gribbin's. In particular his treatment of the cautionary tale of NMR quantum computing is masterful and his assessment of non-fully universal quantum computers, such as the D-Wave One, is insightful. Dowling also gives an excellent account of quantum technologies beyond quantum computing and cryptography, such as quantum metrology, which are often neglected in popular treatments.

Chapter 6 is also interesting, although it is a bit of a hodge-podge of different topics. It starts with a debunking of David Kaiser's thesis that the "hippies" of the Fundamental Fysiks group in Berkeley were instrumental in the development of quantum information via their involvement in the no-cloning theorem. Dowling rightly points out that the origins of quantum cryptography are independent of this, going back to Wiesner in the 1970's, and that the no-cloning theorem would probably have been discovered as a result of this. This section is only missing a discussion of the role of Wheeler, since he was really the person who made it OK for mainstream physicists to think about the foundations of quantum theory again, and who encouraged his students and postdocs to do so in information theoretic terms. Later in the chapter, Dowling moves into extremely speculative territory, arguing for “the reality of Hilbert space” and discussing what quantum artificial intelligence might be like. I disagree with about as much as I agree with in this section, but it is stimulating and entertaining nonetheless.

You may notice that I have avoided talking about the first few chapters of the book so far. Unfortunately, I do not have many positive things to say about them.

The first couple of chapters cover the EPR experiment, Bell's theorem, and entanglement. Here, Dowling employs the all too common device of psychoanalyzing Einstein. As usual in such treatments, there is a thin caricature of Einstein's actual views followed by a lot of comments along the lines of “Einstein wouldn't have liked this” and “tough luck Einstein.” I personally hate this sort of narrative with a passion, particularly since Einstein's response to quantum theory was perfectly rational at the time he made it and who knows what he would have made of Bell's theorem? Worse than this, Dowling's treatment perpetuates the common myth that determinism is one of the assumptions of both the EPR argument and Bell's theorem. Of course, CHSH does not assume this, but even EPR and Bell's original argument only use it when it can be derived from the quantum predictions. Thus, there is not the option of "uncertainty" for evading the consequences of these theorems, as Dowling maintains throughout the book.

However, the worst feature of these chapters is the poor choice of analogy. Dowling insists on using a single analogy to cover everything, that of an analog clock or wristwatch. This analogy is quite good for explaining classical common cause correlations, e.g. Alice and Bob's watches will always be anti-correlated if they are located in timezones with a six hour time difference, and for explaining the use of modular arithmetic in Shor's algorithm. However, since Dowling has earlier placed such great emphasis on the interpretation of the watch readings in terms of actual time, it falls flat when describing entanglement in which we have to imagine that the hour hand randomly points to an hour that has nothing to do with time. I think this is confusing and that a more abstract analogy, e.g. colored balls in boxes, would have been better.

There are also a few places where Dowling makes flatly incorrect statements. For example, he says that the OR gate does mod 2 addition and he says that the state |00> + |01> + |10> + |11> is entangled. I also found Dowling's criterion for when something should be called an ENT gate (his terminology for the CNOT gate) confusing. He says that something is not an ENT gate unless it outputs an entangled state, but of course this depends on what the input state is. For example, he says that NMR quantum computers have no ENT gates, whereas I think they do have them, but they just cannot produce the pure input states needed to generate entanglement from them.

The most annoying thing about this book is that it is in dire need of a good editor. There are many typos and basic fact-checking errors. For example, John Bell is apparently Scottish and at one point a D-Wave Systems computer costs a mere $10,000. There is also far too much repetition. For example, the tale of how funding for classical optical computing dried up after Conway and Mead instigated VLSI design for silicon chips, but then the optical technology was reused to build the internet, is told in reasonable detail at least three different times. The first time it is an insightful comment, but by the third it is like listening to an older relative with a limited stock of stories. There are also whole sections that are so tangentially related to the main topic that they should have been omitted, such as the long anti-string-theory rant in chapter six.

Dowling has a cute and geeky sense of humor, which comes through well most of the time, but on occasion the humor gets in the way of clear exposition. For example, in a rather silly analogy between Shor's algorithm and a fruitcake, the following occurs:

“We dive into the molassified rum extract of the classical core of the Shor algorithm fruitcake and emerge (all sticky) with a theorem proved in the 1760s...”

Continued on next page

If this were a piece of student writing, Dowling would surely get kicked out of class for it. Finally, unless your name is David Foster Wallace, it is not a good idea to put things that are essential to following the plot in the footnotes. If you are not a quantum scientist then it is unlikely that you know who Charlie Bennett and Dave Wineland are or what NIST is, but then the quirky names chosen in the first few chapters will be utterly confusing. They are explained in the main text, but only much later. Otherwise, you have to hope that the reader is not the sort of person who ignores footnotes. Overall, having a sense of humor is a good thing, but there is such a thing as being too cute.

Despite these criticisms, I would still recommend Dowling's book to physicists and other academics with a professional interest in quantum technology. I think it is a valuable resource on the history of the subject. I would steer the genuine lay reader more in the direction of Gribbin's book, at least until a better option becomes available.

–Matt Leifer

QUANTUM INFORMATION
BY NICOLE YUNGER HALPERN

If "CHSH" rings a bell,
you know QI's fared, lately, well.
Such promise does this field portend!
In Neumark fashion, let's extend
this quantum-information spring:
dilation, growth, this taking wing.

We span the space of physics types
from spin to hypersurface hype,
from neutron-beam experiment
to Bohm and Einstein's discontent,
from records of a photon's path
to algebra and other math
that's more abstract and less applied—
of platforms' details, purified.

We function as a refuge, too,
if lattices can frustrate you.
If gravity has got your goat,
momentum cutoffs cut your throat:
Forget regimes renormalized;
our states are (mostly) unit-sized.
Velocities stay mostly fixed;
results, at worst, look somewhat mixed.

Though factions I do not condone,
the action that most stirs my bones
is more a spook than Popov ghosts;[1]
more at-a-distance, less quark-close.

This field's a tot—cacophonous—
like cosine, not monotonous.
Cacophony enlivens thought:
We've learned from noise what discord's not.

So take a chance on wave collapse;
enthuse about the CP maps;
in place of "part" and "piece," say "bit";
employ, as yardstick, Hilbert-Schmidt;
choose quantum as your nesting place,
of all the fields in physics space.

[1] With apologies to Ludvig Faddeev.

Nicole Yunger Halpern *is pursuing a physics PhD at Caltech, because Caltech had produced more quantum poetry than any other school she applied to. Nicole blogs for Caltech's Institute for Quantum Information and Matter at quantumfrontiers.com.*

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Editor

Ian T. Durham
Department of Physics
Saint Anselm College
Manchester, NH
idurham@anselm.edu

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Contributions from readers for any and all portions of the newsletter are welcome and encouraged. We are particularly keen to receive

- **op-ed pieces and letters** (the APS is *strongly* encouraging inclusion of such items in unit newsletters)
- **book reviews**
- **review articles**
- **articles describing individual research** that are aimed at a broad audience
- **humor** of a nature appropriate for this publication

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Editorial policy

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FQXi has announced their 2013 Large Grant awards for research on the physics of information. A number of the recipients are GQI members or are active in the quantum information community. Awardees include **Joseph Emerson, David Cory, and Dmitry Pushin** (Waterloo); **Patrick Hayden** (Stanford); **Olimpia Lombardi** (Austral/Theiss); **Ottfried Gühne** (Siegen), **Adán Cabello** (Seville), and **Jan-Åke Larsson** (Linköping); **Jonathan Barrett** (Oxford); **Dagomir Kaszlikowski** (CQT) and **Pawel Kurzynski** (CQT/Adam Mickiewicz); **Philip Goyal** (Albany); **Gerardo Adesso** (Nottingham); **Wojciech Zurek** (LANL); **Jacob Biamonte** (ISI); **Adrian Kent** (Cambridge); **Çaslav Brukner** (IQOQI); **Jonathan Oppenheim** (UCL); **Ian Durham** (Saint Anselm) and **Dean Rickles** (Sydney); **Sumati Surya** (RRI); **William Wootters** (Williams); **David Wolpert** (SFI); **Donald Spector** (Hobart & William Smith); **Jens Eisert** (FUB); **Noson Yanofsky** (Brooklyn); **Giulio Chiribella** (Tsinghua); **Alexander Wilce** (Susquehanna). A complete list of winners may be found here: http://fqxi.org/grants/large/awardees/list

THE QUANTUM TIMES SEEKS NEW EDITOR

After more than seven years at the helm of *The Quantum Times*, I have come to the conclusion that it's time for new blood, new ideas, and new energy. As such, it is my intention to step down as Editor prior to the APS March Meeting in Denver in 2014. If you are interested in being Editor or in nominating someone to be Editor, please e-mail me at idurham@anselm.edu. As evidence that this is not a stressful position, I have included before and after pictures of myself (gray hair is not from this job).

Before

After