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# THE QUANTUM TIMES

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NEWSLETTER OF THE APS TOPICAL GROUP ON QUANTUM INFORMATION,  
CONCEPTS, AND COMPUTATION

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## Summertime Greetings

As shocking as it may sound to those of you who work at universities and colleges: *the summer is almost over*. If you're on a semester system that means the fall term starts anywhere from the end of August to the beginning of October. As a theorist with no research students this summer I pondered the quantum world from the confines of my library, workshop, kayak, or the beach. Though I have enjoyed being at home in Kennebunk every day, I am looking forward to the start of classes – and the peace and quiet of my 160-mile round-trip commute (two kids + two dogs = decibel levels rivaling a rock concert).

On the opposite end of New England, in the mountains of western Massachusetts, the subject of our inaugural *Times* interview, Bill Wootters, did have a few research students working on discrete phase space descriptions of systems of qubits. Also in this issue, rather than a report from the chair we have a report from the chair-elect, Carl Caves. Secretary-Treasurer Barry Sanders provided a report on the DAMOP meeting in Tennessee that includes information on next year's meeting to be held in Barry's neck-of-the-woods, Calgary.

Straddling the line between serious and light is the reprinting of the inter-blog debate between Scott Aronson and Dave Bacon concerning the difference between computer scientists and physicists. Dave writes on behalf of physicists – even though he is employed in a computer science department! Closing out the issue, *The Lighter Side* takes a look at a perplexing convergence of b's in my "electronic" life in recent weeks. Not sure what that means? Turn (or click) to the last page and find out...

Finally, on a practical note, please be aware that there are one or two operating systems that seemed to have trouble viewing the photographs in the last issue. I am hoping that problem has been resolved for this issue, but if for some reason you have trouble viewing the pictures or if the formatting is a problem, please let me know via e-mail ([idurham@anselm.edu](mailto:idurham@anselm.edu)). I am endeavoring to make this as platform-independent as possible while still creating it on my Mac.

Hope you're enjoying the summer!

-Ian T. Durham  
Saint Anselm College

P.S. This month also includes the debut of page numbers!!!

## Chair-elect's Report

The TGQI Program Committee, consisting of myself, Chris Fuchs, Steve Girvin, and Paul Kwiat, is planning TGQI's contribution to next year's March Meeting, which will be held in Denver, March 5-9, 2007. TGQI will be sponsoring or co-sponsoring five Focus Sessions at the March Meeting: Foundations of Quantum Theory, Quantum-Limited Measurements, Superconducting Qubits, Quantum Information at the AMO/Condensed-Matter Interface, and Materials Issues for Quantum Computing and Quantum Engineering. Members are encouraged to submit abstracts for contributed papers to be presented in these sessions.

Members should be aware that sometime in mid-August, they can go to the March Meeting website to nominate invited sessions and to submit abstracts for contributed presentations. Nominations for invited sessions can also be considered for inclusion in the program of next year's DAMOP meeting, to be held in Calgary, June 6-9, 2007, instead of at the March Meeting. Once the website is open, an e-mail will be sent to all members informing them of the details.

-Carlton Caves  
University of New Mexico

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## DAMOP Meeting Roundup

Quantum information was strongly featured at the highly successful annual meeting of the Division of Atomic, Molecular, and Optical Physics (DAMOP) held this year in Knoxville, Tennessee. The conference had over 950 attendees, and both the invited and contributed quantum information sessions were well attended. Contributed sessions had peak attendances of between 120 and 150 persons, and the invited session had up to 250 attendees. I attended the vast majority of talks, and I found that the talks were by and large excellent in both presentation and oral delivery. Poster sessions are always popular, and there were long queues to speak to poster presenters.

Whereas quantum information sessions at the March Meeting focused on foundations, linear optical quantum computing, condensed matter, and many-body physics, and theoretical talks were more prevalent, DAMOP featured a very large proportion of experimental talks. My impression was that the overlap between attendees at the March Meeting vs the DAMOP Meeting was small, as the two conferences seem to cater to different aspects of quantum information research.

One of the most popular topics concerned atomic and molecular qubits, and how to perform two-qubit controlled-unitary gates on these systems. The choice of atoms or molecules varied depending on the desired operational parameters and experimental convenience. Favored neutral atoms/molecules included Yb, Sr, Cs, Rb, Cd, and ultracold polar molecules. Several presenters discussed collisions for controlled-unitary gate operation, with Ramsey interferometry an important tool for measuring decoherence and for creating gates.

Ion trapping featured strongly in both invited and contributed sessions. David Wineland presented a clear exposition of trapology and directions in sympathetic cooling. In the next talk, Richard Slusher proposed some exciting ideas about using silicon very-large scale integration methods for quantum computation with thousands of ions per square centimeter. He also discussed using micro-electromechanical spatial light modulators for atom optical traps to achieve arbitrarily re-configurable, optical traps. Richard was characteristically cautious in his presentation and referred to the prospects for his proposal by saying, "Nothing is impossible, but it is too damn hard." In fact I found that, by and large, presenters concentrated on realistic medium-term goals such as obtaining low-error single-qubit gates, which

will feed into the long-term goal of reaching error correction thresholds.

There were several talks on implementations. We saw recent results from the University of Michigan that showed Grover's algorithm with  $^{111}\text{Cd}$ , with success defined by exceeding a classical bound of 50% success probability. Quantum cryptography was unfortunately almost completely absent from this meeting, but we did hear about the NIST approach to exploit holes in the  $\text{H}\alpha$  Fraunhofer band to transmit through the atmosphere. Quantum simulators are becoming increasingly popular, and a group at Los Alamos National Laboratory reported their efforts to build a quantum simulator for the Hubbard Model using trapped  $^{88}\text{Sr}^+$ . Other implementations considered atom chips, optical beams with vortices, and Bose-Einstein condensates.

-Barry Sanders  
*Institute for Quantum Information Science  
University of Calgary*

### Further Information on DAMOP

For a speaker & topic list see:

*Bulletin of the American Physical Society*, **51**, 3, May 2006.

<http://meetings.aps.org/Meeting/DAMOP06/>

For information on the 2007 meeting:

<http://phas.ucalgary.ca/DAMOP07/>

Note that next year's DAMOP meeting will be joined with the annual meeting of the Canadian Association of Physicists' Division of Atomic and Molecular Physics (DAMP) and held in beautiful Calgary, Alberta.

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### QIP Workshop 2007

This is the first announcement for the tenth QIP (Quantum Information Processing) Workshop, to be held in Brisbane, Australia, from January 30 through February 3, 2007. The deadline for abstract submission for contributed talks (long and short) and for posters is November 4, 2006. The deadline for early bird registration is November 24, 2006.

For further information:

<http://qipworkshop.org/>

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**The *Times* Interview:  
Bill Wootters, Williams College**

I recently had the pleasure of discussing a variety of quantum-related issues with Bill Wootters. Bill is the Barclay Jermain Professor of Natural Philosophy at Williams College in Williamstown, Massachusetts. Bill's research, carried out in Williams' Department of Physics, involves developing simple and elegant "laws of entanglement" as well as investigating the use of Wigner functions to describe a quantum computer.

**What are you working on right now?**

My students and I are working on various problems related to a discrete phase space description of systems of qubits. In the scheme we're using, the axis variables of the phase space – that is, the discrete analogs of position and momentum – take values in a finite field (a Galois field), instead of the field of real numbers. What attracts me to this area of research is the intriguing way in which the strange arithmetic of finite fields seems to mesh so well with the complex-vector-space structure of quantum mechanics. (We couldn't set up this sort of phase-space description if the quantum state space were real, for example.) I'm hoping that discrete phase space will give us a new and interesting perspective on both quantum mechanics and ordinary phase space.

The specific questions that my students are working on this summer focus mostly on states with non-negative Wigner functions, in the spirit of recent papers by Cormick et al and Gross. So far we've written one longish paper on the subject:

Kathleen S. Gibbons, Matthew J. Hoffman, and William K. Wootters, "Discrete phase space based on finite fields," *Phys. Rev. A* **70**, 062101 (2004). Kate and Matt did this work for their senior theses.

**You also work on developing simple and elegant "laws of entanglement." Have you ever used Wigner functions in one of your descriptions of entanglement?**

One would think I would do that, but I haven't. This is because the particular phase-space constructions that I've been studying lately, which I find interesting because of their high degree of

symmetry and their close analogy with ordinary phase space, do not fully respect the tensor product; for example, one doesn't necessarily take the product of single-qubit Wigner functions to get the Wigner function of a product state. So entanglement doesn't have a simple signature. But I know of at least one paper (by Franco and Penna), based on a different construction, that does find connections between entanglement and the Wigner function.

**In exploring simple and elegant "laws of entanglement" have you and your students and collaborators uncovered anything that has proved insightful regarding the quantum/classical dichotomy?**

I think the deepest insights into the quantum/classical dichotomy have come from direct studies of decoherence rather than from laws of entanglement per se (e.g., laws restricting the sharing of entanglement). Of course these two subjects are closely related.

Now that you ask, it occurs to me that it might be interesting to try to set up a toy theory that is not quantum mechanics but that imposes in a natural way a restriction on entanglement sharing (suitably defined), and ask whether a "classical world" would arise naturally in the toy theory.

**Since we've moved into the abstract a bit here, I wonder if you could share some of your thoughts on Hilbert spaces. Specifically, how do you personally reconcile the abstractness of Hilbert space with physical reality?**

One interpretation of [your] question is this: Do I think that the ultimate physical reality resides in Hilbert space? The answer to that question is no. I think measurement results are more real than Hilbert space. While it is certainly true that the relations among measurement results are very simply described in terms of Hilbert space and state vectors, this doesn't lead me to conclude that the ultimate reality is a state vector. Rather, I think of Hilbert space, and the quantum formalism, as summarizing in an elegant way the result of some more fundamental calculation for which we don't yet know how to ask the question; maybe it's a combinatorial problem whose solution comes out to be easily expressed in terms of complex Hilbert space (and maybe only in a certain limit). I'm not arguing here for hidden variables in the usual sense. I have no desire to return to the deterministic paradigm of classical physics. In fact I expect that physics will only get stranger and further from



**Bill Wootters**

classical physics as our understanding advances. But I think of Hilbert space as something that needs to be explained rather than as the locus of ultimate reality.

**I would guess, then, that you are of the opinion that, as with most aspects of physics (at least within a certain philosophical school), it is simply a useful mathematical model. I think the trouble is that, unlike other mathematical models in physics, Hilbert spaces are so abstract – they don't even really model reality; they simply produce verifiable results.**

Many would argue that a state vector of the universe (with no collapse) is indeed a description of reality; it's just that the reality it describes is not wholly accessible to us. This is by no means an unreasonable position, and I think it's becoming more widely accepted.

My own view, though, is more in accord with your last sentence: the Hilbert space formalism of quantum mechanics produces verifiable results but is not to be taken as a model of reality in the way that, say, classical electrodynamics can be taken as a model of reality. (For the record, note that I have not been consistent on this issue over the years. Sometimes I have accepted and used the notion of a universal state vector. But that has not been my usual inclination.)

**You mentioned that Hilbert space was something that needed to be “explained” in the context of quantum physics. Do you have any thoughts on where this “explanation” might eventually come from or what form we might expect it to take?**

There exist axiomatic approaches, such as the elegant one worked out by Lucien Hardy. In Lucien's approach, one arrives at Hilbert space by insisting on certain axioms that are reasonable in that they require the complexity of the description to match the complexity of the observations. I think this counts as real progress, but ultimately I would like to have something more constructive.

**Constructive in what way?**

I've always been intrigued by Leibniz's principle of the identity of indiscernibles; an extension of this principle might be called “the proximity of barely discernibles.” The idea here is that one can begin to build a geometry once one has a notion of distinguishability. Back in grad school, I tried to build Hilbert space geometry in this way, starting with the assumption that repeated measurements

can give probabilistic results (the probabilities then provide a notion of distinguishability), but what came out was closer to the geometry of a real vector space, not a complex vector space. More recently I've thought further about the notion of a repeated measurement.

In practice we are able to group individual instances of measurements into classes, such that all the instances within a given class are called “different repetitions of the same measurement.” Can we formalize this notion of “reidentifying measurements” in a way that would be helpful? That is, I don't want to take for granted that we automatically know what it means to repeat a measurement. Maybe the process of “grouping into classes” is itself part of the explanation of the Hilbert space geometry.

That's quite vague, isn't it! I guess “I don't know” would have been almost as informative an answer. But maybe this gives you some sense of the kind of explanation that I have vaguely in mind. Of course the probability of barking up the wrong tree on this sort of question is quite high!

**How do your students deal with Hilbert space? Due to its abstract nature it seems as if it would provoke some conceptual quandaries in the minds of undergraduates.**

Of course one has to get used to the fact that the entity for which quantum mechanics gives a simple law of evolution is not directly observable. But students seem to get used to this notion fairly quickly. It helps, I think, to emphasize that this is really different from classical physics.

**Since we're on the topic of students, what aspects of teaching at a liberal arts college do you enjoy most?**

Here are some of the things I like about teaching at a liberal arts college.

- (i) I enjoy teaching the undergraduate physics curriculum. I never find it boring, because even after all these years, there are always new questions that I have to think about, including some great questions that students ask.
- (ii) The departments at the college are small so one tends to interact frequently with people in other departments.
- (iii) There are occasional opportunities to teach interdisciplinary, team-taught courses. One learns a lot from one's co-teacher in such a setting.

**Has the liberal arts environment had any effect on your research?**

I have no doubt that being at an undergraduate college has affected my research. For one thing, I'm always looking for problems that an undergraduate can work on. So the problems are never very far removed from elementary quantum mechanics. Quantum information theory has been a great area of research in this respect. Also, I often like to work on problems that are a little out of the mainstream. Progress in research at a liberal arts college just isn't going to be as fast as at a research university; one doesn't want to be doing slowly what someone else is doing faster.

**At a faculty workshop I attended a year ago there was some consensus that the liberal arts environment might be especially conducive to theoretical work. Have you found that to be true?**

I think both theory and experiment can thrive at a liberal arts college, but in both cases one has to choose the problems carefully.

A few years ago the Kavli Institute at Santa Barbara hosted a small conference on the challenge of doing theoretical physics at an undergraduate institution. One metaphor that emerged was that one could look for good problems "near the trunk" of the discipline as opposed to looking at the ends of the longest branches.

**What is the earliest point at which the basic concepts of entanglement (or related topics) are introduced at Williams?**

We have a 100-level course that's mostly quantum mechanics, and we often introduce the notion of entanglement there. I co-teach, with a mathematician, a 300-level interdisciplinary course called "Protecting Information," which includes some quantum cryptography and quantum computation. The quantum mechanics in that course is entirely self-contained, because many of the students are math or computer science majors.

**Have you ever incorporated (or considered incorporating) some sort of entanglement experiment into the laboratory portion of a course, akin maybe to what they've done at Colgate University?**

Last summer some students started working towards developing new experiments based on down-conversion, and I believe we'll be able to

include such an experiment in the quantum mechanics lab this year. But in doing this, we're following [the] work [developed] at Colgate.

**Let's switch gears here and talk a bit about the discipline that has grown out of John Bell's work. This question may sound a bit "sensationalist" but I think it raises some intriguing historical questions: if John Bell were still alive today do you think that, at the very least, he might have made the "short list" for the Nobel Prize at some point?**

His famous discovery certainly has an interesting status, doesn't it. It's a theoretical discovery whose main effect is not to explain or predict some phenomenon, but rather to show that a certain philosophical position on quantum mechanics, a position based on a long tradition of thought about the physical world, is untenable. I think it is indeed a very important discovery, on a par with other great discoveries of twentieth century physics, though of a different nature.

Your question about the Nobel prize is difficult, because on the one hand, I can't think of any discovery like Bell's for which a Nobel prize has been awarded, but on the other hand, I can't think of any other discovery quite like Bell's, period!

**My main point about Bell and his work was that it spawned an entirely new discipline. In any case, you say Bell's discovery is unique in a certain way, but couldn't you include it in the same category as the uncertainty principle and exclusion principle?**

I see your point about the uncertainty principle and the exclusion principle. But I do think there's a difference. Those two principles are really part of quantum mechanics, whereas Bell's theorem is not a theorem in quantum mechanics. Rather, it tells us what quantum mechanics is not. I should add that I had never given any thought to the issue of a Nobel prize until you asked so my answers are not the product of years of reflection.

**Finally, let me ask you about the state of quantum information research today: what research these days (in both theory and experiment), aside from your own, most excites you?**

On the theoretical side, I get most excited about research that applies ideas from quantum information theory to other areas of physics. One

example is the study of entanglement in many-body systems, a subject on which many papers have been written in recent years. Another nice example is John Smolin's and Jonathan Oppenheim's use of the notion of information-locking to make an important observation about the problem of unitarity in black hole evaporation. On the experimental side, I find a lot of the experiments impressive, but I pay particular attention to demonstrations of quantum key distribution and related advances (e.g., reduction in the rate of dark counts).

**Thanks again for taking the time to share your thoughts. I hope you enjoy the rest of your summer – only about 6 weeks or so left!**

I wish you hadn't said that!

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## THE LIGHTER SIDE

### A Convergence of B's

Let me begin with a qualifier: I came to quantum information, specifically quantum foundations, from cosmology (and two previous non-physics careers). Perhaps more telling is that my dissertation was on Arthur Eddington's *Fundamental Theory* which has been derisively (and a bit unfairly) labeled numerology by many a physicist, astronomer, mathematician, philosopher, etc. (but who's counting?). So I might be excused for sounding a bit nuts once in awhile. And though I am a solid believer in mathematics and science, one must admit that the quantum world can be a bit strange.

And so it was that upon perusing Dave Bacon's blog recently that I came across (thanks to some nifty new software Dave is using) the cover of one of my favorite books: Jorge Luis Borges' *Labyrinths*. Personally I was introduced to this interesting collection of writing in a college Philosophy of Literature class (perhaps my philosophy minor explains the nuttiness). Dave informed me that our (TGQI) fearless leader Charlie Bennett is also a Borges fan (and we already know from the first issue of *The Quantum Times* that Charlie and I have the same taste in printed clothing). No comment yet from Charlie on Borges.

Not long after this subtle collection of B's began congregating in my Inbox and browser, Syd Barrett, erstwhile founder of the band Pink Floyd, died rather prematurely causing me to reminisce to

such classics as *Bike* and *Baby Lemonade*. Of course my apparent doppelganger, the aforementioned Dr. Bacon, is also a Barrett/Floyd fan. The question, of course, is Bennett? Hmm. Bennett. Barrett. I wonder if the substitution  $r \rightarrow n$  is gauge invariant? Maybe that's what really happened to Syd all those years ago...

Of course it was also the youthful (baby-faced?) Bacon who first brought it to my attention (via his blog) that next year's QIP (Quantum Information Processing) Workshop is in Brisbane, Australia. At the time I read *this* I was, of course, working on a calculation involving the B-field inside a Stern-Gerlach device. Later the same day I received a note from one of the little voices in my head, better known to most of you as Barry Sanders (come to think of it, maybe it's not the numerology or the philosophy at all – it's those bloody voices!).

And what meaning (if any) can I take away from this sudden convergence of b's? Well, it has been argued by more than a few people that, at least in quantum mechanics, the vector potential **A** is more fundamental than **B**. That would force me to conclude that Aronson is more fundamental than Bacon in this issue's big debate. However, given that I cannot find similar A's for Barrett, Bennett, Borges, blog, or Brisbane, I suppose it all just might be a simple case of listening to one of my son's favorite songs a little too often (that would be *Who Put the Alphabet in Alphabetical Order?* by They Might Be Giants) or perhaps reading one too many alliterative children's books.

Well, in theory – quantum theory anyway – nearly anything is possible even if it isn't very probable. Maybe the next time around it will be C's. I already have an e-mail or two from Carl Caves, DAMOP is in Calgary next year, and I am still a closet cosmologist. But let's not get ahead of ourselves.

Blissfully yours,  
Ian

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### Reminder

Keep in mind that there will be several items coming up for either a vote or discussion soon regarding the Topical Group including the by-laws and the name. You can always keep abreast of the latest at our website:

<http://www.aps.org/units/gqi/>



## Aronson & Bacon sound off on Computer Scientists & Physicists



**In this tête-à-tête Aronson threw down the gauntlet after “listening to a talk ‘showing’ that a fault-tolerant quantum computer would need at least 100 physical qubits for every logical qubit.” Though his first verbal parry was ostensibly aimed at physicists in general, he specifically issued a challenge to Dave Bacon, referring to His Holiness as a “closet” computer scientist.**

One day a group of physicists ran excitedly into the computer science building. "Guess what?" they cried. "You know how you're always trying to prove lower bounds, but you almost never succeed? Well, today we proved a lower bound!"

"What did you prove?" asked the computer scientists.

"We proved that to pull a wagon through a forest, you need at least five oxen. It's physically impossible to do it with four oxen or less, regardless of what other resources you have."

"How did you prove that?"

"Well, we looked up the strength of a typical ox, the weight of a typical wagon, the size of every forest in a 30-mile radius..."

"Yeah, but what if you had an ox the size of a Brontosaurus? Or what if the forest was only two feet across? Or what if the wagon weighed less than a fingernail?"

The physicists snickered. "These are clearly unphysical assumptions. As long as you stay within a realistic region of parameter space, our impossibility proof is airtight."

"Ah, but how do you know there couldn't be some completely different method of pulling wagons -- maybe even a method that's not ox-based at all?"

"Look, we physicists are interested in the real world, not complexity-theory la-la land. And at least in the real world, when people want to pull wagons, oxen are what they use."

The physicists weren't heard from again until almost a decade later, when they once again barged into the CS building. "Guess what?" they cried. "We just discovered a loophole in the famous Five-Ox Theorem -- the one we published years ago in Nature!"

"What's the loophole?"

"Elephants! If you had an elephant pulling the wagon, you wouldn't need any oxen at all. With

hindsight it's almost obvious, but what a paradigm shift it took!"

The computer scientists stared blankly.

"You see," said the physicists. "This is why we never trust so-called impossibility proofs."

**And the Pontiff responded with his own fable (parable?). In his own words: “Of course I would have liked to respond with a fable directly related to Scott’s main point, but that is damn hard, so instead I present to you a fable which may, or may not bear some relationship with Scott’s post and whose moral is mostly to harp on computer scientists not trusting physicists. If confronted I will immediately deny that I wrote the fable below or that the fable represents anything coming close to my true feelings on the subject.” Since we’ve reprinted it, I guess he’s stuck with the latter.**

Once upon a time and a very good time it was there was a Physicist coming down along the road and this Physicist that was coming down along the road met a nicens little boy named Computer Science. [[Pontiff's] note: bonus points if you recognize this mangled famous opening line; Editor's note: 'nicens' is not misspelled – take that as a clue.]

Physicist, being ever interested in learning new things, began to have a conversation with the nicens little boy named Computer Science. Computer Scientist, it turns out, he had all of these really fun toys which were all labeled by combinations of letters and numbers (like P and NP and BPP and AC\_0.) Physicist was quite confused by all of these letters. What did they mean? Why were there so many? It almost sounded like his friend Old-School Biology to him [ed note: if you're going to hammer CS, why not hammer other fields equally?]

Computer Science patiently explained to Physicist what all of these strange letter combinations were and the accompanying beasts which they described, but really the physicist only listened to a bit of what he said. He really liked the complexity classes P and BPP, and understood the deep dark hatred everyone should have for complexity classes like NP-complete or NEXP. He was a bit confused by PSPACE and wondered, in a bout of extra-illusionary intelligence, whether he should tell Computer Science about Special Relativity. But he really didn't understand

Computer Scientist's obsession with the endless array of complexity classes.

After blabbering on for quite a while, Computer Scientist noticed that Physicist was nodding off. "Why aren't you paying attention, Mr. Physicist?"

"Well, you keep talking about this endless array of complexity classes, and while it all sounds quite fascinating, I'm wondering if you could get to the point?"

Computer Scientist then launched into a diatribe about "proving" all sorts of different things (which the computer scientist insisted on referring to as "theorems." This made these things seem big and important, and secure, like how he felt when he was safe in his fenced suburban home.) Physicist couldn't take much of this diatribe, so he interrupted, "But if you really feel strongly that these complexity classes are different, but you can't prove it, why don't you just accept it and move on? I mean you've got ample experience telling you that P does not equal NP, right?"

At this point Computer Scientist's head bulged, his veins began to stand out from his neck, and he emitted a long loud shriek which sounded a lot like fingernails being dragged across a chalkboard...and the nails breaking. Computer Scientist, however, had long mastered the art of immediate Zen meditation, and so began chanting to himself and calmed himself down by dumping the core of his memory and then rebooting.

"But, Physicist, if you can't prove something, then how do you know it is true? Won't you spend all of your life worrying about whether it is true or not?"

"Let me tell you a story" said Physicist, and therein he launched into a little sub-fable of his own:

*Once upon a time, there was a clan known as the statistical physicists. These statistical physicists studied all kinds of interesting physical systems, their distinction being that they were very good with large numbers of interacting systems. They were particularly good at describing systems which changed their appearance (which physicists like to call "their phase".) In other words they were particularly good at describing phase transitions.*

*Now some of members of the clan of computer scientists were smart enough to learn a little about the theory of phase transitions. Some of these members of the computer science clan, like almost every rational being, were particularly enamored with computational problems which were NP-complete. When they investigated these problems, together with members of the statistical physics*

*clan, they discovered that instances of these NP-complete problems came in different phases and that, just like the phases they studied in physics, they could study the phase transitions between these different instances of the problem. Now this was fun! And so, because this worked for a few instances of the different NP-complete problems, these crazy scientists conjectured that NP-complete problems were distinguished from other computational problems by the existence of different phases and a phase transition. Indeed they were even bolder and claimed that the hard problems, the ones for which no known polynomial time algorithm would succeed, were those problems near this phase transition.*

*Of course this was in some ways both profound and silly. Silly because the scientist could not prove that this was true. Profound because the scientists had discovered that a property of physical systems could be mapped to a computational problem in an interesting, and possibly fruitful manner.*

*But scientists are a skeptical group. So no conjecture will last long without being challenged. So one scientist did. He examined the integer partitioning problem. The integer partitioning problem is, given a set of  $k$  integers between 1 and  $N$  decide whether there is a subset of these  $k$  integers whose sum is equal to the sum of the elements not in this subset. This problem is a beast of the NP-complete kind. So according to what others were boldly conjecturing, this problem should have had a phase transition. But, this scientist claimed, in an article entitled "The Use and Abuse of Statistical Mechanics in Computational Complexity," that this problem did not exhibit a phase transition. It was thus claimed to be a counter example to the bold conjecture!*

*But not everyone was sold on this counterargument. Indeed, numerical results immediately began to dispute this counterargument. And then a member of the statistical physics crowd, Stephan Mertens, approached the problem like a physicist. He showed how to approach the number partitioning problem like a problem in statistical mechanics. He then showed that there was a phase transition in this problem and explained how the effects of working with finite numbers of numbers effected the properties of this transition. Now, Mertens approached this problem with all the tools and lore of statistical physics. These tools involved many things that physicists were comfortable with, including, methods which physicists love known as approximations.*



*So it might seem that the story would end here. Mertens had shown that there was a phase transition in the problem, identified where and how this phase transition occurred, and triumphantly explained scaling effects near where the phase transition occurred. But, no! Why? Well because while Merten had “shown” these results, he had done so using a approximations which physicists were comfortable with, but which were not “proven!”*

*So physicists reading about this would probably have been happy. But not so, for those who work in the clan of computer science! There the approximation was considered an abomination, something so disgusting and foul that it should be banished to the far ends of the earth (no one apparently having told the computer science clan that the world was not flat )*

*Thus a brave group of computer scientists/mathematicians/mathematical physicists decided to see if what these crazy physicists were saying with their abominable approximation was true. And by true, they meant provably true. So Borgs, Chayes and Pittel (the brave group), in a beautiful forty page paper, investigated and proved results about the phase transition in the integer partitioning problem. And what did they discover? They discovered, to their surprise, that Merten’s results, even using his abominable approximation, were correct! While he had made an abominable approximation, Merten’s result agreed with the exact result (in appropriate infinite instance size limits.) Those damn physicists had invoked an unproven and abominable approximation and still gotten the right answer! (As a side note the work of BCP also pens down the finite size effects in a rigorous manner.)*

“Wake up Computer Scientist!”

“Oh, I’ve been awake. I just like to rest my eyes when I’m listening to stories. It helps me visualize the characters involved. For the physicists I was imagining a moocow.” [[Pontiff’s] note: this joke only makes sense if you know where the first line of this post comes from. This is an example of a obsoko: a joke so obscure that perhaps only the author of the joke understands why it is funny.]

“So what do you think about my story?”

“I think that sometimes, even people like you Physicist, can do produce profound results even though you can’t prove why they work. It also seems that you don’t care so much if something is proven as much as if it agrees with your experience. I could never live that way, of course. It seems, so, so...uncertain!”

“Which reminds me, Computer Scientist, have you ever heard of this thing called quantum mechanics?”

“No, what is that? Some kind of car repair shop?”

“Heh. Let’s go to the pub and get some beer and then, boy do I have a story for you....”

**And thus the Pontiff has pontificated.**

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## **Position Announcement**

The Department of Physics & Astronomy at the University of British Columbia seeks applications for one or more tenure track faculty positions in Quantum Information. These positions are primarily intended to be at the Assistant Professor level, but applications from senior candidates will also be considered. Applicants must have a PhD. Degree or equivalent, relevant postdoctoral experience, an outstanding research record and a strong interest in teaching at the undergraduate and graduate level.

Candidates are sought who have interests in solving fundamental physics-related research problems in any area of theoretical quantum information, including quantum computation and quantum communication. The department has related activities in theoretical and experimental studies of decoherence and quantum noise, quantum optics and cold atoms, solid-state based nanostructures, and quantum materials.

The University of British Columbia hires on the basis of merit and is committed to employment equity. We encourage all qualified persons to apply – however, Canadian citizens and permanent residents will be given priority.

Applicants should complete the online application form at [http://www.physics.ubc.ca/cgi-bin/Job\\_Appl\\_Info.cgi](http://www.physics.ubc.ca/cgi-bin/Job_Appl_Info.cgi), making sure to select the Quantum Information competition. A CV, publications list, and statements of research and teaching interests are required and can be uploaded directly. Three letters of reference may be submitted electronically to [jobs@physics.ubc.ca](mailto:jobs@physics.ubc.ca), or sent by mail to (deadline September 15, 2006):

Chair, Quantum Information Search Committee  
Department of Physics and Astronomy  
University of British Columbia  
6224 Agricultural Road  
Vancouver, B.C. V6T 1Z1  
Canada

### Wherefore art thou Ettore?

August 5 would have been (perhaps it actually is) the 100<sup>th</sup> birthday of Ettore Majorana, a person Fermi likened to Galileo and Newton. Among many contributions to the foundations of quantum mechanics and atomic physics, he is credited with the discovery of Majorana spinors (independently discovered by Eddington). His work on Majorana spinors was said to have been originally written in 1932/33, only to be tossed into a drawer where it sat unattended until 1937.

That was roughly a year before he mysteriously disappeared on a boat ride between Naples and Palermo. Two letters that were left behind hinted at suicide but at least two other possibilities have been suggested over the years: kidnapping or an intentional disappearance. Both these suggestions arose due to his involvement with the Italian atomic weapons project. Erasmo Recami has discussed all the possibilities in a book and several journal articles. In addition to those that are listed in Majorana's Wikipedia entry ([http://en.wikipedia.org/wiki/Ettore\\_Majorana](http://en.wikipedia.org/wiki/Ettore_Majorana)), Recami also published the following: Recami, E. 1999. *Quad. Storia Fisica* 5: 19. Recami makes a fairly convincing argument that Majorana ended up in Argentina and reports of Majorana sightings in South America emerged in the 1950s.

A collection of nine of Majorana's papers (including English translations) is due out this year from the Italian Physical Society and the Electronic Journal of Theoretical Physics (EJTP – <http://www.ejtp.com/>) has published a special issue containing 20 articles discussing the impact of Majorana's work on physics today.

So, Ettore, if you happen to still be alive (not to mention reasonably coherent – 100 isn't exactly young, you know) and you're actually reading this: happy birthday!

-ITD

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### Newsletter information

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