Interlude: Gallivanting

Where to go? What to do now? Gina asked herself. She started gallivanting in the science blogosphere, hoping to find some nice blog community to join.

16 The Bell Curve

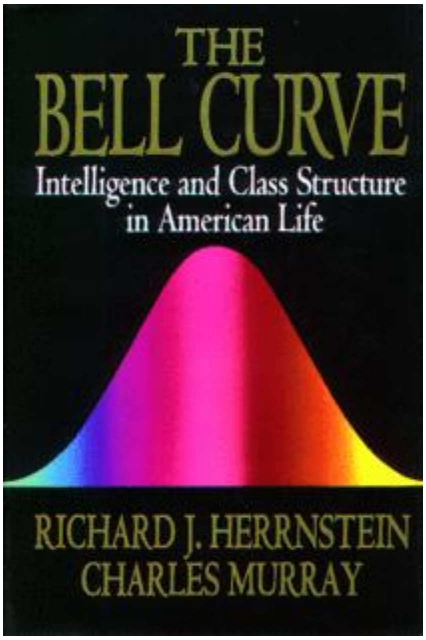
One opportunity for participation was when Gina discovered an old post from July 24th 2006, in:

The Scientific Curmudgeon: John Horgan's blog, titled: **The Bell Curve Revisited**.

The Bell curve was a controversial book about IQ written by Richard J. Herrnstein and Charles Murray in the 1980s. Gina did not like the book for several reasons, one of which was the dishonest description of the "Cyril Burt" case. Cyril Burt, a famous (more precisely, infamous) British psychologist, dominated the educational agenda in Britain for several decades, and his social and educational views were largely based on the British class system. He recklessly supported his views with his research findings, and his research - while in part original and even reasonable - was based on a systematic method of fraud.

Gina says:
October 26th, 2006 at 11:39 am

The “Bell curve” was a dubious book from various aspects. One issue that should have casted doubt on the intellectual integrity of the authors is the way the book treats the “findings” of Cyril Burt.



The bell curve, the graph of the normal probability distribution, as shown on the cover of Herrnstein and Murray's book.

"Too good to be true"

Cyril Burt's famous experiments dealt with the IQ of identical twins. Burt computed the correlation between IQ scores of twins who were separated at birth and found a remarkable correlation.

The first piece of evidence to raise suspicions about Burt’s studies, discovered by Leon Kamin, was the fact that the results of the second experiment Burt conducted completely matched those of the first experiment. This represented an unlikely event, unless the data was modified or made up.

Perfect replications of experimental results, or perfect matches between empirical results and theoretical predictions, are often bad signs. A famous case where experimental results turned out to be too good, thus giving a strong indication that the data had been altered, is the experiments of Mendel in genetics.

Psychologist D. D. Dorfman made a systematic statistical study of Burt's works and found more evidence of fraud. One piece of evidence relates to the "bell curve" itself. Burt expected the IQ distributions to match the bell curve, the graph of the normal distribution. Yet, according to Dorfman, in terms of their correlation with the bell curve, Burt's results are also "too good to be true."

17 The role of rigor

The Blog Gina tried next was:

Musing: Thoughts on Science, Computing and Life on Earth: The weblog of Jacques Distler.

The October 21, 2006 post was titled "The Role of Rigor." The original comment was an interesting discussion of mathematical rigor and physics. Gina felt she had something to say on the matter.

Dear Jacques,

Four quick remarks:

1. In any applied application of mathematics, modeling and interpretation are as important as rigorous theorems. Misinterpretation and over-interpretation of mathematical theorems (and even of mathematical notions) are common mistakes made in application of mathematics and in mathematical formalism. (Correct interpretation is also important in pure mathematics.)

2. There are areas of applications of mathematics where analytic and rigorous proofs are well beyond reach, and heuristic computational methods, numerics and simulations are crucial instead, or in addition to, rigorous theorems.

3. But there is also the flip side. A lot of modern physics, including extremely successful theories, are not supported by rigorous mathematics. Along with the possibility that mathematics is not sufficiently developed, there is also the possibility that the non-rigorous methods hide some extra physics assumptions (and even the more radical possibility that the modeling itself should be corrected). Therefore, truly successful rigorous mathematics for these physics theories is something which may affect the nature of more speculative theories. The input coming from mathematicians studying this notorious question (in the slow and peaceful pace of mathematics) may be as exciting to physics as empirical evidence from the Large Hadron Collider.

4. A strange feature of the non rigorous mathematical nature of modern physics is that mathematical analysis plays a smaller role compared to algebra and geometry.

Posted by: Gina on October 21, 2006 6:40 PM

Physics: computations and words.

The issue of the mathematical rigor of physics computations, notions, and results is of importance. Lee Smolin raised another important point, which is almost in the opposite direction. Smolin argued that the heavy "computation-based" methodology of particle physics, which was responsible for the great successes of the "standard model," is no longer suitable for further progress. Smolin sees more room for philosophical arguments and verbal physics ideas. (Smolin considers his approach to be in the tradition of the great physicists of the first half of the twentieth century.) Other physicists strongly disagree. Clifford Johnson regards "verbal physics arguments," which are not supported by computation, as "popular science." Joe Polchinski regards the transition from verbal description to mathematical formalism as the step in which most of physics ideas fails.

All in all Gina was not satisfied with gallivanting and jumping to and fro between different blogs. She wanted a blog community to belong to. The n-Category café seemed like the worst possible choice. Gina did not even know what n-categories were, and she could not follow most postings. Yet the people there were nice and Gina decided to go for it.

Part II: The n-category café

Gina looked for a new blog community to join. It need not be about strings or physics, she thought.

Back on Woit's blog somebody called ksh95, wrote: "Gina, I have a Ph.D. .., yet I would never be arrogant enough to go to n-category cafe, start posting uninformed nonsense, jump in the middle of discussions I can't fully understand,… **Instead**, I would read every day, try to learn as much as I could, and feel lucky that I was privy to such a high level discussion. Any post I would make would be to thank the blog owners for making such interesting discourse public."

The n-category cafe is a group blog on ``math, physics and philosophy''. Being a group blog means that the main posts are written by several people - three in this case, John, David and Urs. The blog includes a short description of each of them. John Baez is a mathematician and a mathematical physicist, who specialized in quantum gravity and *n*-categories, and who is interested in many other areas as well. Gina did not know what n-categories were, but was looking forward to learning. She was very excited to discover that John Baez was a relative of the legendary singer Joan Baez. Urs Schreiber is a physicist and his work is related to string theory. And David Corfield was working on "the Philosophy of Statistical Learning Theory." Gina likes philosophy and greatly admires statistics.

Challenged by ksh95, Gina was determined to try to participate and make her mark on the n-category cafe!


John


David


Urs

18 From perception of mathematics to dyscalculia

"I think that already commutativity of multiplication is subtler than commutativity of addition, so it's not so surprising that commutativity of exponentiation breaks down entirely," Toby Bartels.

David Corfield's post "knowledge of the reasoned fact" dealt with abstraction and causality in mathematics. David asked: "what to make of our expectation that behind different manifestations of an entity there is one base account, of which these manifestations are consequences." He also referred to the idea that "mathematics has a causal/conceptual ordering".

Let me try to explain David's two ideas with very simple examples. (Perhaps, they are too simple.) We start with the first idea: The fact that three apples plus five apples sum up to eight apples, and the fact that three oranges plus five oranges sum up to eight oranges both manifest the same "base account" which is "5+3=8." As for the second idea, perhaps this is an example: The fact that 8-3=5 comes as a consequence of the more basic fact that 5+3=8 in the causal/conceptual ordering of the truths of mathematics that David refers too.

David Corfield's own example for one "base account" with many different manifestations was the normal distribution in probability theory which is described by the "Bell curve" (Chapter 17). The normal distribution and the bell curve occur in many different places.

Gina was very interested in children's perception of mathematics and wondered if children's perception may be relevant to what David called the "base accounts" of mathematical concepts, and if the order in which children learn mathematical concepts has something to do with the "conceptual ordering" David was asking about. She was also curious as to how things she learnt in university fit into David's framework.

Posted by: Gina on October 24, 2006 4:19 AM

Re: Knowledge of the Reasoned Fact

Dear David,

It appears to me that for some aspects of the issues you raise it would make sense to consider

a) Very basic mathematical facts that we learn in school or early college

b) Naive understanding (by children/students) of mathematical phenomena, their reasoning and the causality relations between them.

Perhaps we need something like Chomskian linguistics for mathematics.

Here are some examples:

Elementary school mathematics: (ask children, compare to what professionals say)   what is the reason for

1. 5+8 = 8+5

2. 6 x 5 =5 x 6

(Is 1. part of the reason for 2.?? is there a common reason??)

3. 42 - 19 = 23.

High school:

4. Why a^b is *not* equal to b^a ?

5. The sum of angles in a triangle is 180 degrees.

College:

6. The square root of 2 is not a rational number

7. A continuous function that takes a positive value at ***a*** and a negative at ***b*** takes 0 in between.

And a question that bothered me as a student, to which I have never been given a satisfying answer:

8. Why is it that a real function that has a derivative at any point may fail to have a second derivative, but a complex function, once having a derivative at every point, automatically has second and third and higher order derivatives??

Anyway, while talking about reasons and causality in mathematics, it may be good to compare insights of laymen and “professionals” and to consider concretely some examples like those suggested above.

This was certainly one of Gina's most successful remarks, and it led to many interesting discussions in various directions. The first three items on elementary arithmetic were especially popular and led to a separate discussion on "commutativity." The items related to college education have led to an interesting discussion on the notion of "continuous functions" and to an exciting description of the notion "elliptic differential equations." John Amstrong related to the idea of studying perception and learning of mathematics, and the structure of mathematics itself in a similar way to Chomsky's theory regarding language.

Posted by: John Armstrong on October 24, 2006 5:33 AM

That's a great idea, but in view of the reality of the situation I am pessimistic.
...
Chomskian linguistics works because everyone uses language and it seems perfectly natural to ask "why" questions about it. By and large most people are content to think of mathematics at even the most basic level as some sort of esoterica. The only reason any of it is true for most people is that that's what they were taught, if they remember it at all.

A long discussion about commutativity followed. Commutativity refers to a very simple yet important rule in mathematics. The commutativity law of addition asserts that a+b = b+a, and the commutativity rule for multiplication claims that a x b = b x a . In her remark Gina wondered how children perceive these rules, are they simply obvious? Are they obvious when it comes to one digit numbers, but less so with many digits?

Toby Bartels said: "I think that already commutativity of multiplication is subtler than commutativity of addition, so it's not so surprising that commutativity of exponentiation breaks down entirely."

Gina said: "I would conjecture that children usually understand why sum and product are commutative and the meaning of subtractions but the algorithms for arithmetic operations on 2-digit numbers obscure this understanding."

John Armstrong said: Children "understand" that 2+3 = 3+2, because they can see it on their fingers and the notion of the order they put their fingers up in is out of mind. "How could it be any other way?" they think.

Toby Bartels proposed set theory as a tool to explain commutativity of addition and multiplication. **John Armstrong replied**: "I think these interpretations of the 'meaning' of arithmetic operations are interesting and definitely worthy of consideration. I also think that they move even further from the question, which is how students learning these arithmetic operations think about them."

It took Gina a few seconds to realize that "the question" John referred to was her own question which had become central to this discussion. "Wow" she thought to herself.

John continued: "Gina's point in raising those particular examples was to suggest a parallel to Chomskian linguistics: ask children learning the concepts "why" they are true, and that will give you insight into their epistemic causality. The idea is that the first stabs of an unsophisticated observer towards an explanation contain a deep insight into how the human mind processes the concepts. This is to be contrasted with the viewpoint of an expert who has already thought long and hard about the nature of the subject, and who cannot simply "unknow" that knowledge."

Next came David, who explained the difference between his own quest for an ordering of all mathematical statements based on causality, and Gina's interest in the perception of children. And John and John engaged in a little exchange of details about "new math". John Baez said: The New Math may not have helped everyone, but it helped me and John Armstrong concluded : "The New Math foundered on many shoals, but its heart was in the right place."

John Armstrong

Gina enjoyed the wide spread discussion that followed, but children's perception of mathematics was closer to her heart.

Posted by: Gina on October 25, 2006 8:35 PM

Dear all,
I agree with John that a Chomskian study of children's learning, reasoning and insights about counting, arithmetic and mathematics will have a limited scope compared to a similar study pertaining to languages. It may, however, still be useful. Besides the philosophical issues of mathematical causality, it can be relevant to the understanding of **dyscalculia** - learning disabilities related to mathematics (Math Dyslexia).

Children's learning disabilities is a loaded and complicated subject, thought Gina. When it comes to mathematical abilities and disabilities, it appears that very little is known. The amount of Google hits for "dyscalculia" is two orders of magnitudes lower than for "dyslexia." This is a topic awaiting further study.

Chomskian Linguistics

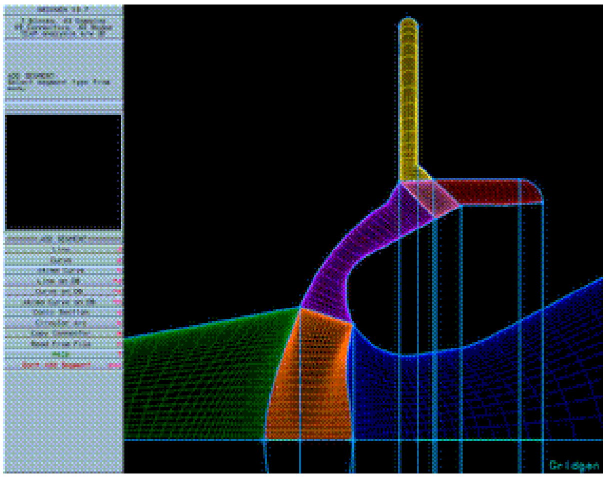
The Chomskian revolution in linguistics is comprised of three elements. The first is finding common structures and formulating common rules that apply to all human languages (to a much greater extent than before). The second is relating linguistics to studying and making hypotheses about the way children acquire languages. And the third is studying mathematically very abstract forms of languages. Chomsky's theory of generative grammar is important in all three aspects.

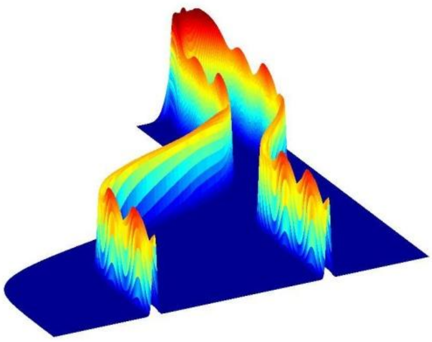
Chomsky's perception and demonstration of the unifying concepts behind different languages have impacted the way languages are perceived by linguists and by philosophers, and dramatically changed the way linguistics is practiced. Most relevant to the thread above, Chomsky saw a direct link between the way children acquire language and the internal structure and logic of languages. His works in this direction are regarded as part of the cognitive revolution in psychology. While emphasizing the universal rules behind different grammars, Chomsky also made a strong point regarding the uniqueness of the cognitive aspects of language as compared with other cognitive abilities. He had a famous debate with psychologist Jean Piaget on this subject. Chomsky's mathematical works on formal languages and the related concept of "automaton" are now fundamental in theoretical computer science.

Chomsky is criticized for being too dominant in the area of linguistics and for leading to unmotivated sharp turns in his own theory. The decline of individual language studies is regarded by some as a negative side-effect of the Chomskian revolution. Others argue that without a major additional statistical ingredient, formal mathematical structures á la Chomsky's generative grammar and "transformation rules" are insufficient for understanding the structure and acquisition of languages.

19 From Lehrer's song on continuous functions, to Baez's lyrics on elliptic PDE

"In my youth I mainly did hyperbolic partially differential equations (PDE), since I liked physics and especially waves. I looked down on the elliptic folks for working on static phenomena like bubbles and taut drumheads - no life, just sitting there, perfectly smooth. But, elliptic PDE certainly have their charm,"
John Baez, October 26, 2006 1:58





The last items on Gina's list were about college education. One question was about "calculus" and the notion of continuous functions. "Continuity" is a notion you learn in calculus that is difficult to fully understand. This led to an interesting little thread devoted to continuity, and Gina was especially grateful to Blake Stacy who provided a link to a song by Tom Lehrer:

Blake Stacey, on October 24, 2006 3:10 PM
Gina asked about the following...:

7. A continuous function that takes a positive value at ***a*** and a negative at ***b*** takes 0 in between.

How can we explain what a "continuous function" is? A textbook might do so as follows: In everyday speech, a 'continuous' process is one that proceeds without gaps of interruptions or sudden changes. Roughly speaking, a function $y=f(x)$ is continuous if it displays similar behavior, that is, if a small change in x produces a small change in the corresponding value f(x). This statement is rather loose and intuitive, and intended more to explain than to define. To give a real definition, we break out the machinery of limits and begin employing deltas and epsilons, as Tom Lehrer describes here:
Tom Lehrer, "There's a Delta for Every Epsilon",

There's a delta for every epsilon,
It's a fact that you can always count upon.
There's a delta for every epsilon
And now and again,
There's also an N.

But one condition I must give:
The epsilon must be positive
A lonely life all the others live,
In no theorem
A delta for them.

How sad, how cruel, how tragic,
How pitiful, and other adjec-
tives that I might mention.
The matter merits our attention.
If an epsilon is a hero,
Just because it is greater than zero,
It must be mighty discouragin'
To lie to the left of the origin.

This rank discrimination is not for us,
We must fight for an enlightened calculus,
Where epsilons all, both minus and plus,
Have deltas
To call their own.

Performed for Irving Kaplansky's **80th birthday celebration** (19 March 1997). **[Editor's comment: follow this link!]**

We come to the last item on Gina's list. Gina's last question was especially complicated. It was about functions over the complex numbers. The complex numbers are obtained by adding an "imaginary" number "i", the square root of -1. When you take the ordinary numbers (which are called "real numbers") and add 'i', and allow addition and multiplication, all sorts of miracles occur.

Gina asked about complex functions and about one of those miracles. In response, John Baez talked about "partial differential equations" (PDE in short). Studying PDE is important in mathematics and in most of its applications. John related the miracles of complex numbers and complex functions to a special family of PDE – elliptic PDE. Understanding John Baez's reply requires some college mathematics, but perhaps the beautiful Baez music can be appreciated even without understanding the lyrics.

Posted by: John Baez on October 26, 2006 1:58

Gina writes: Why is it that a real function that has a derivative at any point may fail to have a second derivative, but a complex function, once having a derivative at every point, automatically has second and third and higher order derivatives?? I would be happy to hear if there is a "reason" or "intuition" behind the miraculous difference between real functions which have a derivative at every point, and complex functions with the same property.
There certainly is a reason, and when I teach complex analysis I try to explain it.
After all, this is one of the biggest pleasant surprises in mathematics. In real analysis you have to pay extra for each derivative: for example, most functions that are 37 times differentiable do not have a 38th derivative. But in complex analysis being differentiable *once* ensures a function is *infinitely differentiable!* It's as if you bought a doughnut at a local diner and they promised you free meals for the rest of your life! And some people say there's no such thing as a free lunch....
So, we have to understand this seeming miracle.

The first fact I try to impress on my students is that for a differentiable function f on the complex plane, the amount by which f(x+***i***y) changes when you slightly change y is ***i*** times the amount by which f(x+***i***y) changes when you slightly change x. Why? Because a tiny step north is ***i*** times a tiny step east, and the derivative is a *linear* approximation to f.
That's simple enough. But, the fact that a step north is i times a step east makes linearity far more powerful on the complex plane than on the real line, where you could only take, say, 2 steps east, or -3 steps east.

Now, just to be differentiable, a function must *satisfy a differential equation:* **the Cauchy-Riemann equation.**

$$\partial f/\partial y = i\,\partial f/\partial x$$

This allows all sorts of great things to happen. For one thing, it means we can't change f in one place without changing it in lots of other places, as well: if we mess with it in a tiny neighborhood, it will no longer satisfy the Cauchy-Riemann equation at the edge of that neighborhood.
So, differentiable functions on the complex plane are not "floppy" the way differentiable functions on the real line are. You can't wiggle them around over *here* without having an effect over *there*.
In fact, if you know one of these functions around the edge of a disk, you can solve the Cauchy-Riemann equation to figure out what it equals in the middle!
So, such a function is like a drum-head: if you take your fingers and press the drum-head down near the rim, the whole membrane is affected.
Indeed, the height of a taut drum-head satisfies the Laplace equation, which also holds for any function satisfying the Cauchy-Riemann equation:

$$(\partial^2/\partial x^2 + \partial^2/\partial y^2)f = (\partial/\partial x + i\partial/\partial y)(\partial/\partial x - i\partial/\partial y)f = 0$$

So, the analogy is not a loose one: you really can understand what complex-analytic functions look like - well, at least their real and imaginary parts - by looking at elastic membranes.

And if you do this, one thing you'll note is that such membranes are really, really smooth. One way to think of it is that they're minimizing energy, so any "unnecessary wiggliness" is forbidden. We can make this precise by noting that the Laplace equation follows from a principle of minimum energy, where energy is

$$\iint |\nabla f|^2 \, dxdy$$

So, the reason why a once differentiable complex function is automatically smooth is that:

1) North is ***i*** times east

So

2) To be differentiable, a function on the complex plane must satisfy a differential equation

And

3) This equation makes the function act like an elastic membrane.

This is a remarkable combination of insights, none particularly complicated, which fit together in a wonderful way.

Posted by: Gina on October 26, 2006 1:00 PM

Many thanks, John, for this beautiful reason. It looks very appealing and quite different from the way I remembered it. Now, I wonder if your explanation (which is very inspiring) will qualify as "causation" (of the kind David asked about), even on a heuristic level. To examine it, we should ask: Does such a reason apply in other cases? Namely, are there any other examples, perhaps a whole class of them, where your 3^{rd} point applies: a differential equation that forces every differentiable solution to have derivatives of any order?

John Baez's response led to some further exchange between David and John, dealing mainly with the possibility of extensions to the "Quaternions", a miraculous and mysterious system of numbers invented by Hamilton in the 19th century. The highlight of the discussion was, no doubt, John Baez's additional explanations about "elliptic partial differential equations."

Posted by: John Baez on October 29, 2006 7:57 AM

Gina wrote: "... Now, I wonder if your explanation (which is very inspiring) will qualify as "causation" [...] namely, is there any other example, perhaps a whole large class of them, where your 3rd point applies... "

I'm glad you enjoyed my little explanation. It's sad that most classes on complex analysis don't explain this stuff.

Yes, there is a vast class of partial differential equations (PDE) such that any solution automatically has derivatives of arbitrarily higher order! These are the so-called **elliptic** differential equations. If you talk to experts on PDE, you'll find they often prefer to concentrate on one of these three types of equations:

Elliptic: here the classic example is the Laplace equation

$$\partial^2 f / \partial x^2 + \partial^2 f / \partial y^2 = 0$$

Elliptic equations often describe static equilibrium.

Hyperbolic: here the classic example is the wave equation

$$\partial^2 f / \partial x^2 - \partial^2 f / \partial t^2 = 0$$

Hyperbolic equations often describe waves.

Parabolic: here the classic examples are the heat equation

$$\partial f / \partial t - \partial^2 f / \partial x^2 = 0$$

and Schrödinger's equation

$$\partial f / \partial t + i \partial^2 f / \partial x^2 = 0$$

Parabolic equations often describe diffusion.

The techniques for dealing with the three kinds of equations are very different. They have completely different personalities. When it comes to obtaining many powerful results, elliptic PDE are the easiest to prove, in part because "elliptic regularity" guarantees that solutions are smooth.

To see if a linear PDE is elliptic, you write it down like this:

$$((4 + \sin x)\partial^4 / \partial x^4 + \partial^4 / \partial y^4 - x^2 \, \partial/\partial x) \, f = 0$$

and peel off the differential operator involved:

$$(4 + \sin x)\partial^4 / \partial x^4 + \partial^4 / \partial y^4 - x^2 \, \partial/\partial x$$

Then you replace the partial derivatives

$$\partial/\partial x, \quad \partial/\partial y$$

by new variables, say

$$p_x, \ p_y .$$

You get a function called the **symbol** of your PDE:

$$(4 + \sin x) \, p_x^4 + p_y^4 - x^2 \, p_x$$

The **order** of your PDE is the highest number of partial derivatives that show up. In the example above, the order is 4.

To see if your PDE is elliptic, just look at what happens to its symbol as the vector $\mathbf{p}=(p_x, p_y)$ goes to infinity in any direction. If the symbol always grows roughly like $|\mathbf{p}|^k$, where k is the order of your PDE, your PDE is **elliptic**.

In the example above, the symbol indeed grows like $|\mathbf{p}|^4$ as $\mathbf{p}$ goes to infinity in any direction. So, it's an elliptic PDE and , any solution is automatically **smooth**: it has partial derivatives of arbitrarily high order!

If you have some spare time, you might convince yourself that the wave equation, heat equation and Schrödinger's equation are *not* elliptic. In these examples the order is 2, but there are some directions where the symbol does not grow like $|\mathbf{p}|^2$.

(I've given examples of PDE involving just two variables, x and y. Everything I said works for more variables, too.)

In my youth I mainly focused on hyperbolic PDE, since I liked physics and especially waves. I looked down on the elliptic folks for working on static phenomena like bubbles and taut drumheads - no life, just sitting there, perfectly smooth. But, elliptic PDE certainly have their charm.

Posted by: Gina on October 30, 2006 11:32 PM

Thanks a lot, John, for this beautiful explanation. I wish students who are under the impression that complex analysis is a piece of heaven, while PDE is down-to-earth "applied" stuff, would read it.

20 Foundations

In David Corfield's next post titled "Foundation" he asked: "Something that has long troubled me is the question of why philosophers have shown what, to my mind, is an unwarranted interest in 'foundational' mathematical theories which make little or no contact with mainstream mathematics." The case for category theory as a foundational mathematical theory was prominent in the thread.

In response to David, Gina said that she was always puzzled why philosophers were interested in mathematics in the first place. She also asked what category theory could offer philosophers and computer scientists.

Posted by: John Baez on October 30, 2006 7:38 PM

Gina asks why computer scientists should be interested in category theory. That's a good question. I should answer it sometime, in a nontechnical way. This thread is not the right place.

For now, I just want to note that computer scientists *are* interested in category theory. They don't need any convincing. There are lots of conferences on categories and computer science, just like there are lots of **books** and **review articles**. So, there's no need for any categorical proselytizing, at least in computer science. The train is already rolling. Here at the *n*-Category Café we can just **jump on**!

Why should computer scientists be interested in category theory? Gina was reminded of the following dialogue:

A: "Why should there be 'z' in 'money'?"

B: "But there isn't 'z' in 'money'!"

A: "So why isn't there a 'z' in 'money?"

B: "Why should there be 'z' in 'money'?"

A: "That's what I asked you in the first place, why should there be 'z' in 'money'?

Kenny Easwaran explained why philosophers are interested in mathematics:
Logic has clear philosophical relevance, as does our knowledge of logic.
The case for basic arithmetic being of philosophical interest is only slightly trickier. The notion of ontological commitment is extremely important in analytic metaphysics. When I say “the number of planets is 8”, there’s a question of whether I’m just committing myself to some planets, or to a number as well.

Gina was drawn to what Kenny Easwaran wrote but, at the same time, she could not understand it at all. Kenny continued:

Frege, and recently others, have wanted to show that this commitment actually already comes from logic and from some constitutive principles about the meaning of “number”, thus suggesting that it isn’t really a problem for metaphysics. Many philosophers are skeptical about whether areas of mathematics beyond logic and basic arithmetic are actually of general philosophical interest.

Can Category Theory serve as the foundation of mathematics?

Usually the foundation of mathematics is thought of as having two pillars: mathematical logic and set theory. We briefly discussed mathematical logic and the foundation of mathematics in the story of Gödel, Brouwer, and Hilbert (Chapter 2). The story of set theory is one of the most exciting in the history of mathematics, and its main hero was George Cantor, who discovered that there are many types of "infinity."

Mathematical logic was always considered as a very abstract part of mathematical activity, related to philosophy and quite separate from applications of mathematics. With the advent of computers, however, this perception completely changed. Logic was the first, and for many years, the main mathematical discipline used in the development of computers, and to this day large parts of computer science can be regarded as "applied logic."

While mathematical logic and set theory indeed make up the language spanning all fields of mathematics, mathematicians rarely speak it. To borrow notions from computers, mathematical logic can be regarded as the "machine language" for mathematicians who usually use much higher languages and who do not worry about "compilation." (Compilation is the process of translating a high programming language into machine language.)

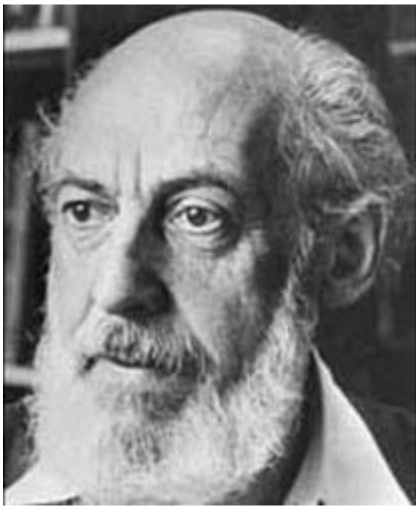
The story of Category Theory is markedly different from that of mathematical logic and set theory. It was invented to explain abstractly a certain area of mathematics called "algebraic topology." Specifically, the area of algebraic topology was based on a certain mathematical trick of associating algebraic objects to geometric objects, and category theory began by giving an abstract explanation to this trick and, along the way, to various other tricks from different areas of mathematics, which seemed unrelated. Category theory can be regarded as an abstraction of mathematicians' practices, even more than of mathematical notions. Amazingly, this abstraction of a single mathematical area turned out to be a very useful language, and a practical way of thinking in many (but not all) mathematical areas.

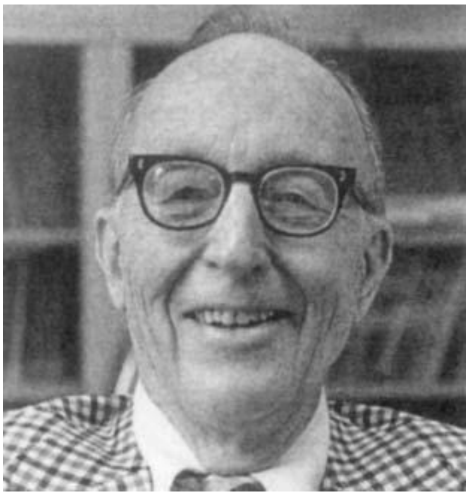
It is not easy to give a popular explanation of what categories are, because the notion is based on a familiarity with modern mathematics. One experience we have when we study mathematics is that the same method, or even the same equation, may solve very different problems.

This seems to express the "abstract" power of mathematics. Within mathematics itself, we are often interested in knowing when two mathematical structures are essentially the same. The technical word that expresses the equivalence of two mathematical structures is "isomorphism." Category theory can be described as adding one more level of abstraction: trying to understand, in an abstract way, "isomorphism" and related notions.

Here are a few more details. Many mathematical areas can be described in terms of the "objects" studied in them, as well as in terms of certain notions of "maps" (functions) between the objects. (So in set theory the objects are "sets" and the maps are "functions," and in group theory the objects are "groups" and the maps are called "homomorphisms.") Categories are mathematical gadgets that put these common structures on common abstract ground. Every category has "objects" and "morphisms" with some abstract properties. An important notion is that of a "functor," which is a way to relate one category to another. Categories indeed seem to play a pivotal role in the foundation of mathematics, or at least in some of its major areas, but they constitute a different sort of foundation. If we compare logic and set theory to the "machine language" of computers, we can regard category theory as an extremely useful universal programming tool.

Even if you did not follow the details about category theory, perhaps you got the correct impression that category theory is part of the mathematical trend to make things more and more abstract. Is this a good trend? Some opponents of category theory refer to it as "general nonsense." But even they concede that it is sometimes extremely powerful. There is a healthy tension in mathematics between the ongoing efforts to understand things more abstractly and the efforts to understand more and more concrete issues and examples.

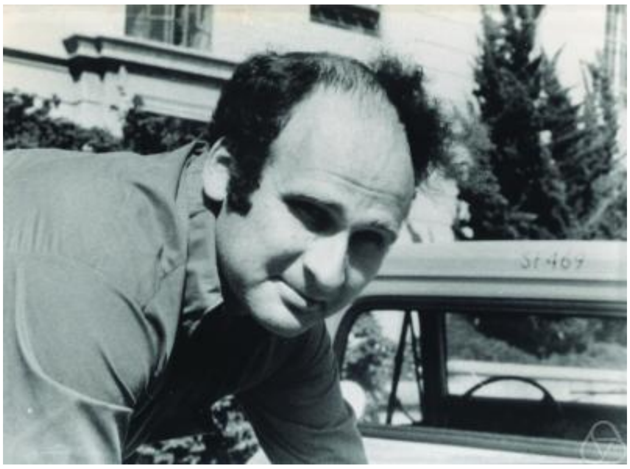




Samuel Eilenberg (left) and Saunders Mac Lane who introduced category theory.

And here is an ad-hoc discussion on this matter between the editor of this book, Menachem Magidor, and Azriel Levy.

Following is an ad-hoc discussion about category theory as a foundation for mathematics with Menachem Magidor (a set theorist and the president of the Hebrew University of Jerusalem) and Azriel Levy (a set theorist and Magidor's thesis advisor), which takes place in the main corridor of the Mathematics Department, where Menachem is standing and smiling the smile of a person visiting his beloved hometown after a long absence.



Menachem Magidor

GK (surprised): Hi Menachem, what are you doing here?

MM (tries unsuccessfully to look offended): What do you mean? Am I not wanted here?

GK: No, no, no, no, I was just wondering if the state of the Hebrew University is so good that you can afford to visit us.

MM: No, on the contrary, the situation is hopeless... Seriously, I was just taking a book…

AL: (enters the corridor, surprised) Shalom Menachem, what are you doing here?

MM (once again tries unsuccessfully to look offended): What do you mean? Am I not wanted?

AL: No, no, I'm just surprised that you have the time…

GK: (interrupts) Guys, I have a quick question for both of you, can category theory serve as the foundation of mathematics instead of set theory?

MM: (eagerly) Hmm, it is actually an interesting question, there is a result that a certain topos theory has the same power as a set theory with certain axioms … of course you need some separation and replacement axioms ... do you remember, Azriel?

AL: The crucial question in my mind is that sets are very easy to explain. Can you explain topos theory to high school students?

Other people arrive and the discussion is interrupted.

21 Computers, categories, analogies, and greatness

"Good mathematicians see analogies. Great mathematicians see analogies between analogies." Attributed to Stephan Banach

The following post a few days later was a pleasant surprise:

A Categorical Manifesto

Posted by John Baez

A while back **Gina** asked why computer scientists should be interested in categories. Maybe you categorical computer scientists out there have your own favorite answers to this? I'd be glad to hear them. To get you going, here's one man's answer:

Joseph Goguen, **A categorical manifesto**, *Mathematical Structures in Computer Science* **1** (1991), 49-67.

Gina was of course very flattered that her comment started a whole thread. The only remark which really dealt with computer science was by Dan Piponi, who explained some of the connections between Category Theory and computer science. The subsequent remarks dealt with category theory and not with computer science. But then Jonathan Vos Post put a nice twist into the discussion.

Posted by: Jonathan Vos Post on November 11, 2006 3:09 AM

Ulam on Banach; Re: A Categorical Manifesto

In his autobiography, Stanisław Marcin Ulam, a mathematician of the Lvov School of Mathematics, attributes the following to Banach:

“Good mathematicians see analogies. Great mathematicians see analogies between analogies.”

Vos post continued to discuss Banach's statement but Gina remained skeptical and was in mood for a good debate:

Posted by: Gina on November 13, 2006 1:23 PM
Banach (according to Ulam) said: “Good mathematicians see analogies. Great mathematicians see analogies between analogies.” And Jonathan asked if we can even say that ‘very great mathematicians see analogies between analogies between analogies between analogies’?”

Actually, Banach’s saying sounds appealing but rather suspicious, as shown by Jonathan’s suggestion that seems to push it towards the absurd.
We can check this with some examples: I am sure there are many examples of useful analogies in mathematics (what are the best ones, in your opinion?) but can you give a few examples of analogies between analogies? And can you give a single example of an analogy between analogies between analogies??

Does Banach’s saying (or Jonathan’s) make sense if we replace “mathematician” with “scientists”, ”police detectives”, “medical doctors”??

So when scrutinized, Banach’s statement may not be that much better than the common public perception:

“Good mathematicians can multiply 2 digit numbers in their heads and great mathematicians can multiply 5-digit numbers in their heads!”

The discussion that followed was fairly technical, and Gina tried her best to hold to her skeptical point of view even without understanding all the technicalities. The readers who are not familiar with topological spaces, fundamental groups, function fields and number fields can very safely skip to the next chapter.

John Armstrong replied: Here is an example

Analogy: A topological space X is to a covering space Y of X as the fundamental group of X is to the fundamental group of Y (a functor is an analogy). Better: The collection of covering spaces (up to homeomorphism) is like the collection of subgroups of the fundamental group (up to conjugacy).

Analogy: A field K is to an extension field K' as the group of symmetries of K is to the group of K'. Better: the collection of extension fields is like the collection of subgroups.

Analogy between analogies: Both of these situations express an adjointness, and the analogies between covering spaces and subgroups of the fundamental group, and between extension fields and subgroups of the symmetry group are similar.

("I got him", thought Gina)

Gina asked: Thanks, John, for the two very nice analogies. Now we can empirically test Banach's statement. Is it true to say that the mathematicians who found the first and second analogies you mentioned are good mathematicians, while the mathematician who found the analogy between the analogies is a great mathematician?

The mathematician behind the second analogy you mention is Galois, right? As for the first example, is it, perhaps Poincaré? (I am not sure; he discovered the fundamental group, I think.) They were, to be sure, good mathematicians. Who is the great mathematician behind the analogy between the analogies?

David replied and mentioned an analogy between "function fields" and "number fields" (related to John's Galoisian/Poincarean one) that could easily be cast as an analogy between analogies.

At the end**, Gina** was somewhat convinced (or tired) and said: "Banach could have just said: 'among various other things, good and great mathematicians spend their sweet lives by seeking analogies and analogies between analogies.' But this would have been much less impressive."

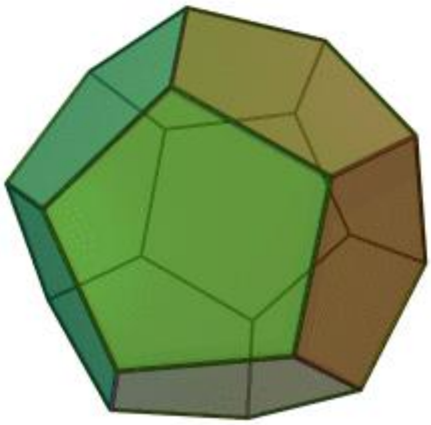
"But apropos Banach's saying," continued Gina, "what about 'greatness': is it **the** moving force behind it all? Let's discuss Greatness and the pursuit of greatness."

Greatness is an intriguing subject to pursue, thought Gina. But this topic was not picked up.

Philosophy, proofs, and computers

If the interest of philosophers in mathematics derives to some extent from its formal notion of "proofs," computer science has various new types of proofs to offer. "Randomized proofs" are proofs that verify a statement only up to some very high probability – "beyond a reasonable doubt." Interactive proofs are proofs that rely on a formal "protocol" of interaction. Interactive proofs are similar to proofs in court: if the alibi you present is refuted this goes a long way toward proving that you are guilty. Zero-knowledge proofs are proofs that reveal no additional information beyond the statement they prove. From an ordinary proof of a mathematical theorem it is possible to construct a probabilistically checkable proof, namely, a proof of which reading even a small, random part will convince you that the theorem is correct.

22 The Dodecahedron



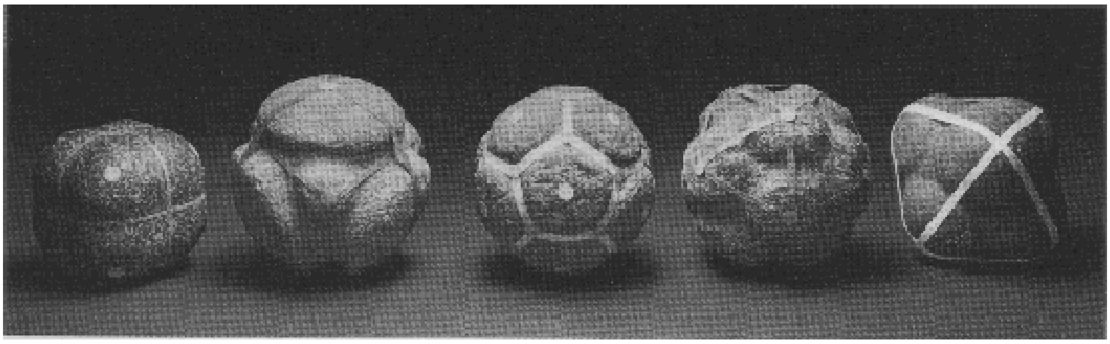
John Baez's post on November 12, 2006 and the link to his lecture on the dodecahedron were fantastic, Gina thought.

John Baez provided a link to his lecture, **Tales of the dodecahedron: from Pythagoras through Plato to Poincaré**, **Reese Prosser Memorial Lecture**, Dartmouth College, November 10, 2006.

Abstract: The dodecahedron is a beautiful shape made of 12 regular pentagons. It doesn't occur in nature; it was invented by the Pythagoreans, and we first read about it in a text written by Plato. We shall see some of its many amazing properties: its relation to the Golden Ratio, its rotational symmetries - and best of all, its use for creating a regular solid in 4 dimensions! Poincaré exploited this to invent a 3-dimensional space that disproved a conjecture he made. This led him to an improved version of his conjecture, which was recently proved by the reclusive Russian mathematician Grigori Perelman - who now stands to win a million dollars.

He further wrote: (By the way, when I say that the Pythagoreans invented the dodecahedron, I'm not claiming it had not been invented before! According to

Atiyah and Sutcliffe, these blocks were first found in Scotland around 2000 BC.



Should we count them as Platonic solids even if they're rounded? That's too tough a puzzle for me. Even less do I want to get into the question of whether the Platonic solids were invented or discovered!)

Symmetry

The dodecahedron is a highly symmetric body. Rotate it and notice that it looks the same in many angles. Use the Ctrl key and click on the picture of the dodecahedron above to see it rotating.

"Group theory" is the mathematical theory of symmetry. Emerging in the nineteenth century, it was one of the most important concepts to have been studied at the time. The books "Symmetry and the Monster" by Mark Ronan and "Finding Moonshine" by Marcus du Sautoy give a beautiful account of the modern theory of groups.

Notions of symmetry are of importance in many areas of science, and especially in physics. Each fundamental force is associated with a certain "group of symmetry," and every elementary particle is associated with a certain "representation" of this group. There are different views on the question whether symmetry is a fundamental notion or rather an emerging notion in physics.

It was a good time to bid farewell to the *n*-category café. Once again there were regular polytopes in the background. This time, it was a friendly goodbye and a temporary one. Gina wanted to move back to the string theory debate.

Part III: *Asymptotia* and Lee Smolin's "The Trouble with Physics"

"Asymptotia" was the weblog of string theorist Clifford Johnson. Clifford ran a series of posts titled "Tales about storms in a tea-cup I, II, III, IV, V and VI" (and later VII) dealing with the string theory controversy and, in particular, with Peter Woit's and Lee Smolin's positions and books. In this part, can read about Gina's adventures mainly in the remarks section of a single post: "Tales about storms in a tea-cup VI."

Clifford was Gina's favorite blogger. He seemed relaxed and let the discussion flow. He did not behave like an ultimate authority and, in fact, did not interfere too much. Gina did not agree with Clifford's overall approach towards the debate. She wrote: "Clifford's name of the series of posts on this controversy "More scenes from the storm in a teacup" may seem demeaning, but it need not be, 'intellectual storms in teacups' may very well be an important part of what academia and science are about."

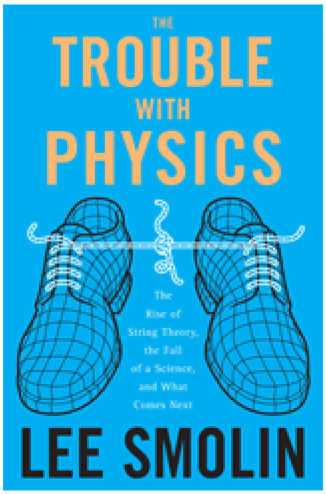
23 Can strings be fractals? and all sorts of other things

Debates and controversies

Some people complained about the cover of Lee Smolin's book and found it too insulting. Others considered the analogy with the naked emperor insulting. On the other side of the debate some people even suggested that negative book reviews are unfair since they cause people not to buy books they would otherwise buy. Gina strongly disagreed.

73- Gina Nov 12th, 2006 at 4:44 pm

There is nothing wrong with the cover of Smolin's book, in fact it is nice. (There was also nothing wrong with Lubos Motl's Amazon review of Smolin's book.) There is nothing wrong with the naked emperor metaphor either. And there is nothing wrong with the provocative name "not even wrong", in fact it is also nice. Exact-science guys are simply not used to practicing and maintaining debates and controversies. (They are either being extremely puritan or occasionally unreasonably aggressive.) Try to consult your colleagues from the humanities and social sciences!



The cover of Lee Smolin's book

The Hype

90 - Gina Nov 18th, 2006 at 9:13 am

It was claimed in these discussions that string theory is over-hyped, and perhaps it is. But let me argue that this hype served not only string theory but also, to a large extent, physics, mathematics, and science as a whole. Society's exposure to sometimes bold (but sincere and serious) claims about science can serve science. On the other hand, hype cannot change even a little bit the factual scientific reality or ease even slightly the immense difficulty in exploring it.

Can strings be fractals?

gina Jan 21st, 2007 at 8:03 pm

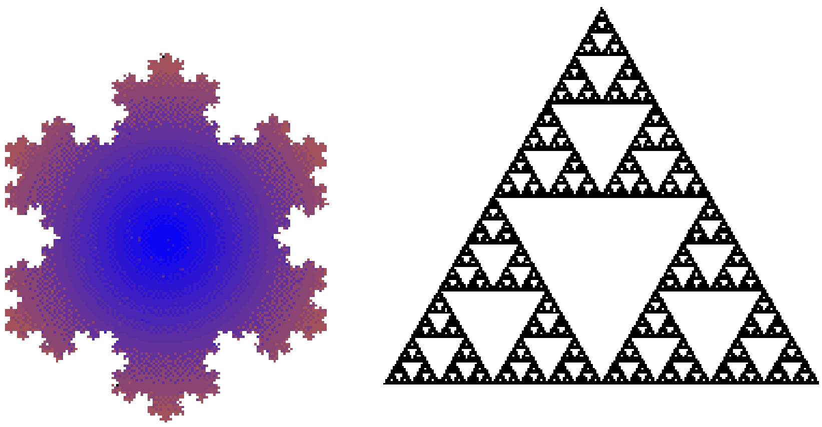
Dear Clifford,

I have the following question about strings. (Probably it puzzles others as well.) From what I saw in Wikipedia (string theory) strings are like arcs that can close up or remain open, and if you add time, you get a nice and smooth two dimensional picture like a cylinder. Is this the right mental picture to have in mind?

My question is, why one-dimensional and not, say, 1.25-dimensional? Do you regard string as a nice looking curve that over time looks like a nice surface? (Or it is just the pictures.) I remember that there are curves that look very different: like the famous Peano plane filling curves. These curves have dimension 2 and there are even curves that have a dimension which is not an integer number. So isn't it better to assume strings are some sort of fractals? Say they are 1.25-dimensional (or 1.63-dimensional) arcs in space? Does string theory rely (beyond the nice pictures) on thinking about strings as smooth and nice, and their "spacesheets" as nice behaving surfaces? Does it make a difference if strings are fractals?

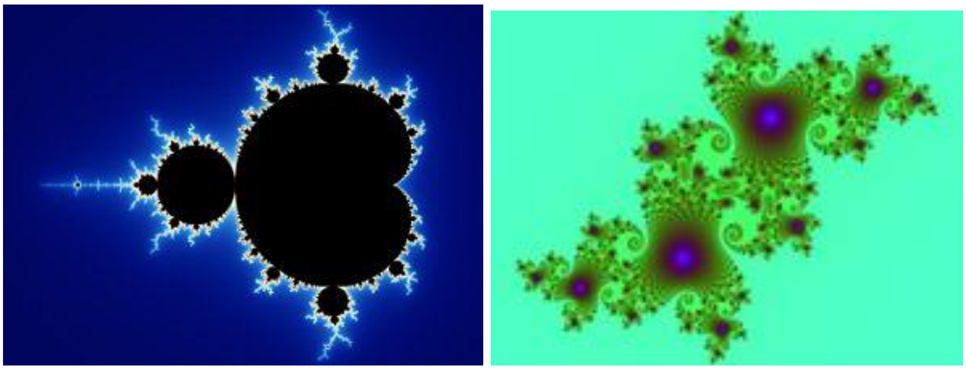
Fractals

Fractals are beautiful mathematical objects whose study goes back to the late 19th century. The Sierpiński triangle and the Koch snowflake are early examples of fractals which are constructed by simple recursive rules.



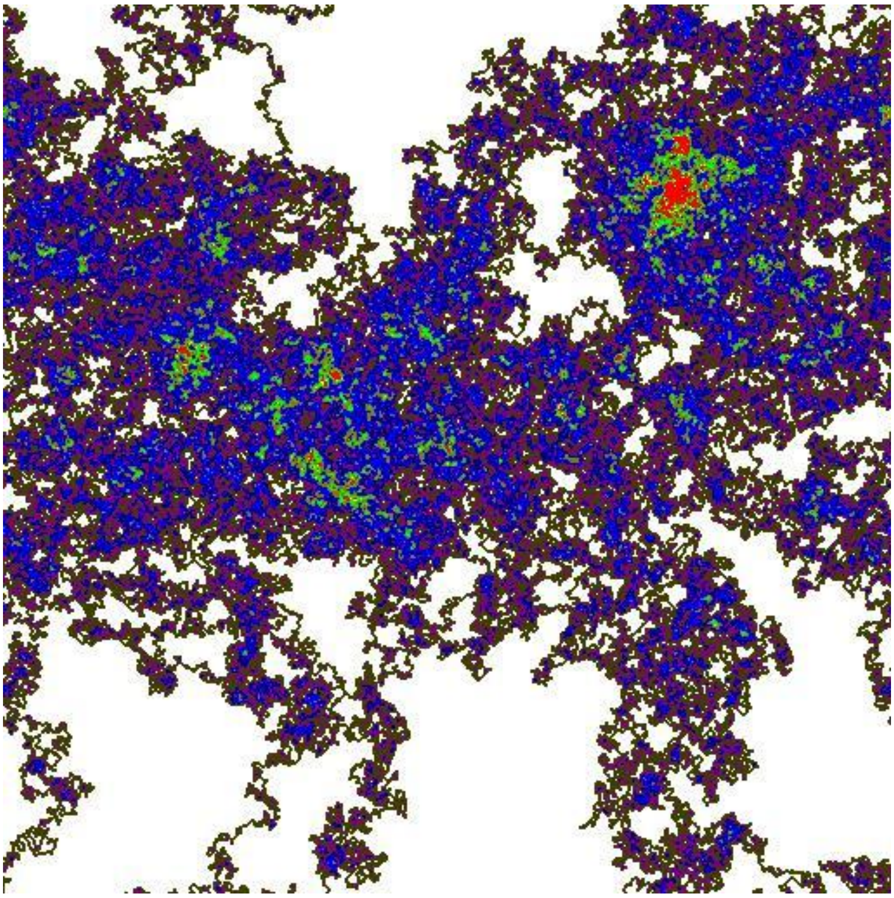
Koch snowflake (left) and Sierpiński triangle (right). (Note: The Koch snowflake is just the "boundary" of the blue shape in the picture.)

Other examples are based on the study iterations of simple functions, especially functions defined over the complex numbers.



Mandelbrot set (left) and Julia set (right).

Still other examples come from various stochastic (random) processes. For instance, the outer boundary of a Brownian motion in the plane, and the boundary of the percolation process (random Hex game) that we considered in Chapter 9.



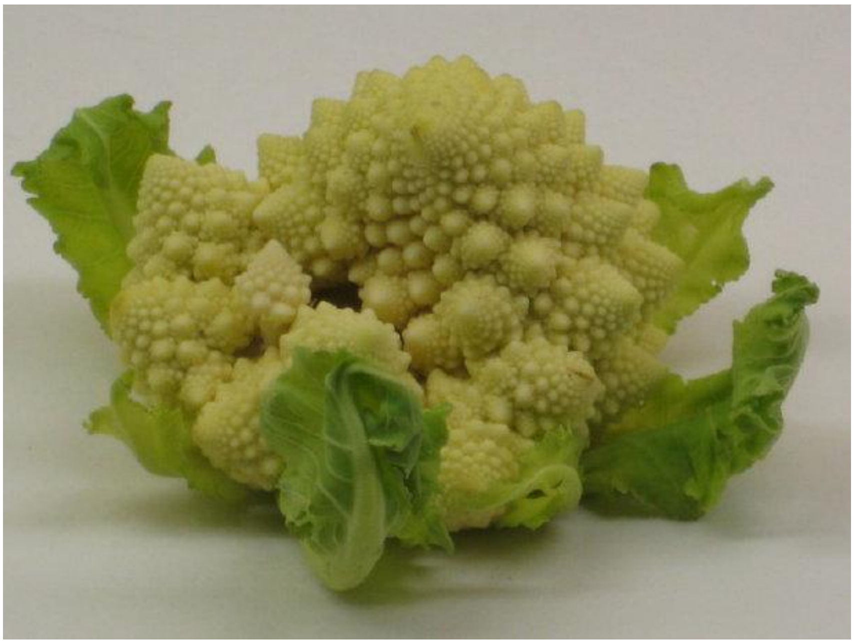
Brownian motion in the plane. The boundary is the "border" between the white areas and the colored areas. (If you get the impression that "boundary" is an important notion in many areas you are correct!)

We already mentioned the importance of the notion of "dimension" in mathematics. A point has dimension 0, a line has dimension 1, the plane has dimension 2 and the space is 3 dimensional. Fractals often have "fractional dimension". The Koch snowflake has dimension 1.2619 , and the Sierpiński triangle has dimension 1.5850. The "boundary" of the Brownian motion in the plane is a fractal; Mandelbrot conjectured that its dimension is 4/3 and this was recently proved by Lawler, Schramm, and Werner. The (marked) border between the white and grey areas in the percolation picture of Chapter 9 also leads to an interesting fractal.

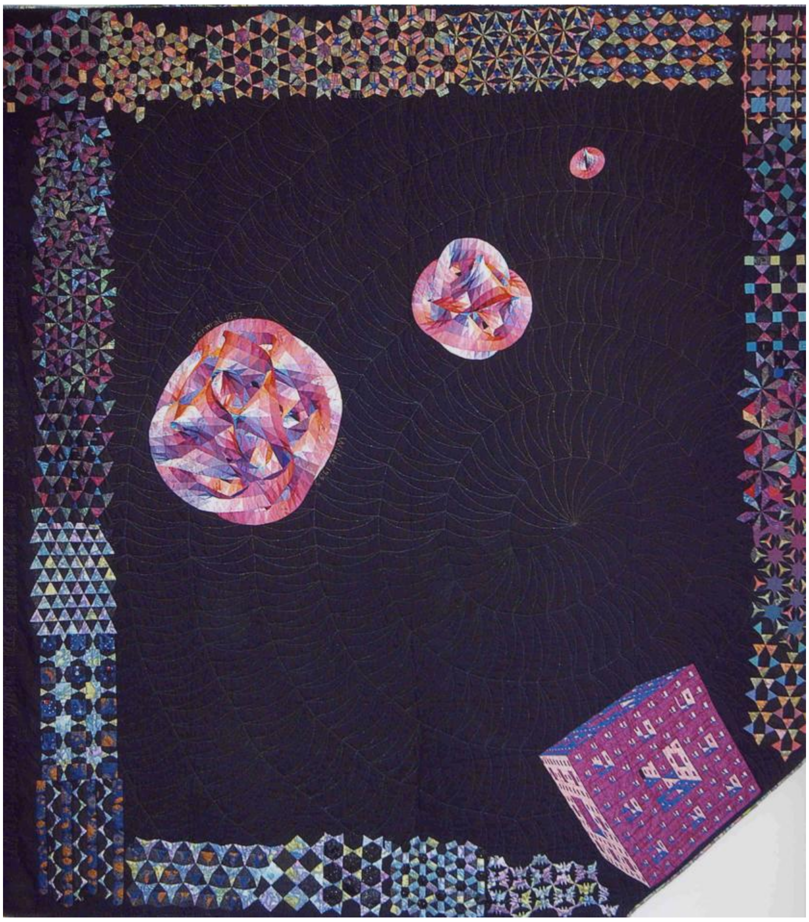
The term fractal was coined by Benoît Mandelbrot, who in his book also proposed the following informal definition of a fractal: "A rough or fragmented geometric shape that can be subdivided into parts, each of which is (at least approximately) a reduced-size copy of the whole." An important property of fractals is referred to as "self-similarity", whereby a small part of the big picture is very similar to the whole picture.

Mandelbrot also understood and promoted the importance of fractals in various areas of physics. Indeed, today fractals play an important role in many areas of modern physics (and there is also some controversy regarding their role). Mandelbrot also wrote a paper concerning applications of fractals in finance.

The notion of self similarity is also important in other areas. In computer science, the self similarity of a problem is referred to as "self-reducibility", and this property facilitates the design of efficient algorithms for solving the problem.



A fractal-shaped cauliflower, photographed, presented, (and eaten(?)) by Clifford Johnson.



A quilt by Anna-Maria Brenti, which is based on several mathematical constructions. In the bottom-right corner we see the “Menger- Sierpiński sponge,” a fractal of dimension 2.7268 constructed by taking away pieces of the unit cube.

24 The Trouble with Physics: Gina's sixteen objections

At some point down the discussion, Lee Smolin complained that people hardly relate to, and try to argue with what he actually wrote in his book. Gina found this complaint rather justified, and also an opportunity for discussion on some specific claims made by Lee. [Editor's note: apparently Gina referred to sixteen as "baker's 15."]

139 - Gina Nov 30th, 2006 at 4:45 am

> [Lee Smolin:] "I can count on one hand the number of times someone has made in print or online an argument of the sort, 'Smolin, or Penrose or … makes the following argument….. leading to the following conclusion…., but it is wrong because he makes the following mistake in reasoning…..' "
>
> Here are a baker's 15 points, some central, which, in my opinion, point to the incorrect, or, at least, problematic aspects of Lee's argument. (Dear Lee, I truly think that trying to attack your argument can be of service. There are, of course, things I like in the book.)
>
> I will be happy to elaborate on each of these points.
>
> *1. Asserting that there was no real progress on the five major problems of physics described in Chapter 1 of Smolin's book is incorrect, or at least very problematic.*
>
> 2. (Even ignoring 1.) Interpretation of the lack of progress, or the limited progress in answering the five major problems as a definite failure is very problematic.
>
> *3. The interpretation of the" Mandelstam story" as mainly damaging to string theory is incorrect.*

4. The interpretation of Maldacena's conjecture is biased. Lee's interpretation is not sufficiently positive towards the ST endeavor, even unreasonably so.

5. The substantial reliance of Lee's case on how to interpret Maldacena's conjecture is in itself damaging to his case.

6. The issue of pluralism should be studied from a wider perspective.

7. Lee overlooks (or neglects) a more serious concern about plurality in the high energy physics community (which is potentially problematic in terms of his other claims, however.)

8. It is strongly implied by Lee, but not explained, that it is not reasonable that string theory and drastically different quantum gravity approaches should develop separately, e.g., that string theorists should be on top of development in LQG (loop quantum gravity). As a matter of fact, it seems quite reasonable that (,e.g.,) ST and LQG would develop separately.

9. Contrary to Lee's suggestion, young scientists should not be encouraged to attack fundamental physics problems head on.

10. The negatively portrayed reactions to sporadic bold theories and ideas (like 't Hooft's) are, in fact, very reasonable. The current attitude of scientists is essentially the correct attitude.

11. Some natural, often welcome social processes in academia (regarding academic judgment and decision making) are depicted negatively. This is a mistake.

12. At a closer look, many claims, stories and quotes against the string theory community are not really damaging to the ST community, and mentioning them weakens the quality of Lee's argument.

13. Simultaneously talking about scientific and social issues, including funding, is in itself very problematic. Not being sufficiently aware of this difficulty is a mistake.

14. Although a substantial part of Lee's argument is about sociology, there appear to be no real interactions with social scientists.

15. Lee's ideas on revolutionizing universities are not detailed, and the hints we get are not promising.

16. Lee claims that science clearly calls for a revolution, as it is now more conservative (anti-revolutions) than ever. This is incorrect. In fact, maybe the opposite is true. The whole "revolution" terminology is off-base.

140 - Lee Smolin Nov 30th, 2006 at 8:00 pm

Dear Gina,

Thanks very much. I'd be very interested if you expanded some of these points. To make it easier to manage, shall we take one at a time? If you elaborate on one and explain why you disagree I will respond accordingly.

Thanks,

Lee

"I am in business", thought Gina, and was at the same time a little terrified as well.

Baker's dozen and fault tolerance

Gina's reference to sixteen as "baker's fifteen" is probably based on the old notion of a baker's dozen. (The following is taken from Wikipedia.) A baker's dozen, also known as a long dozen, and as Devil's dozen, is 13, one more than a proper dozen. The expression found its genesis in 13th-century England.

The oldest known source and most probable origin for the expression "baker's dozen" dates to the 13th century in one of the earliest English statutes, instituted during the reign of Henry III (r. 1216-1272), called the Assize of Bread and Ale. Bakers who were found to have shortchanged customers could be liable to severe punishment. To guard against the punishment of losing a hand to an axe, a baker would give 13 for the price of 12, to be certain of not being known as a cheat. The practice can be seen in the guild codes of the Worshipful Company of Bakers in London.

Baker's dozen is an early example of "fault tolerance" which refers to the property that enables a system (often computer-based) to continue operating properly in the event of a failure of (or one or more faults within) some of its components. Mechanisms for fault tolerance guarantee the operation of a complex system faced with noise and errors. The practice of baking 13 items for an intended dozen was to prevent "short measure", on the basis that one of the 13 could be lost, eaten, burnt or ruined in some way, leaving the baker with the original dozen. Mechanisms for fault tolerance and error-corrections are important in natural biological processes. Arguably, fault tolerance is also important in designing economic and political systems.

A Brief Description of Smolin's Book

Smolin's book has four parts. The first part discusses physics, its main insights and achievements as well as its main research problems. It begins by stating the five great problems in theoretical physics, and goes on to discuss the beauty of physical theories and the "beauty myth", and the role of geometry, the importance of "unification," which brings together different theories under the same roof. Finally, Smolin reaches the question of quantum gravity- the unification of quantum mechanics and Einstein's theory of gravitation.

The second part describes string theory, which (among other things) is a theory of quantum gravity. Smolin describes the first revolution in string theory – the realization of the important role of supersymmetry, and the second revolution – the emergence of a new "M-theory" that unified earlier different string theories. He describes the string theory landscape (that Smolin himself was among the first scientists to discover,) and discusses the anthropic principle, an attempt to deal with the difficulties posed by the landscape.

The third part describes speculative physics not directly related to string theory. Smolin skeptically examines many truths from physics and offers some radical alternatives and ideas. He mentions several alternatives for quantum gravity, including "Loop quantum gravity," which is a theory he was among the first to introduce, "twistor theory" which is mainly associated with physicist Roger Penrose, and "non commutative geometry" developed by mathematician Alain Connes. Smolin raises questions concerning the foundation of quantum mechanics, and mentions a theory called MOND introduced by physicist Mordechai Milgrom. This theory suggests that Newton's law of gravitation breaks down for highly accelerating bodies.

The fourth part of the book is about sociology in science and academic life, and about fundamental issues regarding the question of "what is science." Most of the discussion in our book is related to this final part, which Smolin regards as the most important contribution of his book.

A little road-map

Here is a little road map to the various points Gina raises and to the relevant chapters in which these points are later discussed.

A. Physics: Point 3 (Mandelstam) is discussed in Chapter 26 (and revisited in Chapter 36). Points 4 and 5 (Maldacena) are discussed in Chapter 28. The overall evaluation of string theory: Points 1 and 2 are discussed in Chapter 29.
B. Pluralism: Points 6, 7, and 8 are discussed in Chapter 30, and point 7, about more traditional areas of high energy physics, is revisited in Chapter 39.
C. Foundational studies and bold theories. Point 9 about encouraging young scientists to tackle big foundational questions is discussed in Chapter 31, point 10 about treating fantastic claims of great people is discussed in Chapter 33,
D. Sociology and economics of academia: Point 11 about disturbing sociological aspects of universities is discussed in Chapter 34, point 12 about Smolin's evaluation of the string theory community is discussed in Chapter 35, points 13, 14, and 15 regarding various social and economic matters are discussed in Chapter 40.
E. Scientific revolutions: Point 16 about scientific revolutions, is discussed in Chapter 41.

25 TTWP: Mandelstam; optimism

"Give me a couple of years free from other duties, and I shall complete the task – the task of copying the whole Book of Nature, and of writing the new science." Francis Bacon, according to Carl Popper.

The story about "finiteness of string theory" and the results in this direction by physicist Stanley Mandelstam play an important role in Smolin's argument and occupy a very large percentage of the debate. String theory replaces point particles with curves and studies them using certain complicated infinite sums. Each individual term in those infinite sums is itself the outcome of a complicated mathematical process. Miraculously, at the right dimension, these terms become meaningful. This is the content of Mandelstam's effort and results. Gina felt that, although this issue was certainly important, it had gradually become a "red herring." Too much was made of this particular point.

143 - Gina Dec 1st, 2006 at 11:56 am

> Thank you very much, Lee, for your willingness to discuss this matter with me, and thank you very much Clifford for your willingness to host it in the back yard of this old post.
>
> As you suggested, Lee, let's take it one at a time. I will elaborate on each one of my points in my free time, you are most welcome to reply, as you see fit. Unless emergency occurs, I will not reply to your replies; here, we do need to converge. Points 3-5 are furthest away from my own interests in this debate so let's start with them. My third point was:

3. The interpretation of the" Mandelstam story" as mainly damaging to string theory is incorrect.

Here we refer to the story about the "finiteness of string theory". Important early evidence that string theory is going in the right track was that certain mathematical difficulties (called "anomalities") were resolved for the models proposed in string theory. Some of this evidence was based on the work of physicist Stanley Mandelstam. (Some evidence was found even before Mandelstam.) As Lee argues in his book, Mandelstam did not present a proof but rather partial mathematical evidence and it took, according to Lee, 17 (!) years until further stronger mathematical evidence was presented.

This story is overall not damaging to string theory. Mandelstam's story is cited by Lee in the context of string theorists' optimism (or excessive optimism). The meaning and nature of optimism in science is probably a topic for a whole separate discussion. Here we have a case where, as far as I can judge, both the overall optimism and the mode of operation of "moving on" were justified.

144 - Peter Woit Dec 1st, 2006 at 3 :09 pm

Gina,

It seems to me you completely mischaracterize and misunderstand Smolin's argument here. Why do you think it is a good idea to publicly criticize Smolin over an issue you admit that you don't understand?

Smolin was not saying string theorists should not have "moved on" and conducted research under the assumption that the conjectured multi-loop finiteness would work out, he was just saying that he was surprised to find that almost all string theorists he asked thought that this conjecture had been proven by Mandelstam, when it hadn't been.

Gina was quite surprised by Peter Woit's comment. On his own weblog he accused Gina of wasting his time and here he volunteered to respond to a question addressed to Lee.

Peter continued and described another example of what he referred to as "this phenomenon:" Peter was very excited by results that indicated that a certain theory called "N=8 supergravity" is finite, although string theorists conjectured all along that this theory is not finite. (Gina thought that Peter is over excited about these interesting developments, and that having conflicting and confusing pieces of information is a very common phenomenon when new scientific theories emerge.)

146 - gina Dec 1st, 2006 at 4:38 pm

> Hi Peter,
>
> I raised 16 points where I felt Lee's arguments were either not correct or problematic. This is an academic discussion and not public criticism, and I truly think that such critique can be useful, even if I am wrong on **all** 16 points.
>
> Three of my 16 points centered on more technical issues, but I feel that I can understand Lee's logical argument even without understanding the precise technical nature of "finiteness of string theory". I think that my interpretation of this issue is reasonable and my critique stands.
>
> This was an example which was supposed to have shown that string theorists were over-optimistic. But in this case the optimism appears to have prevailed. A much better example would have been a central case where string theorists claimed one thing and eventually the opposite was proven.

Gina thought that Peter Woit's comment reveals a certain flaw in his approach to science and continued:

When you talk about "this phenomenon" it seems that you have a certain difficulty in the understanding of how science is practiced. At the end (in the best of cases) all pieces fit together and it all looks wonderful. But along the way there are many misunderstandings and conflicting pieces of information, and sometimes even mistakes.

Lee's reply was more to the point and quite interesting. He uses the term "perturbative finiteness" to describe Mandelstam's disputed result.

149 - Lee Smolin Dec 2nd, 2006 at 3:17 am

Dear Gina,

Thanks. The issue has a scientific component and a sociological component. Science first:

-Perturbative finiteness is a major element of the claim of string theory as a potential theory of nature. If it is not true then the case for string theory being a theory of nature would not be very strong.

-Perturbative finiteness has not been proven. There is evidence for it, but that evidence is partial. There is a complete proof only for genus two, which is the second non-trivial term in an infinite power series, each term of which has to be finite. The obstacles to a complete proof are technical and formidable; otherwise we would certainly have either a proof or a counterexample by now.

This is not an issue of theoretical physicists' rigor vs. mathematical rigor. There is no proof at either level. There is an intuitive argument, but that is far from persuasive.

Is string theory in fact perturbatively finite? Many experts think so. I'm afraid that if there were a clear way to a proof it would have already been found and published, so I find it difficult to have a strong expectation, either way, on this issue.

The sociological issue is not that most string theorists chose to continue working on other problems in the hope that someone would eventually resolve it. This is fine, we all do what we can do, and many of us theoretical physicists do not have the inclination or talent to work on such a difficult and subtle mathematical problem.

The problem is that many presentations of string theory, for the public as well as for colleagues, seem to have been misleading on this issue. Many people I spoke to were under the impression that perturbative finiteness was an established fact. Most review papers for physicists and popular books gave the impression that perturbative finiteness of string theory is a fact. Only a few characterize the situation correctly.

Was this a problem? I think it was. I don't believe anyone was deliberately dishonest about this, but there was an atmosphere in which belief in the theory went beyond the evidence in the published papers. Was this conducive to the fact that many people, both in and out of physics, were under the impression that the problem of quantum gravity was already solved by string theory? I am not an historian or sociologist, but I worry that this might have been the case.

You claim that my discussion of this issue is "not damaging to string theory". I don't think it is, if by "string theory" you mean that string theory is of interest as one of several interesting approaches. It is damaging to the claim "string theory" is "the only game in town", as some proponents continue to insist.

Thanks,

Lee

Optimism: beat this I

Here is a quote from Karl Popper's paper "Science, Problems, Aims, Responsibilities" about Francis Bacon:

"According to Bacon, nature, like God, was present in all things, from the greatest to the least. And it was the aim or the task of the new science of nature to determine the nature of all things, or, as he sometimes said, the essence of all things. This was possible because the book of nature was an open book. All that was needed was to approach the Goddess of Nature with a pure mind, free of prejudices, and she would readily yield her secrets. Give me a couple of years free from other duties, Bacon somewhat unguardedly exclaimed in a moment of enthusiasm, and I shall complete the task – the task of copying the whole Book of Nature, and of writing the new science."

Optimism: beat this II

Here is a tale from Arundhati Roy's book "The God of Small Things". In the book Margaret Kochamma tells the following joke to Chacko in Oxford, England, where the two meet: A man had twin sons… Pete and Stuart. Pete was an Optimist and Stuart was a Pessimist... On their thirteenth birthday their father gave Stuart – the Pessimist - an expensive watch, a carpentry set and a bicycle… And Pete's - the Optimist's - room he filled with horse dung... When Stuart opened his present he grumbled all morning. He hadn't wanted a carpentry set, he didn't like the watch and the bicycle had the wrong kind of tyres… When the father went to Pete's - the Optimist's - room, he couldn't see Pete, but he could hear the sound of frantic shoveling and heavy breathing. Horse dung was flying all over the room… "What in heaven's name are you doing?" the father shouted to Pete…

A voice came from deep inside the dung. "Well, Father," Pete said, "if there's so much shit around, there has to be a pony somewhere." (*The God of Small Things,* Flamingo, London 1997, p.243).

Francis Bacon (left) and Arundhati Roy

Stanley Mandelstam, on the occasion of the 1991 Dirac Medal Award Ceremony

26 How to measure progress?

The next item on Gina's list was Smolin's view of Maldacena's conjectures. This was a very mysterious topic! Before embarking on that topic, however, Gina wanted to make sure she understood the gist of Smolin's criticism, and so wrote:

> My basic understanding of Lee's book is that his argument asserts the failure of string theory to reach any progress in the basic questions of physics in the last three decades. If Lee's argument regarding string theory (as a scientific theory) is just that string theory has not succeeded **yet**, or that there are some concerns about specific results, as well as about the whole endeavor, then this is a different matter. I do not think what Lee writes in his book supports such a mild interpretation of his stance.

Lee referred to Gina's question regarding his overall argument. No, he is not claiming that string theory has failed. This was most interesting!

> To Gina
>
> You wrote: "If Lee's argument regarding string theory (as a scientific theory) is just that string theory has not succeeded **yet**, or that there are some concerns about specific results, as well as about the whole endeavor, then this is a different matter." Yes, of course, this is the basic premise of my argument.
>
> The conclusion I draw from this is that since there are other very well-motivated directions of which the same could be said, string theory should be considered and treated as one of a number of important approaches to quantum gravity and unification that are

pursued. I also argue that it would be best for the progress of science if all those who work on these different approaches consider themselves as a single research community, within which we try to mix people doing different things, as well as vary our own research interests, because presently different questions are best approached with different approaches.

My major argument is not "against" string theory; it is against the idea that string theory is "the only game in town" or that it is the "modern paradigm," suggesting that it alone deserves intensive investigation within a community specializing in working on it alone.

Interesting, thought Gina, but she was not convinced that Lee's strong rhetoric, of "the rise of string theory and the fall of science" supports such a mild interpretation of his stance. Next, Gina related to another general issue central to the debate. How do we measure progress?

One issue that is relevant to all the previous items and to the whole discussion is the matter of "how to measure progress" and "how to measure partial success." This is an important question here, as in other areas of science (and life). If we are too quick to declare progress, we may end up nowhere, or with self-illusion and self-deception. If we are too slow, we may not even get started. I suggest keeping in mind the issue of "how to measure progress" for the discussion of other points of disagreement.

In response, Lee expressed his view on the question of evaluating the partial success of scientific theories.

To Gina,

I agree that the key question is what to do about research programs where we have presently partial but not total success. I suspect that what is confusing you is that my view of what is wise in these circumstances does have a tension in it. I do think that we must at all times be aware of the high risk these research programs entail, and therefore be prepared that partial success may be the best that we can accomplish. We live in an unfortunate period in which there are many research programs in theoretical physics that, in spite of being well motivated, achieve only some partial success but never go beyond that. So I believe that individuals should, of course, continue to develop programs that are partially successful, but that we should do everything we can to leave room for someone to invent a new program that may be more successful.

Thanks,

Lee

27 Maldacena

"My own reaction to this was basically: 'there ought to be a different way around this' ", Roger Penrose [explaining his reaction to high dimensions required by string theory], The Road to Reality.

"You have this solid octopus and you hit it on its head and hear (wonderful) music, say, a Mozart sonata..." Gina, [trying to explain the ADS/CFT correspondence and the holography principle without understanding it,] Dec 2, 2006.

Juan Maldacena's conjectures and results are considered as a crowning achievement of string theory. Yet the content of these results, which are referred to as "the ADS/CFT correspondence" and the "holography principle," is very difficult to understand. (If this helps a little, ADF stands for "anti de-Sitter" and CFT stands for "conformal field theory.") Lee Smolin tried to find some weaknesses in the picture portrayed by Maldacena's work and the huge subsequent literature. As a scientific "devil's advocacy," such attempts (with merit) can be of interest. But to make these ideas part of a case against string theory was mind boggling. Gina could not understand Lee's overall argument regarding Maldacena's work.

160 - Gina Dec 2nd, 2006 at 4:42 pm

> Dear Lee, Thank you for your reply and good spirit. I will elaborate on my 4th and 5th comments that are both related to Maldacena's conjecture, and will take the simpler and more important 5th point first. (If you find it fit to reply please indicate the number of the comment.)
>
> 5. The substantial reliance of Lee's case on how to interpret Maldacena's conjecture is by itself damaging to his case.

The fact that such a major part of Lee's argument deals with the validity and interpretation of Maldacena's conjecture speaks for the viability of string theory. Maldacena's conjecture is not a 19th century conjecture that defied proofs for centuries, but something that happened less than 10 years ago. Lee's argument against string theory without Maldacena's works around would have been stronger.

The issue of proofs and precise interpretation of Maldacena's conjecture is secondary to the issue of whether string theory is viable enough to produce developments of such magnitude in the future. I do not see any reason why we cannot expect such further development.

Will there soon be some new developments in string theory of the same magnitude as Maldacena's works (or bigger)? Nobody could know. And Gina considered this uncertainty to be an appealing feature of science. It took some more time to list the specific points regarding Maldacena's conjecture.

161 - Gina Dec 3rd, 2006 at 10:19 am

4. The interpretation of Maldacena's conjecture is biased. Lee's interpretation is not sufficiently positive towards the ST endeavor.

In addition to a very nice and non technical description of the conjectures and their relations to earlier physics discussions, Lee's assertions on this matter can be summarized as follows:

1) Even if true, the Maldacena conjecture does not quite bring the theory to where I would like it to be.

2) There is only strong support to a weak form of the conjecture, with much less consequences.

3) These are not conjectures in the strict sense of mathematics, as the objects themselves are not mathematically defined.

4) Because of the excessive optimism, Maldacena's conjectures are assumed to be true (in their strongest form) and very few people are working towards a proof.

I see no merit in points 1 and 2. Regarding the third point, there is nothing special about this case. The third point reflects a certain “divorce” between mathematics and physics which occurred decades ago, and resulted from the earlier spectacular successes of physics. (Which shows you that successes are not devoid of problems.) The excessive optimism of point 4 is not a sociological issue but rather a bold scientific methodology applied in string theory for better or for worse.

Gina added a remark about how she perceives, in her laywoman eyes, Maldacena's discoveries.

Following Peter Woit’s criticism of my first item, I did try to learn more about the ADS/CFT issue and looked at Penrose’s book, "The Road to Reality". I did not make much progress but this was a delightful experience. Penrose discusses the technical details of string theory with affection and even enthusiasm, like talking about a beloved child, even if he does not subscribe to the theory at all. His skepticism is also charming, and he is also skeptical about his own skepticism. For example, look at what Penrose says about the high dimension idea - “My own reaction to this was basically: ‘there ought to be a different way around this’” - how simple, how nice! You do not need more, and it is such a good line for many purposes - “There ought to be a different way around this”. My heart goes to Roger! (And according to the pictures in Penrose’s, book the universe is simply a sort of amoeba or an octopus.)

Reading Penrose on top of what I already learned from Lee and other popular sources, I developed the following mental picture on this ADS/CFT: You have this solid Octopus and you hit it on its head and hear (wonderful) music, say a Mozart sonata. Now, you take just the very thin external skin (called the boundary) and you hit that. And you hear precisely the same wonderful music. Precisely the same!

Juan Maldacena

Here is Roger Penrose next to a famous tiling he discovered

169 - Lee Smolin Dec 7th, 2006 at 6:08 am

Hi Everyone

Sorry to have fallen behind. Here are some replies to recent posts: With respect to Maldacena, I don't want to repeat the arguments in my book and papers on this, but I want to make it clear that I believe that at whatever level, it is true the ADS/CFT conjecture is a major result in mathematical physics. But I do not think it is a good thing to treat the strong form of the conjecture as essentially true, given that it posits the existence of an isomorphism between two structures which have not even been written down.

28 The overall evaluation of String Theory

It was time, Gina thought, to address Smolin's overall scientific evaluation of string theory. In his book, Lee lists the five main problems in physics, claims that no real progress has been made in three decades and regards it as a failure of string theory and, more generally, of the prevalent approaches to physics in the sixties and seventies, which he considers to be no longer adequate.

Gina Dec 10th, 2006 at 7:24 am

> Dear Lee, Thanks for your interesting comments. Let me try to move ahead with my critique.
>
> 1. Asserting that there has been no progress on the five major problems of Chapter 1 is incorrect or at least very problematic.
>
> Perhaps Lee's assertion can best be seen through Roger Penrose's words on the back cover of the book:
>
> "… so his [Smolin's] claim that string theory is responsible for the lack of real progress in fundamental physics for the past quarter century carries considerable weight."
>
> Penrose attributes an even stronger statement to Lee: that string theory is not only responsible for its own alleged failure but also for the lack of progress in other areas of physics!
>
> In my opinion the claim of "no real progress" should be rejected.
>
> The first question is, of course, what "real progress" means.
>
> Here is an example: In the early 80's Richard Hamilton proposed a program to solve the Poincaré conjecture. Looking back at (say) 1992, did Hamilton's program represent at that time "real progress" towards the solution of the Poincaré conjecture?

In hindsight, the answer is of course yes, Perelman's 2002 proof of the Poincaré conjecture relies on Hamilton's program.

I have chosen the year 1992 because it was a decade *before* the solution of the Poincaré conjecture, and I was told that at that time most experts were quite pessimistic. Of course, some people thought that the Poincaré conjecture may very well be false. Even if true, it was not clear whether Hamilton's program was a good avenue towards a solution, and even if it were, whether the technical problems could be overcome.

My definition for "real progress" requires that the theory in question have a substantial chance for making, or contributing to a definite progress. By this rule, I would have regarded in 1992 "Hamilton program" as representing "real progress" (regardless of the ensuing turn of events), just as string theory should be regarded today.

We previously discussed the interesting question of how to test the partial progress of a theory. Here are some possibilities, none of which is ideal.

a) To try to evaluate the likelihood that the theory will achieve its goals.

b) To compare the likelihood of the theory achieving its goals to current alternatives.

c) To evaluate the contributions of the theory, CONDITIONED on the assumption that it will fail to achieve its central goal.

Criterions a) and b) measure progress in terms of promise, they are subjective and can be influenced by hypes and by crusades á la Woit. Criterion c) is indirect and is based on a gloomy worst-case scenario. In terms of the latter, I am not aware of any current tentative theory in any of the sciences that is more successful than string theory.

2. Interpretation of the alleged lack of real progress in answering the five major problems of physics as a definite failure is wrong.

Large parts of theoretical physics are becoming more remote from empirics and more mathematical in nature. To a large extent, theories are tested by their internal integrity and consistency and not by experiments. The time scales we are familiar with in the area of mathematics may be more appropriate for appreciating progress. When it comes to major problems in mathematics, three decades are just not long enough.

Five hard pieces: Smolin's list of five great theoretical physics problems:

1) The problem of quantum gravity: Combine general relativity and quantum theory into a single theory that can claim to be the complete theory of nature.
2) Resolve the problems in the foundations of quantum mechanics.
3) Determine whether or not the various particles and forces can be unified in a theory that explains them all as a manifestation of a single, fundamental entity.
4) Explain how the values of the free constants in the standard model of particle physics are chosen in nature.
5) Explain dark matter and dark energy.

29 Pluralism and controversies

Important in Lee's argument was the claim that plurality of ideas and methods are not encouraged in high energy physics. Gina's first point regarding pluralism was that the issue of pluralism should be studied from a wider perspective.

Gina Dec 10th, 2006 at 7:37 am

> There are many scientific areas where there is a dominant theory and a number of competing approaches. I do not think that what we observed in other areas is much different than what is seen here.
>
> Of course, pluralism is an interesting issue in more general contexts: when it should be encouraged, when it should be tolerated, and when it should be suppressed.
>
> Her next point was about physics
>
> For somebody looking from the outside, like myself, the unusual thing about string theory is not that we do not see even more revolutionary theories, like those suggested by Lee, but that string theory became so dominant in high energy physics at the expense of more traditional approaches.
>
> Isn't there too much emphasis on fundamental problems? And not enough effort on identifying interesting "second line" problems, based on progress already achieved in fundamental problems? After all, foundations are needed as the basis for full fledged constructions, not just for their own sake.

Gina had one question for Clifford on this matter:

Gina Dec 12th, 2006 at 9:53 am

> Dear Clifford,

You said: "…string theory seems to be the most well-developed of the ideas out there, and there is a ton more to do." After three decades of extensive research, string theory is undoubtedly the most developed idea out there. Do you think a fresh, yet-to-be-developed alternative may have a chance to be seriously considered?

Clifford Dec 12th, 2006 at 9:59 am

Yes.

-cvj

Controversies

Controversies and debates in and around science – between researchers within the same discipline (for instance among string theorists), between competing theories, between competing fields, and between accepted scientific viewpoints and viewpoints rooted outside science – are common. Is there global warming and is it caused by high emissions of CO_2 by humans? Or is global warming perhaps a myth, or rather an established fact caused by changes in the sun's radiation, which has little to do with us? Is quantum physics correct? Can quantum computers, which have superior computation power that can crack the codes used for most commercial communication, be built? Are the teachings regarding free-market economy scientifically based? What is rationality? What is the weight of psychology in understanding economics? What is the value of mathematical tools in the social sciences? What are the limits of artificial intelligence?

What is the origin of the Scrolls of Qumran (the Dead Sea scrolls)? Were they written by a sectarian group living in a village close to the caves where they were discovered, as the dominant theory asserts? Magen Broshi, a senior Jerusalem-based archeologist who subscribes to the central theory regarding the scrolls, said once in a public lecture: “There are at least twelve theories regarding the origins of the Qumran scrolls. These theories are conflicting, which means that eleven of them are wrong; let me tell you which is the correct one.” A voice came from the audience: “At least eleven!”, which was followed by an immediate reply from another audience member: “You must be a logician!”

Is the theory of evolution correct? And is “intelligence design” a serious scientific alternative? Is there a hidden code in the Bible that can reveal details about the present and can it be used to predict the future? Are we witnessing the “end of science”? The “end of history? The “end of civilization”? A major clash between civilizations? Are the claims that the HIV virus is not the cause of AIDS of any merit? Do highly diluted homeopathic medications have any real effect? And is the “Mozart effect,” the claim that – unlike any other similar activity – listening to Mozart’s music before an exam will greatly contribute to its successful outcome, correct?

30 High risk endeavors for the young

OK, thought Gina, we can move to areas in Lee's case which do not require technical understanding of physics and mathematics. Lee advocated in his book that scientists should be encouraged to attack fundamental problems early in their career. Gina disagreed.

167 - Gina Dec 5th, 2006 at 5:24 am

> 12. Contrary to Lee's suggestion, young scientists should not be encouraged to attack fundamental physics problems head on.
>
> Here are three reasons. The first is the main one.
>
> 1) They will fail
>
> Young scientists should learn the taste of failure but they should also learn the taste of success. And they have to be able to identify success: real and definite success. This you do not learn by attacking fundamental problems head-on or by engaging in very high risk projects.
>
> 2) Thinking "big" is the natural tendency
>
> Everybody would prefer to find a substitute to quantum mechanics rather than compute a complicated integral. Going "big" is a natural tendency of scientists - especially inexperienced ones. In science, as in surfing, or skiing, or playing the piano, you first have to learn to suppress the natural tendencies in order to do things right. (And then you can try to express them again.)
>
> Young scientists should experience the great difficulty of getting a small problem right and the great satisfaction and value of craftsmanship.
>
> 3) The fractal nature of science

While the danger of looking too much at the big picture is that you will see nothing, in science it sometimes happens that you look at your small little problem and discover something which affects the whole picture. A film located in the wrong place at the wrong time, which gets ruined, can turn physics on its head. Innocent experiments in iterating simple functions on a hand calculator may have a tremendous effect. Young (and old) scientists should be prepared that when such an opportunity occurs - they should take it (and they should also be prepared for an unpleasant crash).

"On the other hand," remarked Gina's friend, Oded, "the good scientist will know about the big picture and will keep it in mind while working on her little problem. Moreover, you must seriously think about the big picture from time to time in order to internalize it."

Gina was very happy with the many interesting topics involved in the discussion and wanted to share her excitement with others.

168 - gina Dec 6th, 2006 at 3:27 pm

Let me interrupt the flow of elaborations on my points (5 down to 11 to go,) with a few remarks. The first is obvious. Lee's book contains many nice, interesting and even moving parts and I am specifically trying to attack some of his arguments. Of course, the physics descriptions are very interesting. Several far-reaching ideas about physics are described and, while not remotely in a position to judge them, I am in a position to appreciate them and be impressed.

Lee also presents a bold and far-reaching view of how science should be practiced which, based on Kuhn's philosophy of science, can be regarded as a practical road-map. (Kuhn gave a prominent role to scientific revolutions.) I will try to argue with this approach but at the same time I find it a beautiful line of thought.

Look what we have here beside physics and mathematics: The issues of pluralism, measures for progress, optimism, and risk-taking, especially in science; philosophy of science - scientific revolutions and scientific conservatism; incentives for progress in science, sociology of science - scientific communities, power and influence in academia. Great stuff!

Gina forgot to mention skepticism, arrogance, tolerance, beauty in science, debates and controversies, the role of analogies, and quite a few more issues. She was truly excited by the many interesting facets of the story. Gina remembered that when she was a young child, her father told her that "everything is interesting! Every subject, no matter how marginal and unimportant or boring it may look to you, Gina, when you study it in depth you will find it fascinating! Every single subject, Gina!" And Gina says: "While being a skeptical person, I took this advice without questioning it; maybe because it was from my father."

This time, Gina quite enjoyed Lee's response.

170 - Lee Smolin Dec 7th, 2006 at 6:33 am

Dear Gina, with respect to "young scientists should not be encouraged to attack fundamental physics problems head on", I agree with the dangers you point out. And I absolutely agree that the training of scientists must include problem solving skills and experience in solving doable problems. But, in spite of everything we do to try to steer young scientists to do incremental work by which they may advance their career, there remain a few who insist on breaking away and inventing and developing original ideas to attack fundamental problems head on. My question is, what do we do about them? In my experience, two things are true:

1) Such people often have a hard time making progress in their careers,

2) In the long run, they contribute a great deal to the progress of science.

To mention some examples, they invent string theory, or the idea of quantum computation, or spin foam models, they find the correct physical interpretation of general relativity or invent new approaches to the interpretation of quantum theory (the many worlds interpretation was proposed in a PhD thesis by someone who never got an academic position in spite of the theory's great influence.)

Still, given that they are not many in number, and are also easy to identify, I think we can do much better for them, and in doing so speed the progress of science.

In his book Lee refers to scientists who study fundamental big problems from new angles as "seers", contrasting them with "normal" scientists, who are referred to as "craftspeople". Gina suggested calling the former group "dreamers", and remarked that while we already have many dreams and many dreamers the difficulty is to find some "spoilers" who will look very carefully at these wild ideas, study them, and explain to those who have them why they cannot work. Where can one find these "spoilers"? she wondered.

At a later stage Lee changed the terminology he used and referred to "seers" as "valley crossers" and to "craftspeople" as "hill climbers". Gina liked these new names much better. She always imagined scientists of all kind as some sort of mountain climbers...

Röntgen and Feigenbaum

"A film located in the wrong place at the wrong time, which gets ruined, can turn the whole of physics on its head," wrote Gina, and referred to the accidental discovery of Röntgen Rays by Konard Röntgen. The famous Mathematician Paul Erdős liked to tell the following story: A few years before Röntgen's famous experiment, another prominent scientist named William Crookes observed that leaving a photosensitive film near the cathode-ray-tube causes damage to the film: it becomes exposed. He concluded that "Nobody should leave films near the cathode-ray-tube." Röntgen observed the same phenomenon a few years later and realized the significance of his discovery, that indeed changed physics and had a great impact on medicine.

Gina's reference to "Innocent experiments in iterating simple functions on a hand calculator may have immense effect," is a somewhat inaccurate reference to the work of Mitchel Feigenbaum on chaos. Computations with a small HP-65 computer led Feigenbaum to discover in 1975 universal properties of chaotic dynamics and fractals.

31 Scientists and mountain climbers

"… with a normal job, there is even a chance you will pay attention to your wife and children when they talk to you, rather than day dream about physics", attributed to Lee Smolin, in Gina's story

Gina thought about the young scientists and compared them to mountain climbers, with whom they share the sense of beauty evoked by undiscovered territories, by the loneliness, the prolonged difficulties, the need for stubbornness and by the need for flexibility. There are dangers in both professions; Crevasses, for instance, are a prominent hazard for mountain climbers. These, according to Wikipedia, are the slits or deep chasms formed in the substance of a glacier as it passes over an uneven bed. They may be open or hidden. In the lower part of a glacier the crevasses are open. Above the snow-line they are frequently hidden by arched-over accumulations of winter snow. The detection of hidden crevasses requires care and experience. Scientists' crevasses are not deadly but they exist, Gina thought, as does the danger of getting lost and of losing one's skills. Gina's objection to encouraging young scientists to take high risk endeavors was part of her view that they should not be encouraged *at all*. Here is how Gina fantasized a meeting between Lee Smolin and a young Ph. D. candidate called Jeremy, would transpire.

Professor Lee Smolin: Please sit down. (Looks at Jeremy's file) I understand you graduated last year from Harvard University… straight A's, … hmm junior thesis on the paper: "On the relationship between quantum and thermal fluctuation," What brings you here, Jeremy?

Jeremy: I would like to write a Ph D thesis under your supervision.

LS: (looks at the file, this is the best file he has seen in years...): I see. Let me tell you something right away Jeremy. Success as an undergraduate does not always mean success in research, but looking at your file I am willing to take the risk and say that I think you will be able to write a good Ph D thesis.

J; Thank you.

LS: You must know that an academic job afterwards is not guaranteed. It depends on your success in research but *also* on many other factors.

J: I am aware of that fact Professor Smolin.

LS: (after a long pause). Young man, let me ask you a question. You did brilliantly at Harvard and you can *really* succeed in life. You can get out of here in one year with a Master's degree and then go and become a successful man. If you want to be rich, you can go into business or high-tech, and if you want to build things you can go into engineering, and if you want to help people you can go to med school. And wherever you go, you will be surrounded by, and working with *real people*, not only with formulas and computers. And, you know what, Jeremy, with a normal job, there is even a chance you will pay attention to your wife and children when they talk to you, rather than day dream about physics…

J: But I want …

LS (interrupts): Look, don't give me an answer right away. Think about it. Take, two three weeks to think about it, Jeremy, and I will be happy to continue our conversation then.

Lee of course knows that Jeremy will most likely come back, and will complete an excellent Ph D thesis. And then at those times when he is cold, wet and lonely on these mountains he will remember that going to graduate school was not something he was dragged into, but was truly his choice.

32 How to treat fantastic claims by great people

Anatoli Boukreev, Gina remembered, was the greatest mountain climber in the world. An article in a mountain climbing journal challenged his greatness and attributed his success to his practice of climbing up the easy side of the mountains when the weather was good. Boukreev ascended the next mountain from the difficult side. In the winter of 1997, Boukreev was attempting to climb the south face of Annapurna I (8,078 m) and was hit by bad weather. He did not survive.

The next topic of discussion was about the reaction of the scientific community to bold and speculative theories and scientific suggestions. In his book, Lee Smolin describes how revolutionary scientific claims are dismissed by the community even when they are proposed by great scientists. As an example, Lee describes the dismissing approach of most physicists towards the recent ideas of Gerard 't Hooft, a Nobel Laureate and one of the greatest physicist of our time.

288 - Gina Dec 16th, 2006 at 11:38 pm

Let me continue with the elaborated critique of Lee's book.

13. The negatively depicted reactions to sporadic bold theories and ideas are, in fact, very reasonable. Scientists' current attitude is essentially correct.

The attitude of the high-energy physics community towards very revolutionary scientific ideas and theories, even when these are proposed by great scientists, is portrayed negatively in Lee's book. I beg to disagree:

The basic principle in scientific and academic activity (unlike other areas) is that everybody, no matter how famous, has the burden of convincing the community from scratch about the merits of a new theory or a new idea that she or he presents. Everybody, Smolin, 't Hooft, Witten, Penrose, Chomsky, … Everybody.

Amazing possibilities

Understanding our fundamental limitations is among the most important contributions of science and of mathematics. At the same time, various fundamental limitations stated by many great minds turned out to be wrong, sometimes rather quickly.

There are quite a few cases where things that were considered to be impossible turned out to be possible. Immanuel Kant claimed: "No finite Reason can hope to understand the production of even a blade of grass by mere mechanical causes." This quote is from the *Critique of Judgment* (1790). Elsewhere Kant wrote: "It is absurd to hope that another Newton will arise in the future who shall make comprehensible by us the production of a blade of grass according to natural laws which no design has ordered."

Auguste Comte claimed: "Of all objects, the planets are those which appear to us under the least varied aspect. We see how we may determine their forms, their distances, their bulk, and their motions, but we can never know anything of their chemical or mineralogical structure; and, much less, that of organized beings living on their surface ..." (*The Positive Philosophy*, Book II, Chapter 1 (1842)).

Spectroscopy was developed by Gustav Kirchhoff in the 1840s, and the first spectroscopic analysis of the sun appeared about ten years later, less than 20 years after Comte's statement.

A slightly different example relates to the philosopher Wittgenstein. As usual he is more cryptic. He gives an example of a person making claims that seem crazy to us. Writing before expeditions to the moon became a reality, Wittgenstein speaks of a person who claims that "men sometimes visit the moon." The following excerpt is taken from “On Certainty.”

“What we believe depends upon what we learn. We all believe that it isn’t possible to get to the moon; but there might be people who believe that it is possible and that it sometimes happens. We say: these people do not know a lot that we know. And, let them be never so sure of their belief, they are wrong and we know it.”

There are famous quotations of predictions made by Lord Kelvin. Kelvin did not believe that heavier-than-air flying machines were possible and he regarded X-rays as a hoax. Kelvin’s ingenuity was manifested even in cases where his overall predictions were wrong. He gave a lecture on the state of physics at the turn of the twentieth century, and – not unlike Hilbert's famous lectures in mathematics – claimed that physics was nearly complete and all problems would soon be settled. He mentioned, however, "two clouds on the horizon," the unexpected behavior of ether in the Michelson–Morley experiment and the problem of the spectrum of the black body radiation. His genius as a physicist was manifested by the fact that of all the scores of open problems in physics present at the time (as there always are), he pinpointed the two problems that subsequently led to revolutions: the ether problem led to relativity, and black body radiation to quantum theory.

[Editor's note: the items and citations of Kant, Comte, Kelvin, and Wittgenstein were contributed by Itamar Pitowsky, a philosophy professor at the Hebrew University of Jerusalem and my former classmate there. Itamar also drew my attention, in the summer of 2006, to the weblog discussions regarding string theory, and especially to Peter Woit’s weblog.]

33 Sociology of universities

Old people are simply young people that have turned old along the years,

Gina, Dec 5, 2006.

Sociology was a big issue in Lee's book and Gina tried to understand his point of view.

166 – Gina Dec 5th, 2006 at 5:15 am

Let us move to general matters of universities and academia.

15. Some natural, often welcome social processes in academia (regarding academic judgment and decision making) are depicted negatively. This is a mistake.

Here, I refer to Chapter 19 in Lee's book that deals with the sociology of universities and academic life.

Let me quote a few sentences and add some brief comments:

1. "The idea of changing the way science is done in universities will no doubt appeal to some, while horrifying others. But it's probably in no danger of happening. To understand why, we need to inspect the dark underbelly of academic life. Because as the sociologists tell us, it is not just about wisdom, it is about power: who has it and how it is used."

I find it impossible, even after reading the whole chapter, to understand what is said in these sentences which open the chapter and which draw a dark and gloomy picture of universities, of the way science is done in universities, and their inability to change.

2. "In the academic world, with few exceptions, the people who evaluate you are older than you and more powerful."

This may seem bad at first, but it is actually not the case. Overall, I think that it is very reasonable that older people should have more influence.

I do not see anything wrong with old people. My perception is that older people are simply young people that have turned older along the years.

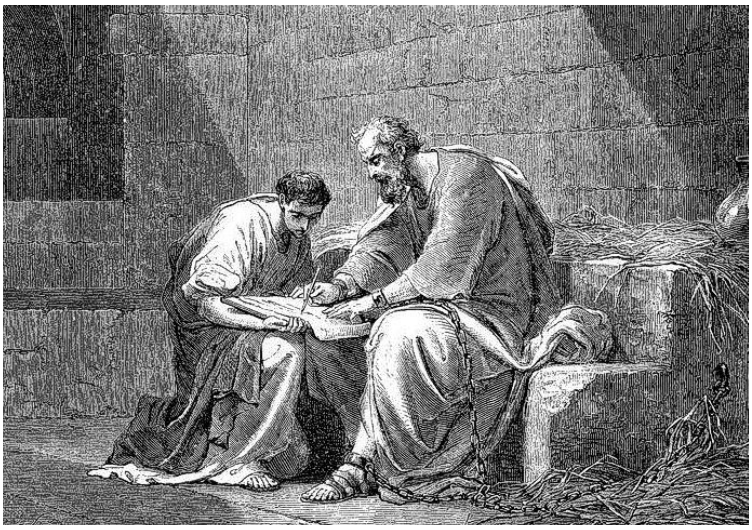
Mathematician Daniel J. Kleitman in 1967 (left) and around 2000 (right).

Curiously, part of the reason for this observation by Lee is not sociological at all, but purely statistical. If you are young, the people who evaluate you tend to be older, and if you are old the people who evaluate you tend to be younger.

3. "... a process of enforced consensus in which older scientists ensure that younger scientists follow their directions"

When there is a need to make collective decisions, the notions of power and influence automatically emerge. It is overall positive rather than negative that the amount of influence is positively correlated with age and with prominence. I am not aware of any mechanism which will lead to making the optimal decisions or which can eliminate the possibility of abuse of power. If Lee has ideas in this direction it would be great to hear them.

4. "A professor will shamelessly write letters slanted towards his or her own students"



Yes, many people do various things (usually fully legitimate but sometimes a little dubious,) to promote their points of view and many are biased towards their own opinions, style, direction of research, and even students. This is a positive rather than a negative fact of life.

Reading a book is always an emotional experience and not just an intellectual one. Lee's discussion in Chapter 19 conveys an obvious mood of disappointment, and, to some extent, despair of universities, of the way they do science, and of their ability, or lack thereof, to change.

In his reply, Lee explained his position starting with his ideas on mechanisms for collective decisions.

To Gina,

I made a number of suggestions in my Physics Today article as well as in the book. But these are standard issues that concern business, as well as academia. My impression is that we in academic science are behind the curve in several of them. For example, in investment banking, when considering the promotion of X, they tell me they do a "360", which means they seek the views of all who work with her, at both less and more senior levels. In law firms, banks, newspapers, and other businesses people who hire and manage other people are required to go through training aimed at combating these problems. In investment banks and successfully managed endowments they deliberately choose fund managers with a variety of approaches and views of the market, in order to guarantee a diversified portfolio.

Gina was especially skeptical about taking investment banks as role models.

Lee continued to describe different forms of collective decisions in universities and concluded with his belief that matters can be changed.

My point is that we should never regard the “sociology” of a field as something we just have to live with; it is the result of concrete choices about the practices that guide hiring and promotion. So if people perceive problems with the sociology of their field, rather than complaining about it over lunch, as one commonly hears, one should work within one's department or university to change the practices. One might even take a comparative approach and consider the different practices around the world as experiments, whose results are to be evaluated.

Thanks,

Lee

Power and collective decisions

"It is not about wisdom but about power," said Lee. What is the wisdom regarding power when it comes to collective decisions like those in university committees or in general elections? Can we measure power? Can we plan a social mechanism that will be based on evenly spreading power and will lead to satisfying collective decisions?

In the 1950s and 60s, two important mathematically-based notions of power were introduced. The Banzhaf power index for a member in a committee or a voter in an election was based on the probability that this member has the decisive vote after the other members have cast their votes. The Banzhaf power index is related to the "Penrose method," proposed by Lionel Penrose (Roger Penrose's father), for determining the number of delegates for every county in a world-parliament, with the objective of giving each county power proportional to its population. The power index proposed by Shapley and Shubik derives from a more general concept in game theory and is based on a set of axioms. It can also be based on the probabilities of members or voters being decisive.

Even in a context where these indices of power do not seem appropriate, the principle of relating power to decisiveness can still be useful.

The idea of using the language of mathematics to describe and study issues of "political science" goes back to the French mathematician, philosopher, and early supporter of democracy and feminism, Marie Jean Antoine Nicolas de Caritat, marquis de Condorcet (1743-1794). Condorcet is famous for his "paradox" asserting that in an election with three candidates – Alice, Bob, and Carol – it is possible for a majority of voters to prefer Alice to Bob, for another majority of voters to prefer Bob to Carol, and for yet another majority of voters to prefer Carol to Alice.

In 1951 Kenneth Arrow found a far-reaching extension of Condorcet's paradox, and showed that under some simple conditions, the only voting method where this paradox is avoided is dictatorship: the outcome is always determined by the same individual. The interpretations of these particular results, as well as of issues regarding mathematical modeling in the social sciences, are subject to intense controversy.

34 The string theory community

The next item, Lee's description of the string theory community, was perhaps the most delicate one. Gina gave considerable thought as to how to express her criticism in the correct manner.

206 - Gina Dec 14th, 2006 at 12:33 am

> 9. Many claims, stories and quotes against the string theory community are not really damaging to the ST community and bringing them weakens the quality of Lee's argument.
>
> Let's look at some specific claims (mainly from Chapter 16 of Smolin's book) against the string theory community:
>
> Lee talks a lot about "group thinking" and about the need "to fight" group thinking. It is not clear what precisely he means. Research based on large collective pools of ideas and results is a very positive phenomenon which characterizes string theory and other prominent research areas. When exactly does this blessed and crucial practice become the negative "group thinking"?
>
> I especially disliked the analogy drawn by Lee between "group think" in string theory and "group think" that brought about NASA's failure to prevent the Challenger disaster, and "group think" that led to the current war in Iraq.
>
> There is also a related claim that string theory "works by fashions," so a large amount of efforts is geared towards directions which look promising. Again, I see nothing wrong with it. It reflects string theory's methodology to try to send long and thin in-depth sensors and get some picture of what the emergent big theory looks like, while neglecting or delaying other directions due to technical difficulties.

> The repeated claim of "arrogance" does not make much sense either, and it is not really supported by quoting a specific physicist (among thousands of physicists) who made a similar claim.
>
> Overall, the feeling I got reading the book was that Lee's description of the string theory community is **fundamentally unfair** and incorrect.

Groupthink and Collective Intelligence

Irving Janis, who wrote extensively about groupthink, defined it as: "A mode of thinking that people engage in when they are deeply involved in a cohesive in-group, when the members' strivings for unanimity override their motivation to realistically appraise alternative courses of action." The term itself was coined, according to Wikipedia, by William Whyte in 1952, and resembles words like "doublethink" from Orwell's book *1984*. Two examples often mentioned in the context of groupthink are those of NASA prior to the Challenger disaster, and the decision to invade the Bay of Pigs.

In contrast, an old idea that can be traced back to the French philosopher and mathematician Condorcet is that collective decisions can lead to better outcomes and thus have some effect of "fault tolerance." A mathematical theorem of Condorcet called "Condorcet's Jury theorem" demonstrates this effect under some assumptions. Modern jargon refers to "collective intelligence" as a superior form of intelligence that emerges from the collaboration and the competition of many individuals.

The notion of groupthink itself has come under criticism, as have some specific claims regarding groupthink. Regarding successful decisions as the outcome of "collective wisdom" and failed decisions as the outcome of "groupthink" does not seem to represent a genuine understanding of collective decision-making, and outcomes of empirical studies on some hypotheses concerning groupthink are inconclusive. In collective decisions there is a tension between the desire to take various different opinions into account and the ability to reach a coherent collective decision.

Even the claim that groupthink was a major ingredient in the Challenger disaster is quite questionable. It appears that now, as before, there is a substantial probability (a few percent) that a space shuttle flight disaster will occur, just as there is a substantial probability for disaster when climbing to the peak of Mount Everest.

35 The finiteness-of-string-theory war

The discussion on Mandelstam's work (Ch. 28) ignited an extensive debate regarding the results achieved by Mandelstam himself as well as by Atick, Moore and Sen (AMS), and by d 'Hoker and Phong, which all assert the "finiteness of string theory". Gina did not consider the weblog environment as suitable for a technical discussion on this matter, and she was rather interested to know (but did not get an answer) whether Mandelstam was related to the famous Russian poet Osip Mandelstam. The debate spanned over 100 comments for several weeks with a lot of heated hair splitting. The debate also spilled over to other blogs. Lubos Motl gave an interesting account of the finiteness issue on his blog, and offered Jacques Distler a "medal for patience". Lubos' post was characteristic of his bold approach towards string theory and mathematics. His overall view was that, with the progress and triumph of string theory, the mathematical difficulties in mathematical physics (and perhaps in all mathematics) would be resolved as well. Gina used the opportunity for a quick interview with Jacques.

245 - Gina Dec 14th, 2006 at 11:13 pm and **253 - Jacques Distler** Dec 15th, 2006 at 7:37 am

> [**Gina**] Dear Jacques, congratulations on the new medal for patience. Let me try and stretch your patience a little and ask a few questions:
>
> 1. Is it correct to say that neither the work of AMS, nor even that of d 'Hoker and Phong can be regarded as solid proof in the mathematical sense of the word.
>
> [**Jacques:**] I don't think physicists strive for (nor should they) a level of rigour characteristic of Mathematicians. Nevertheless, with a finite (but sometimes very large) amount of effort, it is usually the case that a "Physicist's Proof" can be turned into one that would satisfy a Mathematician.

[Gina] 2. According to your explanation, the new proof is "constructive", while AMS proof is not. Can you explain in non-technical terms what the difference is, and why it is of importance to find constructive proofs?

[Jaques:] A non-constructive proof says: even though we cannot write down the answer explicitly, we *can* prove that it has properties A, B and C. And this is sufficient to show that desired outcomes X, Y and Z are achieved.

A constructive proof says: Look! We can write down the answer explicitly and you can see, by inspecting it, that it has the desired properties.

Constructive proofs are always preferable in Mathematics, but even *more so* in Physics, where we would like to use the answer to calculate stuff.

[Gina:] 4. Smolin's concerns on this issue were that string theorists were too optimistic to interpret partial evidence (Mandelstam's) as definite. Do you agree with Smolin at least on this point?

[Jacques:] I *really* don't understand Smolin's account. Mandelstam's announcements generated a lot of excitement, but, when he never wrote a paper about it, everyone figured that he had found a flaw in his method.

[Gina:] 5. What is your take concerning this "divorce" of physics from mathematics. (I learned this term from a paper by David Corfield posted on the n-category cafe.) Is it something physicists should worry about at all?

[Jacques:] As to the larger point, there are many things in physics that we "know" are true, because we have amassed a lot of computational evidence to support them, but which have never been formally proven.

It was especially interesting to learn from Jacques about the importance of proofs in physics for providing effective tools of computation.

> Gina was overall quite skeptical when she heard scientists talking about "solutions" and "proofs". Ever since she heard the nice story, that we tell in the next chapter, about Jeffrey and Miles, she asked herself: Are solutions really solutions?

Debates

Debates are fascinating human activities that are a mixture of logic, strategy, and show. Not everybody shares this fascination. The German author Emil Ludwig considered debates to be the death of conversation. Jonathan Swift regarded debates as the worst sort of conversation, and debates portrayed in books, as the worst sort of reading. Public debates pose various interesting dilemmas. A debate between two positions gives an impression of symmetry, and engaging in a debate against an obscure or illegitimate position gives it some legitimacy and emphasis. On the other hand, ignoring obscure or illegitimate positions may also pave the way to getting them public legitimacy or to making them mainstream. A common form of debate is one in which an uninformed decision-maker extracts information from two (or more) informed debaters who hold contradictory positions on a certain issue. Weblog debates are especially interesting, as they allow an unusual amount of interaction between the debaters and the uninformed audience. Common debate practices include ample repetitions, not giving up on seemingly small issues, never admitting a mistake, trying to undermine the professionalism and integrity of an opponent and not just his logic. We can ask ourselves if these debating practices are rational and optimal in terms of influencing the audience. They probably are.

36 A mathematician's solution to the proliferation problem

"Modeling, modeling, modeling. If you want one important notion in this business it is mathematical modeling. The art of modeling, the science of modeling."
Miles, around 2002.

Miles does not look well, thought Jeffrey. Miles was a young mathematician at Oxford. He was working on something called K-theory and Jeffrey could not figure out what it was about. Miles was a good mathematician, at some point he was the youngest reader in the whole kingdom. Jeffrey was used to seeing Miles troubled at times by a math problem and it was difficult to talk to him on such occasions. But this time Miles looked even worse than usual.

"What's on your mind, Miles?" asked Jeffrey.

"Well Jeffrey," answered Miles, "I am deeply troubled by the problem of proliferation."

"Proliferation?" asked Jeffrey "not that I intend to understand, but tell me what proliferation *means* in K-theory?"

"No, no" said Miles "I am talking about the problem of proliferation of nuclear weapons"

"I see," said Jeffrey.

Bad news, thought Jeffrey. Proliferation is a troubling problem indeed, but he was around long enough to realize that getting really upset over it was a bad idea. And from Miles' look and his rather incoherent thoughts on the matter, it all looked like a one way street.

Fourteen months later Jeffrey met Miles again. To his surprise Miles looked completely fine and even quite cheerful. "What's up, Miles?" he asked. Miles got right to the point. "Well, you remember that I was troubled by the problem of proliferation, I spent much of my time in the last months studying this problem. This was a difficult project that led me to many interesting areas."

"But, Miles" asked Jeffrey, "I never thought about this problem as a problem in mathematics. It is a problem about politics, isn't it? Can you come up with a formula in mathematics which has something to do with proliferation?"

"Not a formula", said Miles, "but a model."
"Modeling, modeling, modeling" Miles continued "if you want one important notion in this business it is mathematical modeling. The art of modeling, the science of modeling."

Then he paused,
"Jeffrey", asked Miles "did I mention '**modeling**'?

Jeffrey was used to Miles' strange sense of humor. "Yes, Miles, you did mention modeling."

"Building the relevant mathematical models was the most important part in my project" said Miles.

"But Miles", said Jeffrey, "this whole issue involves a lot of uncertainties. You cannot know how things will turn out".

"You are absolutely right," said Miles. "Probability, probability probability" he continued, "if you want one important notion in this business it is most probably the notion of probability. The art of probability, the science of probability."

Then he paused,
"Jeffrey", asked Miles "did I mention '**probability**'?

"Yes, Miles, you did mention probability," said Jeffrey

"A major part of my project was in the realm of probability and statistics. Without probability I would not have the language to talk about the problem at all."

"But Miles," said Jeffrey "this issue is related to *people*, and how people behave. How can you translate all this into mathematics?"

"Rationality, rationality, rationality" said Miles "if you want one important notion in this business it is Rationality. The art of rationality, the science of rationality."

Then he paused,
"Jeffrey", asked Miles "did I mention '**rationality**'?

"Yes, Miles," said Jeffrey "you did mention rationality."

"A careful analysis taking into account the multiple approaches to this notion of rationality, irrationality and bounded rationality was a major part of my project, Jeffrey."

"But Miles" said Jeffrey, "When you analyze how one person or country behaves, you have to take into account not only what this agent prefers the most, but also how his action affects the behavior of other agents. This looks very complicated."

"What you are talking about, Jeffrey, is called strategy," said Miles, "Strategy, strategy, strategy" Miles continued "if you want one important notion in this business it is mathematical strategy. The art of strategy, the science of strategy."

Then he paused,
"Jeffrey", asked Miles "did I mention '**strategy**'?

"Yes, Miles, you did mention strategy," said Jeffrey.

"Game-theoretic strategic models played an important role in my work, Jeffrey".

"But then Miles, when you model the behavior of the different agents and take into account strategic behavior, what comes next? How do you actually solve the problem?" Jeffrey asked.

"Optimization, optimization, optimization" said Miles, "if you want one important notion in this business it is mathematical optimization. The art of optimization, the science of optimization."

Then he paused,
"Jeffrey", asked Miles "did I mention '**optimization**'?

"Yes, Miles, you did mention optimization," said Jeffrey
"Once modeled, all these issues of rationality and strategy translated into mathematical optimization problems. This was a major part in my project, Jeffrey."

"But tell me Miles," said Jeffrey, "what is the meaning of all these models and mathematics?"

"Interpretation, interpretation, interpretation" said Miles "if you want one important notion in this business it is mathematical interpretation. The art of interpretation, the science of interpretation. "

Then he paused,

"Jeffrey", asked Miles "did I mention '**interpretation**'?

"Yes, Miles, you did mention interpretation," said Jeffrey

"Jeffrey," said Miles "I built probabilistic models and took into consideration matters of strategy and rationality, I set the relevant optimization problems, and, being cautious of misinterpretation and over-interpretation and the effect of noise, I can say that I feel quite comfortable now …"

"Do you have a solution?" interrupted Jeffrey. This all sounds crazy, thought Jeffrey, but many people will be interested in a solution.

"Let me just say," said Miles "that I feel pretty good about my overall approach and the emerging insight, yes, you can call it a solution"

"But what ***is*** the solution?" asked Jeffrey.

"The emerging solution is simple" said Miles "and I can almost say it is elegant," he continued. "Rather than bore you with mathematical formulation and details, let me try to describe my solution in non-technical informal terms. We are all going to perish."

37 Shocking revelations

Clifford, I acknowledge that this was something I should not have done and apologize for it. - Peter Woit Dec 16th, 2006 at 9:25 am

With more and more comments and very heated debates, a discussion between Clifford and the blogger named Christine Dantas caught Gina's attention. Christine Dantas was herself a well known blogger and had a blog devoted to quantum gravity and other topics in physics called "Christine's background independence". Reading the exchange between Christine and Clifford, Gina could not escape the thought that Clifford did not read Smolin's and Woit's books, so she decided to ask him about it.

262 - Gina Dec 15th, 2006 at 2:35 pm

> Dear Clifford,
>
> Did you read the books by Peter Woit and Lee Smolin? Sure, there are various aspects and details of these books that can be criticized, but what you wrote is an oversimplification of these books and, even more than that, of the many interesting issues involved in this discussion.

263 - Clifford Dec 15th, 2006 at 7:03 pm

> Hi Gina,
>
> Please read what I've been saying on this issue -in great detail- in discussions with Smolin and Woit for the last year and a half. I know their positions very well, and, overall, I don't agree with them.
>
> … I have not read their books, but it is well known that those views are also represented in their books. I am free to make my own

independent objections to their publicly expressed views, and I have made them. I did not write any book reviews, which would be wrong, since I have not read their books.

(By the way, contrast that statement with the fact that Woit made up out of whole cloth the public statement/accusation that I negatively reviewed his book for CUP (Cambridge University Press). If he makes up stuff like that at random, how can a non-expert trust what he has to say about the host of other things he says, about research in the field of string theory? You don't make up things to cast a bad light on someone's professional reputation (he also accused me of somehow influencing KC Cole to write her LA Times review!) and put them on a blog. See here.)

Best,

-cvj

Gina felt all along that Peter Woit got it wrong regarding the identity of the Cambridge University Press referee, but she did not expect that the factual matters would be unfolded.

271 - Peter Woit Dec 16th, 2006 at 6:53 am

Clifford,

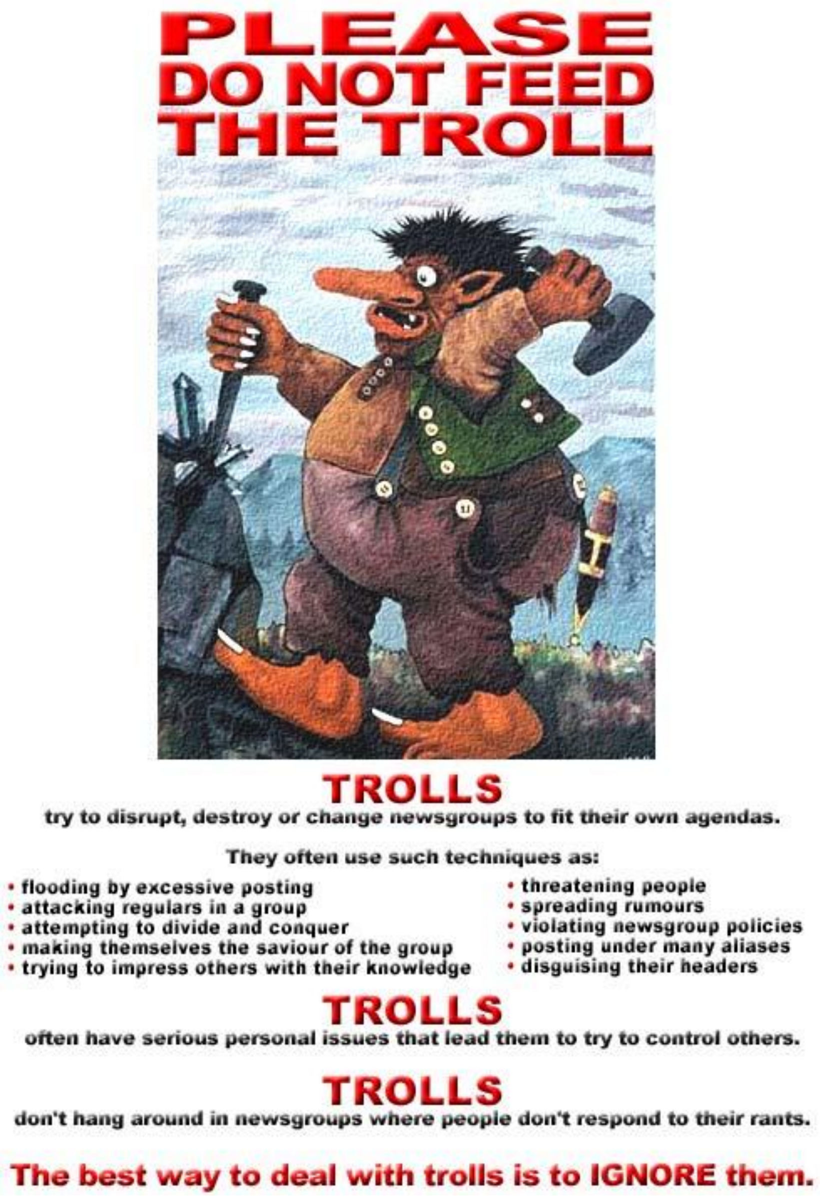
Thanks for making it clear that you were not the CUP referee, had nothing to do with the KC Cole "review", as well as acknowledging that you haven't even read my book or Lee Smolin's. My apology for having made erroneous suggestions in my posting, and for misinterpreting your later comments about it. I will update that posting to include this apology.

So, as it turned out, Clifford did not read Woit's and Smolin's books and Woit wrongly accused Clifford of being the hostile referee of his book for Cambridge University Press. Some factual matters were cleared up after all, albeit not being directly related to the blue print of our universe. Gina felt good about her participation, despite a comment by a blogger called "anon on the Hudson", who argued that "while the experts are discussing important points, Gina's comments are superficial and annoying", and accused her of blog-trolling. "Am I a blog-troller? Thought Gina? She looked at the definition of the term in the urbandictionary and thought about the interesting emerging blog culture. She was not an expert and she could be annoying at times, but she did not accept being called a troll.

Gina continued to be puzzled by Clifford's decision not to read Peter's and Lee's books and confronted him with it, to which Clifford, in turn, responded. With some effort she could understand Clifford's reasons for not reading the books, but this led to misunderstandings and attacks which occupied dozens, perhaps even hundreds, of further comments, and Gina still wondered whether Clifford made the right call.

Language, norms and ethics in the internet communities

The rapidly developing internet communities provide a laboratory for examining the emergence of norms and the formations of ethics and rules. Along with these, a whole new language is developing.



Trolls: The Enigmatic Creatures of Scandinavian Myths

By: Anat Lotan

"Troll sat alone on his seat of stone,
And munched and mumbled a bare old bone;
For many a year he had gnawed it near,
For meat was hard to come by.

Done by! Gum by!
In a cave in the hills he dwelt alone,
And meat was hard to come by."

Thus begins J.R.R. Tolkien's poem "The Stone Troll", which appeared in his collection of poetry *The Adventures of Tom Bombadil* (1962). The lonely, slightly comic figure of the troll depicted in the opening lines, soon displays more menacing character traits, evident in his monstrous plan to gobble up Tom Bombadil:

"'For a couple o' pins,' says Troll, and grins,
'I'll eat thee too, and gnaw thy shins.
A bit o' fresh meat will go down sweet!
I'll try my teeth on thee now.
Hee now! See now!
I'm tired o' gnawing old bones and skins;
I've a mind to dine on thee now.'"

So who or what were the trolls? Were they fiendish brutes who terrorized humans, or simply solitary creatures who enjoyed playing mischievous tricks? Originating in Scandinavian folklore, these mystical ancestors of the contemporary internet-trolls were as diverse in character as they were in appearance.

Nature played a vital role in the emergence of trolls in the human imagination. Scandinavia's bitter cold winters, long hours of darkness and sparse daylight ignited visions of supernatural creatures who roamed the region's dense forests.

The trolls of the Scandinavian myths and legends vary in shape and form. According to the prevalent tradition in Norway, the trolls were perceived as gigantic and ungainly creatures with hideous, beastly features. At the same time, the trolls of Southern Scandinavia were considered to be smaller in size, bearing a strong physical resemblance to human beings. Distinctive attributes such as an unruly lock of hair or - even more so - a tail hidden away in their clothing, were indicative of trolls. For an unsuspecting passerby who inadvertently stumbled across a troll - these tell-tale signs certainly came in handy!

Possessing an uncanny ability to become invisible, the trolls were also keen shape-shifters, assuming the form of both animals and objects. Whereas the large, brutish trolls led a rather solitary existence, the smaller trolls were more sociable in nature, raising families and living in a communal structure in the depth of the forests. Their typical dwelling places were mountainous caves, lakes or special underground constructions.

Were the trolls inherently evil creatures? Here, too, there is no clear-cut answer. Granted, when angered, they were prone to become harmful and vindictive. They were quite capable of committing intentional acts of malice such as stealing food or, even worse, abducting babies and young girls. At the same time, they often repaid the kindness and generosity of humans when these were bestowed upon them. The trolls thoroughly enjoyed playing mischievous tricks, yet since they were not particularly intelligent creatures, they were usually quite easily outwitted by humans.

Whether vile or kind, whether frightful or simply amusing creatures, the trolls have long since established themselves as an icon of popular culture; the hideous beast has become a furry, fuzzy doll, which not only entertains children but which is also thought to bring good fortune.

Whatever their shape or form, these mystical creatures of ages past are still very much alive, boldly roaming cyberspace as they once did the dark forests of Scandinavia.

Trolls: beastly giants or fuzzy playmates?

- Historical background taken from Wikipedia and from Moshe Jeger's book: *Trolls and Humans: Swedish Folktales and Legends* (Translation of the original title in Hebrew).

38 A dialogue with Moveon

> Moveon: "Is it legitimate to discredit a whole generation of hard working scientists by claiming that their work is worthless?"
>
> Gina: "The answer is: yes, it is legitimate".

A blogger named Moveon responded to Gina's question concerning the more traditional areas of particle physics, and offered his judgment of string theory critique.

207 - Moveon Dec 14th, 2006 at 12:57 am

> Gina asked: "Aren't there any interesting research areas closer to QED/QCD/ the standard model? Isn't there too much emphasis on fundamental problems? And not enough effort to identify interesting "second line" problems based on progress already achieved on fundamental problems?"
>
> Yes, there are plenty of those more conservative areas of research and very many people work in more phenomenological directions. It may be that the situation at certain US universities is not always so balanced, but in other countries, especially in Europe, there are more phenomenologists than string theorists. So as for theoretical particle physics as a whole, it is just nonsense to say that most efforts in particle physics would go into string theory.
>
> Now there seems to be a new trend, namely writing a book to complain about the lack of support for alternative approaches, if one feels that one's own field of research doesn't get enough recognition. That's a cheap way to try to bypass the battle in the marketplace of ideas, where it is the peers that need to be convinced, not the clueless public. If everyone who thinks that his

own pet theory is not sufficiently recognized would write a book, the market would be inundated.

Gina was thankful to Moveon for his explanations on that aspect of particle physics called phenomenology. She could not agree with his last paragraph, though. She felt that books and weblogs were part of the "marketplace of ideas".

215 - MoveOn Dec 14th, 2006 at 7:32 am

Gina, I appreciate your continued efforts to get an unbiased and objective judgment of those matters.

"I think the current controversy and discussions are of value. The more books are written, the higher the quality of the books that will be published. " (Quoting Gina.)

I think this is manifestly not true, judging from two recent books. Books and blogs with malicious intentions can do great damage to the "clueless" public who have no means to determine what is true or not. The relevant marketplace of ideas is indeed among the experts, and not the general public. The recent attempts to move the battle away from the scientific ground to the public and the internet, just reflects the failure of certain people to leave their marks on the academic battleground; claiming sociological rather than scientific reasons for that.

Gina challenged the claim that she was objective and explained that it is impossible to be completely objective. While not being previously biased, she was now biased by the (partially uninformed) judgments she had already made in these discussions, and by her tendency to argue all the time. She really did not regard scientific debates as "battles" and "battlegrounds," she did not think there was any real danger of "great damage", nor did she see people with "malicious intentions."

293 - Moveon Dec 17th, 2006 at 9:07 am

[Gina:] "I do not see here people with 'malicious intentions'."

You mean discrediting a whole generation of hard working scientists by claiming their work would be so bad and/or pointless that it is "not even wrong", is NOT malicious? Do you really believe that orchestrating a media campaign in order to damage the whole field of particle physics stems from a sincere concern about it? I can hardly believe that anyone with good intentions would act in such an irresponsible manner.

295 - Gina Dec 17th, 2006 at 9:41 am

Dear Moveon,

Essentially what you are asking is:

"Is it legitimate to discredit a whole generation of hard working scientists by claiming that their work is worthless?"

The answer is "yes, it is legitimate".

There are many sweet aspects to scientific work but one of the drawbacks is that it may turn out that your work is worthless. And another drawback, a smaller one, of the sweet academic life, is that somebody will claim that your work is worthless without justification.

39 Social science and economics

It was time to address the remaining social-science aspects of Lee's argument, and Gina first felt a need to share some general insights, and to describe some basic concepts using examples from Lee's book.

323 - Gina Dec 22nd, 2006 at 1:45 am

Let me talk a little more about the sociological aspects of Lee's argument, start with general remarks about this issue, and introduce some useful notions. Let me briefly talk about "goals," "opportunity costs," "incentives," and the tension between "rationality of actions" and "rationality of rules".

1. **Goals (or "utility")**. What do we want to achieve? I think the vague term "progress in science" can represent the goal that is the basis of discussion, or even more specifically, "progress towards answering the fundamental problems of physics." Of course, when it comes to universities and other academic frameworks and communities, teaching and scholarship are also important goals, and there are other goals as well.

2. **Opportunity cost.** Perhaps the most relevant notion from economics and the social sciences to quite a few aspects of the discussion is that of opportunity cost. Opportunity cost is the cost of something in terms of an opportunity forgone (and the benefits that could have been received from that opportunity) (Wikipedia).

To a large extent, the whole discussion is about opportunity cost. The question is not whether having research in string theory is good, but whether human, financial, and other resources can be better used.

We can study this notion using two little examples from Lee's book. Lee mentions the complaint that string theorists do not get interested quickly enough in "noncommutative geometry." Of course, it is good for string theorists to know and try to use every piece of mathematics (and they come amazingly close to doing so). But the "opportunity cost" approach would be: perhaps rather than climbing further on the mountains of Geometry towards the very, very new theory of "noncommutative geometry," it would be better for string theorists to pay more attention to the large fruitful valleys of Analysis or the mysterious hills of Stochastics.

Another example is Lee's suggestion to teach freshmen quantum mechanics, which is mentioned in several places in the book. Again, the question is not whether it is a good idea for freshmen to know quantum mechanics, but rather what is the "opportunity cost."

3. **Incentives**. This is a famous notion. An incentive (again, as Wikipedia tells us) is a name for any factor that provides a motive for a particular course of action.

Here is a little example. Lee compares "peer review" to "jury of one's peers":

"This is called peer review. It's a funny name, because it differs markedly from the notion of jury of one's peers, which suggests that you are being judged by people just like yourself, who are presumably fair and objective. There are real penalties – prison – for jurors who conceal a bias."

Lee gives a simple example of an incentive: prison for jurors who conceal a bias. Lack of similar incentives in universities leave us, according to Lee, with people who "shamelessly slant" their recommendation letters.

4. The tension between **rationality of actions** and **rationality of rules**.

To explain this notion we go back to the quantum mechanics course for freshmen. While at Yale, Lee's suggestion for a first-year quantum mechanics course was rejected by his conservative colleagues. The story was different at Harvard, where the Dean rejected Lee's initiative out of hand without considering its merits, because the proposal did not pass through the requisite committees. "If we let everybody teach what they wanted to," he said, "we would have educational chaos."

The Dean followed the rule of not allowing, and not even considering, a teaching initiative without the appropriate standard approval procedure, in order to prevent educational chaos. This indeed appears to be a rational point of view that reflects "rationality of rules," even if Lee's proposed action was justified.

Here is another example of the tension between rationality of actions and rationality of rules: Our nc [a prominent blogger in the string debate] mentioned how long it took the scientific community to accept the discovery of the bacteria *Helicobacter pylori* that causes ulcers. (See below.) Again conservatism may have led, in this particular case, to some delay in recognizing an important discovery (even to loss of lives), but overall, as a rule, the conservative and careful approaches in science, and certainly in modern medicine, are very rational.

The last example is a rare issue where Lee presents a conservative rather than a radical point of view, and it concerns what Lee calls the "ethics of science." Lee's position, which can be seen as giving priority to rationality of rules, is that even if you belong to a large dominant group in some field, the tradition and ethics of science require that you also support research in competing directions that you regard as unpromising.

After this long introduction, Gina felt ready to address her further concerns with Smolin's social and economic suggestions. There was one item she never got a good answer to. Is it a good idea to have a quantum physics course for freshmen?

324 - Gina Dec 22nd, 2006 at 2:00 am

14. Lee's ideas on revolutionizing universities are not detailed and the hints we get are not promising.

Gina analyzed in detail one proposal by Lee to have a special program for young scientists involved in fundamental physics. She tried to figure out the incentives to individuals and to universities if Lee's proposal were adopted, and concluded that it would make the situation for these young scientists even worse. Gina had an alternative suggestion which she described with a little sarcasm:

You want to promote young scientists doing foundational work? There is no way around **convincing** the academic community of the importance of their work. This is what you should do: Identify some brilliant young people who do good, highly original, foundational work, against all advice: differentiate between them, identify the better ones and try to support them in the ordinary university frameworks. Help them to get academic positions, spend time in following and criticizing what they are doing; and when they come up for tenure write a detailed recommendation letter, which tells about the candidate and not about the writer, and if they deserve tenure, give them a break: slant it a little, shamelessly.

After dealing with a few remaining points Gina said:

OK, of my 16 points, 15 down and one to go, and a juicy one indeed (scientific revolutions).

Helicobacter pylori and ulcer.

The blog participant 'nc' wrote: "The Nobel laureate, Barry Marshall, who discovered that *Helicobacter pylori* is present in all duodenal ulcers (contrary to mainstream ideas about stress-causing ulcers), took it on himself to demonstrate that it can cause ulcers. Still he was generally ignored from 1984–97." The story of understanding ulcers is indeed one of the great scientific stories of our time. Barry Marshall and Robin Warren, his long-term collaborator who shared the Nobel Prize with him, are great scientific heroes. For centuries, scientists did not believe that bacteria can live in the acidic stomach and thus be the cause of ulcer. Marshall and Warren observed the presence of spiral bacteria in autopsies and conjectured that these bacteria cause ulcers. It was not a whole new conceptual revolution in the human perception of diseases, but what matters most is that it was simply true. Still, it took over ten years for Marshall and Warren to *prove* their hypothesis and convince the scientific community.

40 Scientific revolutions

326 - Gina Dec 31st, 2006 at 3:13 am

1. Morgenbesser (1921–2004)

"Brother, can you spare a paradigm?" – Sidney Morgenbesser

16. Lee claims that science clearly calls for a revolution. Perhaps the opposite is true. Lee argues that science is now more conservative (anti-revolutionary) than ever. This is incorrect. The whole revolutionary terminology is crooked.

This point is mainly a philosophical disagreement I have with Lee.

2. Philosophy of science

We often see attempts in the debate about string theory to apply insights from philosophy of science, whether it is the Popperian notion of falsifiability, or the earlier probabilistically-based notions of verification (like the Bayesian notion), in order to derive definite conclusions concerning various aspects of the debate. Lee's reliance on Kuhn's philosophy of science is in the same spirit.

All these attempts have a limited potential. Philosophy of science theories have very partial descriptive and normative value. Rather than giving clear normative rules how science should be done, or a clear description how science has been done, philosophy of science is important for understanding what science is, and for introducing language and tools to talk academically and scientifically about science.

3. Kuhn

The notion of scientific revolutions was coined by Kuhn. One interesting thing I discovered reading Kuhn's book is his description of how Newtonian physics (!) had been suffering for centuries from problems that resemble the problems of string theory today: lack of experimental conformation, and worse, evidence that appeared to be in conflict with the theory.

The terms of art coined by Kuhn of paradigms and periods of normal science and of paradigm-shifts were very influential and very controversial in and of themselves. But the notion of "revolutions" was what most captured people's imagination. This was a great brand-name which overshadowed everything else.

Kuhn agrees (but some of his followers do not) that the notion of scientific progress is real and that the ultimate goal is progress. One problem with the revolution idea is that for some people revolutions are more appealing than progress, and are becoming the goal. One reason may be that progress is hard to define or to agree upon, and another reason is that a revolution is great excitement.

Even if we accept the notion of a revolution, another problem which is very explicit in Lee's writing, but which is also already presented in Kuhn's book, is the tendency to automatically identify highlights of scientific progress (and other human activities) with revolutions. Does Mozart's music reflect a revolution? In my opinion it does not, and it is artificial to regard it as such, despite it being a highlight of human achievement.

4. The high-energy physics revolutions.

Let's look at some prominent "revolutions" in this debate. Is the "second string revolution" a revolution? There were five potential string theories studied. As can be fully expected, some unexpected relations between them were found. And then there was the nice idea and supporting works suggesting that all five of these theories emerge in a larger 11-dimensional description of the universe. This solves some problems and leads to some others. In my opinion, this is a clear case where the revolution metaphor is out of place.

But whose opinion counts? This is a point Kuhn himself discusses. One of his statements that opened the path to "Kuhnism" and "relativism," to which Kuhn himself later objected, was his view that a revolution is in the eyes of the involved scientists, and not the eyes of an objective observer.

Was the standard model a revolution? In an article criticizing Kuhn's book, physicist Steven Weinberg claims it was not. Weinberg's opinion as a major player in the making of the standard model should count, even according to Kuhn. Was string theory itself (or the "first superstring revolution") a revolution? Maybe. It looks to me more like the non-revolutionary great progress that we see in music. Time will tell whether it is like Mozart, Chopin, or Paganini.

5. Pulling off a revolution

"More than any time in the history of science the cards are stacked against the revolutionary. Such people are simply not tolerated in the research universities. Little wonder, then, that even when the science clearly calls for one, we can't seem to pull off a revolution." (Lee Smolin, TTWP, p. 348)

It seems that all the revolutionary rhetoric distorted the scientific agendas. Science clearly calls for progress, also for some inner reflection about previous progress and about goals. Sure enough, some changes, even sharp changes and some backtracking, are required. But the revolutionary spirit is obscure. One change "clearly" called for is to let the revolution idealization and rhetoric rest and not only in science.

Happy 2007, everybody!

42 How to debate beauty?

After New Year 2007, Gina's participation in the string war weblog discussions slowed down considerably. This chapter brings a few highlights. Gina took part in another round of discussions on the blog of Clifford Johnson, who remained her favorite blogger, and she even participated in a marathon thread (500+ comments) entitled "String theory is losing the public debate" over the blog "the cosmic variance." She supported her claim that the string public debate is of interest only to a few with a comparative study of the cities of Santa Barbara and Gilroy. In Gilroy, the "garlic capital of the world", Gina claimed, you smell and feel garlic everywhere. But in Santa Barbara with its famous physics you do not feel or smell any physics. Gina enjoyed discussing technical matters which were well over her head. And she even wrote up a remark she had planned long before about the debate on beauty.

[Admitting Failure] (The Cosmic Variance)

Peter Woit Mar 27th, 2007 at 4:02 am

…Right now, the only motivation I see for people pursuing this [string theory] is not that they have a scientific argument about why it might work out, but that they are forced into it in order to avoid admitting failure of the string unification program.

Gina Mar 27th, 2007 at 6:36 am

Dear Peter,

This amazing "admitting failure" concept of yours is really a new contribution you made to the scientific discussion. We can imagine the following dialogue between PW and a scientist S (any scientist in any field).

PW: Why are you studying what you are studying?

S: Because I think it is interesting!

PW: No, you are studying it because you do not want to admit failure!

S: No, I really think it is interesting.

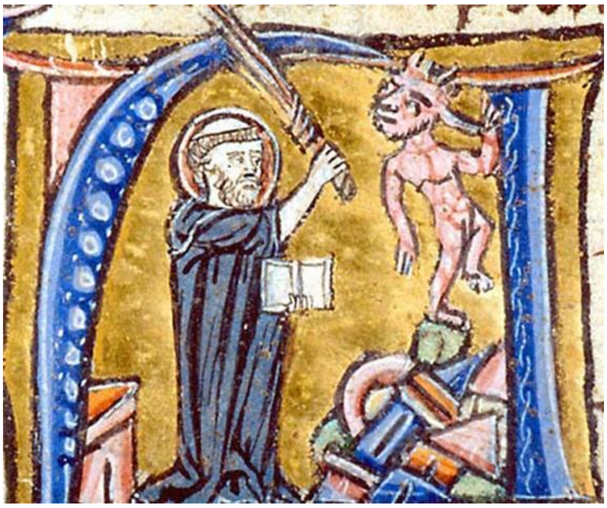
PW: No, No, admit failure at once!

S: If I admit failure, can I still continue studying what I am studying?

PW: Yes. But why would you?

S: Because I think it is interesting

Lubos Motl devoted a post to this remark by Gina and added a nice picture:



[The no-free-lunch theorem and evolution theory] (Asymptotia)

Gina got into a little argument with Lee Smolin about the theory of evolution. She was surprised to see that Lee gives some merit to very weak arguments regarding evolution (which are central in the "intelligence design" literature).

Lee Smolin Mar 25th, 2007 at 8:58 pm: Gina asks, (Mar 25th, 2007 at 7:58 am) "Are you saying that what nature produced is so fit to the extent that the evolution process cannot be simulated, even in principle, by a computer?"… There are also results like the "no free lunch theorem" which states that no single optimization procedure works better than random search on all possible landscapes, implying that unless you already know a lot about the landscape you cannot design a good optimization procedure for it. This means that no single computer program can both anticipate all possible species and efficiently simulate evolution on it.

Gina Mar 28th, 2007 at 1:36 pm Kauffman's argument that Lee mentioned based on the "no free lunch theorem," is supposed to say something about the impossibility - in principle - of optimizing in the context of evolution. People I talked to were very skeptical that the no free lunch theorem is saying much about optimization in general, and were especially skeptical about any relevance to the theory of evolution. (Gina provided a link to a paper by Olle Haggstrom on this issue.)

[Making your mark on a blog's long thread] (Cosmic variance)

Blogging has its exciting rewards!

401. Mark Srednicki on Apr 13th, 2007 at 1:21 pm

Damn! I missed making the 400th comment!

412. Gina on Apr 15th, 2007 at 3:51 pm

Guys, let's keep the 500th for Mark!

499. Mark Srednicki on Apr 23rd, 2007 at 12:56 pm

Gina wrote, "According to Mark, in order to have a mathematical rigorous theory for QED, string theory or 'something like string theory' is needed." Actually, as I said in #495, I'm agnostic on this issue. For QED, there's probably (an infinite number) of technicolor-like theories that could provide an ultraviolet completion.

500. Mark Srednicki on Apr 23rd, 2007 at 12:57 pm

500th comment! Do I get a prize?

[Polchinski's take on rigor] (Cosmic variance)

Joe Polchinski on May 29th, 2007 at 1:06 am : **Hendrik** #35 and **Gina** #32: I agree that I am going a little too far on this rigor thing. This is partly a response to Lee's going way too far in the other direction, but also partly a reaction to my own misspent youth. I used to focus too much on rigor and formalism, and have become a much more creative and productive scientist since learning, very slowly, to see through these to the physics.

[Non-Fock representations to the rescue?] (Cosmic variance)

Gina discusses non-Fock representations with a blogger named Hendrik

Hendrik on Jun 3rd, 2007 at 2:06 am : I suspect that the dimension 26 requirement is purely due to the incompatibility between the Fock representation and the gauge transformations…. So, if one abandons the Fock representation, I think the dimension 26 restriction may be lifted.

Gina on Jun 6th, 2007 at 10:49 am : Do you expect, Hendrik, that abandoning "Fock representations" will allow working in any dimension and the 26 dimension restriction will be lifted entirely? Or perhaps you expect that the dimension that will "burn" the anomalies will depend on some parameter of your hypothetical theory, like the cosmological constant.

[**Debating beauty] (cosmic variance)**

The issue of beauty and physics is quite prominent in this discussion. Lee Smolin warns against adopting a physics theory based on aesthetic consideration and brings Kepler's theory relating the five planets and five platonic solids (regular polytopes) as an example. Peter Woit makes (repeatedly, again and again and again) the claim that string theory is simply ugly, very ugly.

Well, beauty is a subjective matter. I remember my dear grand uncle Lena telling me:" Gina, aren't we very lucky that people see things in a subjective way? If men were objective they would have all fallen in love with my own beloved wife (her name was incidentally also Gina,) who is clearly the most beautiful woman. This could have caused all sorts of complications."

I, for example, regard string theory as very beautiful. Supersymmetry which grew up along string theory is an extremely beautiful notion. (In my view, supersymmetry has a natural form of beauty while string theory has an exotic and peculiar beauty.)

But the really interesting question in my mind is how to debate beauty. Can beauty be argued and debated at all?

Here is a story about arguing beauty in court, which may be of use. It was a case where the defendant was accused of a terrible crime.

The attorney for the defendant said in his opening speech: "Look at the defendant. Look how beautiful he is and look at his blue eyes, eyes of an angel. Do you really think he is capable of committing this ugly crime?"

At first, the prosecutor thought of ignoring this remark altogether, but then the remark was repeated and similar sentiments were expressed by some witnesses. The prosecutor watched how this non-issue was becoming an issue, and was worried that the beauty claims might convince some jury member.

The dilemma was not a simple one. Trying to argue that the defendant was not beautiful might convince a few jurors but would strengthen the belief of others that having beautiful eyes is indeed an impediment to being a criminal. Trying to argue that there is no connection between the innocent angel look and the crime may give this whole beauty business some credibility, and may cause those jurors who believe in this connection to take for granted that the defendant is indeed beautiful.

This is what the prosecutor said in his closing argument:

“Ladies and gentlemen of the jury," said the prosecutor, "there are two types of beauty. There is beauty that reveals a beautiful soul and there is beauty that covers up a corrupted and distorted personality. It is very difficult to distinguish between these two types of beauty, and often our initial hunches and intuitions turn out to be wrong.

We have carefully proved during this trial that the defendant committed the crime he is accused of, and therefore you must **conclude** that to the extent you find him beautiful, this is beauty of the bad kind, beauty which covers a corrupt personality capable of committing terrible crimes.”

Who is the most beautiful queen of cards? Opinions vary.

Epilogue: The Real Border

And this is a small piece Gina wrote about her brother Steve, and posted on Scott Aaronson's weblog "Shtetl-optimized." (Aaronson's post was about the war between Israel and Lebanon in the summer of 2006.)

Anonymous Says:
August 8th, 2006 at 4:12 pm

The Real Border

"I see holocaust in the street" says Steve. I try to reason with him. "Not here," I say, and "But Steve, our own relatives immigrated well before the war," and I even ask, "What precisely is it that you see?" But Steve only says: "I see holocaust in the street and also in my cup of coffee."

My brother is schizophrenic. His illness makes him suffer. This is an intolerable suffering which has been shattering and corrupting his identity and personality for forty years now.

On the other side of the real border there are those whose suffering is unbearable and inexplicable. The innocent victims of the war, Lebanese and Israelis alike, are on that other side of the real border. But the wisdom, the knowledge, the understanding, the justice is on our side of the border.

The more I understand, the more I succeed, the happier I am, the deeper the border between me and my brother.

[Editor's Epilogue:] The future of string theory as the definitive high-energy physics is unclear. Much depends on the outcomes of the Large Hadron Collider, which are expected in several years, and on theoretical developments in and around the theory. String theory can prevail in ways we cannot anticipate at present and it can also fail in unexpected ways. This book is not about string theory. It is more about delicate boundaries between greatness and megalomania, between humility and arrogance, between fantasy and reality, between wisdom and bullshit, between people of different stature and standing, between skepticism and harassment, between sanity and its loss, and between truths and fallacies. These are delicate boundaries that we witness in academics and in science and even in blog discussions. This story offers a little salute to people's passion for understanding their logical and physical reality, as well as for understanding themselves.