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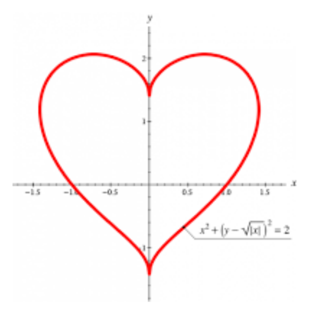
Thursday, December 08, 2016

No, physicists have no fear of math. But they should have more respect.



Heart curve. [Img Src]

"Even physicists are `afraid' of mathematics," a recent phys.org headline screamed at me. This, I thought, is ridiculous. You can accuse physicists of many stupidities, but being afraid of math isn't one of them.

But the headline was supposedly based on scientific research. Someone, somewhere, had written a paper claiming that physicists are more likely to cite papers which are light on math. So, I put aside my confirmation bias and read the paper. It was more interesting than expected.

The paper in question, it turned out, didn't show that physicists are afraid of math. Instead, it was a reply to a comment on an analysis of an earlier paper which had claimed that biologists are afraid of math.

The original paper, "Heavy use of equations impedes communication among biologists," was published in 2012 by Tim Fawcett and Andrew Higginson, both at the Centre for Research in Animal Behaviour at the University of Exeter. They analyzed a sample of 649 papers published in the top journals in ecology and evolution and looked for a correlation between the density of equations (equations per text) and the number of citations. They found a statistically significant negative correlation: Papers with a higher density of equations were less cited.

Unexpectedly, a group of physicists came to the defense of biologists. In a paper published last year under the title "Are physicists afraid of mathematics?" Jonathan Kollmer, Thorsten Pöschel, and Jason Galla set out to demonstrate that the statistics underlying the conclusion that biologists are afraid of math were fundamentally flawed. With these methods, the authors claimed, you could show anything, even that physicists are afraid of math. Which is surely absurd. Right? They argued that Fawcett and Higginson had arrived at a wrong conclusion because they had sorted their data into peculiar and seemingly arbitrarily chosen bins.

It's a good point to make. The chance that you find a correlation with any one binning is much higher than the chance that you find it with one particular binning. Therefore, you can easily screw over measures of statistical significance if you allow a search for a correlation with different binnings.

As example, Kollmer *at al* used a sample of papers from Physical Review Letters (PRL) and showed that, with the bins used by Fawcett and Higginson, physicists too could be said to be afraid of math. Alas, the correlation goes away with a finer binning and hence is meaningless.

PRL, for those not familiar with it, is one of the most highly ranked journals in physics generally. It publishes papers from all subfields that are of broad interest to the community. PRL also has a strictly enforced page limit: You have to squeeze everything on four pages – an imo completely idiotic policy that more often than not means the authors have to publish a longer, comprehensible, paper elsewhere.

The paper that now made headline is a reply by the authors of the original study to the physicists who criticized it. Fawcett and Higginson explain that the physicists' data analysis is too naïve. They point out that the citation rates have a pronounced rich-get-richer trend which amplifies any initial differences. This leads to an `overdispersed' data set in which the standard errors are misleading. In that case, a more complicated statistical analysis is necessary, which is the type of analysis they had done in the original paper. The arbitrarily seeming bins were just chosen to visualize the results, they write, but their finding is independent of that.

Fawcett and Higginson then repeated the same analysis on the physics papers and revealed a clear trend: Physicists too are more likely to cite papers with a smaller density of equations!

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I have to admit this doesn't surprise me much. A paper with fewer verbal explanations per equation assumes the reader is more familiar with the particular formalism being used, and this means the target audience shrinks. The consequence is fewer citations.

But this doesn't mean physicists are afraid of math, it merely means they have to decide which calculations are worth their time. If it's a topic they might never have an application for, making their way through a paper heavy on math might not be the so helpful to advance their research. On the other hand, reading a more general introduction or short survey with fewer equations might be useful also on topics farther from one's own research. These citation habits therefore show mostly that the more specialized a paper, the fewer people will read it.

I had a brief exchange with Andrew Higginson, one of the authors of the paper that's been headlined as "Physicists are afraid of math." He emphasizes that their point was that "busy scientists might not have time to digest lots of equations without accompanying text." But I don't think that's the right conclusion to draw. Busy scientists who are familiar with the equations might not have the time to digest much text, and busy scientists might not have the time to digest long papers, period. (The corresponding author of the physicists' study did not respond to my question for comment.)

In their recent reply, the Fawcett and Higginson suggest that "an immediate, pragmatic solution to this apparent problem would be to reduce the density of equations and add explanatory text for non-specialised readers."

I'm not sure, however, there is any problem here in need of being solved. Adding text for non-specialized readers might be cumbersome for the specialized readers. I understand the risk that the current practice exaggerates the already pronounced specialization, which can hinder communication. But this, I think, would be better taken care of by reviews and overview papers to be referenced in the, typically short, papers on recent research.

So, I don't think physicists are afraid of math. Indeed, it sometimes worries me how much and how uncritically they love math.

Math can do a lot of things for you, but in the end it's merely a device to derive consequences from assumptions. Physics isn't math, however, and physics papers don't work by theorems and proofs. Theoretical physicists pride themselves on their intuition and frequently take the freedom to shortcut mathematical proofs by drawing on experience. This, however, amounts to making additional assumptions, for example that a certain relation holds or an expansion is well-defined.

That works well as long as these assumptions are used to arrive at testable predictions. In that case it matters only if the theory works, and the mathematical rigor can well be left to mathematical physicists for clean-up, which is how things went historically.

But today in the foundations of physics, theory-development proceeds largely without experimental feedback. In such cases, keeping track of assumptions is crucial – otherwise it becomes impossible to tell what really follows from what. Or, I should say, it *would be* crucial because theoretical physicists are bad at this.

The result is that some research areas can amass loosely connected arguments that follow from a set of assumptions that aren't written down anywhere. This might result in an entirely self-consistent construction and yet not have anything to do with reality. If the underlying assumptions aren't written down anywhere, the result is conceptual mud in which case we can't tell philosophy from mathematics.

One such unwritten assumption that is widely used, for example, is the absence of finetuning or that a physical theory be "natural." This assumption isn't supported by evidence and it can't be mathematically derived. Hence, it should be treated as a hypothesis - but that isn't happening because the assumption itself isn't recognized for what it is.

Another unwritten assumption is that more fundamental theories should somehow be simpler. This is reflected for example in the belief that the gauge couplings of the standard model should meet in one point. That's an assumption; it isn't supported by evidence. And yet it's not treated as a hypothesis but as a guide to theory-development.

And all presently existing research on the quantization of gravity rests on the assumption that quantum theory itself remains unmodified at short distance scales. This is another assumption that isn't written down anywhere. Should that turn out to be not true, decades of research will have been useless.

In lack of experimental guidance, what we need in the foundations of physics is conceptual clarity. We need rigorous math, not claims to experience, intuition, and aesthetic appeal. Don't be afraid, but we need more math.

Posted by Sabine Hossenfelder at 7:40 AM Labels: Physics, Sociology of Science

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