Rationally Speaking #211: Sabine Hossenfelder on “The case against beauty in physics”

Julia: Welcome to Rationally Speaking, the podcast where we explore the borderlands between reason and nonsense. I'm your host, Julia Galef, and I'm here with today's guest, Sabine Hossenfelder.

Sabine is a theoretical physicist, focusing on quantum gravity. She's a research fellow at the Frankfurt Institute for Advanced Studies, and blogs at Back Reaction. She just published the book, "Lost in Math: How Beauty Leads Physics Astray."

"Lost in Math” argues that physicists, in at least certain sub-fields, evaluate theories based heavily on aesthetics, like is this theory beautiful? Instead of simply, what does the evidence suggest is true? The book is full of interviews Sabine did with top physicists, where she tries to ask them: what's the justification for this? Why should we expect beauty to be a good guide to truth? And, spoiler alert, ultimately, she comes away rather unsatisfied with the answers.

So, we're going to talk about "Lost in Math" and the case for and against beauty in physics. Sabine, welcome to the show.

Sabine: Hello.

Julia: So, probably best to start with: what do you mean by beauty, in this context? And is it closer to a kind of “beauty is in the eye of the beholder” thing, like, every physicist has their own sort of aesthetic sense? Or is more like there's a consensus among physicists about what constitutes a beautiful theory?

Sabine: Interestingly enough, there's mostly consensus about it. Let me put ahead that I will not try to tell anyone what beauty means, I am just trying to summarize what physicists seem to mean when they speak of beauty, when they say “that theory is beautiful.”

There are several ingredients to this sense of beauty. The best known is probably simplicity, so theories should be simple… [like] in art, where we also see there's a minimalism, for example, in photography.

There is another criteria, called naturalness, which has been very influential, especially in high energy particle physics. By this, they mean that a theory should not appear like it has been handmade. It should not have some conspiracies among the numbers, that they have been tuned to be just right, but it should all be natural. Not like it's due to human choice, because someone wanted it to be just exactly this way. So, that's naturalness.

Then there is a third criterion, that you can loosely refer to as elegance, and that's the hardest to capture. It's when people say that a theory should not only be simple and natural, but it should also hold some surprise. You know? It shouldn't give away everything too quickly. So, you should have this, "Ah-ha!" Effect, that something unexpected comes out, or that things fit together in a way that you didn't anticipate. So, these three criteria, together, are what makes a theory beautiful.

Julia: So, just to clarify the naturalness one, the second type of beauty — would an example of an unnatural theory be if the current model didn't quite explain the data, and so the scientist had to add in an extra term, to make it come out with the right answer?

Sabine: No, naturalness is really about the proportions in the theory. I can give you an example that maybe illuminates this. It's also historically where I think this notion of naturalness was first documented in the literature.

It's when astronomers, several hundred years ago, were trying to switch from the geocentric model, where the earth is in the center of the solar system, to the heliocentric model. They noticed that if the earth goes around the sun, and the stars are on some celestial sphere, then over the course of the year, we should see the stars shift in their relative position to each other, just because we're moving. So, if we are in the center of the sphere, we wouldn't see it, but if we are moving around the center, then we should see the shift. This shift is called a parallax.

So, they were looking for it, but they didn't see it. The conclusion they drew from this, is that either we are in the center of the sphere, and that's why we do not see the stars change their relative position, or the stars are so far away, that we would not notice this tiny shift. So, we know today that the right answer is the stars are indeed so far away that they could not see the shift — because we can measure this shift now, it's just that back then they didn't have the greatest telescopes, so they couldn't resolve this small shift.

But, they discarded this option, because this would have meant that the stars would have been much further away than the other stellar objects that they knew about. These were the planets.

So, the way that I think Tycho Brahe put it, was that he said that God would not like this mismatch of proportion — because you had these kind of similar distances between the planets, but then the stars, which they thought were stuck on a celestial sphere, would have been much, much, much further away. So, this is where this notion of “not natural” comes from, because you have to put in this gigantic difference in scales.

We still have this notion of naturalness in physics now. For example, in particle physics, we have a big gap between several energy scales. One energy scale is called the Planck mass, you know, that's a pretty large energy scale. It's about 15 orders of magnitude larger than the heaviest particles that we know, for example, the Higgs boson. That brings up the question where were does this large ratio come from? This is what we call unnatural.

Julia: I'm just so confused about why people would expect any particular ratio of magnitudes to be...

Sabine: Yeah. You know, that's a very good question. Actually, my mother was one of the first people to read the book, and she asked me exactly the same thing. You know, we read the whole book, and I explain that people use it, but she was like, "Yes, but why? What's the problem with this...?"

Julia: So interesting.

Sabine: But that's what they do. That's what they think is ugly, if you have a large number like this. So, there are more technical versions of it, where you can actually mathematically define exactly what you mean by it, but that's the gist of it. You know, you do not want to have these large differences in proportion. That's why the Higgs mass is a particular headache in particle physics, and they try to explain it. That's what they cook up new theories for.

Julia: It's a headache because it seems too small? Is that-

Sabine: Yes.

Julia: Am I understanding right? Relative to this other, well defined quantity.

Sabine: Exactly.

Julia: Okay. Well, actually, before I go on with my questions about beauty, I should just clarify: What parts of physics are you referring to, with your thesis that aesthetics are overvalued? Or all of it?

Sabine: I'm referring to the foundations of physics. So, that's where we deal with the things that are fundamental in the sense that they cannot be derived from anything else. That's currently space and time, and 25 particles that, for all we currently know, are not divisible any further.

These are the subjects of study in four disciplines. One is high energy particle physics, that we just spoke about. Then there's quantum gravity, so that's the question what are the quantum properties of space and time? Then there are certain parts of cosmology where we're talking about the question like how did the universe begin? And what happened after the big bang? And that kind of stuff. Then there are the foundations of quantum mechanics.

Julia: What's an example of a field of physics that would not meet these criteria?

Sabine: Everything else. Condensed matter physics, for example, where people deal with fluids and solids, and everything that's made up of a lot of particles. Of course, you also have theoretical physicists in these areas, but it's a completely different way that they go about developing new theories.

Julia: Okay. So, well, I guess this dovetails nicely with the next question I was going to ask, which is:

What is the justification that the proponents ... Well, I know from reading your book that physicists will explicitly defend the use of beauty as a criterion for judging the a priori promisingness, or plausibility, of a theory. And also, even after we've looked at the relevant evidence that we have, scientists will explicitly defend holding onto more beautiful theories, even if the evidence doesn't yet seem to confirm them. So, they're using beauty in that way too.

So, A) what is their stated justification for why that's a sensible thing to do? And then B) why is this more true in the fields that you are talking about, than it is in other fields like condensed matter physics?

Sabine: The primary justification that at least I've come across, is two-fold.

The first thing is that, in a certain sense, they think it doesn't matter, because if you have a theory, you go and test it, and it's either correct or it isn't correct — so in the end it doesn't really matter just exactly how you came up with that theory. You know? If you use criteria from [beauty], then that's fine, but in the end, no one wants to know just how you came up with that thing.

The other reason that they often give, is that it's a matter of experience, that what we think of as beauty is really something that we have learned from the theories that we know to work well already. And, as a matter of fact, if you look at very successful theories, like the standard model of particle physics, it is natural in a technical sense.

Except for the mass of the Higgs boson. This mass of the Higgs boson is really, really offensive to particle physicists, because it's different. Of course, the Higgs boson is different, as a particle, from all the other particles, and that's the reason why, but they don't like this issue with the mass.

So, I actually think that it is totally plausible as a first line defense. To try and take these criteria that have been successful in the past, and continue to apply them, to see how far you can go with it. So, that's certainly something that people have tried for naturalness, and there are other criteria of beauty for which they have done the same thing. For example, symmetries, symmetries and unification.

This is something which has been very, very successful in the history of physics. We've unified the electric and magnetic forces to the electromagnetic forces, then we have unified this with the weak nuclear force, and a lot of physicists thought, and still think, that this should continue to go this way, that we should actually unify all the forces by way of symmetry.

Julia: What do you mean by unify the forces? Explain them all under-

Sabine: Yes. What we mean by unifying the forces is that they are not actually different forces, but they come out from only one force.

Julia: Right.

Sabine: It's just that in the energy ranges that we have tested them so far, they look like three different forces.

Julia: Ah, okay. Yeah.

Sabine: It's the same thing with the electro and the magnetic force. We know that they are actually the same thing, and in certain circumstances you can actually exchange one for the other in a certain sense. But, fundamentally, they are the same thing. So, this was ... you had a second question?

Julia: Yeah. The second question was, why is aesthetics so much more heavily used in the fundamental parts of physics, than it is in other parts of physics, like condensed matter?

Sabine: Yes, that's a very interesting question. The foundations of physics are a peculiar field in science right now, because they have a large reservation of data.

It's not like they don't have data. They have a shitload of data, usually precise data. That comes, for example, from the Large Hadron Collider. But all this data that they have, only confirms the theories that they know already.

Now, the problem is that we have good reason to think that the theories that we currently have are not complete. We are missing something, and this is why theoretical physicists want to develop better theories, but we do not have any data that tells us just how these theories should look like.

And this has been the situation for 30, 40 years, that there hasn't been any data for new effects.

Julia: Is the reason for the lack of such data just that, like, we would need a giant particle collider? Or that we don't even know what data we *could* get that would bear on one theory versus the other? Or, what's the reason?

Sabine: Well, we don't know. I mean, that's because we don't have the theory, right? And so you're looking for some new data but you don't know where to look because you don't have the theory.

Julia: Yeah, I see. There's a bit of a chicken and egg problem there, yes.

Sabine: Yes, exactly.

Julia: Okay.

Sabine: And now the problem is that in such a situation, theorists should be very, very careful about what theories they develop and then put forward for experimental tests. Because if they continue to make their proposals for theories, you know, for theories that are not very promising, then the experimental tests will not find anything.

Now a null result is, of course, also a result. But it's not a very useful result if you want to develop a new theory.

Julia: Right.

Sabine: And so this gets you into a vicious cycle where you have non-promising theories that are being put to test, but then the only thing you get are null results and you have no guidance for the development of better theories. And that's exactly the cycle that we've been in for more than 30 years now.

Julia: I see. So is the idea then that that kind of leaves a vacuum which then gets filled by aesthetic preferences and heuristics? And that vacuum isn't present in other fields?

Sabine: Yes, exactly. Well, I wouldn't really call it a vacuum. You know, what I would say is that this absence of new data amplifies the problems that are inherent in scientific communities already. Because people have no corrective that comes from the data.

Julia: I see. Okay. Yeah, that's helpful.

So in your book you quoted a physicist named Weinberg giving an analogy to justify why relying on beauty could be a reasonable heuristic:

He said it's kind of like judging, if someone's judging a racehorse. Like an expert in judging quality horses. And they find one particular horse beautiful. They may not be able to explain why they have that aesthetic reaction, but there are likely things that their intuition is picking up on about what makes a successful racehorse. Like maybe it's got a certain posture or a certain kind of focused personality, and those things cash out as beauty in the judge's subjective perception.

But just because he can't articulate what went into that black box beauty-detecting algorithm in his brain, doesn't mean it's not a good algorithm.

And your response to this was, you know, fine, but if you then try to use that heuristic to judge race cars instead of racehorses, you should not expect it to still work similarly well, in predicting which cars are going to be successful. In other words, maybe beauty was a good heuristic in the past — there are successes, as you've noted — but maybe it's not for the new generation of physics problems.

And so my question for you is: Why would that be true? Why should we think that it happens to be the case that past problems in physics were solvable using beauty as a heuristic, but current problems are not? What would be different about them?

Sabine: Well, first let me note that this argument with the horse breeder that you just quoted is an argument from experience that I was just mentioning as a typical justification. So now my problem with that kind of argument, as you just summarized, is that the experience that you gain from watching a lot of horses win races and so on and so forth really only helps you judge something similar.

If you have to judge something entirely new, this conception of beauty that you have developed in the past is no longer useful. And I am afraid that this is exactly the situation that we're in right now. This does not mean, as you seem to indicate, that a sense of beauty, per se, is not something that we should apply to our theories. It just means that this particular notion of beauty that we have from the past theories turns out to be no longer useful. So what we would really need are new ideals of beauty.

Julia: I'm just saying, why? Shouldn't it be somewhat surprising that the past heuristics of judging beauty are no longer as useful as they were before? Like, I wouldn't have thought a priori that the problems we're solving now would have different properties.

Sabine: Why would it be surprising? I mean, it has happened in the past. I have a couple of examples for this in the book where our ideas of beauty applied to the theories of nature have changed.

Like, for example, I was previously mentioning the switch from the geocentric to the heliocentric model. So, as you probably know, there were a lot of astronomers at the time who didn't like the heliocentric model at all because, as Kepler found out, the planets move on ellipses around the sun and not on circles.

And they were kind of really stuck on these circles, because circles were beautiful. You know, they're perfect. And then they had this idea that if there's one circle you can add another circle on it, then this gives you the epicycles. So they were really stuck on this, on these damn circles. But today this isn't something that anyone would argue. You know, what's so great about a circle? Why not an ellipse?

So this is one of the examples, but there are probably examples where this shift in aesthetic ideals has been even more pronounced. Like the shift from an internally static universe, that was once considered beautiful, to one that is actually dynamical and evolving and expanding as we know now.

Julia: Yeah.

Sabine: Or the shift from the pre-quantum classical mechanics to the quantum mechanics. Quantum mechanics is really an entirely different thing. It works entirely differently. And at the time when it was proposed at the turn of the previous century, a lot of people thought it was ugly, because you have this break with the way that physics was done previously. So they didn't like it at all.

But nowadays, for example, I don't think that quantum mechanics is ugly. I think it's a pretty theory. But other people still think it's ugly. So we're having somewhat of a disagreement. But I think it serves to show that we're not stuck on certain ideas of beauty and that, indeed, in the history of physics our conceptions of beauty have changed.

Julia: It seems to me that of the components of beauty that you named before, I would expect some of them to be much more temporally dependent, much more temporarily useful, and others to be more permanently useful.

So naturalness — well, I guess I'm confused about why that would ever be useful. But I could imagine it being a good heuristic for certain kinds of problems. But not indefinitely, as we push the frontiers of what we're studying.

But simplicity — obviously it's not the case that all truths about the world are the simplest possible truths, but in terms of, “All else equal, should we prioritize simpler theories over more complex theories? At least until we get more data or have reason to budge away from simplicity?” That seems more timeless to me.

And it also doesn't quite feel like an aesthetic criterion to me. I mean, I guess it's fuzzy what you call aesthetic. But I would have called it more of, like, an epistemological principle and not a bias.

So, for example, if someone argues for a model in which emeralds aren't green, they're grue. G, R, U, E. Where grue means “Something that is green up until January 1st in the year 2300, and then turns blue for the rest of time.” That theory that emeralds are grue feels really unnecessarily convoluted to me, and I want to reject it in favor of a simpler emeralds are green theory. At least until I have some evidence.

And that doesn't feel like an aesthetic bias to me. That feels like a principle of how to reason. Does that make sense?

Sabine: Yes. You're right that I should have been more careful about what I said about simplicity. So what you just referred to is a relative notion of simplicity. That from two theories that achieve the same, you pick the simpler one.

Julia: Right.

Sabine: That has nothing really to do with beauty. It's just a scientific criterion. It basically says, why would you want to make your life more complicated than it needs to be?

Julia: An appeal to laziness.

Sabine: Yeah, right.

Julia: Argument from laziness.

Sabine: Yeah, exactly. But the type of simplicity that is an argument for beauty is an *absolute* type of simplicity, that the theories that are more fundamental should be simpler in a certain way. Like I mentioned this example with the unification of the forces. Because they would be simpler if you had really only one force, which only appears complicated if we don't look at it the right way.

So there's this idea that if we look closer and closer into the fundamental structure of matter, we should find laws of nature that are simpler in an absolute way.

And there is really no particular reason for this. Indeed, you could argue that that's not what we have actually found because the laws of nature that were known were much simpler before we started to find evidence for what's known as the strong nuclear force, which is really a mess. You know, people started producing a lot of particles that they then had to classify by introducing the quark model and so on and so forth.

But in a certain sense you could say, well, everything would have been much simpler if we wouldn't have had this force to begin with. It's just that this doesn't describe what we see. And so there is really no strong reason to believe that as we keep pushing to shorter and shorter scales, and studying particles ever more precisely with larger colliders, that the laws that we find should continue to get simpler.

Julia: So it sounds like maybe your complaint is less about using aesthetics as a criterion a priori, to help decide which theories to pursue and test, and more about scientists not updating away from their initial simple working hypothesis as data comes in.

It seems to me like it was probably a good call initially to start with the assumption that maybe these different forces can be unified, and see if they can be. And if they can't be, then settle for a more complex model of the universe. But then if scientists are not actually doing that updating, and still sticking to the, "well, this has to be true because it's simple," then that's the problem. Does that sound right?

Sabine: Yes, exactly. So it was a reasonable thing to try. But we have tried it, starting in the 1980s already, and it didn't work. And I think it's about time to draw conclusions from it. Okay, it didn't work. Maybe you should be trying something different. And that isn't happening. So now the interesting question is why isn't this happening?

Julia: Yeah.

Sabine: Yeah, and so this is why I started to think about the feedback effects that you get from community dynamics on the individual researchers.

So you have this very practical problem that you have a lot of really smart people that come together in large communities, counting several thousands, and they mostly talk to each other about their research.

And they constantly tell each other that what they're doing is great and it's beautiful and certainly there's something behind it, and it's such a promising theory, and so on and so forth. And so they continue to do the same thing. And it's really, really hard to get anything started where you do not have some people in your back who are supporting the same thing.

Julia: But why isn't there a strong incentive to be the scientist who just finds the right theory, instead of spinning their wheels for their whole career with beautiful but wrong theories?

I sort of imagine this analogy to an industry where it's the norm for businesses to spend a bunch of money making their offices beautiful or something. And then it seems like there should be a big incentive for some business to come along and be like "We don't need a beautiful office. We can spend that money being more profitable." And then they would just capture a ton of the market share.

So why aren't there scientists being like, “Wow, all these other people are wasting their time on aesthetics. I'm going to not waste my optimization power on that, and [instead] increase my chance of actually winning the Nobel Prize.”

Sabine: So there are certainly such people. It's just that there are very few of them.

The problem is that you literally, in the present situation, you literally cannot afford doing this because you will not get money. You have to understand that these are problems that you typically cannot solve on a timescale of two or three years. And this is the typical timescale on which you will get a research grant or on which you will get a postdoc position, or something like this, and you are pretty much forced to work on something that will produce results in this timeframe, because it's the only way that you will be able to apply for more funding. And that's how the wheels keep turning, and so there are those people who are comfortable with this.

Also, I should add as a warning that this kind of procedure with the two or three year projects and so on, works perfectly fine in a lot of fields, because it's just the kind of time frame in which you can execute the projects that they have there. But if you're talking about questions like, "How do we quantize gravity?" which is something that people have tried for 80 years but have failed, then in these two or three years, you're not getting anywhere and you know this already, so people will not even start trying.

So the current system just totally doesn't work for that kind of thing. Then what happens is that the people who would really like to do this but they can't, they either leave, end up being frustrated, or they just conform and do whatever they can in the hope that maybe on the side they will be able to do what really interests them. And they typically end up being very cynical about what's going on.

Julia: Would you put yourself in the category of people who are the mavericks trying to ignore aesthetics, in your research?

Sabine: Well, the thing is that I originally was using these arguments for beauty in my own research, and then I didn't really understand what I was doing. So I tried to figure out just where do these arguments come from, and is this a good thing to do?

As my book tells, I came to the conclusion that, no it's actually not a good way to pick a research project to rely on these arguments from beauty. And I've tried to avoid it. I'm not sure how good I am at following my own advice, but I'm trying, let me put it this way.

Julia: Interesting. I also wonder if maybe the problem is less a bias towards aesthetics, and more a problem of groupthink, or an echo chamber.

Maybe what we want is for scientists to have intuitions about what theories feel intuitively compelling to them in an aesthetic way, but we don't want the entire field to get stuck on a few criteria for plausibility, the same way that the art world or the fashion world will develop specific things, like, "These are the things that are beautiful, or cool," or whatever.

Maybe that's fine for fashion or art or something. But if what we need in physics is more exploration of different kinds of theories, and less everyone flailing against the same stuck door, then maybe the lever we should be pushing on — to the extent that we can push on any lever — is the groupthink one. What do you think?

Sabine: Yes, I think that's probably right, what you say. So this belief in arguments from beauty is a symptom, not the underlying disease. It's also only one aspect of the problem, like you have more generally a trend for people to work on topics that are productive and popular, so that they can quickly produce many papers, and these papers will be well cited, just because there are a lot of people who also work on this.

This is just ... it happens to be something that works very well with beautiful theories, because everyone likes beautiful theories, so they like to work on it, so that works really well.

But really, the underlying problem is this streamlining, that people by the current organization of the system are really forced to work on what delivers quickly, and what is appealing to their peers. I think it's really a problem with the organization of the academic system right now.

Julia: Yeah. Switching gears a little bit, do you think that it's possible to do purely theoretical research? Like if we just really couldn't get data to distinguish one theory from another, or should we just completely give up, or can we make progress just with theory alone?

Sabine: Well, to some extent you can, but of course you cannot just entirely give up trying to test the theories, because then you are no longer doing science. There is no hard cutoff where you can say, "Well, I've been developing this theory, and if I haven't been able to find experimental test after ... I don't know, 20 years or something, then it's no longer science." That doesn't make any sense. You should not leave the goal out of sight and just settle on this idea that, "Well, I'm happy deriving some things from this theory, and who cares if it has something to do with nature."

Julia: Well... I guess I was thinking of not ignoring data entirely, but just sort of finding a theory that fits the existing data as well as possible, while taking into account various constraints like simplicity, or elegance, or something like that.

It seems plausible to me that out of the set of all possible theories that could fit a certain set of data, we could still have reasons to prefer some over others, and that theoreticians might be able to do valuable work articulating those reasons, and justifying one theory over another.

I mean, whether or not you want to call that science, does that not seem like a valuable project?

Sabine: Well, we do have theories that fit all the existing data. That's exactly the issue, all right? Then we could just say, "Well, let's close the Department for Theoretical Physics, because we don't need them anymore." I mean, we do have problems with these existing theories, it's just that they do not come from data, they come from some internal inconsistencies.

Now, the thing is that it is correct where you say, you can just go and say, "Well then, let's try to theoretically, or you could say mathematically, solve this inconsistency," and that's for example what string theory does. It's an attempt to solve inconsistencies in the existing theories. If you do something like this, you will never know that this is actually the right way to do it, because there will always be more than one way to do it.

Just because you will never know that the assumptions that you started from were actually "true," in scare quotes. In mathematics, you write down an assumption. That's how it is. You've defined it to be that way, but in reality it doesn't work this way. It's just that it's an assumption that has worked pretty well to describe what we have seen so far, but who tells you that it would continue to be this way? The only way to find out is that you actually go and look. So this is why this experimental test remains enormously important.

Julia: Speaking of string theory, there are two books that yours has been compared to, and they are Peter Woit's *Not Even Wrong*, which was actually one of the very first Rationally Speaking episodes eight years ago now, and then Lee Smolin's *The Trouble With Physics*. They're both about string theory in particular, and they argue that it's problematic, because it hasn't made testable predictions.

Do you have any substantive disagreements with Woit or Smolin, or would you say that their arguments are basically your arguments, just applied specifically to string theory?

Sabine: Well, my book is much broader in its aim, so it's not specifically about string theory. Just from my own training, I have my beef more with high energy phenomenology, not so much with the string theory. I actually think that I'm personally, I'm much more optimistic about the promises of string theory, just not for what the search for a theory of everything, or a unification of the forces is concerned.

I mean, we've tried it, and nothing has come out of it. It's quite questionable that you can actually apply string theory to describe the universe that we live in as with it having a positive cosmological constant. You can make it work, but it's somewhat of a stretch, and yeah, it's not great, let me put it this way.

It is also known that a string theory is very closely tied to the theories that we presently use, and so it has turned out to be useful in certain circumstances to borrow calculations from string theory to describe other things. It's just that these descriptions no longer fall in the foundations of physics, which is also why I don't discuss them in the book. It doesn't really fall into this realm.

This is why I think that string theory actually does have its uses. We don't really fully understand exactly how it works, and where the uses are, but there a lot of people working on it, and I don't really have a problem with it. It's just that what I'm- I mean I know a lot of string theorists, and they are nice people, and so it's not like we're enemies or something. The question is, though, "Is this enough reason to believe that really the fundamental structure of reality must be described by string theory?" And I think the answer to this is just, no.

Julia: Yeah. Great, well last question before I let you go. Can you recommend a book, or blog, or other resource that is a good representative of your field, however you want to define your field, something that non-experts could get a lot out of?

Sabine: Oh, that's a very good question. I guess my first choice to get an impression for what we're doing in the foundations of physics is Sean Carroll's book The Big Picture, because it really gives you the big picture. I think the book also captures nicely why I got interested in the field to begin with.

If you want to understand the fundamental nature of reality, you want to know what really are the laws that govern the universe, and that ultimately dictate our own existence, then the foundations of physics is what you look at, and that's what Sean Carroll explains very nicely in his book.

Julia: Okay, great. Well, we'll link certainly to your book, Lost in Math-

Sabine: Well, I hope so.

Julia: ... on the podcast website, and then also to your website, your blog Backreaction, and to your pick, The Big Picture, by Sean Carroll. Sabine, thank you so much for joining us, it was such a pleasure having you on the show.

Sabine: Thanks for having me.

Julia: This concludes another episode of Rationally Speaking. Join us next time for more explorations on the borderlands between reason and nonsense.