Rationally Speaking #172: Brian Nosek on “Why science needs openness”

Julia Galef: Welcome to Rationally Speaking, the podcast where we explore the border lands between reason and non sense. I'm your host, Julia Galef and with me is today's guest, Brian Nosek. Brian is a professor of psychology at the University of Virginia. He's also the co-founder and director of the Center for Open Science.

You might of heard Brian mentioned on this show before or heard him in the news. He's famous for a number of things but in part he's famous for setting up the Reproducibility Project, which we discussed on our episode with Uri Simonsohn a few months ago, that made a splash in the world of social science by trying and failing to reproduce the results of many psychological experiments in top journals. Brian and I are going to talk today about Open Science and what that means for the field. Brian, welcome to the show.

Brian Nosek: Thanks for having me.

Julia Galef: Why don't you start by talking about what you mean by openness in this context?

Brian Nosek: Openness for our purposes is two things, one is referring to transparency. The availability of — not just the outcomes of the research, as in the reports that I write telling you what I found, but also in the content of that research. The data, the materials, the methods, the code, the protocols, and in the work flow that produced those outcomes. I had some process of data generation, of design, of the analyzing that data of coming to inference at the end and sharing that.

Making that openly available makes it a lot easier for someone independent of me to evaluate the outcomes and decide whether they are credible or not.

Julia Galef: Yeah.

Brian Nosek: That is a core part of openness. The other part of openness that we really care about at the Center is openness as inclusivity. That is so that anybody who has interest, motivation, time can be involved in the research process in some way.

Julia Galef: Anyone, even not a scientist?

Brian Nosek: Yeah. Why wouldn't they be a scientist? Yeah, anybody. Why shouldn't they have ways to access and be involved in the accumulation of knowledge for the public good?

Julia Galef: Got it, okay. I want to just see if I can disambiguate between the different purposes of openness.

Brian Nosek: Yeah.

Julia Galef: I think one context in which people are used to talking about openness is with respect to media, or content in general, or I guess access to programs in

general. And in that context people often talk about openness as being about justice. Like, people have a right to this information.

I hear that rationale applied to openness in the context of science also, especially because tax payers are indirectly funding a lot of the scientific research via NSF and NIH funding. The argument goes: it's only fair for tax payers to be able to access that research without having to pay exorbitant subscription fees to these academic journals.

The fairness is one argument but it sounds like another main argument you're making is about the quality of the scientific research. Your goal being to increase the total amount of reliable true knowledge that science is producing, via openness.

Are you focused on both goals or just the one?

Brian Nosek: Yeah, no. It's a very good point. I'm perfectly happy with the moral argument of, if we pay for it we should be able to access it. That is a reasonable argument from my perspective.

But the latter is really the focus of our attention, which is if we want actually science to produce knowledge as efficiently as it can, then it's not just a good thing because it's fair to be open, it's a necessity to be open, for science to be able to do that.

The reason that openness is a necessity in science is that a scientific claim doesn't become credible because a scientist makes it. You don't believe me because I say I found this thing so therefore it's true and you say, "Oh, okay. He's a scientist, he must know."

A scientific claim becomes credible because you can look at how it is I arrived at that claim. What is the approach I use? What's the evidence that I have? How did I come to my own inference about that evidence? In principal you can come to agree or disagree, but it doesn't depend on me. The evaluation of the evidence is the evidence itself.

Without access to the evidence, to the generation process, to the outcomes, you can't evaluate. You can't decide if it's a legitimate claim. For that perspective openness is essential for just having science be science. Irrespective of a moral argument about access.

Julia Galef: Right. How do you feel about the current way that the incentives are structured in science? Are there incentives for openness or are there incentives against openness? How much of a piece of the puzzle do you think the incentives are?

Brian Nosek: I think the incentives are really at the core of the challenge for open science. That is because right now the primary incentive in science is publication. My career is advanced by the frequency of my publication and in the prestige of

the outlets in which I publish.

Openness is a value but it is irrelevant in the incentive structures. That is, it doesn't make any difference for publication right now for me, whether I was open with my data or my content or not. It makes no difference for publication if I show you my workflow, how it is I arrived at those conclusions. All I have to do is give you conclusions that are novel, that are positive results, finding evidence for a new claim, and are beautiful and clean and tidy. If I can give you that then I am rewarded in the current structure of science.

Julia Galef: When I've heard incentives discussed in this context before, people have made suggestions like, "Well, if we could just find some way to get the tenure process to reward people for openness, for sharing their data, for publishing replications, that kind of thing, then maybe that could solve the problem."

That seems really hard. That's a whole system you would have to change. But you actually, I feel like one of your recent approaches suggests a way for that doesn't go via the changing-the-traditional incentives route. Which was: you demonstrated that you can actually significantly boost people's adherence to the norms of openness by giving them an essentially meaningless badge or sticker.

This was both impressive and amusing to me. Can you talk about this excessive via the badge program?

Brian Nosek: Yeah. I'm happy to talk about that. I will agree with that point and then anticipate that I'll end up disagreeing that our own solution is sufficient, and go back to the tenure and promotion.

Julia Galef: Oh, okay. Excellent.

Brian Nosek: The idea of badges is that if we can provide some way to signal behaviors then if those behaviors are valued, people will adopt them.

This is very basic psychology. There are things that people do and they're hard to communicate, but signals are really useful. Signals can be used to communicate people's beliefs, their mindsets, their behaviors, lots of things that they want to have other people understand very quickly.

Badges are a very simple instantiation of that. When a large group of people work together in creating specifications for badges for open data, open materials, and preregistration, defining the study you're going to do before you actually do it. The Journal of Psychological science was the first journal to adopt those badges and they adopted them on January 1st, 2014. About a year and a half after that we started a evaluation of, did the badges have any effect on increasing rates of sharing?

The idea is the journal adopts badges and then they give authors, when their article's accepted, an opportunity to get a badge. If you'd like the open data

badge, then you have to just meet these criteria, put your data in a repository, make it so other people can read it and then you'll get a badge on your article saying that you did that thing.

Now that is trivial. It's just a little sticker, as you say, on the article. Scientists are largely grown ups, do they really actually need stickers to get credit?

No, in one sense — but in another sense that sticker is simply a signal of a behavior that scientists already value. The notion of openness and transparency are values that are accepted by almost all scientists as values in science. They're just not incentivized to do them.

The arguments against openness are pragmatic ones — I'll get scooped, other people might have concerns, people will attack me. There's lots of things that people worry about with openness but that's because the culture isn't open now. The having an incentive, even a trivial one like that, is a way for researchers that already believe in it and want to do it, but aren't getting any credit, to get some credit.

Psychological Science adopted badges January 1st, 2014. In the two years prior to that, the average rates of sharing data in Psychological Science was three percent of the articles. And we had comparison journals that were about the same.

Post-adoption, those rates started to increase to the point that in the first half of 2015, a year to a year and a half later, 39% percent of the articles had open data.

Julia Galef: Wow.

Brian Nosek: That's a 13-fold increase for that little dash. Then no change in the other comparison journals.

The point isn't that badges actually are these huge motivators and they just force people into doing something crazy, the point is that the value is already there. Researchers recognize this is useful. They just don't have any reason to do it but if I want to signal to the readers of my article that I have a lot of credibility, I have a lot of trust in my evidence, I value the core practices of science then the opportunity to earn a little signal of that might be sufficient for me to go ahead and do those behaviors. Clearly, that had a big impact.

Julia Galef: Wow. It's a huge impact, even huger than I would have predicted from that logic ahead of time.

Brian Nosek: Oh yeah, it was much bigger than I predicted, that's for sure.

Julia Galef: That's so striking. Do you think that there's any reason to think that this particular context, this particular journal or time period or something was unrepresentative in any way?

Brian Nosek: Yeah, there are good reasons to think that the impact when badges are adopted across journals and disciplines won't be quite as strong. The particular reason to think that is that the concerns about reproducibility are at the forefront of researchers minds, particularly psychologists' minds.

Julia Galef: Yeah.

Brian Nosek: There may be some degree of compensatory reaction that is facilitating that of, "No, no, no. We're doing well here," and so I feel extra motivation to do it. Whereas if it's in a community where no one is talking about those issues, they may say, "Pfh, badge? I don't need no stinking badge," and they move on.

Julia Galef: I'm so pleased that you found a way to work that line in.

Brian Nosek: Every time I talk about it I have to find a way to mention it.

That is unknown and we are getting more journals adopting badges so we will have opportunity to see the extent and the impact of that.

Julia Galef: Yeah.

Brian Nosek: That does prompt the earlier point, which is, is this enough? Is it enough to just have badges and other simple signals where people will change the behaviors themselves?

I don't think that's the case, despite being really positive about this particular intervention. The reason is that the incentives for science are embedded in a very complex ecosystem of multiple stakeholders. I am driven both by that journal and the other journals I try to publish in, I am shaped by the funders that decided whether my grants get funded or not. I'm shaped by the scientific societies of which I'm a member that establish the norms and how the styles of how the community operates, what's the right way to behave. I am very strongly shaped by the institutions in which I am a member, I am employed by that decide whether I get a job and whether I keep that job.

Julia Galef: Yeah.

Brian Nosek: All of those are both creating and reinforcing the incentives that drive the researcher's behavior. If, for example, the tenure committees never change their decision practices, and it's all about impact factor and volume of publications…

Julia Galef: Can you just explain what impact factor is?

Brian Nosek: Sure, the impact factor is the citation rate, on average basically, of a journal. Not to say what any article in that journal has been cited but an overall indicator of how many people are citing articles from that journal.

This is a blunt instrument approach to deciding how good someone's work is,

because it can be used just as a heuristic, "Oh, it's in that famous journal that lots of people cite, so it must be good research."

Julia Galef: Right.

Brian Nosek: No tenure committee or hiring committee would say that, all's we do is count the number of papers and count up how prestigious the journals are.

But it's very hard to avoid influence of that. One, because the community now takes it very seriously, and two, because in a lot of these cases the people that are having to deal with the information are very busy. We get 150, 200 applications for a job in our department. What are we going to use? We can't read all of the articles of all the people that have submitted applications.

And so there is an easy tendency to grab on to these heuristics. If those don't shift to some degree, if we don't work on the incentives there as well as with funders, as well as with publishers, then each of them will push back on the attempts to shift on one dimension.

Julia Galef: Yeah. It also seems like a tough coordination problem in that you ... It takes time and effort and you're putting yourself out there. You're taking on extra risk of, as you say, being scooped or being disproven, having someone try and fail to replicate your research. You're taking on all those risks — and that's worth it if you get rewarded, and it's also worth it if that's what the culture expects and you would get punished for not doing that. But it's hard to shift from the equilibrium we're currently at, where that's not considered obligatory or expected, to the other equilibrium.

Brian Nosek: Right, that's exactly right. This is a classic coordination problem. You can hear this talking to researchers, which the conversation often goes, "Yes, I want to do all those things. I want to be open, I want to preregister, I want to do all that stuff but I won't be able to keep my job or I won't be able to get the post-doc that I really need in order to get to the faculty position that I really want." That sense of risk, given the uncertainties, and the lack of incentives directly for them really makes it a harder one to change.

At the same time this is a different situation than coordination problems where people don't agree on the solutions. Here we have a huge opportunity and that is that the values are already shared and people already have, not everybody, but there is a lot of shared sense of what a different reality could be and what would be good. It's just, how do we get there?

Julia Galef: Yeah.

Brian Nosek: This presents all kinds of opportunity, both for small scale interventions moving up to scale and for coordination solutions like pre-commitments. We can imagine a coordinated effort to say, "I am willing to, what's ... Pick one thing, make all of my data openly available if 40% of the researchers in my field are

also willing to do that.

Julia Galef: Yeah.

Brian Nosek: Everybody logs in to a service that records their commitments. At what point will they trigger my behavior? We define the universe of the ... There's 2,000 researchers in your field, as soon as it gets to 800 then you'll get your email, time to go open. Now you're an open researcher.

Julia Galef: Oh, I like that. It's sort of like leveraging a Kickstarter solution for this coordination problem.

Brian Nosek: In some ways, right. There's been, like — changing the Constitution uses this. You have to have so many states agree that this Constitutional amendment should be changed and then it goes. It's reducing the risk, it allows people that actually already hold those values to express, I hold those values and am willing to do it, but doesn't make them go alone to get it done.

Julia Galef: Right. Overall I basically agree with you that we have a sort of consensus on what would be best for the entire group, the entire endeavor of science.

But there's one piece of that question that's not as obvious to me, that there's a clear answer to what the best approach would be. And that is the potential free rider problem with data sharing.

One of the arguments for not being open about data sets is that it takes a lot of effort and resources to collect data that you're going to use for your research and if you know that everyone else gets to use the data that you collect, then that reduces the incentives for people to collect data. Much in the same ways that intellectual property laws are designed to make it so that people have an incentive to put in the time and effort to invent something or discover something.

Are you concerned about the free rider problem at all — or if it doesn't seem like a problem here, why?

Brian Nosek: Yeah. It's a great question and I think it is a problem to the extent that people don't get credit for data generation itself. That's really at the core to me of this issue. Right now all of the credit is for one thing, publication. If we can diversify what one gets credit for, what are the scholarly contributions? It is a scholarly contribution to write beautiful set of code to analyze data. It is a scholarly contribution to design a brilliant study and to collect the data for that study, especially when it's a very hard data set to collect.

If we can shift the model — and there's already work, lots of people trying to think about ways to do this and progress being made — shift the model so that all of those become citable scholarly contributions where I get credit for having generated a data set, then it turns into my interest is to have other people analyze that data and to use that data. If they ignore it, then my scholarly

contribution is ignored, just like my publication getting ignored is not good for me either.

This is understood well enough that even NSF and NIH have both moved to have their description of when you submit your bio-sketch for what you've contributed as a researcher, it's no longer “list your five most important publications.”

It's now explicitly “list your five most important research contributions.” They say that could include software, that could include patents, that could include data sets, or something else. Just say what those contributions are. That is a very nice step, one step towards diversifying what are the rewards of science, not just the publication but the other components of the process.

Julia Galef: That is really interesting. Do you think that researchers currently or that it's plausible that researchers will soon in the near future actually have as much respect for that kind of contribution? For that non traditional contribution, as compared to the traditional, published in a high impact journal contribution?

Brian Nosek: There may be an age effect for speed of acceptance of such interventions but there is ... Because funders are already recognizing that, that's a good thing, journals or publishers are trying to in the sense of data publications and some journals that are really about sharing the products of other things. Of course they're embedding that in a journal article because say, we can't get our mind out of the "it has to be in an article" to give credit for it. Google Scholar is moving toward, it seems, toward acknowledging data sets as scholarly contributions that you can search for.

Then the many many repositories like the one that we operate, the Open Science framework, make all scholarly objects citable units. That being a citable unit where you can actually appear in the reference list — we almost don't need to persuade people that these are things that can be contributed. If they start getting cited they will just become things that provide value for researchers. I think we can do it naturalistically in some ways, rather than having to persuade the skeptics.

Julia Galef: There's one aspect of openness we haven't really touched on yet, which is openness surrounding the process of peer review. Currently, for those listeners who don't know how it works, the scientist submits their article for review to a journal and the journal sends that paper to several reviewers, who are other scientists in the same or similar fields, who presumably have the expertise to evaluate “Is this a good study, should it be published?”

The author of that article is, their name is visible to the reviewers. But the reviewers themselves are anonymous. The original author never finds out who reviews their paper, who decided whether it was good enough or not good enough, the public never finds out.

There are some arguments that openness should also include making the names of the reviewers public, not anonymizing them. Partly because of the risk that reviewers currently can block new research just because it undermines their own theory or it undermines research that is relevant to their theories, and that's bad for the scientific process. But also because not having your name public means there's little incentive for quality control. You can ... Why go through this research with a fine tooth comb to make sure that it has a sound methodology, if no one's ever going to find out that you were the person who let it through, if it turned out to be bad?

Brian Nosek: Right.

Julia Galef: Is transparency of peer review something that you think is promising or not?

Brian Nosek: I do think it is promising for the reasons that you described. Right now, peer review is entirely a service. By that I mean that the researcher gets no credit at all for doing it.

Julia Galef: The reviewer?

Brian Nosek: The reviewer, right.

Julia Galef: Yeah.

Brian Nosek: The most credit that I get being a reviewer is I add a little line on my vita that says I once reviewed for this journal, and that's worth basically nothing.

Julia Galef: That definitely doesn't give you incentive to review more than once.

Brian Nosek: Right.

Julia Galef: Unless they give you a tally mark next time.

Brian Nosek: Right. Peer review is super important. In the current model it is the gatekeeper to what is published versus what is not.

Even in alternate models, peer review is the means of evaluation, of deciding the worth of different scientific contributions. It plays a very important role in science, even though right now it's perhaps not done as efficiently as it could be.

The potential gain of transparency, before talking about the risks, is that if my reviews are known then I get — right now only the editor forms an opinion of me in terms of my quality as a reviewer. If they're known that I reviewed them, then I have a whole new potential source of reputation gain.

Julia Galef: Right.

Brian Nosek: For people that are excellent reviewers, and I've seen enough reviews now

where I've been either the reviewer or the author, where it is amazing scholarship in some of these reviews, like, "Wow, this person really unpacked this issue so brilliantly and identified these challenges and opportunities, et cetera."

… That could be a scholarly contribution of it's own. You can imagine a world in which a researcher who's at a, say, a non research intensive university, they don't have the resources to generate data and research, but they are a brilliant evaluator of research, could achieve tenure based on being a great critic. Of evaluating research so effectively that people say, you have to rely on that person for this field, because they really understand the issues and they can point out things.

Julia Galef: Yeah, that is a huge service to provide.

Brian Nosek: Yeah and why shouldn't it be a way to gain reputation?

Julia Galef: I like that. It's part of the general theme that you've been hitting, about broadening our conception of what counts as a contribution that people should be rewarded for.

Brian Nosek: Exactly and it's an inclusivity step, the other part of openness. Right now so much of the resources, it goes to so few scientists who happen to be at institutions that have tons of resources devoted to research. Almost all the research output is from the top 100 universities.

Julia Galef: Yeah.

Brian Nosek: There are so many really smart, really capable people at places where it's not possible to do a generative research program with any sort of speed because of the resourcing needs to do it. They have so much to contribute in terms of knowledge, skill sets, and everything else, that review is one very obvious way to start to be more inclusive of how that is a real contribution to science.

Julia Galef: One thing that I like about your center is that you aren't just talking abstractly about the importance of openness and trying to promote this idea, you're coming up with these pretty clever and innovative approaches to causing that to happen. Some of which we touched on already.

One recent example in this category is you've recruited people to participate in a prediction market, to predict which studies are going to replicate. Can you talk a little bit about the motivation behind that project?

Brian Nosek: Yeah. This is really a fun addition to the replication work that we've been doing. The idea was started by a couple of economists in Sweden, Anna Drevor and Magnus Johansson, who approached us to say, "Can we try this?" I thought, that's a great idea.

We took a set of studies that we were doing for a large replication project and

ran predication markets on them, invited psychologists or other behavioral researchers to be involved in the markets, gave them 100 dollars each and set bets on the studies. The market price would go between zero and 100 and if you were buying at a higher price, you're betting more that it's more likely to predict, 100 would indicate 100% confidence that this is going to replicate. Zero would be indicator of the zero percent likelihood of replication.

We got a full range of predictions based on the ending market prices for these different replications that were ongoing.

The incentive for the individual participants in the market is that once we got the returns on the results, it would pay out. If it was successfully replicated, anybody that was holding a share would get a dollar for each share that they had. 100 cents. If it failed to replicate, shares you held were worthless. I don't get any money for those.

What we found was that the market was quite well calibrated for anticipating the results that were observed in the replications —indicating on a substance level that researchers have some knowledge about what's likely to replicate or not.

That's useful to know, that when people have priors that say, "Oh, I'm not so sure about that result," that those are worth at least taking seriously. Whether or not they end up being true or not, we don't know, but at least paying attention to that skepticism or non skepticism if people really believe it.

Then other opportunities emerge if predication markets become quite effective at anticipating replication success. For example, prioritizing which things to replicate. We can't replicate everything. Resources are limited and the more we put resources into replication, the less we put resources into innovation. We need to be as efficient as possible between the two.

The opportunity with doing some markets is to identify those projects that are, or those findings that are very important, that the community feels very uncertain about. And prioritize funding for those where it would be devastating for a field, or actually very useful to know that this isn't actually a viable direction, so that the resources on innovation can be placed in other directions to really advance them more quickly.

That's been the real success of that. Now we have a number of subsequent prediction markets ongoing for other projects that are replication projects to see how viable this is as an approach.

Julia Galef: Did you get any pushback about the prediction market idea? I find that people often feel like betting is a little mean spirited, especially if you ... It's a signal of low confidence. Which is as it should be, but when you're talking about events that are high stakes, high emotional investment for people, I find that that can feel calloused to people. Especially if you're benefiting, financially benefiting from someone else's failure.

Brian Nosek: Right, yeah. I certainly can resonate with the reaction of that. There's prediction markets for likelihood of death by different people, or how much damage will occur if you have a war, in this way or in this region. There are prediction markets to predict some really important and then dramatic stuff. You're like, "Wait a second, you're betting money on whether people are going to live or die?" Or in this case, their work is going to succeed or fail to replicate?

I totally understand that sort of feeling, that reaction — but the whole point is to get the person that is making the prediction to be invested in getting it right. That's really the core. We bring to every prediction we make about the world, we bring a lot of our own motivations, what we want to be right, what our ideologies are, how we understand the world.

When money is on the line, the motivation is more focused on the money. I have a particular reward here. It's not showing that my beliefs are right, it's actually giving me — the money actually removes it from my beliefs, what I would *like* to be. Of course I want your study to replicate, I like you, I think you're so smart. But if I have to put money down on it, now I feel a little bit less certain about whether I'm going to bet on it.

It actually is a way to pull people back from all of those feelings and emotions and good things that connect people and make the world all nice and shiny. I think it serves a very important purpose because it creates that investment in accuracy.

Julia Galef: Actually before we continue on this thread, I'm wondering — were you going to talk more about the potential risks of transparency of peer review? Did I cut you off in the middle of a thread there?

Brian Nosek: I forgot about that part. That's really ... It's easy to say, "Oh, we should be transparent and reward people for positive evaluations. The pushback that happens on transparency on peer review is the potential for retribution. If I am a junior researcher critiquing the famous researcher in my field then that person might get angry with me and make it harder for me to get a job or keep a job or otherwise.

Julia Galef: Yeah.

Brian Nosek: Part of that is we have to acknowledge there's unknowns there. It's possible that a transparent peer review process would be more risk for junior researchers than a closed peer review process.

Julia Galef: Yeah.

Brian Nosek: I actually think it's the opposite. That's because the ... It was actually related to the point you made introducing transparent peer review, which is: that senior researcher is able to do a whole lot of things without accountability in a private system.

When you submit your paper that has a different point of view than that researcher, they can kill it, and they can kill it in really egregious ways because they're a senior person and no one knows that they did it except for the editor. Of course the editor is the one that does know but is often more junior than that very senior person, and can't stand up to that senior researcher or risk offending them.

There's all kinds of bad behavior that's easier to do when there isn't transparency. Transparency at least allows you to detect it more easily. If I am a junior researcher critiquing some senior research, other people can see how that person responds and both our reputations will be affected.

This is a prediction, but my prediction is that transparency in the peer review process actually decreases the rate of misbehavior rather than increases it.

Julia Galef: Good. Yeah, I'm glad we touched on that. That seems like an important point. I don't know if this is a feasible experiment to do but it would be interesting to cash out that prediction concretely and put some money on it, to see if we were correct about the effects.

Brian Nosek: Yeah. There has been one experiment that I know about on transparency of peer review. It didn't check about retribution but it did check on if people, when they have to sign their names, are they less critical of the research? That would be the other concern, is everybody just says, "Oh, this is all great." They didn't find that.

Julia Galef: They did not find that?

Brian Nosek: No, they found no difference in the extent of the critique from transparency or not. Take that as one study, but that was the one study that's been done so far that I know about. There's probably others.

Julia Galef: If only they could also check rate of researchers avoiding each other at departmental parties or something!

Brian Nosek: Yeah. We now have all of our cellphones so we should be able to do that automatically. The location tracking —

Julia Galef: Right, right totally.

Brian Nosek: Attach theories to phones and then we'll know whether people — I love it. That's good.

Julia Galef: I like how you think.

In our last few minutes I wanted to continue down this thread of exploring the critique about tone — that this whole open science thing is like, "Yes, openness is valuable and virtuous and so on," but the fact that the pro-openness crowd has been pushing it so hard and criticizing, using openness to critique other researchers' work, that that is kind of mean spirited.

This is not the dominant response to the openness rallying cry but it is ... It's not uncommon. The responses have ranged all the way from “Yes, thank you. Thank God someone is finally talking about this problem,” to “You guys are methodological terrorists,” for example.

Maybe we should give one pretty striking example which was the ... Actually I guess this wasn't about tone. I was thinking of the case of the power posing research, where you had these two researchers who both authored this famous research showing that standing in a powerful pose can make you feel more confident and powerful and has all these good effects.

And after that failed to replicate, the researchers just went in both totally different directions. One of them said, "Yeah, I accept this. I no longer think that power posing is a thing." The other researcher just stood her ground and was like, "No, it's still a thing and the openness side is wrong."

It's just been really striking to see the vast difference in how people responded to it. I guess yeah, I'm wondering if you have any thoughts about whether there is any validity to the critiques about tone, about mean spiritedness or anything else?

Brian Nosek: Yeah, this is a important issue in one way and — unimportant is not the right word, but as sort of a side issue in another way.

The way that it’s a side issue is that people behave badly everywhere. Especially on the internet.

Julia Galef: Yeah.

Brian Nosek: The fact that there is bad behavior among scientists on the internet is not news anymore than any common section on any news website.

Julia Galef: Yeah, I feel like you guys have been pretty polite and restrained in the grand scheme of things honestly.

Brian Nosek: Some have and some haven't. Some have been real nasty.

The example you gave of power posing, the disagreement between Dana Carney who has said, "I no longer believe this research," and Amy Cuddy who is still advancing some of the points of the theories about power posing, that is a normal scientific disagreement. They have two different views of what the state of the literature is. They both have been very responsible, responsive, and careful in how it is they talk about those issues, even though there is clearly a disagreement.

In the same domain, Amy has been called lots of really nasty things and personal things. Not just critiques of the research but critiques of her as an individual in the field. That's just ... It's gross. Why are we doing that? There is a reality there that there are people behind the science and one has to recognize

that of course critique hurts.

I've been critiqued my whole life on everything that I study, and it doesn't feel great to get critiqued but that doesn't mean critique is inappropriate.

We do have to recognize that we are human and we are going to respond to things in different ways. My feeling on the overall issue of tone is, having been on the receiving end of real harsh critique and on giving critique, and I hope I don't do it harshly, I hope I do it constructively. My overall aim is that I can't control my reputation but I can control my integrity. The way that I can focus on how I give and respond to critique is to think about how I want to behave as a person. If other people are going to misbehave and talk nasty and do things that are inappropriate, well that's ultimately on them.

Julia Galef: Yeah.

Brian Nosek: If I spend my time worrying about, "Oh my God, they were nasty to me in the public eye and I'm going to lose my reputation," if I'm worrying all about that then I'm not likely to maintain my own integrity for how I think I should behave.

I think in the long run that's a much bigger benefit to me being productively engaged in what is supposed to be a contentious skeptical environment. Science is all about skepticism and critique and clashing of ideas, and instead try to value each person as genuinely trying to just figure things out while also, of course, having their own ego personal investment in all of this. Just try to tread lightly.

Julia Galef: Yeah. That does sound like a pretty valuable mindset to have.

You know what else you could do? I just thought of this, Brian. You could add a fourth badge that's the “niceness badge,” and you get that if all of your critiques have been polite and respectful.

Brian Nosek: Perfect, yeah, but who gets to be the judge of the niceness badge? I like it.

Julia Galef: All right, we're just about out of time for this section of the podcast. I'll wrap up this conversation now and we'll move on to the Rationally Speaking pick.

[interlude]

Welcome back, every episode we invite our guest to introduce the Rationally Speaking pick of the episode. That's a book or website or something that has influenced their thinking in some way. Brian, what's your pick for today's show?

Brian Nosek: My pick is from grad school when I was learning about these issues of challenges of reproducibility and open science, and realizing that my ideal of

science from second grade is not actually how science is done.

What was stunning to me in learning about these issues is that they have been understood for a long time. There have been papers in the 1950s, 60s, and 70s all detailing the challenges of low power, of lack of planning a research, of flexibility in analysis. They also outline all the solutions that we're pursuing now.

What was amazing in grad school was to realize that the problems and solutions have been known for a long time, it's just that the culture changes so hard that they hadn't been implemented.

So, a paper that really inspired me at that time is one by Tony Greenwald, who actually is my academic grandfather, my most frequent collaborator. He wrote a paper in 1975 called "Prejudice Against the Null Hypothesis." The point of the paper was to show that people think that finding no relationship, a null result in a study, means that it's less meaningful a study and we should ignore it.

Julia Galef: Yeah.

Brian Nosek: He talked about what the consequence of the prejudice is in terms of decreasing the credibility of the published literature. You can find that paper just by Googling it.

Julia Galef: We'll link to it on the site.

Brian Nosek: You read it today and you could be reading it as if it was written yesterday rather than in 1975. To me, that paper along with a variety of others from the same time period was just a revelation. It really has inspired me to do the work that I've been doing.

Julia Galef: That's really cool in some ways that people generations ago were saying this — and also a little depressing that nothing, not nothing, but little came of it. And we're sort of still having to tackle those issues today.

Brian Nosek: Yeah, Jacob Cohen, who is famous for introducing the concept of power, wrote in the 1990s, he had written his initial book in the 1960s, wrote, "We've been talking about this for 30 years and nothing seems to have changed." I just feel like a grouse at this point. I think it really is that the methodologists did figure out what needed to be changed. What they didn't do is apply psychology to the practice of science in order to actually get the change to happen.

Julia Galef: Yeah. Interesting. That is a cool piece of added value that seems like you're hitting an important mechanism there that we are missing before.

Brian Nosek: A lot is happening so it's a very optimistic time I think for science.

Julia Galef: Cool. Brian, we'll link to that paper on the website as well as to the excellent Center For Open Science. I just want to thank you so much for coming on the

show.

Brian Nosek: My pleasure, thanks for having me.

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