Voiceover:

As neuroscientists, our research is at the heart of our everyday lives. From designing experiments to analyzing data to writing grants and journal articles, this work brings us closer and closer to understanding the most fascinating and mysterious parts of our bodies, the brain and nervous system. But for neuroscience to have the greatest possible impact on our world, it must be rooted in a strong foundation of rigorous principles. You're listening to Pathways to Enhance Rigor: A Collection of Conversations, where neuroscientists come together to discuss how to embed rigor into every part of the scientific process. This podcast is a part of the Society for Neuroscience's Foundations of Rigorous Neuroscience research program or FRN, supported by the National Institute for Neurological Disorders and Stroke. FRN is designed to inform and empower neuroscientists at all career levels to enhance the rigor in their research and the scientific culture at large. In this episode, we hear from Drs. Emily Sena, Eric Nestler and Walter Koroshetz. They discuss the rewards of conducting rigorous research, how institutions, journals and funders can step up to enhance rigor across the field, and ways for individuals to shift the needle and tackling this collective challenge. Without further ado, let's hear about Rewarding Rigorous Science.

Eric Nestler:

Hi, I'm Eric Nestler, I'm Director of the Friedman Brain Institute at Mount Sinai in New York City. I also serve as Dean for Academic and Scientific Affairs. I'm a psychiatrist and molecular biologist and my research focuses on understanding the molecular basis of drug addiction and depression.

Walter Koroshetz:

I'm Walter Koroshetz, I'm the Director of the National Institute of Neurological Disorders and Stroke. Prior to coming to NIH, I was a researcher and neurologist at Mass General Hospital. I started in basic electrophysiology, neurobiology and ended up in clinical research really trying to identify biomarkers in the area of stroke and Huntington's Disease.

Emily Sena:

Hi, I'm Emily Sena. I'm at a University of Edinburgh in the UK. I'm a stroke association funded nonclinical senior lecturer. I consider myself a neuroscientist and a preclinical meta research scientist. Essentially I'm interested in trying to increase the validity and utility of animal models of human diseases.

Walter Koroshetz:

Okay, when this topic comes up, I always think back to my mentor, David Corey, who said that, "When you find something really interesting, you have to spend about a year proving it's not an artifact and then you can publish it." I think that is incredibly true that the rigor in which you do experiments is so important. You're in science, everything depends on your reputation. Your reputation depends on doing rigorous science. That is a field that people can trust and depend upon. Now, here at NIH, it's different but even more important because we are the stewards to the taxpayer's investment in neuroscience. Really, it's only high quality neuroscience that we can justify. We've taken on that as a really high priority within the institute and I'm glad to be able to be with this group today because I think we do have a lot of progress that we can make by working together to improve the rigor of science in our field.

Emily Sena:

I 100% agree with you, Walter. I think for me, personally, the benefit of supporting rigorous research is my research is essentially trying to get other people to do rigorous research and highlight the impact

and the prevalence of non-rigorous research. To do that, I think you have to be able to stand up to criticism rigor. It wouldn't work very well if I was presenting evidence and data that in itself wasn't robust in trying to convince people that these things are important. I think, like you say, I think it's been important to my reputation to be rigorous in what I do to show that you can be rigorous and produce good quality research.

Eric Nestler:

For me, every scientist's legacy depends on the record of scientific findings that they've published in the public domain. I've taken this very seriously from the beginning of my training to the present day. That I want every finding reported by my lab to be easily reproduced by others in the field. If we're publishing findings that are important, they warrant replication and will solicit other labs to work on those replications. I'm very proud of my track record that the findings from my lab have indeed been replicated. I think all scientists should adopt that same approach. As Walter said, I'd like to underscore the degree to which I emphasize in my lab regularly how we all have the incredible privilege of spending U.S. taxpayer's money to do all this research. Most of that taxpayer's money is coming from individuals who make less than even many of our post-doctoral fellows. Yet, somehow, we need to convince the public of the importance of the work that we do and the value of the work for society which relies on rigor and reproducibility.

Walter Koroshetz:

I think it's a complicated message. I think scientists need help from people who are experienced in messaging how to get this across. Just to revisit, as I mentioned, doing rigorous research has really been part of science from the very beginning. Our system is such that as an apprentice, you work in someone's lab you're an apprentice. You develop the technology and the methods, the way you think about science are largely influenced by the people you work with. The good news is that if you have really good people, it gets passed down. The bad news is that if the culture of science leads to bias and things that we'd rather not happen, then we have problems. I think we have to face the fact that the competitive nature of science has a psychological effect that if not recognized and checked, can lead to bias in how you design experiments, how you carry out experiments because everybody wants their experiment to work, whatever that means.

Walter Koroshetz:

What it really means is I think I know what I want the answer to be. Anything that goes against that makes me feel really uncomfortable. When you feel like that, the answer is not to make yourself to feel comfortable. The answer is to delve harder into the experiment and see what is really going on. I think that's something that if it's not really drilled into the trainees by their mentors, there's a lot of things that go on in our system that could lead to results being published that are not done rigorously that the results can be due to change. They may even be biased by, say, how someone selects one animal to go into one experimental group versus the other really unconsciously. Emily can talk to this but that's the kind of thing we need to, as our own organizations, really clean our shop, make sure our shop is clean.

Emily Sena:

Definitely. I think with the public, as scientists we've got a responsibility to share how or describe how research should be assessed in terms of rigor. I think the current pandemic highlights, with the COVID vaccine that has come out and then those communities who are scared to get the COVID vaccine because they don't trust science and because of this legacy of things that have gone badly in the past.

There's this mistrust. I think being open, having these communications, public engagement I think is an area of science that hasn't been engaged with as much as it could be or should have been. I think as we get better at that, I think we'll be able to communicate with the public how science works and why science is important and equip the public to be able to say, "I believe what you're doing is rigorous and is for the good rather than just for your careers."

Eric Nestler:

Walter alluded to the problem that we've had of folks in industry not being able to reproduce key findings from academic research. I think that sometimes the lack of reproducibility does not indicate the lack of rigor of the original science.

Emily Sena:

100%.

Eric Nestler:

Any of the findings that industry has had trouble replicating could well be due to the fact that details of the experimental design were not transmitted effectively in the journal of reports that perhaps the expertise and utilizing the same techniques in industry were not the same as they were in the original academic lab and so on. At the same time, we have identified many root causes of the lack of rigor itself within the academic enterprise and perhaps that's something else that we can discuss today.

Walter Koroshetz:

I would just add to what Eric said, is that if you actually look at what the pharmaceutical companies have talked about, they are generally talking about trying to reproduce something that was a brand new singular finding. Maybe it required a lot of technical expertise to get to that finding. That, I think, is different from a body of science in which there's 10 different labs showing that this pathway is important. I think that the pharmaceutical company reports overestimated the problem and it doesn't necessarily mean the work was not done rigorously. I think reproducibility is different. Now, I would say that, and Os Steward knows as well that NIH and NINDS, we've tried to look at reproducibility and in my mind, when you look at what a single lab shows in terms of an effect size and you try and repeat that, say in a multi-center study, the effect size drops sometimes almost negligible. But then if you bring the investigator in and they show what they did and you follow along, you see the effect size go back up again a little bit.

Walter Koroshetz:

It's not necessarily the work wasn't done rigorously but it's just hard to reproduce because of a lot of other confounders that occurred. Some of them become real interesting confounders as well that you could follow up, like the microbiome turns out to be a confounder and why some labs see one thing and another lab sees another thing, that's actually a scientific discovery. But in terms of moving to translation, I think the other concept I like to throw out is the idea of robustness of the finding. A lot of findings can be very sophistically significant but the experiments have to be confined to such a degree that when you expand out to a different species or a different strain or you do the experiment at night or during the day or you mix the females and males, then you get different results because that effect was just that so many things have to be in place to be seen, that is something that we generally ignore. We tend to actually hype up the value of our findings without actually testing how robust it is. When it goes to translation, a drug company, if it only works in one strain of mouse, forget about it trying to go

out to a population with different species and so many different races and ethnic groups. I think robustness is another thing I think that we should put on the table in this general bucket.

Emily Sena:

I was going to say we characterize that as a generalizability of findings or the external validity. It kind of feeds back as well to what you were saying about the difference of rigor and reproducibility in that the phenotype that you observe is a function of the interaction between the genotype and the environment. Depending on these little subtle changes, you're sampling from a specific part of the reaction norm. I think sometimes we sample here in one setting because of all these other confounders and then other times you sample over here because of different confounders. But that doesn't mean the study is not reproducible. It just means that the study has been done with limited external validity.

Eric Nestler:

I like to reiterate what Emily and Walter have both said, that the robustness of findings is absolutely essential. Finding can't be just small magnitude and happens to be statistically significant with a somewhat arbitrary threshold. It has to be functionally meaningful for the organism and then observed across inbred lines, across species and so on. I'd like to provide a tangible example about the importance of experimental detail to achieve reproducibility. This comes from findings from a colleague at Mount Sinai, Paul Kenny, who had developed a knock-out mouse, a line of mouse lacking a particular orphan g-protein-coupled-receptor. He first created the mouse at Scripps where the mouse exhibited a robust obese phenotype. This was not a subtle finding.

Eric Nestler:

When Paul moved to Mount Sinai, the same line of mouse, same genetic background, did not exhibit that obese phenotype. In fact, when construction was done in our animal facility, the animals were under greater noise and vibration stress, the animals exhibited a lean phenotype. Paul was able to relate these differences in weight, first of all to the chow. It gets to the importance of nutrition microbiome as Walter said and other peripheral metabolic factors, as well as ambient stress of the animal. Now when we published the results of our experiments, we rarely incorporate details of food chow, ambient stress and so on in the protocols yet a pharmaceutical company trying to replicate that obese phenotype would have a lot of trouble without knowing those details. I think the responsibility is on us. We could do better with reproducibility if we provided dramatically expanded methods sections for our papers.

Emily Sena:

There's been a lot of innovation in that space, Protocols.io have created this platform that allows you to put really detailed, almost SOP level detail, in this separate platform that you can, you get a DIY and then that's linked in the manuscript. It's a permanent identifier of exactly what was done in the study so that that level of detail can be replicated. This is another point, our conversation about generalizability reminded me of a really interesting slide that I saw at a talk that said, "If you want to generalize to times in the universe, you're doing maths. If you want to generalize to a particular universe you're doing physics. If you want to generalize to all life on earth you're doing biology, to all mice you're doing mouse biology. To C-57 black mice in your lab you might not even be doing science."

Eric Nestler:

Right.

Emily Sena:

I think that's the problem that we have, that there's been a lot of C-57 black six generalizations.

Eric Nestler:

Yeah. Well said. It's as if scientists, too large a proportion of basic biologists have done an experiment on one group of identical twins.

Emily Sena:

Exactly.

Eric Nestler:

That would be the analogy we [inaudible 00:19:05].

Walter Koroshetz:

At NIH, as I said when we started, we have to take real seriously that the science we fund is of the highest quality. Years ago, now, we started to worry that we'd need to be more active in this space. We brought to the attention of the community increased necessity to deal with rigor in experimental design but primarily focused on the idea of transparent reporting. A lot of the early work was then talking to investigators and then to journal editors. I think that has taken hold that there's much more ... Things are looked at much more carefully before they're published. Things like sample estimation, blinding, all the things to get rid of bias now are oftentimes in the publications and in the grants that we fund. The other whoever that we have is in the grant funding, NIH is not, people think of NIH as some kind of agency where we make the decisions about who gets the money. Not really, it's the community that makes the decisions through peer review.

Walter Koroshetz:

The American system is really quite democratic. Your papers are judged by peer review. What we've been doing is really try to educate the peer reviewers to start looking at the applications for evidence of rigorous research design. I think that's caught on but I think it is slower than we all hoped. We've done analysis, reviews, summary statements, it's a little bit patchy, the mention of rigor. But I think this year, just thinking about it going through the grants I'm reviewing for this council, it's actually substantial now where that is brought up in the summary statement. I would tell you if there's a grant on the edge and there's a comment there that questions the rigor, we're not funding that grant. There's real tangible actions that are coming out of that evaluation at peer review. Then also when we do things like, for instance, our Javits awardees where they get grants for eight years. We'll look at their papers ahead of times and assess the papers for rigor and the design. If we have questions, we won't give them that award. That's an honorary award.

Walter Koroshetz:

But I'll just end by saying that NIH's ability to really change the culture is minimal because in people's studies section, it's really hard to tell who is doing rigorous science unless you really know them or they have a reputation. I think that's really up to the community of scientists and the universities. When you train in medicine, you can write five papers but if you can't take care of patients, you're not going to get a license. The residency director is not going to approve you to go into practice. We don't do that in science. As long as you have the papers, you're okay. We really need to start looking at the skill of the

people we're training. It's not just the papers, sometimes it's basically luck if you happen to be at the right place at the right time and you get a great paper. I think the skill of the scientist is really what carries them over the long run. Some more emphasis on building skill sets and evaluating skill sets in the training group, specifically in the area we've been talking about I think would be important for the universities to pick up kind of the medical model adopted. What do people think about that?

Eric Nestler:

I agree, Walter, but I'd actually give NIH more credit for greater impact. We in academia take what NIH recommends extremely seriously. Based, I would say, on large part on recommendations coming from NIH, we have substantially increased the training that we offer our students, post-docs and faculty, too, with respect to rigor and reproducibility. Certainly with the notion of including sex as a biological variable, my impression is that NIH's edict to do that has been transformational for the field. It is no longer acceptable to study one sex whereas for the first 30 years in my career, the vast majority studied male animals only. At Mount Sinai, we've instituted formal training for all of our graduate students, post-doctoral fellows and faculty as well around rigor and reproducibility. We have specific training classes. We've incorporated rigor and reproducibility, robustness of findings into our IACUC reviews for animal based research and IRB for human based research. We expect individuals to go through repeated training every so often. It's not just a one-time thing, it's repeated for faculty, I think, every three years to ensure that we keep up with the changing landscape. Ultimately I agree that it has to be the individual universities and individual scientists who take on that responsibility. But we look very much to NIH for that guidance.

Emily Sena:

I think this is something very institution dependent. Even within institutions it's individuals who really drive this forward, at least in the UK from my understanding it's not something that's really being instituted widely across institutions. It's specific leaders within this space who, within their institutions, have made a lot of noise and have gained some traction but have been supporting each other. In the UK we've got the UK Reproducibility Network. That network has worked together. These people across different UK institutions who work together to support each other to put pressure on institutional leadership. But I think a lot of the movement in this space has been grassroots students, young researchers who have put together groups. In the UK reproducibility network umbrella we've got these reproducibility, they're kind of like journal clubs led by early career researchers where they talk about different activities, tools, pros and cons of approaches to be more reproducible in your work. It's definitely not necessarily the norm as yet, I don't think but there's a huge amount of movement and luckily there are lots of influential who are on board with this, which is why I think we're getting that traction.

Walter Koroshetz:

I think at NINDS, as people may know, we instituted an office for research quality led by a scientist Shai Silberberg who has really been a proponent of improving rigor in science. The plan that we have is kind of similar to what Emily said but formalizing it and giving some financial support to it. That's to develop kind of a consortium of what we call rigor champions. Those people, we would hope we could convene and arm them with the right educational tools and enable them to work from each other's lessons to improve rigor and science from their own environment. It's a pretty ambitious program and it really relies on what Emily said, having people who are, we call them champions. That's exactly-

Emily Sena:

What they are.

Walter Koroshetz:

... what it is. The website is RigorChampions at NIH.gov. I encourage everybody to join. The groundswell...you need sustainability. That's the problem with a lot of things that I see at NIH, it's like a blip, everybody is excited and there's another blip. Then they forget the old blip. This is so important we have to work on sustainability.

Emily Sena:

When I think about, sorry. When I think about when I present on reproducibility and rigor in talks, the most common question I get from the audience, especially from early career researchers is, "How can I convince my PI this is important? I think this is important. I've been following this movement. I want to do more reproducible, more rigorous science. I want to follow things like the open science principles but how do I get my PI on board?" I think this is where senior leadership, these champions have a role to play. I think when you have many senior people on board with these things, I think it's easier to convince them rather than the junior people on their own to convince their PIs that these things are important.

Walter Koroshetz:

Eric, what she just said is actually somewhat...should be disturbing if the young people have to convince their PIs that this is important. What does that say about our system?

Eric Nestler:

I think one of the biggest problems are the journals in which we like to publish our data. That the so-called best journals of the field do not contribute to rigor and reproducibility. They create a culture, we have to take some of the blame also. If a post-doc needs a paper in one of these so-called top journals in order to get a job, that's our fault. But then the journals have policies that are not consistent with rigor and reproducibility. Any finding that is outside a simple linear story is removed by the editors from these papers. There is pressure to keep methods as short as possible and to come up with findings that support this simple, linear story. That puts enormous pressure on trainees but it also creates a lot of pressure on PIs. If you do a quick experiment that's underpowered and by chance you get the result that you want that supports the simple linear story, everyone is happy. The post-doc, the PI and the journal happy and proceed but the scientific enterprise is hurt. I think we should institute as a field a much more stringent requirement on behalf of publication and not accept simple, linear stories.

Eric Nestler:

We don't know enough about biology to have a simple, linear story in my view. In physics and math you can have a simple, linear story. We don't yet know enough about biology or biological systems but where is something that is so simple and perfect, I'm skeptical of it, to be honest with you. I think that a lot of the incentives that have been created for the scientific enterprise go against rigor and reproducibility. It's going to have to be a multi-pronged approach. We'll have to get faculty search committees at universities to not simply look at the impact factor of journals in which people publish, they'll actually have to read the papers. I think search committees have stopped reading papers too often to really assess themselves how good a person's research is. We need to start incorporating increasingly into grant review committees and assess them on reproducibility and robustness of findings. I actually think that latter piece has already happened.

Emily Sena:

Ulrich Dirnagl at the Charité in Germany has been leading an award, the Einstein Award for promoting quality in research, which is really about trying to reward individuals who conduct high quality research or promote or champion doing high quality research. The British Neuroscience Association recently launched the credibility in neuroscience prize, B&A credibility prize, again, to try and acknowledge the individuals who are doing things or those teams who are doing things to improve research quality. I think there's new activity, I think these people who are champions of this work are realizing that we need to create alternative rewards and awards for people because I think the standard reward infrastructure doesn't, as Eric said, it doesn't reward being rigorous. I think these things will stop being valued. Receiving a credibility in neuroscience award I think is something that will be valued.

Walter Koroshetz:

I think this is the crux of the problem. As I said when I started, science is an apprenticeship field. Unless things have changed, my sense is that people who are in the universities know who the rigorous scientists are. Sitting in NIH, it's tough to read the grant but I'll bet you Eric could make a list of, "These are the best scientists in my place, I can believe them. These guys, I'm not really sure if I can believe them. These guys, no way I believe what they say." Now the problem is that science got so big that nobody could deal with that information. Everyone I started, when I was a student and would go to physics conferences and everybody would know who you can believe and who you can't. It was common knowledge that this person, what they say is probably not going to be right. Everybody knew it. Because things got so big, that's gone now. Although I think in truth, the best students gravitate to work with the people who have the reputation for being rigorous scientists. I think that is true. Some may go to people's labs so they can get a Nature paper but typically the younger people, they want to be really good scientists. They know they have to work for really good scientists and that means somebody who is rigorous. I think that still is an incentive piece to getting- [crosstalk 00:35:43]

Emily Sena:

I would maybe push back a little bit on that Walter, just in terms of I'm not entirely sure, given the diversity of scientists and that we have consensus on what makes good quality research. There are some people who will think about rigor in the term that we're talking about rigor but others will say a good quality scientist is somebody who can get nature papers and that's what they value. That's what they think is important.

Eric Nestler:

Emily, I think this is where all of us can contribute to and improve culture because we need to move beyond that. Not just count the number of nature papers- [crosstalk 00:36:21]

Emily Sena:

I agree.

Eric Nestler:

... look at the data and look at the significance of the findings. What impact did that report have on the field? Did it stimulate new research that was reproducible and then extended in important ways?

Walter Koroshetz:

What about the idea of the institution? I think the institutions, at some level they know who is rigorous and who is not.

Emily Sena:

But their incentives are different. Their incentives, you want the scientists who bring in the big grants, who have exposure. I may be being a bit cynical but I think-

Eric Nestler:

One aspect of our culture, at least at my institution is the expectation that a basic researcher's findings will ultimately be translated to humans. Not that everybody does translational work, we believe in fundamental basic science because we don't know enough about fundamental basic science to stop. We encourage basic fundamental science but there's the expectation that a body of work will lead, ultimately, to improve human health. We do inculcate among our trainees and faculty the importance of such translation. I think the expectation of translation encourages then rigor and reproducibility and robustness because only the findings that meet those criteria will be translatable as Walter said earlier.

Walter Koroshetz:

A couple of other things that we've done but they are really kind of nibbling around the edge is to try and set up platforms to do reproducibility. Dr. Steward I think ran one that we did for spinal cord injury years ago. We have one now that's in the middle looking at neuro protection for acute stroke. I don't know if people know this story but this is what got me into this area in 1990, which is when all the stroke trials starting failing in humans. If you were a rat, we could definitely cure your stroke but if you had two legs and walked upright, it wasn't working. What we found was one of the problems was the rigor of the research was not up to snuff. There was a big effort of the whole community to come together and develop criteria for rigorous research to go on to translation called the STAIR criteria. Those are still being worked on now. Now in stroke what we did is we funded a group, it's almost like they're doing a multi-center trial in animals of different stroke protection treatments to see which one is the winner in an unbiased fashion so we could take it to patients.

Walter Koroshetz:

In epilepsy we funded a program that was run out of Utah for the last 40 years where they have rigorous epilepsy models going on. Thousands and thousands of data points over the years that you can come in with your drug and you know if the effect is real or by chance. Then we're also building something like that for pain. These are platforms, as Eric was saying, to really test whether something is robust and a proof that things were as you thought they were. The problem is to do that for all the diseases. That's hard to scale that up.

Emily Sena:

That's difficult. I think what you described is really important, Walter. Back in 2015 I led a group called Multi-Part where we were trying to develop a framework for doing multi-center preclinical stroke studies. Again, one of the big issues that people had, these people came to Multi-Part because they were committed to try and improve translation of acute stroke but one of the issues that they had was, "Well, if we do a multi-center study, how am I going convince my post-op this is a good thing to be invested in because who is going to be the first to author, who is going to be the last to author? Who is going to get the credit?" That, in terms of career and the way we reward science, not thinking of team

science and collaborative science but the first and the last author and that was an issue in terms of how this would work.

Eric Nestler:

One of the other areas that's important for translation is for a basic scientist to design their animal studies in ways that have meaningful translational end points. For example, in my field looking at the biological basis of behavior, we can use rapid behavioral screens if we want to assess the importance of dozens of genes but those rapid behavioral screens per se are not going to provide translatability to the human condition. It really puts pressure on investigators to carry out much more sophisticated behavioral testing where findings and insights can be better translated to humans. Overall, I would say that the responsibility for improved rigor and reproducibility in science falls to several domains. We, in the academic world, look the NIH for guidance and support. We will continue to do so and hope that NIH continues to help us identify what's important for sustenance of our scientific enterprise and support by the public. But there's no question that universities must take on a central role in ensuring the rigor and reproducibility and translatability of data produced by our scientists and physicians. We have better knowledge of the individuals involved at the university level and we have daily access and control over them to educate them and show them the way of how to perform scientific studies on animals and humans, even cells, that are more rigorous, more reproducible and focus on the most robust forms that are most easily translatable.

Walter Koroshetz:

I think we hit on a lot of the topics. This is on one level a complex issue that we have to have a sustainable approach to resolve. But as director of a taxpayer's funded institute, I think NINDS is deeply committed to trying to work with the neuroscience community to do whatever we can to improve the quality and the rigor of the research. I'd say for us, that's our responsibility to the people who pay their taxes. But I think for all of us, as a science community, the reason we're here and the reason we should feel fulfilled about our careers is because we are contributing to the advancement of science. To do that, you have to do rigorous research. Sloppy research actually goes the other way and decreases the speed of advancing. It's just so important and trying to build a community of rigor champions to keep us sustainable, foot on the pedal is what I think NINDS is hoping to do with the help of the neuroscience community.

Emily Sena:

My takeaways would be, to follow Eric and Walter, I guess is there is a huge amount of activity in this space. There's lots of individuals who are committed to improving rigor, reproducibility and translatability of neuroscience research, which is fantastic. I think the reward incentive structure will play a big role in terms of really shifting the needle to ensure we do start valuing rigorous research in the way that I think we should do. I think training is very important. Eric briefly mentioned training in experimental design. I think there's lots of my colleagues would say they don't feel comfortable when it comes to statistics in experimental design. Those who are experts in that field, I think, aren't necessarily valued as much as they should be and utilized as much as they should be to shift the needle.

Emily Sena:

I think we're going to come up with lots of ideas about how we should do these things and how we should improve rigor and reproducibility. It's important that we have ongoing assessments to ensure that these things that we're implementing actually do shift the needle. Otherwise I think we're going to

overwhelm the community with all these different tools and ideas of, "Yes we should do this and you should start doing this and you should do it without any actual real evidence to show that you shifted the needle." That's one area that I think it's been disappointing there's been so little investment in meta research to show that these things are important before we start mandating that people should do these things.

Voiceover:

Thank you for listening to this episode of Pathways to Enhanced Rigor: A Collection of Conversations, brought to you by the Society for Neuroscience, the world's largest organization of scientists and physicians devoted to understanding the brain and nervous system. You can hear the rest of this series on your preferred podcatching app. Be sure to visit neuronline.org/frn to explore the other resources and materials created as a part of the FRN program. That's N-E-U-R-O-N-L-I-N-E.org/F-R-N. We'd like to know what you thought of this episode. Please take a moment and check the episode description for a link to a one minute survey. Supported by the National Institute for Neurological Disorders and Stroke, grant number 5R25NS112922-02. Drs. Os Steward and Lique Coolen are the principal investigators and senior producers. This episode was written and produced by Maya Sapiurka, Tristan Rivera, Emily O'Connor, and Taylor Johnson. Audio engineering and post production services were provided by Human Factor.