Sunday Discussion, Part 3: Epistemology Camp 2024

March 10, 2024 - Arizona

Transcript

[Greg Glassman]
Patrick, I want to ask—I want to answer the question you asked of Dr. Gigerenzer as though you'd asked me, and I want to leave you with what I think could scratch the itch for justice. Science finds validation exclusively, entirely, only through the predictive strength of its models. And then, to evaluate that, because I've always been—I've always—I know Dale's heard me say this a lot—definitions don't come flavored right or wrong. They're just consistent or useful, and I'd like you to look—use that definition as a lens and look how some issues refract through that. It's fascinating.

I like to start with the demarcation problem. We learn there from Wikipedia—just not go there and listen. And I always tell, look at Wikipedia. I love picking on those people. Some of the best and the worst things I've seen written on science sit there, but there's always something to talk about. But I would go on the demarcation problem. They say it's a problem. We learn that this has been a problem that has been unsettled, has been a problem for several thousand years. You're going to go, "Geez, I solved it right here. It's predictive strength. That is the demarcation." You go over to interpretations of probability, and you find that there's a war basically being fought over whether probability inheres in objects or in our heads. And you see there—it's interesting, there's another article, a Wikipedia article, "Foundation of Statistics," and you learn there too that it's Sodom and Gomorrah in terms of the confusion. And somehow the solution to the statistics foundation problem finds resolution in probability theory, and you get over there, and you find out that that's another conflict.

Armed with the understanding that science models find their validation exclusively, solely through their predictive strength, you look at these articles, and there's just not much problem for you. You have to side with the people that think probability inheres in the head. You also have to take exception to the frequentists that tell you that the probability of a hypothesis has no meaning, that hypotheses have to come flavored one or zero, like all propositions. That's the deductive curse that they're living with. And it's interesting, so they do a little probability of the frequentist sort, and you get that p-value. And if that p-value is magically 5%, now my proposition carries a one or a zero. It's all of a sudden, I'm back in deductive land again. It all seems nuts.

What else do we look at? Scientific misconduct. It's abundantly clear that when we unhinge validation from the scientific method, the charlatans are going to have a party. They're going to jump up and down and get all excited. And there's someone who has a fiduciary obligation to say, "You mean all I need is a couple of scientists to put on a masthead a study and some good p-values?" And yes, that's all you need, and it's done all the time. It's done all the time. So, I—and the utility of that definition expresses itself in the application and where it can take you. The ultimate hope is that when someone tells you that if you don't believe them, you don't believe in science, or someone's touting a model that's never demonstrated anything anywhere, you walk away. And I don't care if a guy's got a stethoscope, a computer, a Geiger counter, the white lab coat, all that crap—forget it. Until there's something of predictive strength, you've got nothing, right Malcolm?

Any questions?

[Roger]
I have a question. Is science real in this house?

[Greg Glassman]
No, it's part of my—see the man-made tools of science there on the wall? Science would be on that list too. Science is an interesting thing. My father was of the view that it may be the only discipline where knowing its limitations, boundaries, and the extent to which it can weigh in must be known. And it was on this basis that he said that conflicts between religion and science come about through confusion about what religion is and what science is. You have to be confused on both. He put himself in instant disfavor with the science framework committee in California. They thought he was going to beat up the creationists, but instead, the Science Bear got after them hard and told them that they were no better off than the people they were making fun of. Is that something? Imagine how enjoyable that must have been.

Questions?

We're going to shift gears here. Dale, I'm going to let you introduce Daubert.

[Dale Saran]
Sure.

[Greg Glassman]
I won't even try, but it was my idea to be talking about this here today because I think we have the talent. I'm not the guy—I'm not the—I don't believe in social crusades, but I think this Supreme Court decision could get reversed and done so logically. And we might know the people to put to that task to explain.

[Dale Saran]
So, I don't know that it's quite a departure. I think it's a natural—it's at least a point, it's an offramp in a lot of the things you've been talking about.

I was thinking about it as you were saying it, and it goes back to the question I just spoke with Dr. Garrett about, which is—he asked the question about how do you put quantitative measures to this where you've got both DNA evidence, for example, which has a very serious quantitative side to it, alongside in a context of a legal proceeding where—how do you do that where it's largely qualitative?

And I'll pick up there and say that what you have is—we use some mathematics. In civil trials, for example, you have to prove something to say it's more likely than not. We call it the preponderance of the evidence, and what that really means is—we joke in law school, 51% or 50.1%. It's more likely than not that something is true, and the person who can prove that wins the civil trial to your satisfaction sitting on the jury. It's only more likely than not. And there are other standards for proving damages or certain other things where the law says, "No, no, it can't just be preponderance, not more likely than not. It's got to be another standard: clear and convincing evidence." So where does that fall? You know, it's not quite beyond a reasonable doubt. It's more than a preponderance, so 62%, you know, you give it kind of a qualitative term. You try and paint a word picture to deal with that. And then of course we have this beyond a reasonable doubt, which is not beyond all doubt. So you're trying to put words to some kind of measure to get us to a standard of evidence. And really, to answer Dr. Garrett's question, the way you manage this problem in the law typically, it seems to me, in my experience, is they assign—it's who has the burden for failure to produce sufficient evidence of a particular matter. So in the criminal trial, we say that the government has the burden, and if they can't get to beyond a reasonable doubt, then they lose. So they bear the burden. That's how we assign the burdens where we think they should lie for the failure to produce sufficient evidence.

Now, this comes up where it relates to what Greg asked me, Dan, and others to talk about is scientific evidence in courts. And so just a brief history is in 1927, so I believe the same year as the S conference, was a case came out called Fry v. United States. And Fry was interesting. It was criminal, and it involved a lie detector test, the earliest version of it, you know, measuring your body's physiological responses to questions. And of course, immediately you think when you hear that, "Oh, let me guess. The government was trying to convict a guy using lie detector evidence." And actually, it turns out, no, it's just the opposite. A guy was trying to prove his innocence and said that the court, refusing to consider his clear polygraph, you know, had wrongly convicted him. And the court in Fry came out and created really the standard that would apply all the way through Daubert was the Fry test, and that was this idea about reliability of evidence, how you could call it scientific evidence in court. And it went 70 years almost. The Fry standard reigned. And in the interim, during that time, what happened was the passage of the Federal Rules of Evidence had been sort of compiled and put together in the '50s, and you had the changes to the Federal Rules of Civil Procedure, and then it brings us forward to Daubert, which became the standard. The Supreme Court looking and asking the question was, "Was Fry changed by the passage of the Federal Rules of Evidence?" And specifically, Rule 702 that deals with expert testimony, and the court, the Supreme Court, said, "Yes, in fact, it was changed." And so they threw out Fry, and they put this new standard in. I will tell you that of import, I think it's important to blame the lawyers. I got a plaque when I left flying to become a lawyer, and it was from all my helicopter buddies. And right on it, it just said—on the plaque it said, "Shakespeare was right." And that's what they gave me, you know, the first thing, "Let's kill all the lawyers." And I always thought that was a nice thing to take with me. I know Greg would have loved that. In fact, I might gift that to you at some point. But I think the lawyers bear—and the judges, maybe not the lawyers, but the lawyers in robes, worse yet—bear a good deal of responsibility for what we have now with peer review and all of this.

Because one of the things they found in Fry and going forward was that part of how you could get things admitted, you could get experts on the stand to be decreed experts and therefore to be able to pontificate to the jurors to a scientific or medical certainty, was the law said, "Well, we're going to look at this guy's resume." And that's part of how we decide whether it's scientific or not—are you published in any peer-reviewed journals? And once that became reified by the Supreme Court and decisions over time, to me, if you look at the when of some of those decisions, it tracks very nicely with the rise of these peer-reviewed journals, and suddenly you've got just garbage being pumped out.

I mean, if you're a corporate entity and you know the Supreme Court has said, "Hey, this is what it's going to take to get an expert opinion in," and you just start making journals, printing papers, and getting experts. You can look at whether it's food, nutrition, Pharma—pick any of those things, and you'll find a concomitant rise in the publication of these journals. I mean, look at how—I was just talking to somebody in line—you know, Einstein published the theory of relativity while a Swiss patent clerk. What was his expertise? What were your justifications for saying such a thing? I don't know if he would have passed the Fry standard. Would Einstein have qualified under Fry? Probably not, to be an expert. Can he even offer such an opinion?

And I think that really is at the heart of part of this problem. We've concretized these notions that peer review is how you get to a correct finding. I don't know, Dan, what else you want to say about that?

[Dan MacDougal]
I'll say what Jeff Glassman said: the best science is not peer-reviewed, it's top secret.

[Dale Saran]
It's a difficult thing to explain science to laypeople, particularly where they've got to make decisions. To Anon's point about what do you do when there's DNA evidence and they're going to come in and say, and Kurt talked about it—you try, you know, trying to instruct the federal judiciary and trying to train them that you can't really make the assertions that you're making. And so we kind of play these linguistic games to not allow experts to basically tell people, "That guy's guilty," which is really what prosecutors want, obviously. Or if it's two civil litigants, you know, you want to prove that guy's full of crap, we're right.

And now much of litigation has become a war of experts, but it comes out of these Fry and Daubert misunderstandings and misapprehensions. I think in the materials, Greg pointed to a Susan Haack piece. Did you put that in there? It's worth your time. Those of you interested in this notion of how we know what we know, both epistemological and ontological questions, it comes together in trials. That's where, at least for me, not so much in the laboratory. That's my laboratory; that's where it all comes together.

But I know that we've talked about—and Dan, we're looking at Dan Brother Harry—we're all thinking about ways in which Daubert can be kind of torn down. Sometimes the Supreme Court announces standards, and then in practice, it becomes sort of more observed in the breach than it is in the actual following. Life itself, a lot of times, will dictate how we actually have to deal with these issues of scientific evidence and what counts as expert testimony.

[Emily Kaplan]
It can be cost prohibitive. I had a really interesting conversation with Bobby Kennedy, who's done a lot of these cases, and he was explaining to me about how if you have a population of people that have been harmed in some way by toxic chemicals that have been thrown in, you know, the Hudson River or whatever, and you can tell very clearly there was this and then there was that, but it's corporate. And you have a huge industrial power that's basically saying, "This isn't scientific." He often finds that when he gets to the Daubert prehearing, he can't even move the case forward because it's such a gatekeeper. These small groups of people who have been harmed don't have the resources to go and pay a bunch of experts to do a study and have it be published in a peer-reviewed journal, but the industry side does, and they're ready for it. They've already got their defense made.

I'd love to hear you talk a little bit about that.

[Dale Saran]
I had a great example of this. Daubert has become such a gate that judges are supposed to keep out the junk science. I'll give you a great example of this in a criminal trial where it comes up. We're defending someone—in fact, it was what led to me meeting Greg and going forward. I was in a murder trial, and our client had been analyzed for PTSD, but not just PTSD—what's it called, CTE now, or we called it TBI at the time. So we had a client who had been exposed to multiple large explosions that had had an impact, and this was in the earliest days of the VA looking at this. There was a Dr. Maria Mortius, you can look her up, she was heading Walter Reed at the time, and she was doing all the studies on the consequences on troops of having bombs drop nearby or these other things and what effect it had on the brain.

They have a series of tests they can do to measure cognitive function or executive function, they call it. One of the—they offered, thankfully—the government was like, "Hey, since we've tested some of the other defendants in this case—there were eight co-defendants in this murder trial—do you want to have your guy tested?" And we were like, "Well, what the hell, sure, why not? Let's have him tested." They tested him, and we thought if nothing else, it could be useful if he gets convicted, which it was looking likely since five of his comrades were going to testify against him and had already taken deals. We thought it wouldn't hurt for extending mitigation when we get to sentencing.

They do the testing, and they come back, we get the report, and they basically tell us, "You do know that your guy's brain's badly damaged, right?" And we're like, "Sure." He was a simple guy, you know, he had had a lot of problems passing the ASVAB, and somebody like Patrick would be like, "I don't think you can fail the ASVAB." He did. It took him three times to actually get a score that would get him into the Marine Corps, and he had a really tough life, a black kid from East St. Louis. But he was badly damaged.

We had to have a Daubert hearing. Now, we want to bring this evidence in on the merits because part of the case involved, you know, was he wrong to have made a decision he made during this murder. He was given an order to do something, and defects in executive functions suggest that when you're faced with a lot of different information, you'll just fall back to the easiest heuristic, which is, "Hey, all orders are lawful, I'll just do what I'm told because I can't sort this out." And that was part of our defense—it became at the heart of our defense.

When we tried to get that evidence in—all these tests, clear science, you know, science, they did it—but at the time it was considered cutting-edge, new science. Most importantly of all, the government, who had tested this guy using their doctor who's running the tests out of Walter Reed, now objects and says, "You can't put that in, it's not science, it hasn't passed peer review, and it didn't meet the Daubert factors." We couldn't get past the Daubert hump. Here we are with this wonderful evidence, the government's own sciency people who are now going to come in and testify on our behalf, and it was only largely by begging and groveling and getting the judge to agree that we could put some of it in but not all of it. It was Daubert, it was that exact problem, which is we ran right up against Daubert: it's not peer-reviewed yet, it's not.

The funny thing about Daubert is it's interpreting Rule 702, which claims to be in 702. It says that anybody could be an expert. That's the crazy thing. Laypeople, you all have expertise. There are things you know about that no one can tell you about on a certain subject. We all kind of have our own things. The standard in the Rules of Evidence is if anybody can come in and be an expert, provided that they have testimony that will help the fact-finder in rendering a decision. It doesn't even need to be some recognized scientific field, but this is what we've come up against. When you add these Daubert factors, which is to say we've reified consensus science into valid science, I think that's a large part of why we are where we are.

Now it's a question of finding the right circumstances to take a crack at it, to try and find a way to break Daubert's hold on us.

[Malcolm Kendrick]
I'll just refer you to David Sackett, who you may have heard of. He was essentially the founder of evidence-based medicine. Brilliant guy, I met him a few times about 20 years ago. He wrote a series of articles saying that all experts should be compulsorily retired after three years because he was doing work in evidence-based medicine. He said, "My opinions have become too powerful. People are paying too much attention to me, and I'm not going to write or do any work in this field ever again," and didn't from that point. But he then wrote another article saying, "I have called for the compulsory removal of all experts after three years, but none of them have actually gone."

I think it would be interesting to try and see if we could work alongside that idea of, well, who are these people? Why are they experts, and why do their opinions carry all this weight when actually all they're doing is acting as a bar to progress and they have too much power?

[Emily Kaplan]
You think like, …
…people think something is scientific when it's not, right? So there has to be some litmus test.

[Dale Saran]
I think we tell ourselves a tale when we think that people decide things on a purely rational basis in court.

I was telling some people in line that I was very fortunate as a trial lawyer for many years. I've done hundreds of cases to juries, and I got summoned for jury duty not too many years ago, five, six years ago, in San Diego County. I thought for sure they would never let lawyers be on juries. No lawyer in their right mind would ever want another lawyer on the jury. I would never allow a lawyer to go back and be telling my jury what to think. I only want them to be listening to one lawyer, not that guy, and not even the judge. You don't know what he's doing.

But both the defense and the prosecution in this criminal case in San Diego County seemed to think I was fine to go back there. So now I'm a lawyer sitting back in the jury room. It was the best experience I've ever had for understanding jury deliberations. I've run mock juries and all kinds of things like that, so you get to see jury deliberations, but it's different when it's the real deal. It was an interesting thing to me. I'll say that I continue to have faith in the system. I still believe in juries getting it right. I think it's really hard to fool 12 people.

I think the idea that we need to have only this dry recitation and that's how trials will be decided is misguided. The common law system has served us well for many years. It doesn't always get it right, but I think the times that juries get it wrong are typically because of policy decisions that lawyers make about what they can hear or can't hear. Either we give them information they shouldn't have, or we keep information away from them that they should have. I think that's where you see miscarriages of justice.

Holmes, as much as I hate him because he was a pragmatist, had a great line about the life of the law not being logic but experience. The Rules of Evidence aren't logical rules. For example, there's a rule of evidence that says you can't use propensity evidence to prove that somebody committed a crime. For example, if you go to the bar every Friday night and get drunk and fight people, and now you went to a bar and there's a fight and you're being blamed for it, we don't allow the jury to know that your history is such that you go to the bar every night and get in fights.

There's a great opinion about this where the court says it's not because it's not probative. We're not saying that it wouldn't tend to prove or make it more likely than not that you did it. We disallow it because it's so probative, because it's too probative. The risk is that we will convict innocent people not based on what happened that night but for things they did in the past that we haven't convicted them of. So, there are choices made—evidentiary choices, filters that we impose.

I think it would be unwise to think of ways in which we're going to reduce this to a formulaic recitation. The other part is that courts, if any of you have ever been part of a court—Greg was in the Fanny Willis trial and some of us were talking about it—it's extraordinarily compelling. It's a drama. Court cases and courts are very much like plays. They have actors, and then one goes off stage, and out comes the next act. It's like high school theater productions, but if you're a part of it, there are things you will pick up, things that will be important to you, things that the jurors pick up that no amount of watching and not being there will really attune you to.

I once had a case where a guy got convicted but got a great sentence and didn't get kicked out of the Marine Corps. He clearly had done it. At the end of it, one of the jurors pulled me aside and asked, "Sir, can I talk to you for a minute?" I said, "Sure," and he said, "Did anybody come in here and tell the truth this whole trial?" I had to laugh because he had gotten to the heart of the matter. Everyone who came in lied—the victim, the alibi witness, every defense witness, every prosecution witness. They had all lied. Those of us who knew the facts were just along for the ride. The jury saw right through it and got to the heart of it. I say that because someone reading that record would have concluded something vastly different, but those of us who were participants, even though my guy was convicted, at least he didn't get a bad conduct discharge. I thought the jurors got it perfectly correct. They saw right through it all.

The fix for me would be to tear down Daubert. What would I replace it with? Not lawyers. I think it would be a system in which we trust the juries more and downplay the necessity and expertise of the expertocrats.

[Emily Kaplan]
I don't want to dominate all the questions because Dale, I do have access to Dale, so other people should feel like they can ask questions too. But the other thing is like just procedurally, Congress ratified it, right? So, it's not just up to the Supreme Court to overturn it. If the goal is to get rid of it, what do you do about the Congressional oversight?

[Dan MacDougal]
It's in the federal Rules of Evidence, which is not passed by Congress. It's by committees, judges, and lawyers, and approved from time to time. It's a lengthy process, but the interpretation of the federal rule could be changed by the Supreme Court. I want to address your question of the very charismatic expert who just bamboozles the jury. I don't think that's as big a problem as critics think. The solution is to have a good lawyer cross-examining that son of a bitch. I don't care how many times he succeeds in bamboozling the jury; eventually, he'll come a cropper and won't be able to testify anywhere after that. I've had that happen in my experience.

[Anton Garrett]
Were there trials held on Zoom during the pandemic, and do you think a lot of information was lost in terms of missing body language, seeing people sweat, and such?

[Dale Saran]
I was horrified, and I made my thoughts known when courts started shutting down. I couldn't believe it. There's a standard that the Supreme Court's talked about in some of the cases involving some of the World War II cases. The Supreme Court has said, "Unless there's a war on, we don't shut the courts down." Given what we knew about what Jay had already shown very early on about the likely fatality rate for COVID, I was horrified that courts were willing to shut down.

I'll share with you all that wasn't accidental. For years leading up to this, judges—federal judges and others—had been in lots of training. You can look back where the government was training our judges on how they had to work with the government in the event of a pandemic. Part of the pandemic response included shutting down the courts. The training for that had been put in place years before. I thought it was a clear violation of rights. I know a lot of judges tried to find ways around it, but some people challenged it unsuccessfully. I think people's rights were clearly violated—their legal rights to get a fair trial, to get a jury trial.

[Anton Garrett]
But specifically, do you think it altered results because people couldn't see witnesses sweating, shaking hands under the table, that kind of thing?

[Dale Saran]

No question in my mind. Not a shred of doubt in my mind that you cannot get the same result from looking through a screen. That's why we have trials in person. It unquestionably makes a difference.

[Dan MacDougal]
There are cases where a plaintiff is limping to the witness stand in a personal injury trial, and then a juror spots them sprinting down the hall during a break.

[Dale Saran]
Those things happen, not just on The Brady Bunch for those of us who grew up watching that. That is a real thing that has happened many, many times where witnesses will get on the stand and have some kind of affect in front of the jury, and then the jurors will later see them behaving differently and conclude that the person is a fraud.

[Question]
So, if a juror sees someone outside court engaging in behavior they think is inconsistent, are they mandated to not take that into account, or are they allowed to take that into account?

[Dale Saran]
There’s the schoolbook answer and then the real answer. The schoolbook answer is no, they're not supposed to consider that, and the judge gives them instructions about what they can and can't do, how they should consider evidence. We give them all kinds of instructions: you can only use this for impeachment, not to determine guilt or innocence. Depending on the nature of the impeachment, you’ll be like, "Right," and then they go back into the jury room. The law kind of provides an out and says, "We don’t want to know," because they say, "Hey, no one will ever be able to question you about what happened back in the jury's secret deliberations."

So, I think that the answer is, yeah, they're not supposed to consider extrajudicial matters. You'll see juries get sequestered, told you can't look in the papers, you can't see this, you're not supposed to consider this other stuff. But on the other hand, they're human beings. Suppose you saw something that made you think this guy is innocent. Would you go back there and send that guy to jail because you weren't supposed to consider that when whatever you saw made you firmly convinced that he wasn’t guilty?

Jurors are allowed to talk about their experiences. The rule is that no one else can ask about it, but they can talk about it if they want, though they shouldn't give away anybody else's deliberations. I'll tell you when I was part of that criminal trial, we were told specifically that matters of the law can't

be brought in from outside to be considered by the jury. But the foreperson in my criminal case pointed out that the government had impeached the defendant on the stand with prior convictions. The judge says it's not propensity evidence, we're not saying just because he has two prior convictions involving physical violence or assault that therefore he committed this assault. We're just impeaching with the prior convictions.

We're back in the jury room, and I'm trying to keep my mouth quiet and not interject myself. The foreperson says in California, it’s widely understood that there’s a three-strikes law. After two convictions, number three means it’s coming with a term of years, and it's mandatory. We're not supposed to consider that. I just sat there listening to this, and the foreperson says, “If he's already got two convictions, this one means he's going down,” and all the Californians in the room are nodding. At the end of the day, nobody would vote to convict. I suspect it was because everybody believed if he got convicted, he was going down hard. No matter what you believed about the case itself, the circumstances seemed harsh. Nobody would vote to convict, and he walked. I had no problem with that at all.

So yes, it does come up for real.

[Patrick Whalen]
Dale, it seems like the logic of a jury trial is one of consensus. What's the likelihood of Daubert when it's consistent with the overall logic of the apparatus?

[Dale Saran]
It's interesting you say that. I'm not sure it is because I think both are present. On the one hand, juries are based on consensus. In criminal trials, we want a unanimous verdict, 12–0. The consensus has to be absolute, which favors the defense. All I need is one. So, I think on the one hand, yeah, consensus is part of it. The community has to come to an agreement. Peter mentioned how within the context of science, we have this ongoing process that's iterative and keeps going. But in the context of a case, it’s like, hey man, this thing's got to be over by Thursday. At some point, we're moving on, and that puts a stake in the ground.

I don't know that Daubert couldn't be overturned. I think it could. I think it could, and I don’t think simply because we have civil trials where it’s more likely than not, with a group of people agreeing, means it’s inconsistent. That’s because there's only money at stake, you know.

[Emily Kaplan]
One of the other interesting things with Daubert is that people say it was the right decision because of the specifics within the case. They were looking at kids who had birth defects and tried to blame Dow Chemicals. It later came out that the science said there's no way these chemicals caused those birth defects. So people use the specifics in the case to justify the rule, which should have nothing to do with the specifics of the case.

[Dale Saran]
There's an old lawyer saying that hard cases make bad law. It's one of those things where, because of this time stake we have, we have to do the best we can with what’s here. Why should we give that broader application? Why should there even be precedent in that sense? It’s a problem, particularly when the scientific knowledge changes. Depending on when the knowledge changed, if you can prove it quickly enough, you've got an opportunity for a retrial or reconsideration. There are mechanisms to go back and fix things on appeal, new evidence, those kinds of things. So, there are safety valves built in to do that, but the courts like to move on. I think part of it is the judges don’t want anybody going back and spending too much time laboring over what the idiot judge did.

Susan Haack's piece discusses this. I commend it to all of you. Judges fall for falsification as the demarcation for science. Those of us here, members of the David Stove Society or whatever we want to call ourselves, would disagree strongly with that. It’s a problem.

They really conflated—if you look at that Haack piece and you look at Albert Fry and the judges' opinions, particularly Rehnquist, it's disappointing to say the least. You've got judges who almost just sort of throw their hands up, like, "Who am I but a simple judge? How can I be expected to even know what science is?" It seems to me a conscious lowering of the bar and suggesting that let's just not try to get to know too much here. It seems to me a kind of plea to ignorance on the part of federal judges, but maybe that's just me having higher hopes for my profession than I should. Dan, anything else on that?

[Dan MacDougal]
No. Well, Roger, aren't you friends with Glenn Reynolds? I think what would help change the Supreme Court's mind is kind of a tsunami of law review articles by eminent professors. If we could get some of them on board,

and we might start with Glenn, writing articles criticizing Daubert on different grounds. That's kind of what gives some impetus from the academic sector for them to pay attention and maybe change the rule.

[Greg Glassman]
The amicus brief in Daubert was an extensive list, and it was all academics. There was no one from industry. So Harvard weighed in on what science is and got it wrong. Stanford did, got it wrong. They should ask someone over at Intel.

[Dan MacDougal]
So we get law professors writing law review articles and then file amicus briefs and maybe have a good case. Have to find the case,

[Dale Saran]
I like it.

Yeah, I think Dan's got a good point. I hadn't really thought about that. That is a good recommendation. A lot of the changes in the law—law review articles wind up being very influential. In many of the cases in the 60s, the civil rights cases really came out of law review. They were academics sort of publishing these ideas, and then people would go, "Hey, let's turn that into a test case and then see if we can't," and then you find the Supreme Court using the borrowing language from law review articles. I mean, that's how New York Times v. Sullivan came to be the way it is. Actually needs to be changed back, but law review articles is a good place to start.

Yeah, I'll get that from you. I'd love to see that. Maybe we can start thinking about ways in which—Matt and I have talked about this, and we've been putting this off—but something about the rules of evidence for science, something that can kind of bring these disparate threads together.

[Jay Couey]
I don't know if you're aware, but there was a Supreme Court ruling that happened in 2019 or 2020 that was based largely on a Yale Law School review. It's called the "Antibody Patent Paradox." It changed the way a lot of things happened. Right before COVID, you probably aren't aware, but the monoclonal antibody market was about $150 billion before the pandemic. That meant you could patent an antibody you developed and patent the way you produced the antibody. However, the way you patent it was to describe the methodology. This Yale Law School review noticed that the formulation in these patents was just starting to be repeated. "We baked a cake, we put the frosting on it, and now this is our cake, and we're selling it as a patented antibody." But it turns out that antibodies as a correlate of immunity are a terrible model of how the immune system functions, but have been perpetuated through broken science for decades and perpetuated by broken science also funded by this $150 billion industry. So once the science caught up, it took an actual law review article to express the incongruency between the known science of monoclonal antibodies and what they were representing them as in IP patents. This, in 2020, basically put all of these really valuable patents on incredibly shaky ground, essentially suggesting that all of them wouldn't stand up to scrutiny in court. That's why you see at the beginning of the pandemic we went from monoclonal antibodies to nothing. Monoclonal antibodies were very feared as an investment that wouldn't be protectable in the IP landscape anymore because the biology didn't support their unique inventiveness. That's a really good example of how a law review could change that landscape in a very aggressive way.

[Dale Saran]
Getting it into the best, you know, the mostest bestest of the law, you know, getting it into the Yale Law Review, obviously, or Harvard, and you've got to think about getting past the gatekeepers there. Then of course, there's the question of, given what we're witnessing, which is the collapse of the expertocracy, do you even want to go

there? Do you even want to be there? It's a difficult question. You know, that's where the change would be made, and yet by its nature—I mean, let's be honest, given what we've seen with Harvard's own president, as you alluded to, Emily, with the presidents of both Stanford and Harvard going down in just a flaming pile of dog crap, who saw that coming? I didn't have that on my bingo card last year.

[Greg Glassman]
The Stanford one unwound slowly. It just took too long to keep up with him, but he had pinned some malfeasance on an assistant who left in shame and then came back later with a promotion and is like a girlfriend now or something. It's not good. Everyone saw through the whole thing. It was pretty bad. Am I right on that?

[Emily Kaplan]
Yeah, it's similar. I mean, what I think is interesting is that with a lot of these, it's the college newspapers that are breaking these stories, right? So the Harvard Crimson broke the Dana-Farber story. The Stanford newspaper really was on top of the president more than anybody else. The mainstream media then is forced to do a piece on it, and then they go away, and the kids are still working hard. I mean, that gives me some hope and optimism in the sort of dismal media market right now, but there are these kids doing really great work that is what we would expect from the mainstream, and we're not getting it.

[Dale Saran]
I think the effort's worth it. I just think that—I mean, it's worth it if only for the fact that look at how many jurists come from Harvard and Yale. We're run largely by the Skull and Bones, at least on the legal side of the house, so it's a worthwhile effort for sure.

[Greg Glassman]
I'm recognizing something might be wrong with me and that I can't imagine a greater university experience than removing the dean.

That's cool.

[Dan MacDougal]
Well, you went to Harvard for five years, didn't you, Greg?

[Greg Glassman]
What?

[Dan MacDougal]
You went to Harvard for five years.

[Greg Glassman]
That's right. Five years straight.

Peter's going to talk to us about open-access publishing, if he doesn't mind. How do you like that, Peter? Then we'll take some questions, and I think we're done.

[Peter Coles]
Should I make this quick then, so everyone can…

[Greg Glassman]
Make it unbelievable.

[Peter Coles]
But I'd rather make it believable. So, I just offered to say something for a few minutes about open access publishing and actually open science generally because I hadn't realized that people would talk about this, but a few of the talks yesterday actually hit on one of the problems in academic research now, which is the publishing industry and the effect that it has on research behavior and costs.

So, I'll give you a little bit of autobiography. About 15 years ago, I was sitting at the University of Cardiff in Wales, working in a staff meeting where, as is frequent in the university system anywhere, there was a discussion of the dire financial situation facing the department and budget cuts and things like that. We discovered for the first time ever—we were told how much our university was spending on journal subscriptions in the area of physics and astronomy, which I was working in. It turned out—I don't remember the exact figures—but I do remember that if we didn't pay the journal subscriptions anymore, we could appoint two more faculty members.

Being a physicist, ever since the early 1990s, in astrophysics especially, which was the field that I work in, almost every research paper has been put on the arXiv. The arXiv is a free repository; anyone can download papers from it. It's actually run out of Cornell but it's recently had big donations from the Simons Foundation, so it's not a sort of amateur thing; it's well-funded. And since the early '90s, I don't think I've ever really looked at a journal, a physics journal, because everything is free on the arXiv. All the relevant research papers go on there. Every physics and astronomy department that I'm aware of has a journal club that they run every week. The graduate students download papers from the arXiv and discuss them, and there's never any reference to actually going to the journal website.

So that was the early '90s. I think the arXiv actually started in '93. It was a bit smaller then than it is now. It's actually very good. Actually, if you look at the arXiv, how many of you here have seen the arXiv? There's a few. It still looks like a 1990s web page, which I quite like about it. There's no kind of wasted fancy graphics and things on the front page. It's very direct to what it does.

So, the idea that I had when I heard how much the journal subscriptions were costing was basically, why do we bother with these journals? Why don't we just use the arXiv and forget about the journals? Of course, the answer is peer review. The answer everyone comes up with is journals do peer review, so we need to have that quality mark, so we know that the paper is reliable. The arXiv is not peer-reviewed, so basically anyone can put a paper on an arXiv, and there's no guarantee that it's actually correct. There's some moderation on the arXiv, but it's not equivalent to peer review.

[Anton Garrett]
You need an academic address.

[Peter Coles]
Yeah, but well, if you don't, you can still be sponsored by someone who can get you in if you haven't got an academic address. But there is some gatekeeping there, but it's not the same as peer review.

And then, at the same time, roughly about 15–20 years ago, the open science movement got going. The main logic about this was that, in an area like astrophysics or cosmology, most of the research is funded by the taxpayer. It's blue skies research; it's not making commercial products and so on. And since the taxpayer is paying for this research, it seems to me that it's a moral obligation that the taxpayer should have access to that research that they've already paid for, instead of paying a library subscription of several thousand a year to access a journal.

So this is the open access publishing idea. It's actually only part of the open science movement because the other parts are open data. In my field as well, big experimental result big program observational programs are mandated to make all of their data publicly available. That is something that we do in astrophysics. It's by no means the case in other fields, like in medical research, for example, where people don't generally release all of their data; they keep a hold of the data.

It seems to me we talked a lot yesterday about reproducibility. A key part of science is that if you make a scientific claim of a research result based on some data, somebody somewhere else, some other research team, should be able to take that data and check whether you did it right or not, or even discover other things in the data that you didn't find. This seems to be a principle which we have been doing in astronomy for 30 years, but it's very—you know, in astronomy, if somebody publishes a result based on a paper, and you wonder about it or think it can be generalized in some way, you just email the people there and say, "Can I have the data?" They say, "Sure." And that's not what happens in many other fields.

Open access publishing isn't everything. The principle of open science is that everything that's needed to produce a scientific result described in a paper or any other source should be made available for somebody else to do the analysis again and check that you did it right. That's open science.

So I'm not going to say anything more about that. I'll go back to the open access publishing. What happened with this was the move to say, well, actually, we should instead of people having to pay a subscription for journals or for their Institute library to pay a subscription to a journal, the results of scientific research should be made available free of charge to anyone who wants to see them. Then the question is, who pays for this process? You can't charge a subscription, so how do you pay for it? At this point, the academic journal industry jumped in on this movement and was terrified that it was going to lose its profits. To put that figure in perspective, as in 2020, the global revenues of the academic publishing industry exceeded by about 50% the entire global revenues of the recorded music industry, until the open access movement came along. So that's many tens of billions. The profit margins for big publishers like Elsevier, Taylor and Francis, Wiley, and so on exceed the profit margins for the big tech companies like Google and Apple. Wow! And if you want to know why they're making such big profits, just look at their business model.

So imagine comparing the academic publishing industry to a restaurant. It's a restaurant in which the customers bring all their own food, the ingredients. They're charged to cook the food on the premises. The owners of the restaurant then sell the meal back to the people who are eating them, and all of that is revenue, and their outlay is very low because we do all the cooking, and we're the people who read the papers; we do the eating as well. So we pay twice. Right? So, if we're paying page charges or something, we're paying to eat in the restaurant, we're also paying subscriptions for it, and so they've been making a huge amount of money by stealth, largely because for most academics, this money was coming from a library budget, which is not the budget that they see in their own department; it comes from elsewhere in the university.

We were told about 20 years ago that when digital publishing came in, the cost of academic publishing would go down, which is a not unreasonable thing to happen. It's quite cheap to put papers on the internet. It almost costs nothing to put them, and most journals are now online only; they're not—they don't—it used to be expensive to produce hard copies and mail them all around the world and so on, and there was a legitimate cost there. But now there isn't. Incidentally, the cost of putting a paper and curating a paper kind of forever on the arXiv is about $11 for one paper. So that's how that curates the platform and actually hosts the data that's in your paper; that's on average. Some cost more because they're bigger data sets and things. So the cost of actually publishing a thing on the arXiv is a few dollars.

What happened with the academic journal industry was that they said, "Okay, we'll do open access. That means we can't charge subscriptions, and the only thing we can do to maintain the revenue that would be lost from subscriptions is to charge the authors." So they invented a thing called the article processing charge, or page charges. Some journals have been charging page charges for a while, and that means now that academics are asked to, or research teams, whatever, are asked to pay upfront to publish their papers open access, and the cost is several thousand, even more, thousand per paper for one paper. The journals that I work with typically charge $3,000 as an article processing fee. Now, that cost has nothing to do with publishing a paper. The cost of publishing and disseminating a paper is simply a charge to replace the revenue lost from subscriptions, which you didn't really know about before because unless you looked at the library thing, now you have to pay out of your research grant. Researchers are saying, "Hang on, does it really cost this amount of money to publish a paper?" Of course, it doesn't. It's all profit. That's where the money comes from. The actual cost of publishing has gone way down, but the profit margin has gone up, and as an—it's easy money.

So I thought this is nonsense. We're being—this is a parasitical industry. Back in the 18th century, obviously, printing and circulating scientific papers around the world was expensive and difficult

, and academic publishers fulfilled a useful purpose in doing that. There wasn't any other way of doing it in those days, but we're still stuck with the system of academic publishers even though we're now in a digital era, and I can write a paper, put it on the internet, and instantly anyone around the world can actually access it. It's—the cost is—it's just maintaining a revenue for services which is now redundant, in my view.

So, that's where we are with—so it's worth saying that many funding agencies now mandate open access publishing. So, you know, my funding agencies basically say, "If you write a paper based on this research grant, it has to be open access," because it's taxpayers who funded your research, and they should be able to read it. Not everyone in the world wants to read a cosmology paper, of course, but it's a principle which I think is important.

So, how did we—so basically, the open access movement got hijacked by the academic publishing industry in a way so that it would preserve its profits. And some of us are very angry about this, but there's no point in being angry about things unless you can come up with a way around it, a way of avoiding it.

So, a few years ago, five, six years ago, as more or less as an experiment, I started a journal which is now—the terminology for it is now called a diamond open access journal. There's lots of different kinds of open access—gold open access, green open access, and so on. So this journal is a peer-reviewed journal; it's free to publish in, and it's free to read. It's not, of course, entirely free; nothing's free. There's no such thing as a free lunch, and there's no such thing as a free journal. But the annual running costs of the journal that I run, the Open Journal of Astrophysics, are a few thousand per year, and less than an APC for one paper in a traditional journal. And last year, we published 50 papers. So that's not a big journal output, but we just started, so it's not surprising.

And how do we do it?

It's very easy, journals don't do peer review. We do. Other academics do the peer review; they just do a bit of organization. So, we organize peer review for papers that are submitted to the arXiv. We have two referees. If the referees—there's usually a bit of ping pong between the referees and the authors. If it passes peer review, we publish what's called an overlay, which is basically a pointer to the arXiv entry for that paper, with all the metadata, the authors, and things like that, and a DOI, digital object identifier, which is a unique identifier for a paper that's used in citations and so on.

I remember having an argument with a publisher from Oxford University Press, and they said, "Well, you know, we justify the £2,000 article processing fee because we register all the metadata for the article with Crossref." Crossref is a system which keeps track of citations, contains billions of articles around. Crossref is an amazing thing, but most people don't know anything about it. It's there, behind the scenes when you check your citations for your paper. It's Crossref that has done that. And they say, "Well, this is why we have to register all this, and therefore, that justifies some of the cost."

Well, I register all the metadata for the papers with the Open Journal of Astrophysics, and I can tell you exactly how much it costs to register metadata for a paper. It costs $1 per paper. $1. So, what the other $1,999 of your APC goes to, I don't know. So, the costs are negligible. They're not zero, as I said, but we have a small grant from, essentially, the agency in Ireland which runs library services, and you know, last year's cost was like $25,000. That's in the noise when it comes to research grants, so we can cover that without too much difficulty, without charging authors or readers.

If we ever get to the point where we have to charge authors, if it scales up incredibly quickly, we could levy a small charge, not thousands of dollars, tens of dollars. But I would insist that we give half of it to the arXiv because, without the arXiv, we're nothing.

This started as an experiment. It was delayed a bit by the pandemic a few years ago, and we went along kind of modestly. Last year, we picked up because the big astronomy publication in the UK, which has the rather archaic name of "Monthly Notices of the Royal Astronomical Society" —which believe it or not is a modern journal, and it doesn't come out monthly either—switched to this APC system for open access, £2,500 per paper. At which point, a lot of people in the UK said no, and we started to get a greatly increased submission rate. So, we went up. Last year, we published 50. Our doubling time is about a year at the moment, so this year we'll probably have 100; next year, 200. And the trouble with exponentials is that they get silly very quickly, so you know, I don't know where it'll go.

If you want to know whether this kind of thing can work, I will tell you that we've had papers published by a Nobel laureate, George Smoot, a cosmologist. Many fellows of the Royal Society have published with us. And most importantly to me, we have many papers which are published in it, led by early-career researchers who are actually taking much more of a risk than established professors in publishing in an unfamiliar journal.

We have an—there are lots of reasons that I could talk about why you should not pay attention to journal impact factors, but I know that lots of bureaucrats do. Our journal impact factor—I don't like journal impact factors, and I'm not saying that because ours is bad—our journal impact factor is 8.2, which is higher than the astronomy journals that we're competing with. So, I think we produce quality papers. It's a no-frills service, of course, because the authors typeset their own papers to put them on the arXivs. We don't do copy-editing, we don't do typesetting or anything like that, but when I mention this to authors, they say, "Oh great, you don't do copy-editing. That means that some boob doesn't go messing around with the text that I've very carefully written." Copy editing from journals is not always a positive thing, and in my experience, it's not often a positive thing. So, they typeset their own papers and publish there.

And actually, we reject a higher proportion of papers than the mainstream journals do. That's not deliberate; that's just the way it turns out with our refereeing standards. I think we're a high standard.

[Anton Garrett]
Peter, would you add something about the conflict of interest when the learned societies get into…

[Peter Coles]

Yeah, I was going to say that. So I mentioned with Monthly Notices, Monthly Notices of the Royal Astronomical Society is the Royal Astronomical Society's main source of cash flow. They subsidize the charges from publishing in that, and I think there's an issue there with the learned societies and G. G talked about learned societies. The Institute of Physics in the UK also has a publishing division which makes very large profits and now is taking those profits, via APCs, from research grants to fund things that are not to do with research—things like outreach programs and course accreditations and things like that. Many of those things are worthy, but they're not research-related, and the money that we're spending on APCs is not being used just to cover the cost of publication, which is what it should be.

So there is a way, and so the learned societies, all their profits are spent in the remit of the learned society rather than just going to shareholders. But it's still not a transparent way of funding that because it's siphoning off research funds.

So I just recently resigned from the Institute of Physics because I disagree so much with the policy of essentially appropriating funds from research grants to spend on other things.

So just a couple of other things. I think the—we've heard about the difficulties of some of the impact of the publishing industry on scientific research and academic research in particular. Just think about the incentive that now is if you're charging per paper, and you're a commercial journal, where is the market pressure going? It clearly is in the direction of publishing more papers. That's less obvious when it's a subscription-based journal. Right? So I know of several cases already where academics have resigned from an APC-funded journal on the basis that they've been told directly by the publisher to lower their academic standards of refereeing to publish more papers. And this is where it's got. So the new gold open access, that's called gold open access—Fool's Gold, it is actually—is a pressure to reduce editorial standards and to publish more and more and more papers.

I think we already have too many papers. I said this yesterday; I think the paper itself is a bit of an outmoded idea. But together with, you know, P-values and all this kind of stuff, it's a pressure on people to produce lots and lots of fairly useless papers, and it's busy money for the journal publishers because they're getting paid for everyone. I don't know if you're ever aware of this concept of a vanity publication. You know, I've been contacted by these "Who's Who in Wherever," and you pay us to get your name listed. The new-style academic journals are very much vanity publications. You pay, you publish, and it doesn't do science any good. In fact, the pressures on researchers are negative. They make science worse. It's part of the way science is broken, in fact, is that publishing has taken over. People regard science as being the production of papers, and I don't think that's what it is. It's actually papers are a kind of tangential output of the scientific process. The scientific process is much more than just a collection of outputs, as our administrators calling, "How many outputs do you have this year?" We've been—we are asked to fill in on spreadsheets every year.

I think my output is not just scientific papers in peer-reviewed journals. Anyway, what's interesting about this is that we're lucky in astrophysics because we have the arXiv, and everyone has been using the arXiv for 30 years.

I have been putting papers on the arXiv. Other disciplines don't have the equivalent of the arXiv, and the arXiv is not used by all scientific disciplines equally as well. But recently, there are—there's an archive, a prototype one in criminology. There are ones in medical and biophysics, bioscience. And the overlay model that I described doesn't—is not really fixed with the arXiv; it could be applied to any kind of repository.

And you, those of you who work in universities, will know that your university probably has an institutional repository where all its papers are kept. An overlay could sit on top of institutional repositories. And remember that these things are run at a cost of like $10 per paper rather than $2,500 per paper, or $3,000 per paper. All of that research, all of that funds that could be saved, could actually be spent on making science a bit better rather than feeding the profits and siphoning money off to other things.

So, I encourage everyone here to think about the—well, the landscape is definitely changing, and I think some of these changes are inevitable because people can see that it doesn't cost $25,000 to publish a paper. And it's technology which is driving this; the availability of digital publishing is there; you can't make it go away. And I think the traditional publishers are fighting a losing battle trying to keep their revenue by providing a service which is not necessary anymore.

It's the modern era; we don't need these traditional journals. So, this poses—if we now go to a much more federated system of publications where people use repositories and organizations organize peer review among themselves, which is what we do, we could cut this enormously expensive parasite and spend a bit more money on research. But more importantly, everything is open.

Another point about APCs, which I forgot to mention, is suppose you're from a not wealthy country with not much research income to spend. You're excluded from publishing in a journal which is charging thousands of dollars to publish your paper. You just cannot afford to publish. So, on the pretext of being open access, it's actually closing the possibility of publication off to many people. And also, of course, you might not be with a university, so you have a much wider possibility for people to publish in your journal if you don't have to pay an APC or belong to a university.

So this is just a heads up that I think over the next few years is going to be very exciting. I think the old model of academic publishing is going to disintegrate. How long it takes to disintegrate, I can't predict. Prediction is always difficult, especially about the future. I think that was Neil B. that one. But it's happening, and I think this change to an author-pays model has opened a lot of people's eyes to where all this money has been going for all this time, and they're going to find different ways of doing it.

I'll mention another thing that one of the big offenders in terms of profiteering is the journal Elsevier, which runs a very large—that's one of the biggest profit-making commercial journal publishers. Elsevier also has fingers in other pies as well. Elsevier has a front company called Scopus, which some of you may have heard of. Scopus runs a list of proper journals, as it were, and many funding agencies insist that it's not a proper paper unless it's in a journal on the Scopus list. So Elsevier is gatekeeping this thing as well.

I recently got the Open Journal listed on Scopus. I had to hold my nose to fill in the application because I dislike Scopus at all, but it actually does matter to people in some countries that the journals they publish in are listed on Scopus, so we're actually on there.

Finally, one last thing that I'll mention is about peer review. We do peer review; we do the best job we can with peer review. And I—and some of our reviewers do a fantastic job. Actually, one thing I've learned being an editor of this journal is that if you want really good quality peer review, you ask early career researchers to do it because they write much more cogent and detailed responses than old professors who—actually, a lot of the papers that we do are very technical, and so it's very often the postdocs who are working at the coalface in the field know much more about the details than the people who are in kind of management roles. So they do a good job, and I'm very gratified that the younger generation is actually on board with this because they haven't grown up with the old system and, therefore, are not used to it, and they recognize the absurdity of it straight away.

So that's an optimistic sign. I'll just say that the peer review we've had this comment before—I think it's worth saying again—peer review is a way we have of trying to assure some kind of quality in academic publishing, but it's not perfect, and we should never think that a peer-reviewed paper is the gospel. Reviewers make mistakes, just as authors do, and just as everyone else does.

So another aspect of open science, which is important, is that as many people as possible can scrutinize the result. Referees may have missed a bug in the paper; somebody else reading the paper will find that bug, and you can then go and do a retraction. With our model, it's actually quite easy to publish an amended version because we just change a pointer to another arXiv version.

Peer review is much better if the entire community of your science discipline does the review than it is if there are just a couple of people who the editor invites. So, that's just to say that open access publishing is part of a very important way of mending some of the breaks in science because science is not at its best when it does things in private. It's much better when it's all done out in the open, where the data is made publicly available, where the scrutiny of scientific results is facilitated rather than hindered by the scientists themselves.

I think that's an important principle, and I think many of the difficulties that we've heard about in the course of this event are exacerbated by the current methods of publishing scientific data.

So I'll finish there; it's a bit longer than I thought.

[Emily Kaplan]
But I'll just say one quick story, which was years ago I did a story on Alexandra Elbakyan, who started Sci-Hub. If you guys aren't familiar with her, she was someone who was, I think, studying neurology or something, and she was in Kazakhstan and didn't have any money. She couldn't access any of the medical journals; she couldn't afford to pay for them as a student. So, she was very computer savvy and basically figured out a way of sharing passwords with other people so that she could get their journal articles, and then she could share theirs. This created a repository of basically, I think it's now all medical journal articles throughout history. There's something also called Library Genesis that has done this with books. But she's like an international fugitive. She's in hiding. She has been—I mean, the story was like 10 years ago—she still hasn't really resurfaced because the publishing industry was claiming that she was stealing all their work. But her point was, science has to be open, right? We have to be able to see these things, and you're prohibiting me, in a poor country, from being able to do the work, and I'm curious about it, and I have been interested in it.

I think it was actually like a triple dip by the—how much the workforce is paying for the taxpayers paying for all the workforce and the publications and the subscriptions, and the libraries, if it's a public institution, you're also paying for the tuition of the kids going there and the professor salaries.

But I'll just add, you know, as a former journalist, when you would see something that was peer-reviewed, you were told essentially by your editor you can just report that's true. So the idea that peer reviewers are fact-checkers is a huge fallacy. Most people don't realize that the peer reviewers are told to assume there is no fraud or scientific misconduct, and the data is all right. So they're not doing what a fact-checker would do in old school journalism, where they'd really rip it apart. They look for other sources, and then this goes through the sort of chain of information, and the media gets hold of it, and they assume it has been fact-checked, and they assume that it is legit and verified information. So it creates this real web of bad conduct.

And then just one funny thing, after that story, my dad is someone who knows a lot of people in science and engineering and is friends with, um, Benjamin Lewin, I think his name. He started Cell. And my dad went to him, and he said, "How did you handle peer reviews doing these stories?" And it's kind of crazy that, you know, there aren't any checks and balances. And he apparently responded to my dad when he was like, "I was peer review. There weren't peer reviewers; it was my journal, and if I thought it passed the test—you know, the stink test of me—then it was published." And Elsevier went to him and said, "We want to buy Cell. This is an incredible high-impact journal." And he basically gave out a number that was ten times what he thought the magazine or the journal was worth, and they said on the spot, "Yes, we'll take it." So, he's very, very wealthy now because of that. But he looks at the whole thing and concludes all the same, you know, if it's one person and they're the editor or the publisher, and they say, "Yes, I'll accept this," or "No, I won't," and this is my area of expertise, you at least have some barometer of truth, right? Where you know the standard is being held by this one person.

And I think that's part of this. I mean, Greg and I talk a lot about how, you know, in the military or in technology, there's no peer review. It doesn't mean that things don't pass; they might be top-secret, right? But they still get done because the efficacy and the validation are in the predictability of the outcomes. And I think we do need to get back to something that looks more like that. And I think the business model is—I mean, when I was diving into all of that, I could not believe how much money there was being spent on these things, and that there was like zero labor, right? I mean, it's like a dream business model. So I think, Tom, you wanted to say something too?

[Greg Glassman]
Dale, what were you thinking on the sole reviewer?

[Dale Saran]
I mean, up and down, yeah. I mean, yeah, I was thinking, sure, there are two ways to look at this, you know. Yep.

[Emily Kaplan]
But I would think the market would take care of that.

[Dale Saran]
The market was taking care of it, you know, except when the business defamation started—CrossFit's dangerous; it's killing people, you know.

[Anton Garrett]
Perhaps I should add that Peter should be careful not to be assassinated in view of the size of this industry. Maybe you should borrow a bodyguard from one or two people here.

But, uh, perhaps in the size of this industry—you said it exceeds by 50% the recorded music industry. Are you able to break down the size of academic publishing? Because I think textbooks are still a valuable service. Is that included in that figure, or excluded?

[Peter Coles]
It's all academic, so it's difficult with textbooks because not all textbooks are published by the same people who publish journals. And actually, the global revenues of the academic publishing industry are dominated by four publishers. There's Elsevier, Springer, thanks John Wiley, and Taylor and Frances. And actually, John Wiley and Sons recently acquired Hindawi, which is a publisher that had, what is it, 10,000 retractions last year? It's a very dodgy, but very profitable publisher.

[Thomas Seyfried]
Yeah, let me just say a few words on this. This whole discussion, Pete, you're spot on about this whole thing. We in the biomedical industry are behind in catching up with the arXivs. But you're right; I submitted a paper for the first time to the arXivs, and you get feedback. But the thing about the arXivs, from peer review, is when you send a paper to peer review, you have two or three guys that are making the decision about what they think is here, right? One guy doesn't like it, you know, and it could stay in this go around and around for months, sometimes. Got to do another experiment here, another experiment there. But when you do arXivs, now everybody in the

world is looking at that. You have to be super, super careful for not being a what is it, Drango, or Drago, whatever the H is, you know, because you make a mistake, they come at you. You got a section there where you can comment on this, right? So, um, see you, I tore up some guy who didn't know his ass from a hole in the ground on the arXivs, and you have that opportunity now.

And the guy looking at that, whoa, this guy saying that! So now you're opening the discussions to the world, not just kept behind the peer-review curtain of the journal that you're submitting to. So, yeah, uh, peer review, and the other thing that's nice about BioRxiv is they're also allowing you to see how many people are actually re-looking at your paper, you know. And then you're right, because I get, when I put some of these in the arXiv, we get all these comments from journals that say, "Oh well, please send it to us; we'll peer-review it," and all this kind of stuff. But you know, just leaving it in the arXiv is fine. Let it age, and you can add more stuff to it like you say. So this is like, and it doesn't cost anything, and we're paying so much money, thousands for Frontiers and whatever else journal we're putting in, for them to typeset this. And if the data are correct, then everybody in the world can see; we put everything in there, right?

So, I think this, but we're behind the physics guys; we're just to catch up because a lot of my friends, well, it's not peer-reviewed; it's not going to help me get promoted, it's like that blah, blah, blah. But when you're older, you don't give a shit, you know. The bottom line is you want people to see your work, that's the bottom line, and let people make comments on it because if you screwed up, man, you get blindsided, you feel like a fool. So you have to be on top of that, and I think that's the way the best way for science to move forward, no question about it.

So you're right, it's going to change, and the young people know this, so they're going to be doing this. And I think this is the future, you're absolutely right because we're not being—we're getting dumped upon by all these people taking our money. We should be spending more money on research equipment and research expenses, and we're having to spend all this money on thousands and thousands of dollars to get it into peer review. And then, you know, one of the things I do in my cancer class is we take Cell, Science, Nature, and we go through the so-called peer-reviewed papers, and we point out all the errors, and then the kids ask me, "How do they allow that to be through?" because the peer-review system is broken. There's so much data now. If you ever see the size of the amount of data put into a Cell paper, I mean, you have to spend a month going through all the data, and some of these things aren't correct; you don't have time to look at all this. But you put it in Open Access, and people will see, so that's why you be very, very careful with the arXivs.

But anyway, he's right; this is the future, no question about it.

[Peter Coles]
Can I respond? just a—somebody wanted to come

[Jay Couey]
Oh, that J over there? JB

[Jay Bhattacharya]
sounds so—so, uh, first, like, I'm a big fan of the Open Access model, and I think during the pandemic, it was a real important way to get out word that you couldn't get out through very, very biased period journals. But I should say that I, and many other people who were trying to get our message out through MedRxiv and SSRN and BioRxiv, faced a problem I never thought I'd see from the Open Access journals. They rejected papers that went counter to the public health narrative; they rejected one of my papers that actually ended up being published in a peer-reviewed journal, because, with the excuse that "this is too sensitive a topic to publish in an

open access, free-up Open Access pre-print journal during a pandemic." So, the politicization of these Open Access journals is not something to take for granted because during the pandemic, they failed that test.

The other thing about peer review I think is really important is that there is no incentive to do good peer review. There's none, other than trying to impress the editor. And that, I think, is the central problem that we have, right? I agree completely; what happens afterwards in the Open Access journals, where you have that comment section, is beautiful, right? Or, and the community you describe of peer review, that's beautiful; that's what science is supposed to be. But there is almost no incentive whatsoever, unless you want to get at somebody, to do that comment or do the careful work.

So, the central problem then is, how do you create incentives to do the peer review? How do you get the young people, like the young people that are donating their time to the journals, they're taking advantage of, basically? Like, they should be doing their own science, not—because the peer-review work they get gets literally no attention. So that—so, I think the right thing is, it has to come from universities, it has to come from the journals can help by that, by publishing the peer review, giving it a DOI itself. So, the peer review itself, and making peer review part of how we evaluate scientists, right? If you do good peer review, it's—as the analogy I like to think of is like the movie industry, right? Anybody can publish a movie; just make a movie and put it on wherever, right? And put it on YouTube, and you publish the movie. After the fact, there's like this thick review infrastructure, right? There's like the 10 stars out of 10, and it was fun to watch, people, and then there's like the Roger Eberts who go into some deep dive about some psychological thing I don't care about. And so, they just—it's just a big deal to be a great movie reviewer. We should have that in science, like, it should be a big deal. We already kind of do, except it's only very few people who publicly do reviews. If—but that should be the central part of science, like that kind of thick discussion after the fact. So, the paper doesn't matter; it's what happens afterward, right.

It's the what, how do people evaluate your idea that happens afterwards that really matters, and then we all should grow a thick skin. So that, I mean, I had one of the top altmetric scores of all time from a paper I put in medRxiv in 2020, and well, you should see the comments—they're just like excoriating, it was fantastic. So, it was really fun.

[Dale Saran]
I'll just add to that, the idea, and this was, I got this from Dr. Glassman, and he was adamant about this—and I know Greg talked about this—but his view was that publication meant to make it public. And so when you're talking about these problems, I think I was thinking this when I was listening to you, Peter, is why are we hung up on this notion of peer review? Like, let it out, and if you fall on your face, there's the peer review. You know, let the—I mean, there's some great peer review going on in the YouTube comments, there's also a ton of garbage, but you can find some real gems in there.

And I think the idea should be that publication isn't the province of an elite, and neither should peer review be. I mean, there are things I can look at peer review, I can peer review. I don't need to be an expert on that subject, I just can see [__] when I see it, you know, and I can point to the fallacies. And the under—you don't need to be an expert in, you don't need to be an expert in anything to know you're committing fraud if I can point to the fraud.

[Greg Glassman]
Dale, the exposure of the scientific misconduct, fraudulent science is coming out of independent, private "pajama media" kind of people, you know. So, if they're pointing out the bad science, like the guy who wrote the software to hunt the fake Western blots, it's pretty good work.

You think you can work on the other end too, not just look for the bad but give something a thumbs up.

[Dale Saran]
It just seems to me the idea of peer review is like this overlay that, to Emily's point, gives it this patina of legitimacy and makes it harder to critique and actually peer review because somebody else says, "Oh, it's already been through peer review, and therefore it's been blessed and the pope has said it's wonderful," and now you're excommunicado for suggesting otherwise.

And I think it would be better if we just said, "Hey, clean it up as best you can, publish it, and, you know, it's okay." I mean, Einstein had papers that later he was like, "Oo, uh, you know, there's that famous thing that, you know, people even within his own discipline were like, 'Ah, the guy probably missed it on that one.'" But nobody held it against them, you know. I mean, it's—we all make mistakes. I just think it would be so much better if we met, if we thought of publication as Dr. GL just beat me with this, that publication is the act of making public, that's the whole point. And subject, you know, let's crowdsource it.

The internet produces wonders when you crowdsource.

[Emily Kaplan]
Gary Taubes was recently telling me that when he's gotten really into using AI to translate old German medical journals that he couldn't have read, you know, earlier he didn't have access to, and one of the things that he's been really struck by is that these old scientific societies that were looking at problems that he's interested in, when they would publish, they would publish their results, their argument, their hypothesis, all of that, and they would publish basically all of the criticism they were expecting to get because of prior work or because of conflicting results from another study, and they would rebut it or they would say why they still hadn't solved for that problem yet.

But he said it would be like 40 pages, and with the internet, there's no reason why you couldn't do that too. And so you think if you were truly intellectually honest, you would do that as a scientist. You would say, "This is what we found, this is what we can't say conclusively, this is the other work that needs to be done, this is the prior work." There's plenty of space to do that online. But I think when you're publishing and you're paying $1,000 per page and it's out of your budget, it disincentivizes. I mean, and Tom, you've talked about this with me before too, about trying to be able to see the datasets and the cancer stuff that you used to be able to get access to way more information about how the study was done or whatnot, and now it's just sort of this glossy overview of how great the results were. I think that all should—that all needs to go.

[Peter Coles]
Can I respond to the responses?

Do I need a mic? Probably I do. So I want, there's one thing that I've forgotten to mention, and also I want to respond to some of the things that have been raised, which I think mostly people seem to agree with what I've been saying, which is rather nice.

One thing is this business about peer review. Now, I said peer review doesn't make something absolutely true or absolutely false. The role of peer review in the journal that I run is mainly not gatekeeping but to identify obvious errors or make corrections to, or improvements mainly the latter to the manuscript so that it actually improves the quality of the published article. So this is the reason why we go around a number of times before publishing. Usually, there are errors and ambiguities in it; it's not just yes or no, this is not good enough for our journal, it's there are—it's good, but you could make it even better by doing this. So it's editorial advice, really, rather than gatekeeping.

You mentioned incentives. It's actually, maybe I'm very naive, but in the community that I deal with, the incentive behind most reviewers writing good, comprehensive referees' reports is that they just want the thing to be right. They care about what's being written about, and they will say, "Oh, this is a good idea, but you know, it could be even better if you included this, this, and this." They're actually very dedicated people, and they want the literature to be a good reflection of the science that's being done, and they generally—the attitude is to try and help other researchers get their work published, rather than actually say, "No, you're an idiot, and I'm going to shoot you down."

Well, yeah, I know it's different—well, look, I mean, arXiv, we've had arXiv for 30 years, others are just coming to this. The open science movement has been true in astronomy for a long time.

[Jay Battacharya]
I think the problem is not that—because the communities, the people dedicated to getting it right, in lots of communities, but the problem is that if you have a tremendous amount of money at stake from the output of these things, or court cases, or whatnot, there's a lot of nonsense that goes on in the community. You can't assume what you just said about.

[Peter Coles]
The salient factor probably is that we're in a blue skies research thing where nobody really gets rich by making products based on cosmological observations, so it's kind of more altruistic behavior you get in that community because the stakes are intellectual rather than financial. I think—I think

[Jay Battacharya]
I picked the wrong field.

[Peter Coles]
Can I say another—the point that you made about, you put something on the arXiv, and people can attack it straight away. I actually yesterday made a comment about how I don't really like the idea of scientific papers that much, and they should be replaced by this kind of paper with commentary, and the paper should be updated according to new data and revised according to comments that come in, so it should be a living process that's on display rather than a fixed point. But I do think that the idea that it might make people a bit more careful about what they write because it's going to be immediately made available for people to criticize is not a bad thing. You're not going to look good if it's not right, yeah, so you take a lot more care, and maybe you get fewer junk papers being pushed out. So I don't think that's necessarily a bad thing.

And the final comment I wanted to make was something that I forgot in my bit earlier on, which is about copyright. The journal that I run, authors keep the copyright to all of their paper; we do not assign copyright to anybody. They can use that stuff again wherever they want to, and that's not the case in many journals. Just wanted to mention that the very first paper that I wrote was in 1986, I was just a PhD student then, and I had a sole author paper. I think my supervisor didn't want to be associated with it, so he let me publish it on my own, and subsequently, I was writing a textbook many years later, and I wanted to use a figure that had been in that I'd made for the first paper that I published. And I had to write to the publisher of the journal which was then Blackwells, which is based in Edinburgh, which the publisher of the Monthly Notices at that time, and I said, "Can I use this in this public, in this book?" And they wanted to charge me £150 to—because I'd signed over the copyright of the paper to the publisher, and you know, I'd made the bloody thing, and I didn't see why I should buy it back.

So I thought, "I'm not paying £50 to buy back my own graph." So what I did was, I found the program that produced the graph, and I changed the labels on the axes slightly, and made another version, and used that, and nobody ever complained about it. But that's also a crazy thing, the fact that you produce scientific research and get it published, essentially, you give it to somebody else for perpetuity. That's just wrong as well. So that's my final comment.

[Gerd Gigerenzer]
£150 is cheap. I've been charged more. But I really admire what you did because it sets an example, together with other examples, and I think the long-term program must be to take publishing out of the hands of commercial publishers and all of them, and have an alternative that is non-profit. Yeah, and that saves us lots of money and gets us out of this kind of slavery to do work for Elsevier without pay, as a reviewer without pay, as a—you pay for—you want, and I think we have to think about something, how we do that and get publishing back where it was in the hands of scientific communities, and that should be our goal.

[Peter Coles]
Yeah, so I—I say that actually a lot of people talk a lot about open access publishing. One thing I did is make a decision to—to actually stop talking about it and—and actually try and make a journal that ran on those principles. And I think the main purpose of the Open Journal of Astrophysics is to demonstrate that it's—it's not only possible but it's possible to make a good journal by comparison with other journals. It stands the same kind of level.

[Gerd Gigerenzer]
It's not about open access publishing, just—it's about nonprofit open access publishing.

[Malcolm Kendrick]
Yeah, if I could just put the black on slightly, which is my favorite color, T, normally.

Agreeing with Jay, yes, in cosmology there are not so many issues. I just feel that if—if you're going to go into the medical research world, um, then you have to tread very carefully, much more carefully because there are people out there who will try to obliterate you, and they will try very hard, and they will gain a hold of many researchers if you're going to put something open access who will review your papers and absolutely slam them through the floor, and they will be being paid to do this, and you're going to face a real—a really more difficult position. So I think you need to—it, I—I think peer review should be—you know, after I had a few cups of coffee, I'll have a peer review, um, just across the parking lot, and which is about as useful as peer review is, in my opinion, unless it's not the way you say. So I just think that from regard to trying to get into my world, um, of medical research criticism, I think you need to make it look much more kind of bigger and proper, and if we don't, if you don't achieve that, it—it will probably just be attacked to such an extent that it may be destroyed. I just think that we have to not be naive about the—the difficulties in a field with so much money involved.

[Peter Coles]
Yeah, yeah, so I've got no intention of going into that field myself. I don't know, but, uh, what I would say is that I think, uh, the problems to be solved are simpler in cosmology, as I think we probably agree, and blue sky science generally. If we can't make it work in those fields, it's certainly not going to work anywhere else. So, my—my logic of my approach is, let's—let's make a working model as a kind of prototype for how this could, in principle, apply to other fields. If we can't make it work in astrophysics, then it's not going to work somewhere else because we have such a head start on other fields.

[Malcolm Kendrick]
I think you're absolutely right. And I agree …

…needs a moreful… and of these peer reviewers are important people kind of thing. So..

[bad audio]

[Anton Garrett] How robust is your website to attack?

[Peter Coles]
Well, it's actually—it's hosted on a commercial platform, so we actually—it's a third party that runs, uh, and they have the usual cyber defenses. They're not infallible, but they're stronger than they would be if I—I'd set my own web page up, but it's, um, there are denial of service attacks on—I don't know, uh, as part of the open science, there—movement, there's this open source software movement, which is another part of it, people should share their codes, they usually run off a platform called GitHub, people share their—uh, source codes on GitHub. There was a massive denial of service attack on GitHub last week, two weeks ago, uh, which I think has been resolved. I don't—don't know who was behind it, but one can certainly imagine hostile players, you know, uh, competitors, if you like, uh, encouraging people to—to do that sort of thing. Um, we haven't had any serious issues yet with that, and I think we're fairly well defended, but we're also actually not really a—a large publisher yet. Uh, if we were really competing with Elsevier, I think, uh, we might have a bit more to contend with along those lines.

I don't know, um, there.

[Gerd Gigerenzer]
By the way, there's a—a website by a French mathematician where you can sign up and declare that you will never review a paper for Elsevier anymore, that you will never submit a paper to Elsevier anymore, and you will never be on a—or, you can choose between all of these or some of these. And I have a question to you, uh, so how about what should funding agencies do? So I'm, um, in another job, uh, vice president of the European Research Council, the ERC, which is the largest funding agency in Europe, and we have done a few things, for instance, uh, we have instructed the—uh, authors of proposals to delete all impact factors from their CV, so, J, so many—uh, researchers, they cite their—their paper, and then they put the impact factor, what a work you have to, updated everything. Yeah, so no impact factor, send a signal, and also only to submit their six or so best papers, to give a signal it's not about quantity, but quality. Also, there's an obligation that all those who get a gr—publish in Open Access, but we have so far no means or not found a way to—uh, to, if it could, I require nonprofit Open Access like your—CH, then we could change the entire system, but that probably would, at the moment, being going too much. So, do you have any advice what—uh, funding agencies can do to help you and to help science.

[Peter Coles]
Funding agencies…

Well, what I would like to see—I mean, what they could do—these kind of initiatives like the journal that I'm running are actually very grassroots things. So, um, and we're doing one in the field of astrophysics, so I think funding agencies should look at this model and realize that they could easily propagate this model into different fields and provide publishing platforms between research institutions that allow researchers to publish on those platforms at zero cost and just bear the cost from the research. A tiny fraction of a typical research grant could cover a whole discipline, um, in the immediate short term.

For the Open Journal, we're kind of bracing for ever-increasing numbers of papers coming in. What I used to do—I give a talk about this subject to astrophysicist departments. I've done a lot of them in the last year, and usually, what I say is, I don't ask for money or anything like that to help it because our running costs are quite low.

But I just encourage people to volunteer to be editors for us because it's an unpaid job, and we need more editors if we have more papers. And to not just dismiss requests to be a peer reviewer from us because we all get lots of peer review requests, and a lot of them are from junk journals. So, I'm trying to raise the profile of the journal so it's actually a name of a journal that people realize is not a predatory journal or anything like that.

So, I think a modest investment in infrastructure to make these kinds of platforms more widespread—Diamond Open Access journals—to go back to the question of research assessment and proposal assessment, I'm very pleased to say that I was instrumental in getting the Science Foundation Ireland, which is our main research funding agency, to sign up to a thing called the San Francisco Declaration on Research Assessment, which is a set of proposals to encourage fair assessment of research proposals. So no impact factors in promotions or grant applications and things like that—that's one of the principles. No reference to the Scopus list of allowed publications.

Actually, one of the things I'll say is that in astrophysics, papers get cited a lot; there's a lot of activity in it. So, we have a huge amount of information at the article level of the impact of individual papers. I know precisely how many citations each paper has that we published last year with the Open Journal. So that's an even better argument for the uselessness of the journal impact factor because that's a kind of weird average over all the papers published, divided by the number you first thought of, and whatever.

So, if you can track citation impact at an article level, there's no need at all to refer to the impact factor of the journal. We have a paper that's got 800 citations, for example. And that's not—I just think, you know, that's a good paper. It doesn't make it a good journal on its own. I think I'd rather you think this journal contains lots of good papers than this is a high-impact factor journal. I'd rather talk about the first thing rather than the second.

But I think this general set of principles of how you assess research fairly, not using arbitrary and misleading metrics, is an important thing. So if we can get more countries to sign up to the Declaration on Research Assessment, that would be a very big step forward as well. The ERC has signed up.

Yeah, uh, I think most of the curiosity in Ireland actually is that the Science Foundation Ireland, which is a research body, has signed up, but the Higher Education and Research Department of the government has not signed up, so they're running off different rule books essentially. There, so we haven't quite persuaded the government to sign up to it yet, but in the UK, all research councils have signed up as well, and I'm not sure about other countries in Europe, but the message is getting through that impact factors are junk; you shouldn't be using them. And lots of other stuff is junk as well; you shouldn't be using those either.

I think those kinds of declarations are useful only if they—I mean, if you just go around saying, "This is junk, don't do it," then that's only half the battle because you have to say, well, we do have to choose, we have to fund grants, we have to decide which is the best application, and so on. So we have to assess things in some way. So you have to come up with a better set of proposals of how to do it, and DORA does that.

[Greg Glassman]
Thank you, everybody.

[applause]

Bye.

Hey, Peter. Valentina Zharkova's Twin Dynamo paper from a few years back, the twin dynamo of the Sun, the Valentina Zharkova theory—do you know of it?

[Peter Coles]
No.

[Greg Glassman]
The model? No, okay, I'll send it to you.

Yeah, I wonder—I wonder, it's interesting. She's, she's—well, I'll send it to you. I'm not going to—

[Peter Coles]
I'm not a solar physicist…

[Greg Glassman]
Okay. How about aliens, yes or no?

[Peter Coles]
I think you need to ask that famous expert with Harvard University; they got—is that?

[Greg Glassman]
Who is that?

[Peter Coles]
What's my—all, Clen, have discovered alien evidence for aliens at the bottom of the Pacific Ocean, and oh, what's his name? It—it's—in the UK press, whenever there's a headline that says, "Harvard astronomer does something," that's him, it's always him.

[Emily Kaplan]
Weren't there some Mexican aliens or something recently?

[Greg Glassman]
I don't know, but I stopped reading at Harvard anything.

My favorite recently was—uh, parenting experts at Harvard—just like, slam on the brakes—hard stop.

Yeah, there are no parenting experts, and were there, they wouldn't be at Harvard.

Thank you, everybody.