

The new watchdogs' vision of science: A roundtable with Ivan Oransky (*Retraction Watch*) and Brandon Stell (*PubPeer*).

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Editors' note: On March 3rd 2016 Catherine Guaspare (CNRS/UCLA) and Emmanuel Didier (CNRS/UCLA) organized a conference entitled "Destabilized Science" at UCLA (http://epidapo.ucla.edu/Destabilized_Science). They moderated a roundtable with Ivan Oransky, co-founder of *Retraction Watch*, a site that tracks "retractions as a window into the scientific process" and Brandon Stell, founding member of *PubPeer*, a website that "enables scientists to search for their publications or their peers publications and provide feedback and/or start a conversation anonymously". We questioned them about their experience and vision of contemporary problems with science and what a robust science ought to be. Mario Biagioli (UCD), Michael Chwe (UCLA) and Aaron Panofsky (UCLA) participated in the conversation. Of course, there have been new developments since this 2016 discussion, and our understanding of some of these phenomena have changed. What follows is a transcript, lightly edited for clarity, of the conversation.

We all have noticed that science, and especially the life sciences, are now plagued with a wave of misconduct. Our interest is twofold. We would like to characterize the new conditions and practices of science that are problematic, and second to make some propositions to get out of the situation. First, we need to know who identifies the problems. We would like to know who are the new watchdogs of science compared to who were the old ones?

Ivan Oransky>> My response to that is that there were periodic watchdogs. Politicians, for example. Not ones that we necessarily agree with, but politicians making funding decisions with a very specific agenda, trying to defund this or defund that. I think the press also did play some role. If you look at the case of the Tuskegee study, that study went on for 40 years, and was only ended because in 1972, the Associated Press reported on it, and all that happened after. It was episodic, and it was targeted in different ways. I would say that you had gatekeepers. And you still have gatekeepers. And I think we could all agree that the gatekeeping function of journals is not as robust as we wish it was. But you do have gatekeepers. To me, it's us, it's *PubPeer*, it's a bunch of other sites. We're not the only ones. But I don't know that there was any systematic watchdogging happening before. Maybe I was missing it before.

Brandon Stell>> I would add to that I'm not sure that the watchdogs themselves have changed that much. I think their voice has been raised a little louder by sites like *Retraction Watch* and *PubPeer*, but scientists have been watching themselves.

Ivan Oransky>> I think technology just enabled what we see today. You know, PubPeer is an online journal club. In the past, you'd have a journal club, and then argue about a paper, you rip it apart much the same someone would on *PubPeer*. But then that was it. You'd leave the room, and go have lunch, and do something else. Now, it has a permanence, and it can invite other conversations. Before, a lot of this stuff was happening, just behind closed doors.

Brandon Stell>> My understanding is that the pressure to publish in high-impact journals has changed the attitudes of scientists. Apparently, when the journal *Cell* was launched it also launched the chase for the impact factor. I haven't looked into closely but I've heard other scientists claim that the impact factor became more important after that. Many of the earlier major findings in my field of biology were published in the *Journal of Physiology*, or the *Proceedings of the Royal Society* in London because at that time it apparently didn't really matter where you published your data. Over the years the chase of the impact factor, and the resulting pressure to publish in specific journals became more and more important. Maybe that increased the need for watchdogs.

Do you consider bloggers on science also as watchdogs? Because sometimes, they raise questions on the way things are going in science.

Ivan Oransky>> Well, I would put them in the same broad category as commenters on *PubPeer*. So I mean, they're commenting on specific things. And they may have particular interests. It's very much...there's a sort of speciation that happens. There are sort of ecological niches that come up. I think, more generally as watchdogs, there's also the Office for Research Integrity (ORI). We all now talk about the ORI as if it was around forever, you know? Depending when you start counting, the Office of Scientific Integrity (OSI), as it was initially called, or ORI, it's 30 years old, give or take. Before that, we had really nothing. Other countries are starting to have things that are starting to look like the ORI. They're getting there. The Danish Committees on Scientific Dishonesty (DCSD), and others – Sweden has something similar, Canada. So, I don't know, it depends how you define watchdogs. I mean, it always comes back to the definition. We could crowdsource a list, and then maybe disagree on certain members of it. But I think I would cast a wide net, and then sort of circulate a list of who we thought of as that. I mean, again, going back to politicians, is Senator Grassley here in the US, is he a watchdog? I mean, there's a scientist in prison now who would not have been in prison if Chuck Grassley had not paid attention to the case. So, I don't know.

So, now, let's focus on your two sites, PubPeer and Retraction Watch. They have in common to be very participatory, in the sense that readers participate a lot. Let's talk about these participating scientists. What is, according to you, their incentive for posting on your sites? What do we know about them?

Brandon Stell>> Because they're anonymous I really try not to know anything about them. If the site is ever attacked legally, it's best if I don't know anything about them. So, I honestly try not to look at who's writing the comments, and try not to build up a profile of who these people are. And so, it's difficult for me to give a profile of what their incentive is, or what their personality traits are.

Ivan Oransky>> I would answer cautiously for other reasons. I don't know who the anonymous people are, obviously. I do see patterns, and sometimes I get into email exchanges with people. And because we fact-check comments, sometimes I'll email or someone on the staff will email. And then it turns out they write back from an account that lets me know who they are. It could get challenged in court, but we can claim the shield law, because we are a news organization, and therefore don't have to disclose a

confidential source, thanks to the First Amendment. So, my knowing who these people are is, to me, much less risky than Brandon knowing who they are.

Mario Biagioli>> That's great. At the same time, it does look like there is such an emotional drive there that I suspect are like frustrated whistleblowers. It looks like something went bad in their professional life at some point -- you know, so like a little wounded. They're people who actually have a motivation because they got screwed. Maybe they reported somebody, and the dean fired them or something.

Brandon Stell>> They don't even necessarily have to have been screwed. Just observing some of this stuff coming in on PubPeer frustrates me, and I'm making an effort to distance myself from it and not form an opinion, but sometimes you see some cases that are shocking and there are no apparent consequences. So, I can see how some people get frustrated and that's probably one of their underlying motivations for writing comments on PubPeer.

Ivan Oransky>> I think there's a sort of spectrum. Take the case of a post-doc working in a very prominent lab. He had co-authorship. His boss is not going to get fired for anything, probably. But he's frustrated. He's calmer and very methodical about it, but he's not at all in danger or anything bad happening to him. Then there's something like the very active retired scientist. Then there are people who are very frustrated by all this, but don't go to the heights that the retired scientist does. And then there are other people who are obsessed about just a single case. It's the fascinating case of this poor guy, I mean, he was right. And kind of everyone knew he was right for 10 years. Finally, the NSF agreed. But if you look at what he did over the years -- I mean, I'm not saying he ever did anything that was irrational or even more than just vaguely pushing the envelope. And he remained actually quite calm throughout the whole process.

Brandon Stell>> There are other people that are on the record as being whistleblowers also. There's Paul Brookes.

Ivan Oransky>> Well, there's a gender thing that I think is also at play. There's a case of someone that has been doing a lot of stuff just quietly behind the scenes, where she uses her name when she contacts journals, but she never wants a hat tip on Retraction Watch. She doesn't want it to be known that she was the person, even though obviously it's not completely unknown. And that's also because I think that women are -- they just end up attacked more in very gendered ways, honestly, when they get involved in these things. And so, that's also another little axis that you see.

Michael Chwe>> Oh, I think that age plays a role too. Like the young, it's definitely easier to beat up on the young just because he was a weaker person. I was just going to say like a gender thing, I mean, once I was involved in this thing in my kid's high school. And it was an association that was raising money that turned out to be very corrupt. And we had this decision about whether to announce it or not. And the only people who wanted to announce it in the whole school was me and another woman. And she said it's because we're both from the south. We're both kind of from honor-based cultures. We believe in honor. I do really believe in honor. And some people just don't believe in honor. They think it's ridiculous. I just thought that was an interesting cultural kind of difference. So, maybe there's something there, I don't know.

And what were your own motivations in creating your site?

Brandon Stell>> My motivation was simply to give the community a louder voice in the assessment of the quality of published science. As we started to build the site, we began to see that if we created a

conversation about science, we could potentially change the way that science is published. What is really interesting to me right now is that we're in a position, I think, where we could just completely topple the journal publishing system if the cards fall in the right place. If we can help the community's collective assessment of publications to become more important than the impact factors associated with those publications then there would be no need for journals. I'm very excited about the science I do in the lab, but if PubPeer could help facilitate that then I think that would be a bigger contribution to science than any of the research I do in my lab.

Ivan Oransky>> Obviously I'm not a scientist. I see *Retraction Watch* as very much an extension of my journalistic work. I would like to make it full-time at some point. But I think I've always been interested in accountability journalism. Why do I spend all this time on it? And you know, full disclosure, since November of 2015, Adam Marcus [co-founder of Retraction Watch with I. Oransky] and I do collect some salary, a 20% time salary. And yes, we get other benefits from it. Again, I get to travel around and talk to people. So, it's paying me in that way. I'm not naïve about that. It gives me a profile. But Adam and I, we just have always believed in great stories and in transparency. The rewards of being able to do that kind of work in journalism, especially nowadays, are really considerable, regardless of anything financial. When people want to criticize us or want to discredit us, they say, "Oh, see, it's because you wanted all of that." Now, sure, I can argue that it's an extension of what I do, but it also is very good for my profile. And it allows us to get grants and other revenue, which pay other people. And so, I guess I'm not trying to say that therefore we're all sort of in that category, but I think it's sort of degrees of things. But I think broadly speaking, you do need people who are somewhat motivated by something. I don't know how to characterize it.

What else do you think would help scientists participate more in the evaluation of science?

Brandon Stell>> I think increased use of preprints would help. When you submit a manuscript to a journal, you can immediately put that manuscript on a preprint server for other scientists to read and reference. The only downside of publishing first as a preprint is that there is some fear that posting preprints might preclude publication in *Nature* and therefore impede the chances of getting the next grant. I think that increased use of preprints could change the uptake of post-publication peer review enormously. As more scientists put their work on preprints servers, then reviews are going to become more of a commodity. People are going to start reviewing these preprints more and more.

But why would people react more on preprints than they do on already publicized texts?

Brandon Stell>> I have a friend who submitted an article to a journal 2 years ago. It's still under review. The reason that he's putting up with this process is because he needs that stamp of approval from the journal for his career. That's the absolute only reason why he's doing it this way. He could have put his manuscript on a preprint server and immediately disseminated the information in it to all of his colleagues. I think the incentive for pre-print peer reviewing could come from the authors. By sending their preprints to colleagues and asking for comments, the authors would be chasing down the reviews of their work instead of journals doing it for them. This process might initially produce very positive reviews but the reviews would only "count" if they were public and other scientists might disagree with the initial reviews and write dissenting reviews. And it'll just snowball from there.

This kind of practice doesn't exist?

Brandon Stell>> It exists very much in physics. Theoretical physics has been using preprints for a long time. It exists but hasn't been taken up by the biomedical fields yet. But there seems to be a huge push by Howard Hughes and by the NIH to try to get this to work in biomed.

And why do you think there is resistance to pre-print reviewing in biomedicine?

Brandon Stell>> Perhaps in biomedicine, the impact factor and publishing in the higher journals carries much more value than it does in other fields. If you have a paper in *Science* or *Nature*, you strike gold for your career.

Ivan Oransky>> I also think that there's the role of patenting and licensing. It's true in some other fields, not so much high energy physics, but some of the other physics. I mean, I think that we have to think historically about universities' role in this. The incentives to license things out, to commercialize, I mean, you have these whole offices of tech transfer now that really didn't exist -- well, certainly not before the Bayh-Dole Act because universities weren't allowed to do anything like that. Even NIH does the same thing now. So, I feel like that's much more of an issue in certain preclinical sciences. Not general neuroscience or something, although then you could patent a tool, I guess. But I think that's part of the equation also.

Aaron Panofsky>> In economics, for example, there's a vigorous pre-publication culture, as well as a fetishization of something like impact factor, right? There's the top five economics journals. They matter, and everything else basically doesn't matter. And so, it doesn't bring in the patenting issue, and that might be part of the difference. But I think in economics, you have both. It's really important to get into one of these top five economics journals, but there's also a vigorous preprint culture through the National Bureau of Economic Research and SSCNET. And so, I don't know. I don't know how economists actually navigate that. But I mean, articles generally have a multi-year pre-life before they have their final published life.

Ivan Oransky>> Well, sometimes they don't even get a final published life.

Mario Biagioli>> But is it the case they improve as preprints?

Michael Chwe>> It's true, there's a prepress culture, and it even existed before the Web. Definitely there's a culture of circulating grassroots papers and all that. And also it's true that there are five to six journals which are considered the very best. And there's kind of a drop-off. But you can't get tenure -- I mean, the typical -- like, if you want to get tenure at a top place, you would need at least one of those, one journal, one article in one of those journals. Political science is almost even more that way. Like, there's only two or three journals which are considered the best. But yeah, there's less of a preprint culture in political science.

We have examined the new watchdogs, the new way science is evaluated. Let's question now the new features appearing effectively in the practice of science itself. One of the core expectations is that science must be reproducible. But what are the actual practices of reproducibility of experiments today? What does it actually mean, who would reproduce an experiment, and for what reason?

Brandon Stell>> In our lab, we might read a publication that shows that a certain compound affects the communication between two brain cells a certain way. In the lab we might try to build on that result and try to use the compound to affect a circuit of brain cells. We won't necessarily reproduce the original experiment about how the compound affected the transmission between two cells, but we will use it to see if the network changes. So we reproduce -- in every experiment that we do, we're reproducing some

of the literature that we're building on. Most experiments aren't done specifically to reproduce data, although it does happen.

Ivan Oransky>> I would just say -- and I'm not a scientist, so I wouldn't do any of the replications myself or anything like that, but I would just say that I think that science, or at least the areas of science that I see discussions about reproducibility happening in, are grappling with definitions. I think that it's something that's a little bit unclear. People use the terms differently. So, some people mean replication when they say reproducibility, and vice versa. I don't think it's completely clear what people even mean. You look at the Reproducibility Project: Cancer Biology, and they're not repeating any of the experiments. They're just getting the data sets and doing the analysis again. And so, that's kind of confusing because they're sort of using the same terminology. I'm not saying one's right or wrong. So, we sort of need some definitions. As far as who does it, though, you sort of need to think about that too. And it gets back to the Impact Factor culture. No one's going to give you any credit, really, for replicating. No one's going to publish it. There's now this *F1000 Research* section that says it will publish papers replicating former experiments, and it probably will, but that's not *Nature*, it's not *Science*. So, I think it starts with incentives.

Mario Biagioli>> So, one question, Brandon, about what would you do -- so, in the example you gave us, it was quite different from these other cases, where the reproducibility is assessed by going over the data again. So, there is no actual lab event. You are just reanalyzing the data and see if it matches or not. But in the case where you use some claim that you find in the literature in the experiment you are conducting, if the experiment doesn't work, so first, you'll try to figure out to make sure that it's not your fault. And then what do you do? So, do you write it up on *PubPeer*? Or you write an email to the author?

Brandon Stell>> So, this hasn't happened to me at my lab, but I have a friend that was working in a lab, and he was trying to reproduce some high profile results on learning and memory and they completely failed to reproduce the basic effect in the paper. He was working in the lab of a big shot, so the big shot called the other big shot's lab and said, "Look, we're having trouble. What's going on? Are we not using the right reagents?" And said the response was, "Oh, well, you have to do the experiment in this specific way." So, they said, "Okay." They went back and they tried to do it in that specific way, and it still didn't work. So finally, they were invited to the lab that originally published the data to try to reproduce it in their lab and failed. In this particular instance, it ended there and nobody ever knew about it. This is before *PubPeer* existed, and added to my motivations to create it.

Mario Biagioli>> So, but the invitation to go to the original lab and do it there, that's kind of normal? Say, if you cannot redo it in your lab, you would expect that the author would invite you to his or her lab to do it there?

Brandon Stell>> I don't know of many cases. I think it's probably not too common. And I think that if it was a smaller lab like mine that was having the problem, there would not have been an invitation to come to the lab and try to reproduce the results. Another interesting example that Ivan might know more about, was in the field of economics, where they had access to the original data and they still weren't able to reproduce the results.

Ivan Oransky>> Yeah, there are two studies. There's actually one that came out just yesterday in *Science* also. But that one found 61% of the 18 papers examined were reproducible, which actually is reasonably

high now in our new understanding¹. But the one you may be talking about actually is, I think, it's a preprint by someone that's at the Fed². We refer to it in our post about this new econ paper. But, the thing about econ is those datasets, you may have to buy the data, but they're sort of available. You're just doing different things with the same data. There is a corollary that is clinical trials. So, if you look at reproducibility attempts in clinical trials, now you're starting to see people able to do that because data, you're now mandated to make them available. That's the law. So, there are these interesting parallels in fields that have nothing to do with each other, like econ versus clinical medicine.

What sanctions are actually applied against fraudsters or people that are caught? Very basically.

Ivan Oransky>> Well, I think probably the most common sanction is nothing happens, because people aren't caught, but that's with an asterisk. If people are caught, obviously retraction is a kind of sanction, certainly if it's not voluntary. If you just look at the ORI, I mean, what are the ranges? It goes everything from 3 years of supervision if you want NIH funding again, to a 3 year ban on funding, to a lifetime ban, there's like one of those every 10 years. And that's sort of as far as it goes from the ORI. There have also been cases, one that I referred to earlier, where people are actually going to prison for federal grant fraud. So, it really runs the gamut. But most of it is, I would argue, pretty light. I think 5 years in jail is too much really, but criminal sanctions ought to be on the table in more of these cases. And a lot of people argue, just conceptually, why are we not banning the person if there's a federal finding of misconduct, why is that person ever allowed to get grants again given that there are 100 people who we know haven't committed fraud who would be eligible for that grant?

Brandon Stell>> I think Ivan's first point is extremely important that, most of the time, nothing happens. And there's obvious reasons for that. There are conflicts of interest throughout the entire system. When there's an error that's found, the authors usually don't want to admit that they made a potentially embarrassing mistake. The journal doesn't want to say that there was a problem with one of their articles. The institute doesn't want to say that there was a problem with one of their faculty. There are conflicts of interest built throughout the entire system so that these issues aren't recognized and aren't sanctioned.

Can we imagine sanction, penalties against journals also?

Ivan Oransky>> So, Adam and I called for something several years ago, a Transparency Index³, somewhat theoretically, but also to get a conversation started. The conversation really never got off the ground, so we should try again. But what if journals were held accountable? I don't know any journals, certainly not top ranked ones, that don't have their impact factor right on their homepage. Well, what if they had a retraction. This is where it gets a little tricky, because you actually don't want to punish retractions. So, you need to come up with a system that makes sense, and I don't know what that is. But if you find something that should have been caught by peer review and wasn't, or that the journal published things that are too hyped, for example, or not transparent in some other way, could you sanction them -- I mean, you could, I just don't know how effective it would be. But I do think we need to look at journals. I mean,

1 See <http://science.sciencemag.org/content/351/6280/1433> and <http://retractionwatch.com/2016/03/03/more-than-half-of-top-tier-economics-papers-are-replicable-study-finds/>

2 See <https://www.federalreserve.gov/econresdata/feds/2015/files/2015083pap.pdf>

³ <http://www.the-scientist.com/?articles.view/articleNo/32427/title/Bring-On-the-Transparency-Index/>

the scientific community could do it somehow, but ORI doesn't have any jurisdiction over a journal. The journal doesn't get federal funding.

Brandon Stell>> So, you know, I think *PubPeer* plays a role in that also because on *PubPeer*, we have a page called journals, where you can look at the number of comments for each individual journal. And that page gets a ton of traffic presumably from the journals themselves looking to see if somebody's commented on one of their journals – I don't think most scientists are interested in that. And we're seeing that journals are acting a lot more quickly than they have in the past. And we've also been approached by journals to see if they could get that information immediately. That when there's a comment on one of their papers, they could know about it immediately. So, this is kind of a sanction on journals. You know, it is kind of a black stain on them, and they want to remove that or fix that as quickly as possible.

One of the complaints of the journals is anybody and any kind of comments can be posted on your sites. So, can you explain how is moderation organized on your website?

Ivan Oransky>> Moderation, it takes so much time and effort. We could sort of just decide not to have comments, but that would kill us. Now, how does it work? I mean, functionally how it works is I'm still moderating all the comments. But someone actually, you know, reads all the comments. I mean, I don't know if this is interesting, but we use a system that's pretty typical for journals and websites, where the very first comment you leave has to be moderated no matter what. But then if you come back from the same IP address or same email address and you had one get through, you can actually -- it will be whitelisted. But we do go back and check all of them anyway at some point. It's just that you won't have to wait for it to be moderated. It may go up and then they may remove it. We may unapprove it. But we look for verifiable claims, publically verifiable claims. Because that, honestly, is the definition of libel. If it's a false statement, then we don't have any protection against that. So, there's this sort of hardcore libel thing, but there's also -- we don't want the tone to be -- even if people are saying things that are true, but then throwing in this random, snarky, personal comment, we actually don't let that in.

Brandon Stell>> We temporarily banned two people from commenting on *PubPeer*. They were consistently commenting about people instead of about data and they were derailing a scientific discussion. Our commenting policy requires comments to be based on publicly available facts.

Ivan Oransky>> We haven't banned anyone. We have people on watch lists, that's for sure, but we haven't banned anyone. I guess maybe I'm too sensitive to it as a journalist, but anytime someone can accuse us legitimately of shutting down discussion or debate, I think about that.

Brandon Stell>> Well, we have the same issue. Ban is too strong a word. We put them on ice for a little while.

Do you think the new ways for scientists to participate in the evaluation of science has changed the practice of the good old peer reviewing?

Brandon Stell>> I'm not sure it has had much of an effect on traditional peer review yet.

Ivan Oransky>> I'd probably agree. I still think there have been some discussions that have spilled over a little bit. Anonymity, for example, is one discussion. And there's been a little bit of crossover and maybe confusion because pre-publication peer review, it's blinded, but it's not anonymous, right? So, it's not anonymous to the editor, who knows exactly who everybody is. And it's not even really blinded to the

people being peer-reviewed. So, that sort of fostered a little bit of discussion about it, but I haven't seen any effect. I mean, I'd love to -- the first time someone says, "Hey, I spent more time on that peer review because I don't want to have this paper show up on *PubPeer* or *Retraction Watch*." I'd probably write a post about it if somebody said that, but I haven't heard of it.

In the Voinnet affair beginning in 2014, when the scientist Olivier Voinnet has been accused of misconducts, one key event was when Vicki Vance wrote on PubPeer. She explained that she was leaking her peer review of one of Voinnet's paper, where she had identified important flaws (even lies) but that had not been taken into account by the journal for which she was reviewing. Does this count as an effect of PubPeer on the good old peer review?

Ivan Oransky>> We've published leaked peer reviews. I think it's absurd that you peer review a paper, and then you are sworn to secrecy and can't even say that you peer reviewed it. I mean, I understand why people do it, but I hate that. In my limited experience, I had a case where I wrote a review, I think I concluded with a major revision kind of review. And then the paper ended up getting published with all of my comments -- you know, they responded to all of them in the new version, but in a different journal. He had never withdrawn it from the first journal. And I'm not even allowed to tell you who it was because I allegedly agreed to this nonsense that I shouldn't have agreed to.

Does it happen that in the midst of the discussion of a paper on PubPeer, a commentator comes to the fore and claims she was a peer reviewer but her comments had been ignored?

Brandon Stell>> That's happened. And people have come out on *PubPeer* saying that they were one of the reviewers, and they raised these issues, and they weren't addressed, and they're still not addressed.

Michael Chwe>> Like, some journals in social science, you write the review and you just send it in. You don't even know anything what happens. Sometimes, you don't even know what the result is. In some journals in social science, they will send you the result, and also the other people's report. So, at least that's kind of interesting -- the *American Journal of Sociology* is like that. And so, the cool thing about that is you get to see -- you feel like more of a participant in discussion about the paper as opposed to just sending in your own little thing. So, I think that sometimes, it would have been nice to have kind of the reviewers kind of engage in some discussion among themselves and still be anonymous. And that's something which -- like, I guess that model of *PubPeer* could be used and just being the standard journal referee. That would be interesting.

Do you think that the people who engage in that conversation -- so, they would be actually voluntary peer reviewers, right? Do you think that they should be credited?

Brandon Stell>> There are some journals that are doing that. There are some journals that will print the names of the peer reviewers on the paper. I think *Frontiers* is doing that.

Ivan Oransky>> *BMJ Open* or *BMJ* as well.

Brandon Stell>> I think that's a good idea. We allow anonymity on *PubPeer* because we want to get as many people involved as possible. But if we could flip a switch so that everyone felt like anonymity wasn't necessary anymore I think I would flip it. I think I would probably prefer if everything was out in the open. That's my personal opinion. But I feel very strongly that *PubPeer* needs to be anonymous right now in order to attract the participation of everyone and build the community.

Ivan Oransky>> There's also *Publons*. You know *Publons*?

Mario Biagioli >> It's a way to credit -- you can get credit for your reviews. And there's also another one that's on the sort of flip side where you can bring your reviews from one place to another. It's called *Rubriq*. I don't know that either of them have taken off very much, but they're sort of interesting start-ups.

Brandon Stell>>I think *Publons* is a good idea if we're going to stick with journals. You know, in the long term, so that reviewers should get credit for stuff that they're reviewing and the time they put into reviewing. Personally, I think that we should completely abandon journals.

What should be the relationships between post-publication peer review and the grants-awarding institutions?

Brandon Stell>> One of the goals of *PubPeer* is to provide something other than the impact factor that can be used to evaluate a body of work. We hope the granting agencies and selection committees will use the information on *PubPeer*.

Do you have already signs that something's happening, or no?

Brandon Stell>> There are committees that are looking at *PubPeer* comments, and using them to make hiring and funding decisions. The lawsuit we were involved in was based on a hiring decision. There was somebody that was up for a job in Mississippi, one of the universities in Mississippi, and lost that job because of comments on *PubPeer*. So, there is this specific example as well as some others that are evidence that *PubPeer* is having an influence, and committees and funding agencies are apparently using it to help make decisions.

You said you have a journal page on the website? So, do you have the equivalent for granting agencies?

Brandon Stell>> No. We probably could. I guess that data exists on *PubMed*, and we could take that in from *PubMed*. But there aren't that many funding agencies.

Ivan Oransky>> I'm not sure. There are plenty around the world. But I'm not sure how easy those metadata are to pull in. You might be able to sort industry-funded versus not industry-funded, but that's not granular enough. But I do think it's interesting, this phenomena that might be worth picking up on that Mississippi case. I mean they're actually admitting or they're acknowledging that they're making decisions based on allegations. That's far different from what you usually get with a university, which is they don't do anything except when based on obvious and clear evidence.

Brandon Stell>> But wait. I think you guys reported that Mississippi ended up saying that they took a look at the data, and they agreed with the *PubPeer* comments and that's why they rescinded their job offer. It wasn't because there was something nasty said about him on the internet.

Ivan Oransky>> Yes, you are obviously right. I don't mean that they just sort of blindly did it. Well, I get -- maybe you're right. Maybe the nuance here is not so much that they are actually acting on it, but actually saying that they're acting on it and where it came from. That's still fairly unusual, and I think so, I think *PubPeer* is having this -- it's almost forcing people to look at these things. You know, Paul Brookes did a very interesting paper on *PeerJ*. That's worth looking at because his claim, his conclusion was that the

same kinds of allegations made public have like seven times the likelihood of any sanction, or any retraction or correction, than if you had the same ones just sitting in a file drawer somewhere.

Are there some negative unintended consequences to this reorganization of the field? To say it in another way, is the value of what is to come greater than its cost?

Ivan Oransky>> I mean, I think one of the things that can happen in the short term and that people criticize us for is, does the public trust in science actually decline as they learn more? And our response to that is always, well, the real problem is when scientists keep saying there's no fraud and it doesn't matter. And then a big story comes along every year, and then you have this problem of the cover-up being worse than the crime. But I think in the long-term, that will actually go away. But I don't know. I mean, maybe that's one thing. I'd like to hear what Brandon has to say. I mean, I think that there's also a risk of -- and this is sort of related, of weaponizing post-publication peer review. You can weaponize anything. I think I sometimes do feel like retractions are weaponized. Again, it goes back to politics. You know, you could certainly do the same thing. So Brandon, did you see the story in *the Australian* about David Vaux? This is someone who's really been taking the fight to the people. He's a senior scientist and he's trying to fight misconduct and fraud, and all the things we're trying to fight. And so, people have now started leaving a lot of comments about him and his work on *PubPeer*, in fact, and *the Australian* ended up picking it up. David does a lot of looking at manipulation of things. But then, of course, there's the backlash. So, you can have that consequence, which is real. And look, Brandon and I, I'm sure also people find ways to attack us. That's the price of doing business for us.

Brandon Stell>> One thing that I'm really excited about is taking the focus off the journals and putting it on the data. And this, I hope, will eventually remove the need for journals. Some arguments made against that idea are: How are we going to know what is the good science? How do we sift through it all? How do we find research that's relevant to us? My response to these questions is that I think we'll find ways of curating papers better than the journals are doing right now and post-publication peer review will help to separate the strong papers from the weak papers.

PubPeer and Retraction Watch focus the attention on a particular set of problems, but they don't help identify problems that have been identified in other venues such as conflicts of interests in the way that science is funded, ghost writing, the file drawer problem of just not publishing stuff that you're not interested in. So what might be at stake by addressing some problems and leaving others relatively unaddressed?

Brandon Stell>> If peer review is happening out in the open, then these conflicts of interest are going to be less of a problem. Also I believe that putting negative or uninteresting results in the file drawer would be less of a problem if we can move away from this case of the impact factor. Therefore I believe that *PubPeer* has the potential to address these issues as well.

Ivan Oransky>> We're conscious of this all the time. Partly because in my day job, I think about clinical trials a lot. I've actually been trying to push people to start posting on *PubPeer* about clinical trials. They haven't quite taken that up yet, though. And you know, we see some clinical trial retractions, but it's not a lot of what we do. So, on the one hand, I'm very conscious we're searching for keys under the street light. But we try to expand our focus. We now have this weekly column in *STAT*. We write op-eds in various places. We do other big long-form pieces that we try and take some steps back and put stories in context. And whether it's successful, others will have to judge. On the other hand, we have captured the attention

of a lot of members of the press, of the media in the same way Brandon has, *PubPeer* has. And partly because people know us, we're in the media. And using us to draw the big picture of science has become a real opportunity for other reporters. And for us sometimes to weigh in, that is great. We want to encourage other people to be thinking about it. So, I think that it's a little bit of a canary in a coal mine. Finally of course am a little defensive sometimes about letting perfect be the enemy of good. We're going to do what we can do. We can't even keep up with all of that.

Michael Chwe>> Let me just add that scientific conduct and integrity makes students even more defensive, and more kind of short-sighted. So like, I think that more graduate students think "I've got to do everything right. I've got to store the data right way." You know, the IRB and all this stuff which I didn't have to think about. And it just puts more obstacles in their way, and makes them think that if I just do the following things correctly I'll get a thesis, as opposed to thinking about an idea. So, I worry about that. I think that that's a potential problem too.

A question for you, Brandon. When you describe the science you would like to see, you said that you would like to see robust science. You use this word a lot, robust. Can you explain on this a little bit?

Brandon Stell>> By robust, I mean strong data. Data that's repeatable and stands the test of time.

Mario Biagioli>> Well, I think robust is an adjective that in science and technology studies have been going around a lot because people get kind of self-conscious about using the T word. So, people don't use truth or true. There is a shame factor, like you know, we are too cool to use that term. So, robust and robustness has effectively taken over the truth slot without sounding -- so, people have been doing -- and you understand the predicament. You know, people try to have a term that both conveys robustness, but I don't think there is a definition. And also, I actually have the same question about conflict of interest. I'm quite conceptually fascinated by it because of course I am against it. And of course I believe that disclosure will go a fairly long way toward taking care of the problem. But at the same time, conflict of interest is desired because, in a sense, it's like to say, well, you know, we're talking effective about relations. So, everybody says, "Yes, science in one way or another is based on collaboration, linkages, relations, and so on." But then there are relations that are not good. So, in a sense, I think it would be a serious challenge to do a philosophically sound definition of conflict of interest. And you understand I'm not saying conflict of interest is a stupid term, we should not use it. You know, we need to fight conflict of interest. But actually, I think that the definition of conflict of interest is tricky. And I think that people say, "Well, let's make everything transparent." Yes, but I don't think that transparency by itself would take care of it.

Brandon Stell>> For science, a better term for conflict of interest, might be the word "bias". Because if the conflict of interest is causing a bias, then it's a problem in science.