

Peer Review Review

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Christoph Bartneck: [00:00:00] In October 2023, I had the privilege to talk at the Nerd Night in Christchurch. This event series operates at the intersection of comedy, popular culture, and science. I talked about my adventures in exploring the peer review process. Some of them to the annoyance of my fellow scientists, conference organizers, and predatory publishers. But always with a nod to comic effect and a focus on the over competitive beast we call academia. Please enjoy this live recording of the Peer Review Review.

This is the Human Robot Interaction Podcast. I'm your host, Christoph Bartneck.

Hi, [00:01:00] I'm Christoph and I'm an academic. You've been to this kind of meetings before, right? You know how this works. Okay, let's try again. Hi, I'm Christoph and I'm an academic. Thank you.

I've been an academic for many years and I'm addicted to investigating the world, how it works, how people work, how robots work. And I conduct studies where I try to figure stuff out and unlike influencers, I don't just post this on my blog or I go on the socials and make a blast. I expose myself to the peer review process.

And the way this works is that I find [00:02:00] something out and then I write a paper about it. And this paper then I send to a journal. And the editor of the journal then goes about to send this to a group of experts who will look at this work and check if it's okay, if it's novel, if it is correct. And they, this goes through a couple of cycles of feedback and revisions.

And if I'm lucky, only if I'm lucky, does the paper actually get published. And then you get to read it, for a princely sum, of course. Of which scientists guess absolutely nothing. Now, in science studies, what we do is that we look at how scientists do science. So we look at this peer review process to see [00:03:00] how it works.

And, well, maybe it doesn't work. And as a scientist, of course we do a controlled experiment. Right? That's science. So, we have good papers, we have bad papers, and we can send them to the journals, and either they get accepted, or they get rejected. That's science for you. And, if a good paper gets accepted, that's great.

Working. That's how it's supposed to be. If a bad paper gets rejected, it's also fine. We're doing good. The problem is when good papers get rejected, or bad papers get accepted. That's an indication that the whole process failed. I'm going to talk today about some of my adventures that I had in trying to conduct studies like this, and, well, how they were received by the scientific community, let's say.

Let's start with good papers getting rejected. Now, [00:04:00] the problem here is that first you need to have good papers, I'm happy if I can write a couple of them a year and that's it, but it's never clear if a paper is good or bad. I mean, how do you know up front? So back in 1982, Peters and Ceci came up with a brilliant idea.

They went to journals, in this case psychology, and they took 12 papers that were previously published. They went through the peer review process. These were good papers because otherwise they shouldn't be there. And they just resubmitted them. They changed the title a bit, changed the authors, resubmitted it.

From these twelve, three of them were detected as plagiarism. Okay. Laughter So, guests, please help me. Of the nine that made it through, how many of those do you think were rejected? Five. [00:05:00] Four? It was 8. 8 out of 9 papers were rejected, and not because of a lack of novelty. Because that would be an obvious reason, right?

Hey, we've read this before, this is already known. No, it was deemed that these papers were of insufficient quality, and therefore they got rejected. Now, that's problematic, I suppose. And it inspired me, because, well, why not? So, I went to the best conference in my field, and I thought, Let's do this. So I took 12 papers, and I, these are the papers, this is one of them, and I changed, took all the meta information away so we couldn't tell it has been published, and I changed the title, and I rephrased the abstract so it's not so obvious, and then I submitted it.

I thought I was reasonably careful because these days you can do a bit of tracking, you can [00:06:00] figure out who submitted this paper, you can check the IP addresses, you know. So I was careful, I created some fake identities, like Gmail accounts, new names. I used a VPN to mask my IP address. And I thought I had it all worked out.

And then, a couple of weeks in, I started to receive emails to my fake email account saying, Hey, we think you've done something naughty. You submitted all these papers which we had already received. And then I was like, okay, cool, yeah. I explained to them, well, this is the experiment, this is what I'm doing, and I would really appreciate it if you would let this go, so that we can learn something here.

Can you please let these papers into the review process? And originally they were like, yeah, we'll consider this. But first, you have to tell us who you are. Now, I was scared. I didn't like that. And so I was a bit hesitant. [00:07:00] And eventually, they found me out.

Now, the interesting thing is, how did the organization respond? Because after it became clear who I was, The whole idea of continuing this experiment was stopped. And the only thing they did was essentially an investigation and a prosecution and they wanted to throw me out of the ACM and all sorts of upheaval and with a couple of email exchanges it was clear that this was nonsense and nothing ever really happened.

The interesting part here is that the conference was not interested in a self reflection and a self evaluation. And that I think was pretty, pretty bad. But of course, there's an underlying problem that is good papers get rejected and that shouldn't be the case. So I thought to myself, how can we solve this?

What is the solution? So I came up with a brand [00:08:00] new scientific journal called Interaction Studies. And here the idea was if you have a paper that got rejected three times, then you automatically get accepted at this journal.

The only thing I ask of you is please submit the peer reviewed comments you received and you can write a response and we publish everything together in a nice package And then people can read the discourse. What happened? How do people see this? And I thought this was a really really good idea We recruited a team of editors and we advertised it and here's another question for you How many submissions do you think we received in the first year?

You got it. Zero. No scientists wanted to admit that their papers was previously rejected. So, [00:09:00] that wasn't really good, I suppose, either. It was a bit disappointing, and you might think, oh, this is a pretty wacky idea for a journal, right? This is not how it's supposed to be. But believe me, my colleagues in science are much smarter and much better than me. So, for example, we've got the journal of trial and error, which celebrates failure, because there's so much to learn from failure.

And my absolute favorite is the Journal of Universal Rejection.

You can feel comfort in the predictability of the peer review process. Every paper that is submitted gets rejected. And if you write a good paper, the editors actually write you a nice letter back explaining why it was rejected. But it's nicely predictable. Now let's switch over to bad papers that get accepted.

And this is easier because it's so much easier to make bad [00:10:00] papers. So I got invited to a nuclear physics conference and Yeah, sure, nuclear physics, why not, I can do that. So I took my iPhone, and I'm, you know this, right, you text message, you type something, I typed in nuclear physics, and I just type autocomplete, autocomplete, you know, just whatever came up, I just hammer it.

And that resulted in that piece of text there, and I again created a fake identity and took the first Wikipedia graphic I could find, fake, um, fine, send it off. A few hours later, I got a notification that the paper was accepted.

Well, so, I blogged about it, and the media caught on, and we all had a giggle, and we all thought it was funny, and it is.

But if you think, again, that, again, I'm nothing [00:11:00] compared to my colleagues, my absolute favorite accepted paper ever is, Get me off your fucking mailing list.

This paper was accepted in the International Journal of Advanced Computer Technology. Bravo! They had graphs, they had data, fantastic. So, we can keep on doing this. You know, for every bad journal or conference, we can send crap their direction. And of course, these days with ChatGPT, it's become so much easier.

But of course, you know, how often can you repeat this joke? So I, again, I received an invitation this time it was for the International Conference and Expo on Robotics and Artificial Intelligence in Rome. So I thought, okay, this time I've got to do something different. And so I took a really deep dive into the organization because I wanted to understand who are the people behind it.

Who's doing this? Cause make no mistake. This is a business. They, they charge people. Um, and so it's just [00:12:00] business. And so I made this deep network plot of the people and the companies and how they relate. And of course I wrote a crap paper. And then I thought, okay, you know, I kind of like warmed up to them and I communicated with them and I said, Hey, you know, your conference is a really great idea.

You know, I'd really like to interview about it. Is that possible? I've got a podcast series and can I help you promote your conference? And I said like, yeah, sure, let's do that. And. We had an interview and, um, we started talking and I kind of asked them, how does your peer review process work and what do you do?

And they, of course, told me the story that everybody wants to hear. And eventually I revealed to them, look, I wrote a complete nonsense paper. Why did you accept it? And they just hang up. And if you want to listen to that episode, this is my podcast. And, you know, have a listen. It's really quite interesting to see.

And since then, I've got like a little page where I collect bad conferences, flaky [00:13:00] conferences, predatory journals. And every once in a while, I get a message where people start to complain about, Hey, we're going to tell the New Zealand government if you don't take us off from this list. And at this point, my success is measured by the enemies I make.

So, I'm thoroughly enjoying that. But there's so much more. So there's the conference on human robot interaction. I feel dearly for this conference. And they're slightly different because they don't have any of the peer reviewed process, but at the end of it, all the editors come together. They fly to a city, and we had around 50 people there.

And the idea is they discuss everything. And what we looked is, well, from the rankings before, I mean, you get, you, papers get points. Before and after, we compared what, what changed and so it was around, so we estimated that bringing everybody together costs around 200, [00:14:00] 000. It does not include the salary of the people, just flights, hotels, everything.

And there were eight changes to the program. Eight. Which means that every change costs around 25, 000 to make. And this is not big changes. People, papers that were previously deemed acceptable and they were demoted to reject it. There was not suddenly like something terribly wrong with them. There was small adjustments or the papers that were previously rejected and suddenly were accepted.

They were already good papers. It was just a little bit over the hump and we wrote a paper and we submitted it and not to the, actually we sent it to the organizers first because we wanted to first discuss it and they didn't like it. The feedback we got was like, you actually conducted an illegal experiment because we didn't know about this, and you can't do that, and But really, they just didn't want to.

And the real question is, [00:15:00] why is that? Why are scientists who are supposed to be interested and curious so reluctant to reflect upon themselves? It's really puzzling.

Now my interpretation is that this is largely based on the facts of what I call over competitiveness. A competitive system, at some point, you can make it more and more competitive, and at some point, it just breaks. So, let's start a little quest, and please don't be shy. Who of you has got a PhD? Two, three, okay, I mean, whoever shows up to an event like this, I mean, it's not really representative.

But let's say it's a small number, from the general population. Now, this is data from the UK and around when you finish your [00:16:00] PhD, half of the people finish a PhD immediately leave science and never come back.

Now some of them start an early career and 70 percent maybe go to government agencies and the people who survived early career. Again, 26 percent leave science. Only around 3. 5 percent make it to a permanent position in academia. And less than half a percent of the people become a professor. Now, if you want any kind of definition of overcompetitiveness, this is it.

It's crazy. And People are competing all the time. I compete with all my colleagues about resources, funding, students, teaching obligations. It's crazy. It's absolutely insane the amount of pressure that you have. And just to give you an example, University of Canterbury, shall we try? If you want to have a career at University of Canterbury, these are [00:17:00] the promotion criteria.

There are four levels and you have to do academic citizenship and service. You have to do teaching, you have to do research and you have to do more research. And this is just the higher level. So if you drill in, like, okay, now only, only research now, what exactly do you have to do? Then you come up with advancing knowledge and here are examples of things you're supposed to do, quality of journal published in Q1 journals and everything, and reputation appropriateness of book publishers.

You're supposed to supervise and mentor and here are all the things you're supposed to do. You're supposed to lead groups and fostering interdisciplinary research. You're supposed to translate and bring your products to the market, commercialization. You're supposed to engage with the community. You're supposed to support Vision Maturanga.

It's like a lot of things you're supposed to do. And the point is, nobody can. It's, it's, it's the everything is important [00:18:00] problem, where there's so much to ask that it's just impossible. And at this point, the system breaks. And that's when academics, not just in some sort of unknown university somewhere in India, start to misbehave.

No, Stanford, Harvard, scientists start to invent data so that they can write more convincing papers and have a better career. As a matter of fact, right now, I can go to the internet and buy myself authorship of an accepted scientific paper. There's a marketplace for that. It's possible. In New Zealand, we've got the performance based research funding.

Every couple of years, I get evaluated and I get a school grade about how super duper of a researcher I am. And depending on that, money is given out. And the idea here is, of course, competition, right? So, it's not like [00:19:00] everybody gets the same. The good people get more money and the bad people get not so much money.

That's the idea competition But of course this money right now goes into the big part of the university and me as a researcher I get zero from the whole performance based research funding. I get zero dollars to do any research No extra research is done because of it. It just disappears. So In this competitive environment where everybody's constantly afraid and scared and competing and people, of course, researchers become reluctant to challenge the game.

So think about what we've done so far is we challenge the rules of the game while playing the game. That's like, I don't know, playing soccer and suddenly you want to discuss the rules of offside. It doesn't work. You can't do that. But the problem is that the game never [00:20:00] stops. There's never a break where we sit down and say, Hey, wait a minute, is this actually a good set of rules?

Should we do this? It doesn't happen, it continues. And of course, if you previously submitted to a conference, and suddenly it's exposed that this is a bad conference, then your reputation will suffer. So nobody wants that. So, the whole system becomes very conservative and afraid of change. And what we have to ask ourselves is, Is the university just a company?

Is teaching and research a means to earn money? Or is money a means to do teaching and research? That is the fundamental question that we have to ask ourselves. And these days, it looks like a university is just a company.

And if we abandon truth for money, then might is right. Thank you.[00:21:00]