CARNEGIE COUNCIL for Ethics in International Affairs

Reinventing Discovery: The New Era of Networked Science

Public Affairs

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Introduction

JOANNE MYERS: Good afternoon. I'm Joanne Myers, director of Public Affairs Programs, and on behalf of the Carnegie Council I'd like to thank you all for joining us.

Our speaker is Michael Nielsen. He is not the usual foreign policy expert that you are used to hearing at the Carnegie Council, as he was originally trained as a theoretical physicist and worked on quantum computing and related topics before leaving academia to work full time as an advocate for open science. Still, what he is working on has the potential to impact all of us.

Today he will be discussing his recently published book, *Reinventing Discovery: The New Era of Networked Science*, in which he examines how the online world is revolutionizing scientific discovery and why the revolution is just beginning.

While most clichés lose their force through overuse and time, the idea that the Internet is changing the world, especially the world of discovery, cannot be repeated often enough. With each new passing day, powerful new cognitive tools enabled by the Internet are greatly accelerating scientific discovery regardless of national borders.

Reinventing Discovery is about the potential to transform the way scientific discoveries are made. As Michael will soon tell us, it is now possible to connect vast numbers of researchers, and even ordinary citizens, who might have useful expertise to bring to bear on a scientific problem. This opens the way to the possibility of solving problems that would be completely intractable in the traditional mode of scientists working alone or in small groups.

For example, he tells us about the Polymath Project, where mathematicians come together over the Internet to solve an interesting puzzle. He also writes about new ways of tracking medical outbreaks, to ways of identifying galaxies—all projects which, because of the Internet, individuals from all over the world were able to participate in in order to create solutions and shed light on some of the mysteries of life.

As exciting as all this is, there are still many challenges. The biggest is that scientists are often reluctant to share their ideas and data in ways that speed up the advancement of science.

So the question is: How can we convert individual insight into collective understanding in a manner that can improve the way science is done? For the answer, please join me in welcoming Michael Nielsen to our program this afternoon.

Thank you, Michael.

Remarks

MICHAEL NIELSEN: Thanks, everybody.

I started my career as a scientist about 14-15 years ago now. If you had told me at the beginning that I would end up working on issues relevant to policy, I would really actually have been shocked. And through the start of this talk it won't necessarily seem that the issues are necessarily all that relevant. But I hope as we move towards the end that in fact you will see that there is actually quite a bit of relevance.

I am going to start, in fact, just with a couple of stories about how online tools, about how the network, is impacting science. I think it's better to start that way rather than to start with some abstract description.

I am going to start with a mathematician, a fellow at Cambridge University named Tim Gowers. Gowers is a professor of mathematics at Cambridge, certainly one of the world's leading mathematicians. He is the recipient of an award known as the Fields Medal, which is often compared to the Nobel Prize in mathematics. There is no Nobel Prize in mathematics.

He is also, in fact, a blogger. He is perhaps not your standard profile for a blogger, but it's surprisingly common amongst leading mathematicians. There are 42 Fields Medalists now living and four of them, so about one in ten, have actually started a blog.

So we have this Fields Medalist blogging. In January of 2009 he wrote a very striking post with the title, "Is massively collaborative mathematics possible?" What he was proposing doing in this post was using his blog as a medium to attack a difficult unsolved mathematical problem, a problem which he said he would love to solve, entirely in the open, using his blog to post his ideas and his path of progress.

What's more, he issued an open invitation inviting anybody in the world who thought that they had an idea to contribute to post their idea in the comments section of the blog, and his hope was that by combining the ideas of many minds he could make easy work of this hard mathematical problem. He called his experiment the Polymath Project.

Actually, things got off to a very slow start. The first seven hours not a single person made any suggestions at all—perhaps not so surprising.

But then, a mathematician from the University of British Columbia, a fellow named Jozsef Solymosi, wrote in just a brief suggestion. Fifteen minutes after that, a high school teacher from Arizona named Jason Dyer made a comment. And three minutes after that, a mathematician from UCLA, a fellow named Terence Tao, also a Fields Medalist actually, made a comment. Things basically exploded at this point.

I wasn't actually a substantive contributor, but I was following along very closely right from the start. Ideas were being proposed very, very quickly, and then often rapidly developed and mutated and changed form. Sometimes they'd be discarded and sometimes they would come to be part of the accepted canon of knowledge about this problem.

In fact, over 37 days, 27 people contributed 800 substantive mathematical comments containing 170,000 words. That's a lot of mathematics very quickly. I found this out in a really visceral way, because it was actually quite hard to just keep up. Just reading it was hard, never mind contributing.

Gowers summed it up actually rather nicely. He said that "the polymath process is to normal research as driving is to pushing a car." Actually, I had to push a car up the highway once as a teenager and have some sense of the difference. Quite a difference.

So at the end of the 37 days—the reason I mentioned that number—Gowers announced on his blog that the problem had most probably been solved. They did have to go back and check a number of things, and everything did in fact check out.

The point was that the back of this problem had indeed been broken. They had solved their problem.

This process has been repeated a number of times since then, with I should say varying degrees of success. Not every project has been as successful as this first one. But overall I think it has been very successful, and some of the later projects have actually involved many more people.

Of course, the reason I'm talking about it tonight is not because of the details of this particular mathematical problem or anything like that. It's not important because of that particular problem, no matter how much you might think that problem matters or doesn't matter. It's important because of what it suggests.

I think that what it suggests is that actually we can use these tools as cognitive tools, we can use them in some sense to amplify our collective intelligence. By that I just mean speeding up the solution of some of the very hardest problems.

If any of you have read, say, James Surowiecki's book *The Wisdom of Crowds*, these kinds of notions, when people think about group intelligence, they're often thinking about relatively simple problems. But this kind of problem was right at the limit of humanity's ability to solve. It was challenging certainly some of the brightest people in the world. It's very interesting and striking that these tools helped those people solve that problem—and maybe not just in mathematics, but maybe broadly across many different fields.

There are many such experiments going on in networked science.

I want to change fields completely and come at this from a totally different direction now with a second story, partly just to try to get some triangulation on the different ways this can happen.

I'm going to talk about a project called Galaxy Zoo. This is kind of a website at some level. You can go to galaxyzoo.org and log in and create an account.

What it is, is it's a cosmological census. A couple of astronomers at Oxford University in 2007 had the idea—what they had was they had photographs of a million galaxies that had been taken by a robotic telescope in New Mexico. When you've got photos of a million galaxies, you've got a problem: it's great to have all this data, but it's hard to analyze it. What do you do? Do you sit there?

Well, one of these two people, Kevin Schawinski, had actually sat for an entire week classifying 50,000 of those galaxies as being either spiral galaxies, like our Milky Way, or elliptical galaxies, just a big ball of stars, for a project he was doing. If you figure it out, that means that if he was spending 12 hours a day doing it for seven days, he had to classify a galaxy every six seconds. He said at the end of the week basically he didn't ever want to look at a galaxy again. And you've still got a million to go. So he had a problem.

They decided that they would create this website that would recruit online volunteers. Anybody who wanted to sign up could come along, and you sign up and you get shown a picture of a galaxy. In the first case, you just get asked: Is it a spiral or is an elliptical galaxy? That was all you were asked.

People got a little bit of training, but after you complete their ten-minute training course, you can do it.

The reason they were doing this, I should say by the way, is it turns out that actually human beings are still better than the best computers at solving this problem of determining whether it's a spiral or elliptical. A lot of these images, if you see them in books or on television, they always show you a beautiful spiral, George Lucas-type spiral galaxy, that's really pretty. But actually most of these images are tiny little smudges, and you look at them and peer, and it's hard to tell.

How successful have they been? They've got 250,000 volunteers to do more than 150 million galaxy classifications. That's a lot of galaxy classifications.

Why does this matter? Well, let me tell you the story of just one of the discoveries that they've made.

About the middle of 2007, just as the project was getting started, somebody involved in Galaxy Zoo, using the handle Nightblizzard, of all things, posted to the forum to say, "I've seen this funny little galaxy. It's a little green galaxy, which was unusual. It was very small, it was very compact, and it was extremely bright." He said, "This doesn't look like any sort of galaxy I've ever seen before. What type of galaxy is this?" Well, nobody knew, including none of the professional astronomers who were following the forum.

A few other people in the ensuing weeks made similar observations about small green, very bright galaxies. But nobody connected the dots.

And then, in August of 2007, a Dutch schoolteacher, named Hanny van Arkel, did something. It was inspired. It was a joke, but it was inspired. She found one of these little galaxies. She also had posted to the forum, but she made a joke. She dubbed the galaxy a "green pea galaxy," because it does indeed look like a little green pea, and she titled her post, "Give peas a chance." [Laughter]

A whole bunch of the participants thought this was hilarious. They did two things. They started digging up their own Green Pea galaxies with a similar profile and they started making terrible pea jokes. My favorite was, "Peas stop." [Laughter]

Okay, big deal. Who cares?

Well, a hunt started to take shape. At first it was humorous, but gradually it gets more and more serious. As of today, the hunt is still going on.

There are more than 20,000 posts involving hundreds of people in this hunt. What they did was they gradually taught themselves all sorts of relevant astronomy. In particular, they taught themselves a field called spectroscopy. It doesn't matter if you don't know what that is. It's just a way of understanding all sorts of stuff about the galaxies. They developed a basic theory of these Green Pea galaxies.

First of all, they confirmed with professional astronomers that it was indeed a new type of galaxy. It's a very bright star-forming galaxy, very compact, surrounded by very, very hot ionized oxygen, which turns out to be very unusual for galaxies.

Probably the really interesting thing is that, even though it's a very small galaxy, about a tenth the size of our Milky Way, it's forming about 40 new stars every year. The Milky Way forms about two or three new stars every year. So it is a star-forming galaxy.

You see this interesting thing takes place: they do become more and more sophisticated. Eventually, they are sending sophisticated database queries with all kinds of spectroscopic criteria off to this database server to do automated searches to find matching galaxies.

I'm a theoretical physicist. At first, it's easy for me to understand what they are doing. Eventually, I can't follow because it has become very, very detailed.

There is a little industry now of people, professional astronomers, writing papers about this new class of Green Pea galaxies.

So take a step back and think: What does this mean? You know, it's nice that there is an industry of professional astronomers doing this. But a bunch of amateurs discovered an entirely new class of galaxy. What did you do over your summer vacation? That's what they did. That's remarkable. That's truly remarkable. It's a new type of galaxy. Overall, the galaxy has led to 22 scientific papers so far.

Let me tell you just about one of the most prolific contributors to the project. It's a woman named Aida Berges. She is a 53-year-old grandmother of two—or she was 53 when the profile I'm basing this on was written—from the Dominican Republic. She lives in Puerto Rico now. She's a stay-at-home mom.

She classifies roughly 500 galaxies per week, plus does many, many forum posts, and she participates in several side projects to Galaxy Zoo. When she was asked how she found out about it, she said she saw something on CNN about Galaxy Zoo and—this is a quote—"I went to Galaxy Zoo and my life changed forever. It was like coming home for me."

I just want to tell you about one of the things that she has discovered. There are these things called hypervelocity stars. What a hypervelocity star is—we have the Milky Way galaxy. These are stars that are sort of inside it but they are shooting through so incredibly rapidly that they are not bound to the galaxy; they'll eventually shoot off out into the rest of the universe if you wait long enough.

The first of these were discovered in, I think it was, 2005 by somebody at JPL [NASA's Jet Propulsion Laboratory]. There are about 29 now. Aida Berges discovered two of those. Pretty interesting kind of a thing for a 53-year-old grandmother of two to be doing from Puerto Rico.

The way I like to think about it, this kind of project, Galaxy Zoo, is an example of what you might call citizen science. At some level, if we think about society as a whole and then the scientific community, to some extent the scientific community has historically carried on its deliberations, its process, rather separate, in many ways, from the rest of society.

We have a few bridging institutions which bridge those two. There are certainly people documentaries and this kind of thing, people doing science outreach of various kinds; there's the education system; there's a few other ways in which science enters our society. But this citizen science is a new type of bridging institution between science and the rest of society. It's in the very early days. Nobody knows what it will ultimately lead to.

These people starting Galaxy Zoo started with no budget. Basically, *chutzpah* is what they had. They had no idea that it was going to be so popular. So who knows what else will be possible?

I think there is some potential here, depending on how those institutions grow and change, to have some impact on the relationship between science and society as a whole.

A couple of things I've talked about is this idea that maybe networked science can perhaps help speed up the process of discovery, and also the idea that it can potentially help change the relationship between science and society. This is a very optimistic point of view, obviously.

The reason I got interested in this is actually for very pessimistic reasons. What I want to do now is talk about some of my reasons for pessimism, particularly the idea that scientists have in fact, despite these examples, been tremendously inhibited in how they adopt many of these ideas and many of these tools. I'm going to start with an example.

It's a website called Qwiki. It was started in 2005 by a graduate student at Caltech, a fellow named John Stockton. His idea was a very good one. It was to create kind of a research-level Wikipedia for quantum computing, my field and his field. The idea was that it would be almost a super-textbook for the field. It would contain all sorts of information about the latest research discoveries, the latest research breakthroughs in that field. It would contain descriptions of the big open problems, people's speculation about how to attack those problems, all this kind of stuff. Potentially a very good idea.

It was announced at a workshop at Caltech in August of 2005. I happened to be at that workshop and I talked to a lot of people about it. A whole bunch of people were very skeptical. They said, "This sounds like a complete waste of time. Why would people want to do this?" And a whole lot of people responded very positively, on the other side.

We had these interesting little discussions. They said, "Oh, you could use it to do this, you could use it do that. It would be great if somebody did this, dah, dah, dah, dah."

"Okay, sounds great."

Then I'd say, "So what are you planning to contribute?"

"Oh, no, no, no, no. I don't have time. But, sure, I really hope somebody else will do this."

Well, if everybody has that response, what happens? If you actually look today, the majority of contributors to the Qwiki are spammers, people writing about Viagra or whatever. This is not good stuff. So it has failed.

In fact, if you look at similar efforts, because there have been dozens of attempts to do similar things in all sorts of fields—there's a [inaudible] list which has failed, there's a string theory wiki which has failed, and dozens of others wikis which have failed—despite the fact that in many cases these sites have been very well executed, very well done.

Just to jump to a completely different set of examples, there have been many attempts made to create so-called scientific social networks, kind of a Facebook-for-scientists sort of idea. So the idea is to connect scientists to other people with complementary interests so they can share data, share ideas, share computer code.

This sounds like a very good idea. In fact, millions of dollars have been spent by dozens of organizations trying to create such sites.

They are often very well done. You create an account on such a site, you log in, you look around, and you're in a virtual ghost town. There is nobody present. There is nobody doing anything. So they have failed.

This is not to disparage those sites. There is a systematic reason why this is happening.

The reason—I almost said it before—is, particularly if you are a young scientist—so imagine you are in your mid-20s, you've got your Ph.D. from MIT [Massachusetts Institute of Technology], and you're trying to build your career. You love science. You'd like to get a job at a major research institution, for example.You know that there is just one thing that you can do to get that job, and that is you build your career by getting a very good record of scientific papers and of scientific grants, which is what follows from that.

So even if you think a site like the Qwiki or the scientific social networks are the best idea ever, here's the calculus: Are you going to spend 200 hours this year writing a couple of mediocre scientific papers that nobody will ever read, or are you going to spend that same time making a long series of brilliant contributions to the Qwiki?

Well, if you value the ability to eat and to have a career in science, you will write those two mediocre scientific papers. It's a much better investment of your time. So even if you really believe in these ideas, it's a very hard tradeoff to make.

Projects like the Polymath Project, you might say, "Well, how does that fit in?" Actually, it fits in just perfectly to this.

At the start of that project, they had a discussion: "What are we going to do? What's the output going to be? It's going to be a couple of scientific papers at the end." It was an unconventional means to a very conventional end. So in that sense it was a conservative project.

This might sound like a boring discussion of academic politics. Who cares? It's very dry at some level.

But, of course, if you go back to the basics, what's at stake here is our ability to create cognitive tools which can actually help expand our ability to solve some of these really challenging intellectual problems. That's something that matters for everybody in society. It's not just a question of boring academic politics. That's why I got involved. That's why I care so much about this particular set of issues.

I'm going to finish off by talking about a few small cases—they're actually not so small at all—a few cases which give you some cause for optimism, and suggest some ways forward. They are cases where in fact the way science is done has changed.

The first case involves the Human Genome Project. Probably everybody knows that the human genome data is actually publicly available. You can take your iPhone out and go and download the data right now. Every time you read in the newspapers that so-and-so a gene is connected to such-and-such a disease, you're actually getting the benefit of that open data, because the people who made that discovery are not connected to the original Human Genome Project; they've gone and downloaded the data and they have used it to make that connection.

Why is that data open? Well, actually, there's an interesting story behind that. You go back to the early 1990s and there was a lot of discussion amongst leading biologists about what would happen to the genome data. Everybody could see that it was in humanity's collective best interest for this data to be shared online as quickly as possible. But that doesn't mean necessarily that it was in any individual researcher's best interest to be sharing it online. It's not a publication; you're not going to get a whole lot of credit for it.

But, fortunately, they could all see that this was a problem. So a meeting was convened in Bermuda in 1996 where the leading people in the Human Genome Project; and also Craig Venter, who would lead the private effort to sequence the genome, was there; leading representatives from the grant agencies were there. What happened? They came together and they talked this issue over.

People said, "Well, I'm not unilaterally willing to go ahead and share my data. But if everybody else will agree to do it, sure I'd love it. It's obviously in our collective best interest."

So they came up with what are called the Bermuda Principles, which state that within 24 hours of taking the data it should be uploaded online to GenBank and put into the public domain.

Then the people from the grant agencies went back to the grant agencies and they baked them into policy at the agencies within one year. What that meant was that if you wanted to get money to work on the genome, you needed to agree to abide by those principles. It was an enforcement mechanism. That's why the data became available, in fact why it became available so early in the whole process.

It made its way ultimately to the very highest levels. Clinton and Blair issued a joint statement in April of 2000 praising the Bermuda Principles and urging that every single country in the world adopt similar principles. So that's a very nice story.

Unfortunately, it's far from being broadly the case. Even if you just look at other types of genetic data—here's all of human scientific knowledge and here's the genome. It's important, but it's a tiny, tiny fraction. Even if you just look at other types of genetic data, the situation is patchy, to put it politely.

One biologist of my acquaintance made the comment to me after a talk that he had been "sitting on the genome for an entire species of life for more than a year." So there it was sitting on his hard disk in his lab basically undergoing bit rot. It's a whole species of life. That's not a small thing.

This is generically the case when you talk to scientists. Sometimes when I give talks to scientists I will ask them, "Put up your hands if you systematically share your data." If there's 100 people in the room, maybe four or five people will stick up their hands.

There are a few fields, by the way, where that is not true, where things are much better. Astronomy is a good example. But in a lot of fields that's the case.

That's one nice example.

There's a bigger example, an example from which we are all benefiting right now in many ways: you have to go back to the dawn of modern science to see a really big cultural shift.

Go back to 1610. Galileo has just built his first astronomical telescope. Early in the morning of July 25th he takes his telescope, he points it at Saturn, and what he's expecting to see—this is the first time he has done this—is a little disc. He doesn't see a little disc. He sees a little disc with bumps on either side of it, totally unlike the other planets. Of course what he is seeing is the first ever hint of the rings of Saturn.

It's hard for us to remember now, but at the time that was a huge discovery. The heavens had not changed, or had barely changed, in our understanding of them, since prehistoric times.

So Galileo straightaway knows that this is huge. So what does he do? Does he announce it to the world? No. He writes a description of the discovery down in his private working notes, then he scrambles the letters into an anagram, and within one day he sends it off to four of his astronomer colleagues, including the great astronomer Kepler. What that means is that if, say, Kepler later makes the same discovery, Galileo can reveal the anagram and claim the credit, but in the meantime he hasn't revealed anything at all. Pretty clever, huh?

So was Galileo a bad guy? No. It was common at the time. Leonardo did it, Huygens did it, Isaac Newton did it, Robert Hooke—if you took physics at all in high school, you might remember Hooke's Law; that was revealed as an anagram. Common practice at the time, because there was no incentive at all to reveal this kind of knowledge, or very little incentive to reveal this kind of knowledge.

We take it for granted today that scientists will at least write a paper about their discoveries. But in fact there was a great cultural change that occurred in the second half of the 17th century and in the 18th century. There was a linkage made between your publishing as a scientist and your career progress. That actually took, let's say, a hundred or so years for that connection to be forged.

In the meantime, people like Newton were very resistant. They might publish after decades. They might not publish some of their discoveries at all. Who knows what was lost during that time because of it? So it's a great thing that this transition has occurred. It's a wonderful thing that this transition has occurred. That's why we have benefited so much from scientific discoveries. I said before we are all benefiting from it right now.

We have these incandescent bulbs in here, and it's a good thing that people like Maxwell and Faraday published their results in the early 1800s which established the principles by which those bulbs work. It would be a very different society if they hadn't worked under that kind of system. Faraday actually, when asked the secret of his success early in his life by the chemist William Crookes, said it could be summed up in three words: "Work, finish, publish." In 1600 it was work, finish.

There is a historian at Stanford, Paul David, who studied the question of how did this transition occur. He sums it up. I'll sum up his 120-page paper in two words. The two words are "funder pressure" or "patron pressure."

I'll just give the example of Galileo again. This is my example, not actually David's example, but it's in his spirit. Galileo had earlier made a big discovery. The rings of Saturn were his second big discovery. The first one was of the four moons of Jupiter. This made him famous across Europe at the time because it was the first change in our knowledge of the heavens since ancient times. He discovered that Jupiter had four moons, what we now call the Galilean Moons.

Funnily enough, he actually published an account of these four moons. Why did he do that? It took him about six weeks. Well, he wasn't happy with his living situation at the time. So he wrote off to several potential patrons, including the Medici family, and said, "I will name these moons after you if you agree to become my patrons." [Laughter]

If you look at the cover of the pamphlet that he published, there's the title, *Siderius Nuncius (The Starry Messenger)*; then there's Galileo Galilei; and then, in big font, there is "Medici Sidera." You know what the most important words on that cover were, "the Medicean Moons." It's bad luck for the Medici we now call them "the Galilean Moons." But that's beside the point. So that kind of patron pressure. There were different incentives for the people funding the work.

Well, that's a very similar story to the one I told about the Bermuda Principles and the openness of the Human Genome Project. At the end of the day, it was the grant agencies that had the money that actually were able to—they had the "stick" so to speak—they were able to enforce some kind of openness.

I won't belabor the point, but just a few things that certainly I would like to see happen today, which are:

- Some of the work for the big grant agencies of the world to do—the NIHs [National Institutes of Health] and the National Science Foundations—is to work towards much stronger open data policies for a much broader range of data than they currently have, and earlier in the discovery process. There are many other things that they can do. We can get into that in the discussion if people want to talk about it.

- I would say that as a general principle, I think publicly funded science should be open science. People should not have genomes of entire species locked up on their hard disks. That should not be the standard situation. There are certainly exceptions to that broad general principle —exceptions for confidential and proprietary knowledge and so on, but as a broad general principle publicly funded science should be open science.

The major reason I wrote the book is because I wanted to help make open science a public issue. I think there needs to be a public discussion of what type of scientific culture we want to support by public money. There's not going to be any single silver-bullet solution to these kinds of problems that I have been talking about. I think many steps will need to be taken.

I will just mention one thing that is now going on that is I think significant. These are linked things. There is an act, the Federal Public Research Access Act, that has come before Congress a couple of times. Each time it has gone back to committee. There is a good chance it will come up again before Congress in the near future.

What it would do, if it passes, is it would make essentially all research done by U.S. government agencies—any agency with more than a $100 million-a-year budget—would be made publicly accessible. There is a reasonable chance that this will come back before Congress in the relatively near future.

There is also at the same time a request for information from the White House. They are requesting that any member of the public who wishes to do so make a comment on the issue of public access to scientific research. There is a deadline of January 2 for comments on that issue.

There is an organization, called the Alliance for Taxpayer Access, that is helping coordinate the work on that. If you go to their website, which you can easily find with Google, you will find a lot of information about both of those issues.

If I could wave my magic wand and get one piece of legislation passed, that is probably what I would get passed. It would be a very thing that would certainly make a lot of scientific knowledge much more openly available.

Thank you all very much for coming.

Questions and Answers

QUESTION: James Starkman. Thank you so much.

As a career professional in theoretical physics, let me ask you the 64 trillion light-year question: What do you know, what does the profession know, as to whether the number of galaxies are finite or infinite and whether there are infinite universes or just the universe that we can observe?

MICHAEL NIELSEN: It's a broad question to say the least. Nobody would accuse you of thinking small. [Laughter]

Look, I'm not a cosmologist so I can't really comment with any professional knowledge on the issue. I know it's a hot topic of contention.

I have a friend who has certainly worked on the idea of "baby universes" and things like this.

That's probably the right thing to say, that it is to some extent a contentious issue, which is as it should be. People have different models and they are not yet sure what the right answer is. I'm sorry I can't give you a more definite answer, but that's the way things are.

QUESTION: Harry Langer.

The government has gone public, at least for development basically for innovation to create jobs. Now, if this is publicly given around the world, it sort of defeats the purpose of creating jobs in the United States. How would you handle this?

MICHAEL NIELSEN: It's a good question.

The total budget for publicly funded basic research in the world is about $100 billion a year. The total budget within the United States is about $40 billion a year. A lot depends on exactly where you draw the dividing lines between basic and applied research, but those are good ballpark numbers. They actually come from a National Science Foundation report about a year or so ago.

I think, at least historically, the way people have answered that type of a question is to say, "Let's cut our innovation system into two pieces. We are going to have a basic research system that focuses on things like understanding electromagnetism, just as a way of increasing our general knowledge, and, hopefully, in decades down the track, this will lead to commercial innovations, like light bulbs, that we will all benefit from." So that has been funded as a public good to be shared by everybody on the general principle that a rising tide floats all boats.

Then, on the other side, you have of course the very large, much larger than the basic research system, all the research and development that is done in companies.

There has been a fairly clean split between those two sets of things, and I think it has worked reasonably well.

Actually, I'll give another Faraday quote, his famous, possibly apocryphal quote, when asked by the Chancellor of the Exchequer why he was doing all these crazy experiments with things like frogs' legs and whatnot in his early electricity experiments. He supposedly replied, "Sir, one day you shall tax it." And he was right, of course.

QUESTIONER: Which countries supply the most money for basic research?

MICHAEL NIELSEN: Absolutely the United States. According to those estimates anyway, about 40 percent comes from the United States for basic research.

QUESTION: Susan Gitelson.

Since presumably you were born in Australia, down under, far away from here, and so forth—

MICHAEL NIELSEN: Indeed, yes.

QUESTIONER: There's always a question when we are talking about public budgets about how to allocate the amounts to various forms of science. The galaxies are fascinating, and it's fortunate that people without scientific backgrounds can contribute to the research. But there might be certain priorities, such as basic research to eliminate diseases and things like that. Who is going to decide that?

The other thing on this national question, America is very open. But what about China, what about other countries, that are learning from our research but not permitting us to learn from theirs?

MICHAEL NIELSEN: I guess those are two separate questions.

I'll focus on the second. It's an absolutely fascinating question. How is China going to fare when there certainly seem to be a lot of benefits to relatively open dissemination of knowledge, certainly to scientific discovery—in fact in a lot of ways, but let me focus on the scientific discovery.

My understanding, certainly talking to Chinese scientists, is that there is a tension there internally, which is fact that to do their work they need a lot of access to knowledge. Things like the Great Firewall of China, which blocks off a whole lot of Internet access, can be substantial obstructions. So that is potentially actually a sizable disadvantage to them.

There is going to be over the next few decades a very interesting experiment run where we are going to see just how much—well, the Chinese government is going to have to make a decision about where to put the barriers there, where they want to have those barriers, and how much disadvantage that is actually going to cause to their researchers, because I think it's clear it is going to cause some disadvantage, but we don't know how big a disadvantage.

Unfortunately, I can't give you an answer. It's too early to actually say. But it seems to be an emerging issue.

QUESTIONER: And what about priorities in research?

MICHAEL NIELSEN: Let me see if I understood your question. Yours was a broad question about how does one decide how to balance cancer research versus research on galaxies versus whatever, you name it. It's an interesting question, obviously a very fundamental question.

The way in which it is dealt with at the moment, to a surprisingly large extent, seems to be that actually the scientific community decides. It's not really decided so much at the political level. There are a lot of big line items that go on in the U.S. in very broad ways without being terribly directed and, in the way in which the money is directed, is decided by scientists further down the track.

A good concrete example: the budget for the National Institutes of Health is $34 billion a year, or was last year. For the National Science Foundation, I won't swear to it but I think it was about $5 billion. So that's a decision, right? That's saying buying medicine is worth about seven times as much as what the National Science Foundation does, which tends to be more things like astronomy and so on and so forth.

How do you set that number? Well, the way in which it is done is different groups fight backwards and forwards at the moment. How would I like to see it done? I'm not sure. It's a really interesting question.

QUESTION: My name is Zachary Marco.

Given the exponential rates of technology advancements and speeds of networks and the speed in which scientific progress is being made, I'm curious if you have any ideas or visions or concepts of what the next big step might be that will progress things even faster. You mentioned perhaps through that federal article the idea of the government sharing information. So perhaps it's just more APIs [application programming interface] and government agencies, things of that nature.

But is there any other concept or platform that would be the next level from, for instance, the information sharing on Galaxy Zoo? Is there anything else?

MICHAEL NIELSEN: Like I said, no silver bullet, just lots and lots of steps, like the Federal Public Research Access Act. If that passes—actually, a similar bill was passed, or a similar piece of legislation was passed, for NIH-funded research a couple of years ago.

What you are going to find over the next few years is when you do Google searches on biomedical terms, you are going to start seeing primary research literature more and more, because now if you are funded by the National Institutes of Health, your work needs to be made public after—there's a one-year embargo period, so you're only just starting to see the impact of that now. But in five years' time you'll see a lot more impact.

That is a significant thing for everybody. Certainly, it's a significant thing if you have just been diagnosed with some form of cancer or something like that and you're doing searches. It's a good thing to be able to get the original research literature. The Federal Public Research Access Act will greatly expand that.

What else would I like to see pass, if we are just talking about policy?

Certainly, open data policies, which greatly extended the Bermuda Principles. There's a lot of scope there. So instead of having five people in an audience of 100 scientists stick up their hands and say, "I systematically share my data." Well, I shouldn't have to ask the question, because 95 of them should be putting up their hands.

But there's a lot of other knowledge that is locked up inside people's heads and their laboratories that could actually be very usefully shared.

Scientific code is a very good example. Things like, for example, the big climate models that are used to do predictions of the future climate. At the moment, my understanding is—how many is it? Is it 18 or 19 such models? I believe that the code for all of them is largely kept private.

There is an initiative actually from the Climate Code Foundation to develop an open code base that contains such a model, which will be pretty interesting if it comes to pass. But it would be nice to see some more of that made publicly available.

Those are just a couple of ideas. There are hundreds of things that could be done.

QUESTION: Thank you for the presentation. Very interesting.

Don't you run into problems with military secrets, etc., on some of this funded research?

MICHAEL NIELSEN: Sure, absolutely. I am intending to focus on basic research. It is obviously the case that there is research, for example, of a classified nature, that will still remain behind closed doors. With that said, there are some interesting problems there for those people and those organizations, which is that in some cases it may actually be to their benefit to work more openly.

Just to pick on my own field, the field of quantum computing, actually most of the money coming into quantum computing, almost all of it, is coming from the military. In fact, a huge amount of it is coming from the U.S. military. The reasons don't really matter. It's basically because if quantum computers are ever built, they will be very useful for cracking certain types of code. The military want to read other people's mail, is essentially what is going on. So they have spent billions of dollars on it for that reason.

Here's the interesting thing. They're not doing most of that research in a classified fashion. What they have done is they have made a judgment that actually it's best to do it out in the open. In fact, a lot of that money is going overseas. Why? Because they really want the machines, and the best expertise is elsewhere and they can't get it.

I worked in a research center in Australia that was getting, I think, $2 million a year from the National Security Agency. Why? Well, because if it's a choice between working with their own internal people and working with another organization that has a better chance of actually making the key steps, they have a choice to make.

So to the extent that working in the open can actually speed up the process of discovery, it does start to move the fulcrum over a bit and there are more incentives to work in the open, even for organizations that historically have been very secretive.

The same applies, by the way, in pharma, for example. Some of the big pharma companies have been moving more and more towards open innovation models for exactly the same reason.

QUESTIONER: [Inaudible, off microphone] get patent protection?

MICHAEL NIELSEN: They work very carefully to—I'll repeat the question just in case anybody missed it. You're saying how does this desire to work openly in the case of, for example, pharmaceutical companies, fit in with their desire to get monopolies through patents?

The desire to get monopolies through patents only applies to a tiny, tiny fraction of all the work that they need to do. They get a huge amount of value by concentrating on patents in a few small areas. So there is a lot of, for example, pre-competitive research that they have to do to lay the groundwork for that, that fundamentally they'd be quite happy to work with other organizations to do it. So they have every reason in the world to do it in the open.

Interesting example: A couple of years ago, one of the big companies, Glaxo, released—what was it?—I think they said $150 million worth of data about chemicals, about drug targets, into I think it was the public domain. Why? Well, it wasn't of any competitive interest to them, but they are hoping that other people can use it—it's "the rising tide lifts all boats" idea—to do something interesting that may actually be of benefit to them.

QUESTION: Thank you for your comments. very interesting.

On a personal basis, I'm curious, when did you discover that you were an inventor or a scientist? And (2) what is the current state of quantum computing?

MICHAEL NIELSEN: There wasn't one magic moment at which I decided I was going to become a scientist. Certainly from the time I was a small kid I was interested.

My parents bought the Childcraft encyclopedia when I was five or six. I loved this volume about world and space, volume 4. It's a blue book which I can see in my mind's eye. It had all sorts of stuff about the Big Bang and the planets and the solar system and whatnot. I thought this was wonderful. But I thought many other things were wonderful growing up as well, so it's not like that was the unique event. There were lots of things.

Something I will say and connect it back to my talk that I think is very interesting: I didn't meet a professional scientist until I was 16. It was kind of a significant thing. I was living, compared with most of the world's population, a life of intellectual privilege. I was growing up in this big, rich city, Brisbane, in Australia. But nonetheless I, and nearly all of my friends, had not met a scientist.

There are a few intellectual centers—certainly New York counts as one—where that would not be so common. But outside those places it's relatively uncommon.

And then you think of somebody like Aida Berges, or there are many schoolchildren who have been involved in Galaxy Zoo. It's not that they're meeting a scientist; they are scientists, they are actually doing it. That's an interesting and profound kind of a shift. It's particularly interesting and profound when those people are in out-of-the-way places, I think.

I have a friend who's from a small town in west Texas. How many scientists did he know growing up? Probably not a very large number. Your local MD or GP is probably the only one you would meet.

How do you come to trust a community? Well, you need to know it. That's why I care a lot about this idea that maybe we can create new bridging institutions, because it actually will change the connection between science and the rest of society maybe in some very interesting ways.

When I look at Galaxy Zoo—there are many similar projects; I just mentioned and focused on Galaxy Zoo—I do think they had very little money to develop it, they have really done it on a shoestring, and

I think to myself: What's going to happen as they develop this idea and do it better and better and better? How much are they going to grow and what kind of impact will that have on people? It's an interesting question.

JOANNE MYERS: I thank you for opening the door to discovery science.

Audio

In this fascinating talk, theoretical physicist Michael Nielsen describes today's groundbreaking new era, where scientists, mathematicians, and ordinary people worldwide are working together online to solve problems and expand scientific knowledge.

Michael Nielsen is an essayist, a speaker, an advocate of open science, and one of the pioneers of quantum computing. Together with Ike Chuang of MIT, he wrote the standard text in the field, which is now one of the 20 most highly cited physics books of all time.

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