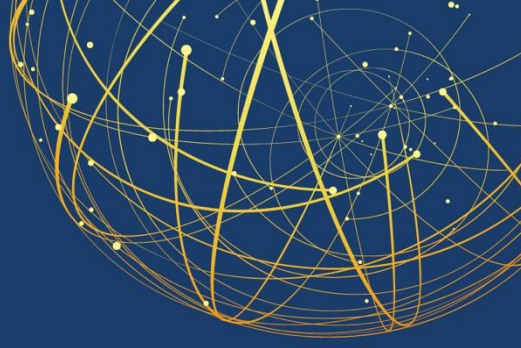

FROM THE CELL TO THE COSMOS: FUNDING THE NEXT ERA OF DISCOVERY

Announcer 00:57

Thank you for joining us. Please welcome the panel to the stage.

Caityln Barrett 01:03

Thank you. We haven't even done anything yet. Appreciate you all. So, I want to welcome everyone in the room to "From Cell to the Cosmos: Funding the Next Era of Discovery." Today, we're exploring how philanthropic, private, and public capital, in addition to the intangible assets of philanthropy, can accelerate discovery. More importantly, we're here to remind everyone of the joy of witnessing scientific progress, from the launch of the Artemis II, to transformational updates to our understanding of the lengths that cancer cells will go to fuel their growth and spread, including stealing mitochondria from neurons. Don't believe me? Look it up. It's a really cool paper. And also, just using ancient DNA to build a detailed snapshot of Neanderthal social structure. One thread that I know that the speakers on this panel are interested in coming back to is how scientific progress actually shows up, sometimes as steady incremental gains, sometimes as a step change, when infrastructure tools and talent line up and bold ideas can be tested. Our goal is to make that dynamic visible and actionable for the funders and builders in this room. Now to the people that will be bringing this panel to life. Sam Gill, the president and CEO of the Doris Duke Foundation, Sam Rodriques, director and CEO of Future House, and Elias Zerhouni, president and vice chair of OPKO Health and professor emeritus at Johns Hopkins University. So, I think what I'd like to do first to open up is start with a visioning moment. I'd love for you to tell me a little bit about your background, of course, and your current role, as you share one discovery that you're really excited about at any scale from cell to cosmos. And I'd love to start with you, Sam.

Sam Gill 03:08

Sure. Well first of all, thanks for the opportunity to be part of this panel. Thanks to everybody for coming, and for Milken for making the conversation possible. So, just so people have context, Doris Duke Foundation's a $2 billion private endowment. We work to build a more creative, equitable, and sustainable future. Doris Duke had really eclectic interests, and so our programs are really diverse, but we've, for the entire history of the foundation, have continued Doris Duke's legacy of investing in basic biomedical research to cure diseases. She played a really significant role in funding HIV/AIDS research, for example, in the early '80s, when this was not something that was a particularly popular thing to do. She took a real interest in diseases that were tough to crack. And so, one discovery that we've been a part of, and I think is, in every way, kind of a testament to both the breakthrough power of science, but also the kind of science system that we've enjoyed over the last 70 years in the US, is that we helped to fund some of the original research that was part of the cure for sickle cell disease that was announced in the last couple years. And that is an example both of the potential, but also the challenges of our system. So, this was not a disease that was seen as particularly popular among young researchers. The fact that CRISPR allowed scientists to move from thinking about treatment to cure made it more appealing, but that was ultimately the role of our philanthropic funding, was to help young researchers to see a career in this. And once they were able to see a career in it, then we began to get the power, the sort of the inertial power of the American biomedical science system, which has the patience and capital for really expensive basic discovery, and then an enormous engine of commercialization that can get working overtime.

Caitlyn Barrett 04:49

Thank you so much, Sam, and I know we'll come back to training and building up that next generation of scientists because I know each of you has a very vested interest in that space. Sam Rodriques, I would love to hear your perspective as well.

Samuel Rodriques 05:05

Absolutely. So, I'm Sam. I run FutureHouse which is a non-profit research lab focused on finding the absolute best biomedical research talent and helping them to use AI to maximize their scientific outputs. And I also run Edison Scientific, which is a for-profit company that does something similar with the pharmaceutical industry, so figure out how to build and train AI agents that can automate the entire process of discovering and developing new medicines. And the thing that I am really excited about, the kind of discovery that I'm really excited about is the discovery in the past six months, it's slightly meta, discovery in the past six months that we now have AI systems that are capable of making discoveries themselves. And so, at a risk of being perhaps circular, the thing about this that's very exciting is we have developed systems at FutureHouse and Edison that in the past six months have generated 15,000 novel scientific findings, for specific scientists that we've worked with. Which we're right at the very beginning of this major inflection point in our ability to go and discover new things about the world, which will come with it a major inflection at the rate at which we can go and advance ourselves technologically, cure diseases. And so, the cure for sickle cell is absolutely transformative. Amazing landmark, very inspiring for me to see that happen as I was in training. And what I want is, we should have another hundred of those. We should have another thousand of those. And I think it's very possible because of the systems that we're building, so.

Caitlyn Barrett 06:52

Really appreciate that from the perspective of someone that worked in a research lab. I would have loved to have had more efficiency in that space. Elias?

Elias Zerhouni 07:01

Yeah, well, so I'm Elias Zerhouni. I'm a physician scientist. Came from mathematics and physics into radiology and imaging sciences, and then biomedical engineering. And then I was at Hopkins and I was one of the first ones to understand the barriers between disciplines were a real problem for me. And then—so I started to create bridges first and then break the walls between the different specialties and so on. That got me noticed and people said, "Oh, that's a new trend." I'm talking about something that happened in the '90s. And so I became the executive vice dean at Johns Hopkins, and I ran the research, and then I was asked to become the NIH director in 2002, 2008. And then I went to head of R&D for a major company, Sanofi, and then I created my own biotech company because I believe in the following. When I was the NIH director, I thought we knew a lot more than I realized we did. And the reason is complexity. So my view, the breakthrough to me comes from resolving the complexity of biological systems on one end, and then the precision that you need to treat an individual.

Elias Zerhouni 08:10

So let me give you some numbers, because I think scale is important here. So when you look at cell to cosmos, you say "Well, what's that?" I mean, cosmos is infinite, right? Well, it turns out that if you look at our cells, you, it's 37 trillion cells. All of these cells have replicated from your conception to today. And each time they replicated, there were about two, three mistakes that were made. So the cells you have now are not equal to the cells you were born with. And if you look at your neurons, 70 percent of your neurons don't have the same DNA in the details than you had when you were born. Now, there are about 80 to 100 billion neurons. Each of them has 5 to 10,000 connections. So if you look at that, it's 1,000 trillions. It's a 12 to the 15, right? I mean 10 to the 15, sorry. Number. It looks like a huge number, doesn't it? And then I can tell you something extraordinary about you. You can generate 10 to the 18 antibodies. That's why your immune and you protect yourself because in yourself, amongst these 7 trillion cells and immune cells, you can generate a diversity of antibodies that no organism can generate that complexity. The lab doesn't do that. We can't generate that many antibodies specifically for something or another. But more mind-boggling than that is the fact that the transformation that I see, and you ask me, "What really changed your vision?" and makes me wish I would start my career over again is really AI and really the AlphaFold 3 program that received the Nobel Prize and David Baker work on the RF diffusion, solving the two problems. If you have a sequence, can you see the structure? If you desire a structure, you can see the sequence, right? Those were the back and forth. Now, think about it. You know the game of Go, which really made Hassabis and the team famous. You know how many combinations you can have in the game of Go? It's about 10 to the 130 power. Now just so you know, the cosmos has about 10 to the 80 atoms. And the number of small molecules that can really act with the body is about 10 to the 40th power. So we are 10 to the 18, molecules 10 to the 40, and then what the universe contains is 10 to the 80, and then

what AI can compute is 10 to the 130. We've never had, as a human species, the power to compute and the power to understand. And combine your diversity, because none of you—even two twins are not clones of each other. Yeah, there's no clone of you in the universe. If you look at your fingerprints, your iris, you're different than everybody else. So that individuality requires precision. But AI, I think, is the transformative factor, and you heard about it, is going to allow us to understand this multidimensional complexity that life is.

Caitlyn Barrett 11:31

I'm so glad that you mentioned AI. And I think that we've got a lot of expertise here, and I would really like to dive a little bit more into FutureHouse and what you're doing with AI right now, because I think that we all see there's a lot of promise but there's also a lot of foundational need underlying. There's infrastructure that needs to be there. There are people in the background that are working with AI, and I'd love to know how you're thinking about that interrelationship between man and AI.

Samuel Rodriques 12:05

Yeah, it's a great question. So, the thing that we find over and over again is that there's a base layer where you can think about how to use AI. And that base layer when you're doing scientific research. And that base layer consists of, you know, you can ask questions about the literature, and you can ask it to go do analyses for you and things like that. But then, much as you point out, one of the things that's very exciting about AI is the ability to consider possibilities that are orders of magnitude larger than any human can consider. And that is something that I think for scientists today is probably not intuitive, right? There was a huge transition in biology in the 2010s when we went from doing—testing individual things one at a time you know, in a well plate, to testing combinatorial combinations. Where you could test thousands or tens of thousands or millions or sometimes billions of things in a single reaction. And it took five or ten years for people to really grok how they could leverage that technology. And something similar is going to happen with AI in science, right? And so the more sophisticated things that we're seeing are things like, yes you can ask the AI to go and do a literature search for you. But what if you go and you run your AI 100,000 times to go and do literature searches on every protein in the genome across several different organisms and figure out where—what has been studied and what has not been studied, that comprehensively map the white space and identify opportunities to repurpose proteins, for example, from one use to another use? This is where you just get into much more sophisticated kind of ways of thinking about it. And so that's what we do at FutureHouse. Our primary focus at FutureHouse today is basically on identifying extremely talented biomedical researchers who have specific biological questions that could be massively accelerated with AI, but who don't necessarily have the intuition yet about how to use the AI, or don't have access to the kind of engineering expertise and computational resources that they would need. And we supply that expertise, we supply those resources, and we teach them. And then I think the results that we've seen in the time that we've been running this program so far have been really incredible. You get a lot of very exciting research out, and it's been very, very fast.

Caitlyn Barrett 14:48

Yeah, I don't think I'm the only person in this room that is really excited about what you're doing. And I think that when we started today, we started with this recognition of where science is and how incredible these discoveries have been. And I think at the foundation, there's a real need for the ability to fund this kind of work. And I'd love to go back to you, Sam, and talk about the portfolio to protect science, because I think that right now we're in a moment where we are seeing the joy and the value of science, but we are also seeing that there are some areas where we're pulling back from that funding. So, what is the Doris Duke Foundation doing in that situation?

Sam Gill 15:34

Yeah well, we've really depended on, in our work to support biomedical research, both directly and indirectly, a system of public expenditure for research that has been the envy of the world since the '40s and '50s when it was created. And I think, to speak euphemistically, that system's going through the most significant disruption in its entire history. So I think people are fairly well acquainted with the details, but this administration has now in consecutive years proposed budgets that would sort of end public expenditure for science at the levels that we understand them. We're talking about kind of 20, 30, 40 percent cuts to budgets. That funding is what enables people to study science at a graduate level, in addition to funding work within laboratories. What we're also seeing is there's sort of an extraordinary amount of interference from the administration in how that funding is dispersed. Some of that we've all read about has to do with what people are studying, but if you ask people in any major federally funded research institution right now, a research institution that's dependent on federal funding, they're living in just a world of uncertainty that they've never experienced. Their grants might come through, they might not come through, it's really difficult to predict. Now, there are a lot of cultural path dependencies that have accrued within the scientific system that are inhibiting, I think, some of the really important possibilities that the other panelists have already discussed around artificial intelligence. Many people would attribute to the federal funding structure the kind of incrementalism of basic science, rather than looking for transformative breakthroughs. Many would attribute to federal funding structure the real emphasis on lab science, as you point out, as a kind of epistemic frame, not merely a setting in which science happens, a very rigid way of thinking about what questions can be asked, how they can be answered that revolves around a PI. That funding structure has also been responsible for getting in the way of a lot of interdisciplinary research, particularly interdisciplinary computational research, that obviously is essential to harnessing some of these breakthroughs.

At the same time, having said all that, the reality is that I think we would rather face the classical innovation challenge of how you get an entrenched structure to evolve than the novel innovation challenge of trying to get billions of dollars of federal funding back and to restore the idea that researcher independence at private research institutions is a really powerful engine of innovation. So along with Caroline Montojo from the Dana Foundation, who I think spoke yesterday and will speak again tomorrow, Doris Duke Foundation has helped to create something called the Portfolio to Protect Science, and it's really just an effort to help funders, many of whom are very acquainted with how to fund science, but who may not really understand how to fund into policy and administrative systems that help science—that make science possible, to help them connect to existing efforts and initiatives that are seeking to protect kind of a minimum viable science so that we have kind of a substrate, to use a scientific term, that we can innovate on top of. And so some of the projects that we've helped to support are the projects that have turned back some of the efforts by the administration to massively cut science funding if you follow that.

Congress has repudiated this. For example, the efforts that we funded also include projects that sort of help to protect the really valuable role that science plays in public policy. So climate science, vaccine science, things that large systems rely on to help us make smart decisions.

Caitlyn Barrett 18:55

Yeah, absolutely. And Elias, you come from a position where you have been in academia, you are doing industry, and I'd love to know how you think about the funding scenario as well as just how science—what the pipeline of science—

Elias Zerhouni 19:11

Right. I learned a lot about it from my position at NIH.

Caitlyn Barrett 19:13

Yes.

Elias Zerhouni 19:15

And one of the things you said about peer review is very conservative. It's incremental. Reason is that the federal regulations obligate you under a law called the Federal Advisory Committee Act. And if you read that law, there's no way for you to create a jury that is diversified with the people who really know what they're talking about. So you get an averaging and you get a regression to the mean. So when I was the director, I created the Pioneer Award. I had to go to Congress to get an exception to the law. And I went to, at the time, Senator Stevens from Alaska, who had a wife who was very interested in medical research. And I went to him, I said, "I need a waiver, because I can't. I will violate the law." That's what my lawyers were saying. So you have these embedded restrictions, and then he said, "Okay, why don't you do that? But I won't give you the whole NIH budget. I'll give you $25 million." Then he upped it to 50. Today it's the best program that was created in 20 years, according to the National Academy of Sciences, not me. Why? Because it gives the freedom to those individuals who pose the right questions. So I always say great science comes from people who ask the great questions, not from people who have the labs and the activities. There's no question that AI will enable individuals to understand better the systems they're dealing with because of the scale. But I think what we need to do—if I had a dollar, you said to me, "Okay, philanthropy can do something," what would it do, right? And I had three dollars, right? First dollar would be people. People, talent, geniuses, right? And the genius is one in a million person. And it's almost like uranium enrichment. You really need to sieve through the non-active uranium, find a good one. One in a million is really, for our country, we're 350 million, it's 350 people, right? It's not enormous to find great talent. It's been done. I mean the Rockefeller University, private institution, 25 Nobel Prizes from that institution. The Whitehead Institute, the Pasteur Institute in France, 10 Nobel Prizes. So you can see that

the kryptonite is the people, the young folks who have the mindset to go out of the box and ask questions you and I couldn't think about. And whether AI helps that or not, I don't know, because AI is based on past data. So I'm not so sure that it will really discover questions that no one thought about. But certainly, AI can enhance our ability to do research faster, better, at scale, and maybe come up with new things.

I always like to give the example of Einstein. So he was 25 years old. He was abandoned in a patent office, but he was a physicist, and he looked. There were 14 experiments between 1870 and 1905 when he did his work that showed that the speed of light was constant. In those days, people thought that lights traveled in a medium. So if you went with the Earth's rotation, it should get faster than against the Earth's rotation. But no, the data found through multiple experiments, which you will multiply even better, was showing 14 times that the speed of light was constant. And then you say "Well, if the speed of light is constant, then space must not be, time must not be." And boom, the theory of relativity came up. And he was disbelieved for a long time. But then when the solar eclipse came and showed that light was actually bent because of the space-time differences, because of gravity, boom, you had a change, you had a revolution. So if you look at where we are today, it's like physics in 1920. I'm a physicist, so I can tell you, it's about that time. And then beyond that, what happened was teams were put together, accelerators were put together, the Lawrence Livermore Laboratory came together to explore nature at a level that wasn't explorable before by single physicists working in an isolated lab. So the second dollar will be creating integrated, multidisciplinary scientific teams, physics, mathematics, engineering and all that. We don't have that in the universities as much as you think. And I know three, Stanford, Berkeley, and then the Broad Institute at MIT. The others are still siloed. So if philanthropy money could break that and then change the paradigm of how we do research with the means that we have today, I think that would be the dollar that I would spend.

Sam Gill 23:58

Could I just—

Caitlyn Barrett 23:59

Yes, please.

Sam Gill 24:00

I just think in both of those examples, something Elias that you said that I think is sort of really important to bring out for people is, with philanthropy the whole point of the Vannevar Bush paradigm is to fund at scale what we already know will work. And I think one of the biggest mistakes that philanthropy, particularly in science makes, it to help a system continue to do what it already does well. So there's a lot of people in this room who probably think, "Well, what Elias said is that I have to go find the next superstar researcher." And the proxies for that will be that they've won tons of NIH grants, and they're at the head of a really prestigious department in a well-organized field. I think that's the opposite of what you said, right? You said we should look for talent that isn't getting rewarded in the current system, that has new

and different ideas about how to do things. And we should look for the ways that science wants to happen but can't in the existing structure, where it's butting up against the existing structure. AI and computational science is a perfect example of that. If you talk to biomedical researchers who are interested in using computational science and AI to enhance their findings, they'll tell you it's very difficult in an academic setting to actually make this happen. At the practical level, I can't find the right people. I can't pay them the right amount. They're not accorded the right level of prestige within my lab. These seem like really simple reasons that we're not achieving breakthroughs, but they're actually the dominant reasons that we're not achieving breakthroughs. And I think another place where there is a huge amount of potential for innovation and—look, the Portfolio to Protect Science is the exception to what philanthropy should be doing. We want to keep the status quo going enough that we can innovate on top of it. But I think what you've described is the rule. I think the other thing that we need to do is to start rewarding scientists who are actually asking questions that are going against the grain about the purposes of science. So again, one of the things that we've begun to recognize at Doris Duke Foundation is that our system is great at creating new drugs. The NIH has become phenomenal at building a reductive medicine miracle machine, and that's really, really important. But we're also living in a country where, despite the fact that we have those advances, we have declining health outcomes. And a lot of those declining outcomes have to do with whether people have access to those treatments, whether there's breakthrough science to be found in how we treat disease, how we prevent disease, these sorts of things. If you talk to researchers who are studying these questions, first of all, it's much more interdisciplinary science, but they feel maligned within the system that they're in. They can't get NIH grants. So I think implicit in both of your points about where you put your dollars, not just sort of people and working across disciplines, but doing so in ways that the current system has really strongly disincentivized.

Elias Zerhouni 26:24

Absolutely.

Samuel Rodriques 26:24

Can I—

Elias Zerhouni 26:25

I'm sorry, let me just—you're so right. I attributed 35 Pioneer Award prizes, which are five-year grants, no questions asked. You got a great idea, you go for it. One of the criteria was to have been turned down by the regular—

Sam Gill 26:40

Absolutely, that's great.

Elias Zerhouni 26:41

No, it's true.

Sam Gill 26:42

That's fantastic.

Elias Zerhouni 26:43

There's a fellow, Karl Deisseroth, who I predict is going to get a Nobel Prize. He came up with—34 year-old, turned down three times. He came up with the idea of taking a neuron and putting a jellyfish, a channel that was light sensitive, so he could turn off the neuron with the yellow light, turn it on with a blue light. And he came and described this, and I was like, "Oh, he's crazy, he's nuts." And then the jury, only nine people that were not constrained by the rules. And they came up, and four of them were saying, "Eh, that's impossible. Nah, he's kidding. That would never happen because of this, because of that." Five were like, "Wait, why not try?" And so I was the vice president. You get a tie vote. And I asked the jury, I said, "Can any one of you certify that what he's talking about is impossible? You, you, you, you." "No, no I don't know that it's impossible. I don't know." Well, if it's not clear that it's impossible and violates the laws of nature, let's do it. And he created the field of optogenetics. I don't know if you know what that is. It's basically controlling with light the activities of the brain. And so I think that's the kind of thing I'm saying, that our system is not designed specifically for people that are bold enough to be considered crazy by the regular system.

Samuel Rodriques 28:10

And so the thing that people really need to understand if you're interested in philanthropy and science though, is that yes, we absolutely need new structures. The need for new structures is the reason that FutureHealth exists. 100 percent. If you want to do AI in science today at any level that is relevant, you simply cannot do it in a university without a huge amount of additional structure to enable you to hire the software engineers, to enable you to get the compute. It's not possible. But the thing that people need to appreciate is that many philanthropies today are set up in a way that just extremely strongly discourages them from taking risks on new structures. So one of the things that I did before starting FutureHealth, actually, was I came up with a proposal for a kind of structure called a focused research organization, which is basically a non-profit startup for science. That work is being carried forward by my colleague Adam Marblestone and Convergent Research, which if you're interested in science philanthropy, you don't know about Convergent Research you definitely should. And so I strongly encourage you to look it up. But I think the thing that's important to emphasize is that with focused research organizations, you have a problem that's too big for academia, can't be done for a profit. You find a genius, you find a genius like an Einstein or like a Deisseroth or whoever in the prime of their career, and then you go out to philanthropies,

and you say, "Here's the problem. It's a great problem. Here's the person. It's a fantastic person." And they say, "Oh my gosh, we want to fund this. But our board has all kinds of questions about how do we know that you're going to continue to exist in three years? And can we actually take a risk, right? Oh, it would be so much easier for us to just go and give this money to this university that's been around forever, and then we can be sure that they're not going to run into any problems later," right? And so if you want to actually do things that are high risk—everyone likes to talk about doing things that are high risk—but if you want to actually do things that are high risk, you need to be willing to take the risks. And I think that most philanthropic funders in science today are actually not willing to take those risks. And so if you're a philanthropist in science, you need to stand up, and you need to think to yourself, sit down and think to yourself. Have I actually arranged my funding in a way structurally that allows me to take the risks that I need to take in order to find the Einsteins, or to find the Deisseroths, and to fund them, right? I'm not surprised that Congress wasn't willing to do it, right? Or that they only were willing to do it with $50 million. But I find that very funny. It's kind of sad and very funny. But philanthropies should be able to do it. But you need to think correctly about—are we setting ourselves up for failure?

Sam Gill 30:59

Congress actually does have an obligation to think differently about risk. They're using taxpayer money and I think you're exactly—the whole point of allowing there to be this much into—there is nothing like American philanthropy in the rest of the world. And I think it reflects a conviction that is sort of Tocquevillian conviction that some level of innovation comes from below. And so that we need to allow independent capital that's not so large that it eclipses publicly accountable authorities, but is large enough that it can make a difference. And it's interesting, you brought up the example of Rockefeller University. Well, the Rockefeller Institute was an institution of a kind that had not existed in history, and it reflected exactly the same instinct. There was a generation of American doctors who went to study in Germany. They came back and they said, "Guess what? The future of medicine is not homeopathy." Well, I guess we're going back to that now. But the future of medicine is this scientistic study, and there is no place in the United States that's actually set up to do it. And then they had this loony idea. John D. Rockefeller, Fred Gates, and Simon Flexner, brother of Abraham Flexner, who wrote a very famous report on medical education, and William Gates from Johns Hopkins had this loony idea that maybe we should just set up a totally different place to do it. And I think to your point, the university not only borrowed a new structure from Germany, it was created to enable a kind of plasticity of structure that would allow practice to catch up with the insights of new science.

Elias Zerhouni 32:25

Which is why the Broad works. Now, you say oh, you go to a university, you get captured by the old tradition, and the thing, but Broad was smart. Guided by some people. They made a deal with MIT, which freed the Broad from a lot of the regulations and departmental, I would say dynasties that are there. And then they block, in fact, the creation of new teams. So they asked in universities the power of appointment, the power of promotion is the two fundamental powers of university, not money because you know—and so they asked, they said, "No, no, no. Appointment and promotions will be done in the Broad." And what has allowed them to recruit people from different walks of life, including data scientists,

even genome sequencers and so on and so forth. Same at Stanford, and this is a funny story because Jim Clark is the one who created the Bio-X Institute. Bio-X meaning chemistry, biology, engineering, all of the things coming together, right? And after maybe a year and a half, he realized that they were just playing him, and so he pulled his money back. He said, "I'm not going to give you the money to replicate what you've done for 100 years." So the power of philanthropy is to create new models and new teams and really say, "Look, what counts is not what Congress wants." And I know Congress well, and every time they would ask me, "You're doing basic research. My patients are waiting for cures. Why can't you spend your money more on clinical trials and things like that?" That's the fundamental mistake that China is doing right now. People are afraid of China. I'm not so afraid of them because I know structurally they've really doomed their own progress because they spend four times less than we do on basic research. And then they don't really finance innovation. If you look at a biotech company in China, they would sell a drug at a tenth of the price sold here. So when you hear about all these Chinese molecules coming over here, it's because the Chinese government has made a fundamental mistake, which we didn't. We reward innovation through whatever the private sector and startup companies and all the big companies that were created here is because we reward it. The Chinese don't really reward innovation. They control innovation. So that's why I'm saying that the $90 trillion I heard yesterday is the amount of money that will go from one generation to the other generation over the next 20 years. I don't know. That's a golden opportunity. There's never been so much wealth in philanthropy. And if philanthropy really because of its freedom and what you're doing and the ability to reframe the debate and really put the light on the failings of our system, as you know, not only failing involuntarily, but now we're failing because we're cutting the number of grants. So that is where philanthropy, I think, has a role. It's not a money role. It's a vision role. Like sell to cosmos. Be cosmological, not governmental.

Caitlyn Barrett 35:36

Yeah. Lightning round. Okay. What is one thought process, policy, structure that if we could evolve it within our biomedical research system would just launch us into the stratosphere cosmos. Can you give me something quickly that you can think of right now? And just raise your hand if you got something. I don't—yeah.

Sam Gill 35:58

Yeah. I think if we complement reductive molecular biomedicine with a real health solution science, we're going to see just the outsized impacts from that over the next 50 years that we saw from reductive science over the last 50 years.

Samuel Rodriques 36:14

Yeah. I will just say I think that actually, something like focused research organizations, something where you're enabling researchers to go outside of the standard system, will lead to really different outcomes than what you would get otherwise. There have been some philanthropists who have been very visionary

here. Eric Schmidt has been extremely visionary. There are several others now who are coming in. But I think we need way more attention on solutions like that.

Elias Zerhouni 36:42

Are you talking about philanthropy or in general?

Caitlyn Barrett 36:43

In general.

Elias Zerhouni 36:44

Okay, So the power that AI has based on the data that it has in its data bank, right? And the number of token and whatever they do, right? So AlphaFold 3, we went from 25,000 known structures of proteins to 200 million, right? But there's a lot more in the human body. For example, if I had my druthers, I just told you that you can generate 10 to the 18 number of antibodies, right? Wouldn't it be great if we had a database of all the structures of all the antibodies that everybody produced, and the neutralizing ones, non-neutralizing ones in sequence, right? Not wait to inject somebody with a vaccine and find out what antibodies they have and which one works and which one doesn't work. If I had my druthers, I would say this kind of projects would be game-changing, just like the human genome was game-changing. But the proteome is important, and then the immune system is so important. We don't even know why people get inflammation in aging. It's what we call inflammaging, right? So there's fundamental questions that are beyond the reach of any particular one team. It needs to come together. And by the way, I don't think looking for talent, in the competition for talent that we're well-placed. We're a small country relative to 1.4 billion in China and so on. So I think you should go get the talent wherever it is, not just here.

Caitlyn Barrett 38:14

Absolutely.

Elias Zerhouni 38:15

Right.

Caitlyn Barrett 38:16

I am so inspired, and I've rethought how I think about the work that I do, and I thank you all so much for contributing to that. We are now going to transition—so please stay—to the next portion of the session, where we're going to zoom in on a particular area of scientific transformation. Please join my very good friend and colleague, Sylvie Raver in the next panel, in shifting the lens to the brain and how today's neuroscience momentum is shaping everyday life. Thank you.