WILLIAM BONVILLIAN: All right. This is Vannevar Bush, and I had you read *The Endless Frontier,* which is maybe the most important organizational document in US science. Vannevar Bush-- fascinating figure. This is his--

He's considered one of the first great computer scientists, and he creates this amazing device in a room that essentially functions as a computer that can do, fascinatingly enough, differential equations. And it's room size, and it consists of these rods and brass knobs. And you spend a day setting up what you want to do, and then you adjust everything.

And then, you've got the answer. It's not what I'd call high speed, but conceptually, computer scientists view this as right up there with what Babbage thought of and so forth. It's an absolutely foundational tool in developing modern computing.

Vannevar Bush is a professor of engineering at MIT. He understands industry. He was a founder of Raytheon. He obviously understood vacuum tubes, because Raytheon was an early builder of vacuum tubes for a lot of radio communication.

But he decided he didn't trust vacuum tubes. They were too fallible. So when he put his differential analyzer together, he uses these weird rods and knobs. Here he is, by the way. I think that's Frank Compton with Vannevar Bush.

In the course of war, World War II, he becomes, in effect, Mr. Science. Here he is at the ultimate point of fame in that time period, on the cover of *Time* magazine. But he's viewed as the mobilizer of the US science system. And day after day, year after year, it's just a series of "wow" moments for the American public as these scientific advances come onto play, from radar to atomic power. And it's a remarkable story.

So he's quite close to Franklin Roosevelt. He sees the entire system being dismantled at the end of World War II. So he persuades Roosevelt-- let's hang onto something. Everybody thinks world peace is going to break out and be enduring, and that the US military machine is going to be dismantled. Everybody's going to come home from overseas, and we'll go back to essentially having no army.

That's what lies ahead, and Vannevar Bush realizes what amazing stuff has been accomplished in the scientific world in the course of the war. So he persuades Franklin Roosevelt. Let's hang on to some of it.

And Franklin Roosevelt writes to Bush saying, essentially, three things. How do we diffuse scientific knowledge that's gained from the war? How do we organize scientific knowledge, particularly a war against disease? And what's the governmental role in supporting science and private sector research? Essentially, these questions.

Probably, Vannevar Bush wrote the draft for Franklin Roosevelt to send back to him. That's probably-- this is government. That's probably what happens. He calls his work *Science, The Endless Frontier,* and he's appealing to a very interesting historical strain here.

So one of the great historians of the first half of the 20th century was named Vernon Parrington. And Parrington writes about the end of the frontier and its effect on American history and American society and culture. And his point is that one of the things that's very different about America is that the structured, class-ridden society breaks down in American history at the frontier.

And of course, the entire country is a series of advancing frontiers, historically. And that that's the change agent that creates the kind of dynamism in American society, and that deep sense of equality that's lurking there. And when the frontier is officially declared ended, like in 1900, it's a big moment. Because what's the frontier dynamic going to be that's going to keep our society driving towards new things, if we don't have the frontier anymore?

So what Bush is arguing, in this very interesting historical sense, is science is the frontier. And science, by the way, is not going to end. It's an endless frontier. So it's a very interesting appeal to a kind of American sensibility.

So his paper actually comes out in July, 1945. Franklin Roosevelt had died the previous April. So it's presented to Harry Truman. And Bush is thinking through the post-war model for US science and its-- the association-less model dominates World War II. It's public-private collaboration, for sure, as Max pointed out.

In this document, Bush disaggregates science. He separates it back out. And we'll talk in a minute. We'll talk a little bit later about why he went in that direction. But he essentially argues, OK, American taxpayers. Invest in science, and you're going to get some really big things. And he essentially argues for four of these.

So as everything's being dismantled after World War II, he argues, let's keep investment in basic research. That's his case. And if you invest, American taxpayer, in basic research, here's what you're going to get. One-- you're going to get a war against disease.

Roosevelt, Bush, many others had seen the power of the development of antibiotics in the course of World War II, which really scale up. World War II is the first war where people actually died in combat as opposed to disease. The death rate in the armies of World War II is far lower than in all previous wars. It's very dramatic, and that war is being fought in some very tricky regions that are quite disease-prone.

And the antibiotics just dramatically reduce the death rate from disease. So they saw how a governmental effort of supporting that science, and then supporting the production, and then distributing it, could create a war-- pneumonia had been the biggest killer. It was number one, right? And that dramatically goes down after World War II. It has a huge effect.

So they saw the possibility of investing in science. You'll get a war on disease. It's a pretty powerful argument. Now, the next argument, they didn't have to sell to anybody, because everybody understood it as a result of World War II.

Invest in science, you get national security. And the Cold War hadn't started at this phase. It's got another four or five years before people will realize it's on them. But he makes this argument, and it falls on receptive ears, because people understand how powerful radar is. People understand how powerful nuclear power is and so forth. The transformation of technology was dramatic in the war.

The third argument is an intriguing one. American taxpayer, invest in science, and you will get what Vannevar Bush calls public welfare. What's he talking about? The big anxiety at the end of World War II is, we've got these 16 million people overseas. They're going to come out of uniform and they're going to come back the United States.

And we're dismantling all wartime spending, which is most of government spending. And we're dismantling the war machine. We're going to have another Great Depression. That's what everybody thinks. And you can see why. What are these 16 million people going to do? And we're dismantling all these industries.

So Bush is arguing, we're going to get full employment. And he's going after this big post-war anxiety about where these returning vets are going to be employed. And so he argues that essentially, science creates innovation, and that the innovation is going to drive the economy. All these new technologies are going to be at hand.

Fascinatingly, we do get a huge dividend from all the scientific and technological advances that occurred during the war, and we create a whole series of new industries coming out of the war, and it actually works. And just in case there was any doubt about what to do with those 16 million veterans, we had to park them somewhere. So we create the GI bill, and we send them to universities, and they get degrees.

So they take, actually, pretty interesting technical training they got in the military, and then greatly enhance it. So we've got a very strong technical pool of talent entering the economy. And this huge burst-- it's pure Romer right? And there we go. We get this incredible post-war economic growth time period that really lasts until about 1970.

The fourth argument is, nurture talent-- a Romer-like argument. His point is that there's going to be a governmental role in the other three items, but there's got to be a governmental role in backing this talent base up. There's going to be a governmental role in educating scientific talent at scale.

So Bush has got what we can call a "pipeline" theory of innovation. Here's a pipeline. We've talked about this before. The government will dump basic research in at the end of the pipeline. Mysterious things will occur in the pipeline, and great technologies, products, and a fabulous economy will emerge at the end.

That's the vision, right? That's his conceptual framework. That's what he's arguing for. This vision of the governmental role in basic research dominates US scientific thinking to this day. This is where it came from.

Why? What is he up to here? Why is he doing this? Because in the course of World War II, he created this incredibly connected scientific system with unbelievable results, where industry and government and universities were deeply connected with each other-- a highly connected system. And at the end of the war, he pulls that model apart and says, the governmental role is not going to be this connected stuff. It's going to be basic research. What's he thinking?

Interestingly, he's the person who created the two most fundamental scientific organizational models in US history. He does them both, right? They're contradictory, but he does them both. What's he thinking about? Well, to some extent, I think he was influenced by his physicist friends. And physics comes out of World War II-- people like Oppenheimer and the Los Alamos project-- with this huge concern about, what have we done?

We've been hell bent to create atomic weapons, and then we don't use them against Germany, because the war is ended. But then we use them against Japan. And did we really think this through? And what does it mean? What does it mean to have unleashed this unbelievable new force into the world's political environments? Are they ready for it?

So people like Oppenheimer go through a huge rethink. A lot of the physics community does this. Have we been too closely tied to a military industrial complex? As Eisenhower would put it later. So Oppenheimer is a close friend of Vannevar Bush. Bush testifies for him when Oppenheimer's security clearance is pulled later on in the Cold War, which was pretty nervy.

He's influenced to some extent by that, but he's not naive about politics. He understands the importance of national security, and he's an advisor to the Defense Department in the immediate postwar period. I think part of it is just practical reality. The federal government is radically decreasing its budget. The cheapest stage is investing in early stage research. That's much cheaper than applied research or development. Let's hang onto that.

And we created the federally-funded research university. That's an amazing creation. Let's be satisfied with having done that one, and keep that option on the table. I think that's a fair amount of his thinking. He's being really quite practical.

But he is trying to remove science from the fray. He was very concerned in the course of the war by science becoming politicized. Decisions about where to put defense laboratories were based on regional and local politics that Franklin Roosevelt was a master of.

Building consensus in different political regions and putting big research centers in places like Tennessee had logic, but also political logic, as well. And Bush didn't want that. Bush wanted science out of the politics. He wanted to really separate the political system.

So this is his vision for postwar science. And by and large, it becomes the case. So let's-- let me do Blanpied's piece just very quickly-- just a couple of minutes. He writes the early history of NSF.

So Vannevar Bush's design at the end of the war is-- essentially, during the war, he had the NRDC and the OSRD, two organizing committees for US science. And basically, that's probably the only time that we had pretty centralized science in the United States. But when Vannevar Bush ends the war, he wants to do the same thing. He wants to create one tent.

So he has this vision of why to do science in *The Endless Frontier.* Then he has an organizational model, and he wants to create this National Science Foundation as the tent for all of US science. It's all going to go in there.

It's going to be flexible. It's going to be loosely organized. It's not going to be terribly hierarchical. It'll be somewhat like what he was doing in the course of the war, but that's what it's going to be.

And something happens. He writes up this legislation to create this one tent for all of US science, and what became known as NSF. And it runs into this guy. So that's Harry Truman. And you'll notice on the corner of the desk-- it's just a little statement here that says, *"The buck stops here,"* in italics letters, right? It stops on Harry Truman's desk.

So Vannevar Bush wrote up and got Congress to pass his National Science Foundation bill. And much to everybody's total amazement, Harry Truman gets it, and he reads it, and he realizes that the buck is not stopping on Harry Truman's desk for science. It's stopping on Vannevar Bush, and a bunch of scientific elite types will control science, out of political control of the president.

Harry Truman, not unreasonably, says it's unconstitutional. We can't. This is an executive branch agency. The president ultimately needs responsibility. I'm not going to mess these guys up. I like science, but it belongs to me. So he vetoes the National Science Foundation Act.

He's influenced by a very interesting guy named John Steelman who's from a smaller college, but he's effectively a science advisor. So Vannevar Bush and Harry Truman, they didn't get along very well. Harry Truman, high school educated, never got to go to college. He was too poor to go to college. Very smart guy. He's an artillery captain in World War I, and a very successful one.

And artillery commanders in World War I are the smartest. You've got to understand ballistics and all kinds of complicated mathematics, and Harry Truman is very smart. So not a dumb guy. A very talented guy, and became a highly-respected-- criticized a lot, but became a highly-respected president, particularly in retrospect.

So there's no putting this guy down, but he just didn't get along with this kind of Boston Brahmin Vannevar Bush type. And not that Vannevar Bush was part of Boston society. He wasn't. He was from a lower middle class family, and his father was a minister, but they didn't get along.

Bush got along really well with Franklin Roosevelt. Not nearly as well-- really, not at all with Harry Truman. So they're at loggerheads. Vannevar Bush is getting Congress to pass this without, really, Harry Truman being involved.

Harry Truman's got his own advisor, John Steelman. And they have a very different idea of how science can be organized. There's going to be a lot more applied work, and it's going to be much more associationalist is their vision. So the NSF bill gets vetoed, and NSF doesn't get authorized for another five years.

So meanwhile, time doesn't stand still. This is why we have this very decentralized science agency system in the United States, because Harry Truman vetoed the National Science bill. All these other agencies are popping up, because there's jobs that have to be done. So the Office of Naval Research gets created in the Defense Department.

The great energy, nuclear weapons laboratories, Los Alamos, and later, Sandia and Livermore and so forth-- they all get created because the atomic mission is there, and that has to be dealt with, and they need science for that. So what later becomes the Department of Energy Lab system is being stood up. Scientific support at the Atomic Energy Commission is being undertaken to support nuclear research.

The National Institute of Health, which is a little public health research agency benefits more than wartime advances in science and medicine. That becomes the National Institutes of Health that's now the monster in the room. All these things are popping up, and NSF misses the boat. It doesn't get authorized for another five years.

Doesn't really get stood up until Sputnik. It's very modestly funded, until Sputnik. So this is why Vannevar Bush gets his-- he's successful in his basic mission-- governmental support for research universities and basic research. He wins that battle. He loses this battle. He doesn't get science in one tent.

And there's an ongoing, deep debate. Was that good? Are we better off with a very flexible, decentralized system of science agencies that are organized around all kinds of different missions?

I think my view is, a lot of disagreement, that's probably good for us, right? It creates a lot of pathways of opportunities for scientists to pursue. They're not under one tent. They've got a lot of options about what they go with their research, and a lot of different missions converge and come to bear to support science.

So overall, it's probably a positive, I would argue. But the negative is that it is impossible to organize science across these agencies. It is a nightmare. Getting these agencies to cooperate-- and again, behind them are nine appropriations subcommittees-- nine separate appropriations subcommittees. You gotta bring those folks along too. Good luck.

So central coordination of R&D is a huge organizational challenge. And look, it's not as if one single agency owns NANO. You want to make progress on NANO? You've got to bring all this cast of characters together and get them to hammer out cooperation, which is very hard to do.

High performance computing is a beneficial technology for all of these agencies. How do you get them to cooperate on that? These are nightmare organizational problems in our system. We finally created the Office of Science and Technology Policy as the coordination entity, but OSTP does not have budget authority.

It doesn't really control the budgets of these agencies. So it can't, in the end-- federal government money is power. It doesn't have any money. So it works with OMB and moves things along, but we do not have an efficient system for cross-agency collaboration, which becomes very critical when a scientific advance will affect a number of the actors.

AUDIENCE: What year was OSTP created?

WILLIAM BONVILLIAN: Well, Vannevar Bush is considered the first scientific advisor. John Steelman is sort of considered second. OSTP-- Eisenhower has a science advisor, so Killian, MIT's president, becomes his scientific advisor. Jerry Wiesner, also MIT's president, becomes Jack Kennedy's science advisor. Talk about talent. Those are amazing characters.

So whether it's called OSTP or not, there's a scientific advisory system that really endures, starting with Vannevar Bush, with the exception of a brief period of time when Richard Nixon got mad at scientists and he shuts down his scientific advisory system. But that gets revived under President Ford very quickly. So we've had a pretty continuous system. President Trump has not named a science advisor yet, however. So that chop is kind of *Home Alone.*

[LAUGHTER]

But from an organizational point of view, with all these disparate scientific agencies, you do need some coordination entity, from just a practical governance perspective. So I think that will happen over time. So I've forgotten what year the OSTP Act-- Authorization Act actually passed. It's got to be in the '80s, but we've effectively had a scientific advisory system in place since, really, the outset of World War II.

All right, I'm done. It's yours. And you've got-- and yours.

AUDIENCE: Yeah. So how should we-- I could--

WILLIAM BONVILLIAN: So why don't you do Bush, and then we'll go to Blanpied and we'll kind of--

AUDIENCE: Sure.

WILLIAM BONVILLIAN: Because they're these two models. One is basic research support and the other is no single tent.

AUDIENCE: Yeah.

WILLIAM BONVILLIAN: That's the two issues here.

AUDIENCE: OK. So regarding that single tent view, I've noticed that a lot of scientific research has had problems securing funding for long periods. Because often scientific research, by nature, is long-term, and doesn't always produce useful results.

Actually, this is one of the-- I think Adolph Hitler, actually-- he tried to ban all scientific research outside of directly applied science. So he didn't want anything in quantum or anything weird or theoretical like that, where he could not see the obvious applications of it. So with that, I'm curious what your thoughts on our governmental system, and how incentives can be revised such that long-term scientific strategic thinking can take place.

Because as it is, people have-- they are reelected every two, four, or six years, so they have to-- they need to produce results quickly. And if they can't do that with their funding, then they'll lose votes, and they could even get kicked out. So I'm curious what your thoughts are. [LAUGHS]

AUDIENCE: Do you have any interesting thoughts on that?

WILLIAM BONVILLIAN: You know, Martin, I didn't come back to you when we resumed, so--

AUDIENCE: Oh, the point I was going to--

WILLIAM BONVILLIAN: I'm going to give you a chance now.

AUDIENCE: The point I'm going to make, we were talking about should scientists be these kind of political leaders-- I was going to make a connection to VCs. VCs are the investment entrepreneurs. They kind of look for somebody that they say is kind of autistic, which is like they are in their own little world and they're not very-- kind of anti-social, but they focus-- because of that, they focus so much on the science that they're really, really good at it. And you need someone to sell it who's more social.

So I was going to go forth from the model of somebody who's-- it's very rare to have something can be both. [INAUDIBLE] I can't really think of one. I also don't really think of Bill Gates is a super sciencey person. It would be more like if [INAUDIBLE] trying to become-- for president.

And Loomis was this person who listened really well, and then could sell the ideas really well. So I was going to make that point, that connection where I couldn't really see somebody who's really focused on their art. And also to Stephanie's point, where yeah, that scientist might lose the ability to make some key discoveries, but now that he knows how the actual environment works, he can actually make any new discoveries with that framework in mind, which might be better for his career overall.

There's this one idea from scientists, and also artists, where it's your best work comes after your 40s. You can have long period of doing something you think really, really matters, but [INAUDIBLE]

WILLIAM BONVILLIAN: So let me translate your point, which is an interesting one, back to Max, to your question. So in a way, we could argue that the venture function is not designed into the Vannevar Bush's model. That's what he's trying to pull science out of, in a way. I'm extrapolating.

AUDIENCE: So he's trying to find some sort of long-term solution for this, right, and I was trying to figure out, well, what is it that, given our current system-- as opposed to just implementing his model, because it didn't work the first time-- instead of that, is there any way we can adapt what we have currently to try to have almost a hybrid between the two, so it gets the funding, but there's still-- you can still have these separate branches?

WILLIAM BONVILLIAN: You've stunned everybody, Max.

AUDIENCE: Yeah.

WILLIAM BONVILLIAN: It was too brilliant for our group.

AUDIENCE: Or they're just thinking, oh it's dumb question. The first, most obvious thing I think of was just increased term limits. But of course, that means you have-- that's not great. That has its own disadvantages.

AUDIENCE: I don't really see the term limits as much as of an effect on research, because I don't think everyday people are super aware of the research that's going on the NIH and--

AUDIENCE: True.

AUDIENCE: I don't even know how much of MIT funding is coming from the government.

WILLIAM BONVILLIAN: 70%.

AUDIENCE: A lot.

AUDIENCE: Yeah.

AUDIENCE: [INAUDIBLE]

AUDIENCE: I don't know how much public accountability there is to that. I don't--

AUDIENCE: [INAUDIBLE]

AUDIENCE: --if it's as much of a problem as just the lack of funding in general, rather than the lack of funding connected to results.

AUDIENCE: Or I think that voting-- votes are typically cast based on, I would say, more social or moral issues, rather than promises on science advancement. I can't think of a lot of people who have campaigned recently based on-- maybe I'm just not in tune, but I don't think there have been a lot of scientific promises or campaigns run on scientific promises. And I'm not talking about just president. I'm talking about two-term-- or two-year term Congress people, as well.

AUDIENCE: Maybe they should be.

WILLIAM BONVILLIAN: So I want to bring Chloe into this discussion, because we're rapidly moving into your territory. So why don't you pose a question?

AUDIENCE: OK, sort of related to the topic that we were just on, the question that I found the most interesting to think about from the Blanpied reading was-- you mentioned it with Harry Truman and also, I think, Harold Smith, the director of the DOB, brought up similar concerns that it was essentially unconstitutional for the president to allocate funds and power to a group of private citizens, for them to redistribute to fund the scientific endeavors that they deemed worthy.

So where do you all have opinions on where we should draw the line between wanting only our elected officials to be responsible for handling taxpayer dollars, and understanding the value of having our politicians be advised by experts in various fields? And then off of that, who ought to make sense-- broad question-- not to make scientific policy-- politicians advised by scientists, scientists appointed by elected politicians, or-- back to your point earlier-- politicians who are scientists? Is it possible?

AUDIENCE: The first two sound very similar. I'm sorry, go ahead.

AUDIENCE: I think that there are multiple levels of fund allocations though, so I think that there-- in the current system, say what the NSF, yes, politicians, I think, are sort of at the top, and they decide how much-- what the budget is-- overall budget. But then within the NSF, there's a scientific review process. So really the decision on who is funded specifically is farmed out to experts in the field.

So I think that it's an OK system that we have, because-- maybe you're talking about the top level, the original decisions on allocation or budget needs.

WILLIAM BONVILLIAN: Lily, let me build on that for a second, just from a practical perspective, on kind of how it works. Congress actually doesn't interfere much, frankly, with scientific budgets. It leaves NIH alone. It leaves NSF alone. It'll occasionally interfere with one of the defense R&D agencies, particular when they're spending a lot of money on actual physical project that may be related to science, because that affects jobs and employment, stuff like that.

But the government more-- it keeps its hands off DARPA almost entirely. The Congress actually does a pretty good job in controlling itself from intervening. And there's an occasional exception, but they are pretty careful about maintaining a hands-off attitude. In turn, the US is organized-- again, at Vannevar Bush's behest-- it's competitive. It's a competitive system, with the exception of the Department of Agriculture, which has a whole lot of science entitlement funding based on allocations to land grant agricultural programs.

That's the only non-competitive part. Scientists have got to compete for the funding. That was part of Vannevar Bush's original vision, and that remains. And that is a huge strength of US science. Other systems don't do that. There's much more entitlement science, for example, in German science. And they they're actually trying to reorganize to put a more competitive factor in, because they realized the value of having the best minds battling each other for the best research awards.

And it's and it's peer-reviewed, so scientists are analyzing and selecting their own science. So overall, it's a pretty dynamic process, and the government isn't deciding what science is going to get funded. That was Vannevar Bush's big fear.

The other dynamic here is that Vancouver bush came up with a design that fit into a conservative model. In other words, he's not interfering with industry. He's doing basic research. He's staying out of industry's way, by and large, which was another appealing part of the model from a political design perspective is that he wouldn't be, in effect, picking winners and losers in the industrial applied space.

So he pulls science out of the system-- out of the political system, by and large, and that's more or less where it's been. Congress does make decisions about overall funding, but they don't really get involved, except to a very small extent in actual scientific allocations.

So that's kind of how the lines got drawn in the actual system, as it played out. So in a way, Vannevar Bush got a fair amount of what he wanted. His concern about political interference in science has never really been realized. But then, of course, most of it's basic. How about one more point from each of you? Because I'm having to watch the clock.

AUDIENCE: So [INAUDIBLE] actually mention an interesting question that I found. So Bush states that basic research is essentially non-commercial. It's not going to achieve the attention it requires, if it's left to industry. So how common was this belief at the time, and isn't it as common today? And has the situation improved since Bush drafted this report?

AUDIENCE: I think maybe we can argue that basic research focuses on these long-term, not very commercial endeavors because the point is you won't see-- it's understood that you won't see immediate technological or economic benefit from these kind of endeavors.

They're kind of exploratory in nature, and so you're going to get exploratory, only exploratory. And maybe, just maybe you might end up with lasers, or radar, or something. But the probability is also pretty low. And so I think, by nature, what we deem as basic research and what we hope comes out of NSF and NIH funding allocated to these basic research ideas isn't going to be picked up by commercial or private investors, just because of the way things are and work.

And I think we definitely have seen that in the cutback in calls for basic research. And then we haven't really seen the funding levels aggregate pick up from sort of government plus private. Once you do the math, it doesn't equate to the same [INAUDIBLE] funding and basic research. Even if the government decides to drop off, we don't see private investors picking up the slack.

You can argue that the government can play both roles, we can do these public private partnerships and also do basic research. But I don't think private companies can have that flexibility to do both, just because they can't operate on these really long time cycles without any promise of returns.

AUDIENCE: Just a side note. In general, companies that can do side research is because they're a monopoly. That's the only kind of company that can do side research like Xerox or Bell Labs.

AUDIENCE: Yeah, I'd say definitely has improved, at least in my opinion. It seems that research is more easily funded by the government, although, of course, there are definitely ebbs and flows in that.

WILLIAM BONVILLIAN: So the rationale here is that, for early stage research, it's too risky for industries to manage. They can't necessarily assume that there's going to be any kind of return anywhere in the plausible future, so they just-- they're not going to do it. It's really out of range of what their shareholders would accept.

They can do development, as we talked about in the first class, because that's much nearer-term towards results. But the basic research stage is much more problematic for them. And your point is absolutely right, Martin, which is that the end of the monopoly model in-- for example, in the communications sector, with the demise of AT&T, that meant the end of AT&T's great basic research lab system.

AUDIENCE: [INAUDIBLE] I took a class on open sourcing stuff. So I feel like if there's an ability for a private company to do-- they'll be doing interesting research and decide we're never going to do anything with this. Because this happens a lot in companies, where you don't want to disrupt an industry where it's not worth pursuing business-wise, because it's too complicated. They'll go: we've been through this. Do you guys want it? Do you want to buy it? I think that might be an interesting one. I don't know if it's been done [INAUDIBLE]

AUDIENCE: It happens a lot in bioinformatics and computer science. Websites like GitHub, you know that that's the repository for everyone else's scripts that they've already written, and programs and stuff. But for a business, that would be interesting.

WILLIAM BONVILLIAN: Chloe, how about a close out point from you?

AUDIENCE: Yeah.

AUDIENCE: [INAUDIBLE]

AUDIENCE: So I think it's interesting to examine why-- obviously, both Bush's and Steelman's proposal encountered different obstacles at different times, as we've already discussed. But ultimately, it took the threat of technological takeover by the Soviets with Sputnik for a lot of impetus to exist to fund research, to protect the educational STEM pipeline-- which, tying back to our earlier discussion of is it-- do we have a system structured right now such that the US places value on this type of research outside of a stressor like war?

It seems like, at least at that point, it's hard to get that ball rolling. Which again, is ironic, because the whole discussion was started by Bush, who wanted to draft this outline for what funding that research might look like in a postwar era with the idea for an institution like the NSF.

WILLIAM BONVILLIAN: Good. All right, I'm going to jump through so we can finish on time. The next two--

AUDIENCE: [INAUDIBLE]

WILLIAM BONVILLIAN: I'm sorry?

AUDIENCE: [INAUDIBLE] I think it might be cool as kind of a fringe idea. So you might have-- not really investigating, but you might have an idea for a pursuit in maybe basic research or something like that, but your company might decide it's not worth it as a business venture. But maybe it's a secondary system like selling that-- like buying and selling that information might be cool.

So like I have an idea that I can't necessarily execute it for whatever reason, but I can make the idea known, and then someone else can pursue and pick up.

AUDIENCE: Like Elon Musk did?

AUDIENCE: Yeah, precisely.

WILLIAM BONVILLIAN: Right. Some people are working on patent pools, for example, and amassing patent rights, which may, in turn, be transferable and saleable. So there's work going on in some of these points. I'm going to do the closing two readings. And Sanam, they're both yours, right? OK.

So I'm going to quickly summarize this piece from Peter Singer that I put in there. Peter's piece is a terrific research because, frankly-- it's almost like doing genealogical history. We know what the advance is. Figuring out the science that emerge from is a really complicated process.

But Peter, I thought, very thoughtfully dug into and really built a lot of the scientific history around a lot of advances that we live off. The point here is we get a lot of-- I don't want to denigrate Vannevar Bush's. Stokes takes it on, and attacks it, and criticizes it, I think for some good reasons. But we've gotten a lot out of that model.

So federal research support has just been critical to the development of just an incredible array of advanced technologies, and basic research is behind a lot of that. So it's a key enabler. It's a key US comparative advantage. And interestingly, a big advantage of basic research is that it puts you out on the frontier of science, and you can get scientific breakthroughs from basic research.

If you're just doing incremental applied work, particularly at the development stage, you're not going to get the breakthroughs. But if your door is open to the basic stuff, there's a lot of potential breakthroughs lurking in there. Now, getting them into implementation is not simple, but it is, overall, I think an advantage in the US system that we have strong basic research.

What have we gotten? NSF supported the original funding for the Google search engine. GPS was founded on a purely scientific Navy effort around atomic clocks. That's one of the foundational technologies, as well as DOD's Navstar system, and work by DARPA. Supercomputing really came out of the DOE National Labs.

Speech recognition came out of DARPA. They funded all the work behind Siri originally. Siri is Stanford Research Institute. That was the development entity, a basic research nonprofit. DARPA, as you know, did the ARPANET, which led to the internet. NIST did closed captioning. All kinds of critical technologies came out of basic research, as well as defense technologies for the smartphone.

In energy, the whole fracking revolution came out of the Department of Energy's lab research. LEDs came from the Air Force. MRIs came out of NSF and NIH. The list goes on. Human Genome Project, an absolutely fundamental research project, was led and significantly influenced by NIH.

Out of mathematics comes things like reverse auctions, which is a tool that we use in a number of circumstances, such as spectrum auctions. One of the great absolute breakthroughs of early 20th century agriculture, a huge enabler in that sector, is the development of hybrid crops, particularly hybrid corn, which transforms the production of corn in the United States. Absolutely transformative. Comes out of obscure agricultural research stations in places like Connecticut.

So there's a lot that we've gotten out of basic research. You can barely see this, but it is a famous diagram in computer science. So it's called the tire tracks diagram. And you see these kind of lanes and then you see these tire tracks weaving in and out of them. And these are the foundational work in computer science and where it came from, with university work leading and almost initially [INAUDIBLE]-- that's federally-funded university research-- being often picked up at a subsequent stage by industry R&D, fed from university to industry, and then eventually emerging in the green lines as billion dollar product lines, and then eventually, the wider line, 10 billion or more product lines.

So these are foundational research efforts. These are the technology results that emerged. There's a rich literature behind every single one of those lines-- small lines on this map. So that famous tire track diagram is a critical diagram developed by NSF in explaining how the computing revolution came about.

We're now at Donald Stokes, and I'm just going to summarize very briefly. Stokes is a very interesting figure here. He was dean of the Woodrow Wilson School at Princeton. And he dies of leukemia very tragically, and pretty early. This is the last big thing. Pasteur's quadrant is the last big project he works on.

There's a lot of emotion in this book that he puts together. He feels these issues very strongly, as you could tell from his text. And what he's confronting is the relationship between science and government, which we've been trying to dissect earlier on. And he takes on the Vannevar Bush basic research model and talks about its weaknesses and failures.

So federal support of basic research-- his goal was to curtail governmental control of the performance of that research. That was one of the key things Bush was up to. And he aimed to create, through NSF, an entity with cross science authority based on a way, as I said earlier, on his wartime scientific organization attempts.

The rationale behind creating NSF and supporting basic research is that basic research is to be performed without thought of practical ends. That's one foundational feature. That's what it is. Let's not think about practical ends, let's just do the research. And then secondly, basic research will be, he argues-- Vannevar Bush argues-- the pacemaker of technological advance.

So again, he has got a pipeline model. Do basic research. Dump that into the pipeline. Mysterious things will occur. Great technologies will emerge. And his assumption is that dumping that basic research will pace what is going on inside that pipeline. It is a left right model. The government is dumping something in on the left side of the pipeline. Great stuff is emerging on the right side of the pipeline.

Now, remember Richard Nelson. Nelson talked about the way in which technology influences science, as well as science influencing technology. Vannevar Bush does not have a two-way street of interaction between the two. He has organized US science on a one-way street, a left to right-- it's not two lanes, it's left to right.

So Stokes argues that that is a critical organizational failure, that that radically limits what you're going to be able to get out of your system. Skipping ahead here, to conclude that point, the Bush paradigm of the linear relation between science and technology, Stokes argues, bears no resemblance to their actual connection. The reality is it's a two-way street, and we've only organized a one-way street. And therefore, this interaction is not going to occur. There are deep ties between science and technology. They're interactive, they're not linear.

So the Vannevar Bush model lasts and is respected really until the 1980s. And what's going on in the 1980s? We talked about this in class 3. That's the great competition with Japan. And Japan builds a much more connected system, much deeper ties between industry and its research system-- in fact, its research system is industry-led really.

The US has got this disaggregated system. Basic research is off on the side. The argument is the US is coming up with the breakthroughs. It's not able to implement them. It is not turning them into industries. In fact, Japan is taking the US breakthroughs and turning them into industries. That's the story of a lot of consumer electronics in that period of time.

And Stokes is suggesting we've got to get down to basics here. We've got a fundamental organizational problem. It's not an accident that this occurred, it's the way we organize things. That's why it happened. And this is a very anxious moment in US political history when he's writing here, because we're starting to de-industrialize, and with lots of effects on a lot of people in the United States.

So Stokes argues that Vannevar Bush's model is static, whereas, in fact, the reality is dynamic, and the static linear model is not the right model. So he short circuited the thinking process. He assumed, because you do basic research, implementation would follow. But it doesn't necessarily follow, Stokes argues. It's one-dimensional.

Stokes's quadrant picture is an important picture to understand, because this is the classic critique of many of Vannevar Bush model, which is the classic critique of US science organization. So all this stuff we're reading here is pretty noted material in US science history.

Here is his Pasteur's quadrant, which you all saw. So there's four quadrants, Stokes's mind, and there's two axes-- are you searching for a fundamental understanding, yes or no, and are you considering use, considering the utility of what you're researching-- no and yes.

So he has this nice quadrant, and then to the upper-left quadrant, where you're searching for fundamental understanding but you're not considering use, that's like physics in the '20s. That's like Niels Bohr. They're figuring out particle physics. They don't see any utility for this. The idea comes along later about atomic weapons, but that's decades away.

So yes, we can understand that is an interesting moment, search for fundamental understanding, no consideration, really just trying to understand stuff. Then there's what he calls Pasteur's quadrant, and he argues this is the one that Vannevar Bush's model misses completely. He's trying to organize as though its particle physics, but a lot of stuff isn't.

So Louis Pasteur, he's searching for fundamental understanding. He's doing microbiology at a really fundamental scale, but he's certainly considering use. He doesn't want kids to die of bad milk in Paris. He's considering use. He's up to something here. He's after a medical fix.

So that's a very good example of basic research being driven by consideration of use. And Stokes doesn't want to eliminate this quadrant-- he knows how productive it is-- but he sure wants to add consideration of this quadrant. Now, what are the other two quadrants?

No search for fundamental understanding, and consideration of use. So in here he puts Thomas Edison. That's like the light bulb. Thomas Edison doesn't really care about physics. He just cares about the light bulb. But it turns out to be a little more complicated, and we'll talk about this in class 7.

But Edison has got scientists in that room, in that idea factory, as he calls it, in Menlo Park, New Jersey, that frame 100-foot-long building. And they are onto some scientific ideas here. And there is something called the Edison effect, which actually helps explain electron theory, that Edison himself and some of the other crew come up with.

So there is science evolving out of the technology that they're pursuing. So it's consideration of use, but they are finding some fundamental understandings here, because it is a two-way street. Now, the one that makes no sense to me is, is there anybody really is not searching for fundamental understanding and not considering use? Who would do that? Who would do that?

So Stokes says, oh, Darwin. Gets on a boat, has a nice ocean voyage--

AUDIENCE: Sounds like fun though.

WILLIAM BONVILLIAN: --walks around the Galapagos, evolution dawns. It's not the way it happened. He gets on the Beagle because he already has developed a lot of evolution theory. In fact, his father is a scientist who's been working on it before him, so he has a very good idea of why he's getting on that ship.

So I don't really know anybody who quite fits there, because I don't really think Darwin does. Darwin really as a pretty good idea what scientific theories he's going to be pursuing, as he sails around the world. So I'm open to suggestions, if anybody can think of someone. But I think he feels this blank in because it's a quadrant. He has to put something in there.

How does he think it really works? He comes up with this complex diagram, which is very-- it's a little hard to follow. He calls it Stokes's dynamic model. And you can see that use-inspired research is the big one. Then there's pure basic research, and then there's purely-applied research.

And then there's this kind of system between early understanding, improved understanding, and improved technology, and existing technology. And everything swirling around, but this is right at the center. That's kind of his idea of how it really works.

And his critique of Vannevar Bush is you're putting all your bets here, and you have to think about this, as well. So that's Stokes's critique. And the fact that the US was doing great science and not making it is what drives him to this set of realizations about these fundamentals of how we've organized our science and technology system in the US. So Sanam, it's all yours.

AUDIENCE: OK, well, I guess I'll just start with some thoughts that I had on basic research in general from both these two readings. So it seems, we've been reading, a major downfall or obstacle to basic research is that the payoffs are risky and uncertain, especially in the short run.

And that's kind of what shareholders are generally most concerned about is short on profits. And so that kind of hinders the long-term investments in basic research, which potentially have really large positive externalities, as we've been seeing.

And there's also-- I think Stokes mentioned this-- there's also a low incentive because the original investor often doesn't reap the most benefits from whatever eventually comes of the basic research. And the profits are spread throughout society, and not always concentrated [INAUDIBLE]. So investing in and instigating basic research is-- there's a lower incentive for that.

And I think that's an argument to classify it as a public good, which is really in favor of saying the government should be-- or federal support should be there for providing this public good quote, unquote "of basic research." And I think that's a very good argument for the necessity of federal support.

But the other thing I was thinking of is that are there ways of expanding this from just-- the responsibility, shifting it just from that of the federal government to other actors? So some thoughts that I had when I was doing these readings were-- and I'll put this question to you guys-- is there a way to change the payoff structure so that the responsibility to provide funding for basic research can be on industries, as well?

Or is there a way to have an integrated system that encourages multiple and synergistic benefits that both firms, private actors, and maybe even some public actors all want to kind of buy into? And I think that gets us closer to-- Singer mentions the rich ecosystem following post-- after the war, where it wasn't a situation of a lone innovator working by themselves or anything.

So that's one of the main things I've been trying to process with these readings. So I guess the more general question to you all would be with-- how to change the system so that private and public funding relationships can work together, and also can be cognizant of the different agendas and goals of the different actors within the institutionalized science framework that we've been looking at. Any thoughts?

AUDIENCE: I wonder who it connects to the energy industry [INAUDIBLE]

AUDIENCE: I feel like I'm repeating myself, but when I was first here, we talked about patient capital and this breakthrough energy. And [INAUDIBLE] one of the proposals there was definitely a public partner-- a public private partnership with different countries, including the US.

And unfortunately, it's not happening in the US of committing to double their R&D budgets around energy. And then, again, this is just Bill Gates and his billionaires club. But it could be other sources of private capital, and we're starting to see some other sources pledging to pick up what that-- what they were calling mission innovation that bringing basic research ideas further along, pick those up, and incubate them longer.

The MIT Energy Initiative doesn't solve all the world's problems, but there's definitely early stage research that we fund-- that part of every company's membership they put $100,000 a year into a pool, and we fund what we call commonly around MIT different people are doing seed grants. So I could go on about that, but we have--

AUDIENCE: Do you mean like in Media Lab, because the Media Lab models-- I forget how much they get paid for each company [INAUDIBLE]

AUDIENCE: We have something like that, but nobody gets IP at the Media Lab. Nobody gets IP for what I just described, for the early stage stuff. Because we also bring in like $30 million a year of money for sponsored research.

But Bill, I don't know if you want to say anything that ARPA-E. That's also a part of the Department of Energy that's modeled after DARPA, that definitely funds early stage. And a bunch of MIT faculty have gotten those grants. I don't know how much it's under threat at the moment. I guess a piece of ARPA-E is.

WILLIAM BONVILLIAN: ARPA-E is proposed to be eliminated.

AUDIENCE: The total piece.

WILLIAM BONVILLIAN: Yes.

AUDIENCE: It's not even a lot of money, right? It's like $103 million?

WILLIAM BONVILLIAN: It's $300 million.

AUDIENCE: 300, OK.

WILLIAM BONVILLIAN: Only at the federal government would that not be a lot of money.

AUDIENCE: Does any of that address what you were--

WILLIAM But Sanam, you're driving it really interesting points. What are some other organizational

BONVILLIAN: models that might help us get around some of these dilemmas? And it's a really intriguing set of questions.

AUDIENCE: Going back to some of the-- not even organizational stuff, but some of the stuff that we talked about earlier like new accounting methods, or increased tax benefits for doing basic science research and things like that. If you don't mind, I had a question about this reading.

So he kind of pokes holes in Bush's linear relationship between basic R&D and development-- or basic research and development. So he creates this quadrant, and I didn't really gather from it so much what he was recommending from a policy perspective how you de-distribute funding across those three or four sections differently. I didn't see that much in the reading [INAUDIBLE]

WILLIAM BONVILLIAN: Right. In a way, Matthew, you're asking, is Stokes attacking what's really kind of a paper tiger? Does this system that Stokes is describing of the federal government really only funding pure basic research along the Vannevar Bush lines-- does that really exist? And I think there actually is a serious question.

So in the next class, class 6, we're going to dive pretty deeply into that question-- how does the system actually work? I've just described the Vannevar Bush model, which dominates US civilian science and the major scientific organizations that are part of it. So NIH, NSF, DOE, Office of Science are all organized on a basic research model.

But then there's a parallel universe in the US R&D system. The Defense Innovation system is not organized like that at all. It's back to World War II. It's a much more connected system. So we're going to explore that in class 6 in a fair amount of depth.

But the agencies themselves have changed. They've evolved over time. So for example, at NSF itself, which is the classic basic research organization-- NSF's probably greatest director, Eric Bloch, who died tragically recently-- great, great figure in science. He's the only NSF director to come out of industry, so he was part of that amazing IBM team that did a lot of-- that won the president's Medal of Technology for some of their really early computing advances.

So he comes out of the computer science arena. And so he walks into NSF. He sees this basic research agency. He sees that they all seem to want computers, but they have no idea of what computer science is. So he creates a whole new branch of science, computer science, and builds a strong support system for computer science organizations and departments.

DARPA had previously been funding those. Suddenly, NSF is landing in that space, as well. In addition, he embraces the engineering division. He himself is an engineer from RPI. He embraces the engineering division and brings in a whole set of much more connected collaborative engineering research centers and related programs that are going to be much more connected into the system than most of NSF is.

So even these agencies themselves tend to push further down the innovation pipeline than just the early stage research, even an organization like NSF. So there's an evolving story here on how US science organizations is structured. And when you read an article in the next class about new model innovation agencies, we're going to confront some of those issues.

So in the next class, we're going to talk about the parallel Defense Innovation organization, which is very connected, as well as the evolution of these agencies themselves. So it's not as if things have stood still. Stokes's critique has been understood and accepted, in some ways, in some parts of these other agencies, although they still remain basic research-based. So it's an evolving story here, Matthew. Sanam, how about one more question-- or a couple more?

AUDIENCE: Yeah, so getting at the idea of short-term versus longer-term, if we're looking at that top-right quadrant where it's use-considered basic research, do you think that that's still a little-- there's a lot of emphasis on innovation being centered there. Do you think that's still focused on short to medium-term visions, and excluding the longer-term, or do you think there's also the space for more long-term approach?

AUDIENCE: So I think just one example [INAUDIBLE] like DARPA. Because I worked on a project with exoskeletons. It was funded by DARPA, and they-- it was consideration-based. They were looking for the next generation of soldiers. And the projects that they were funding were super early stage, down to [INAUDIBLE] basics of biomechanics, and energy usage, and metabolics, biowalking, [INAUDIBLE]. So I do think there are some examples of a longer-term while still considering use.

AUDIENCE: Yeah, I think it's harder and harder to think of examples of pure basic research now. Obviously, there are still some. Like astrophysics research and understanding the universe might still fall into that category. But I think a lot of what people are doing, people have some sort of idea of what an end goal might be in it.

Like biotech applications can go back to very basic bio research. And now people doing that research are thinking about how it might be able to be used in the future. So I think that is definitely still long-term.

WILLIAM BONVILLIAN: Steph, I like your point. And an MIT scientist once explained to me how it actually works. He said, look, on day one, I'm doing fundamental exploratory science. And I may do that for a considerable period of time. Then I see something. Then I see something. Some kind of opportunity pattern.

Suddenly, I'm doing applied science, because something-- I've seen something. I've seen what the possibilities are. And then I'll play with that, and then it doesn't work, so then I'm back doing fundamental exploratory basic science again for a while. It's a dynamic process here. I think Stokes is right. It's not a static linear process. And often, the scientists themselves will move between basic and later-stage work on a single research project, based upon what they're discovering and finding.

The National Institutes of Health is pretty much a basic research organization, but when you go to the National Institutes of Health, you don't apply to them for money. You apply to the National Cancer Institute. So you're not applying to the National Institute Let's Go Have Fun in the Laboratory. You're applying to the National Cancer Institute.

So there is some use-inspired elements, even in a pretty basic research organization like NIH. How about a closing point, Sanam?

AUDIENCE: Yes.

AUDIENCE: Can I ask a follow-up really quick? You mentioned that this has now become sort of the seminal work in science technology policy. I was curious--

WILLIAM BONVILLIAN: That Stokes is?

AUDIENCE: Yeah.

WILLIAM BONVILLIAN: Yes, it is.

AUDIENCE: I was wondering if you could talk about the immediate reception when he published it-- or when it was published posthumously.

WILLIAM BONVILLIAN: War. A fundamental ethos of scientists is we're doing basic fundamental research, exploratory research. Data has enormous value. And here's Stokes telling us that we're missing something. Shove it was a lot of the reaction. Nobody quite said that, because Stokes was dying of leukemia.

But that was pretty much the tenor. And that's still a ongoing debate here. MIT, in some ways, is a very unique institution, because at the beginning, at the foundational moment in 1860s, it attempted to integrate engineering and science. Remember we talked about at the very beginning-- engineering is organized around-- in the 19th century, it was making stuff. It was doing things. That's what engineering was all about.

Science was natural philosophy. It was observational. It was understanding how the natural world operates. MIT attempted to bring these two fields together and put them on a continuum. So there may be a pure natural scientist at MIT-- and there are-- and there may be some pure engineers, but mostly, they're all on a continuum together.

And the organizational mechanism that MIT uses to do that is not the departments. So most universities have the departments at their core, which are organized around single scientific fields. Departments are important at MIT, because they determine credentialing, and do you have a job, and can you teach, and what are you going to teach. They're important.

But the really powerful elements are the labs at MIT, and they integrate all kinds of fields, and they are use-driven. They are very much results-driven. So they're bringing together science and engineering on a continuum, but with a set of organized purposes. So it's a pretty unique organizational model. It's not the pervasive model in US universities.

So this debate doesn't really break out that much at MIT, because MIT already did the integration. But at many other places, it's an ongoing debate. So Sanam, a closing thought? I keep-- what do you think?

AUDIENCE: Something that's really in my mind, especially right now, is just the direction that this debate is going to take on, especially considering you've got to go through the budget and everything. So I think that means a lot for a lot of areas that are outside the reach of market in general. And especially with science funding, it seems like defense is the clear winner with this new budget. So I think that that kind of might signal a re-prioritization that is going to have some interesting implications for this whole debate.

WILLIAM BONVILLIAN: That's a really important point. This is a pretty seminal moment in US scientific history. So the organizational set of assumptions that the Vannevar Bush put into place at the close of World War II of federal support for research, it's really being-- in question now. And this is the largest cutback of a federal science support in history that's now been proposed.

Is the model ending? Is this amazing system of federally-supported research universities-- is that being phased out? So it's a very critical time to consider these questions. This is the model that we've had up to now. This is the first time I've really seen a pretty fundamental hit on that model. Now, whether Congress takes it or not, we'll see. It's going to be playing out in the next few months.