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WILLIAM BONVILLIAN: So today, we're going to get into innovation at the institutional level. So that's our fundamental topic. Because we've been out for two weeks, let me do a recap so that we're all current with the underlying framework.

Class 1, we talked about, as you remember, growth theory, and we read Robert Solow and Solow taught us that technological and related innovation is the dominant causative factor in economic growth. Old classical economics posited that it was capital supplier labor supply. Those remain important, but the monster in the room, Solow found, to the tune of 60% or more, is technological and related innovation.

We also read Paul Romer. And Paul Romer says, wait a minute. It's not just having an R&D system. You've got to staff that R&D system. It's the talent base. That's a key driver. That's another critical factor. So his phrase is quote, "human capital engaged in research." So if you train great physicists and they're driving cabs, they're not engaged in research, right?

That's a no-net gain. But if they're engaged in that system, that's what's critical. And his prospector theory says the size of a well-qualified talent base makes a huge difference. That's a key driver. Talent on task and a lot of talent on task is better than less talent on task. So those are two fundamental innovation factors that we arrived at in the first class.

Class number 2, we began thinking about innovation systems. So we got two factors out of the first class. Then we began to think about systems. Nelson tells us that there are national systems of innovation that you can analyze, innovation at the systems level, and look at the actors in that system and the strength of the actors and therefore the strength of the whole system.

So that gives us a third innovation factor, which is really-- we'll call it innovation organization. And then we read some other pieces in there. I'll highlight a couple. Don Kash and Robert Rycroft said, you've got to network the actors. It's the network, the strength of the network of the actors, that's now, particularly in a 21st century context, particularly critical.

And we also read Charles Schulze, a noted economist from the University of Minnesota, who was a senior governmental official in the Lyndon Johnson era and then Jimmy Carter era. And he raises the issue of what's the public role? What's the private role as we look at innovation

systems? What should the public function be versus what the private sector should be. An absolutely fundamental set of issues.

So then we did classes 3 and 4. We essentially focused on the innovation challenge ahead in production. And in class 3, we looked at the kind of history of US manufacturing problems. We read Kent Hughes, and he explains the fascinating competition when Japan launches a brilliant new innovation system and production around quality. That's where the rust belt begins. That's where US statistics about growing inequality begin. That's where this problem that we're now worse off at, that's where it commences. And then we looked at Korea and Japan and how they built manufacturing-led innovation systems.

And then in class 4, we looked at the current US manufacturing problems. So I wanted to put a set of real issues, really timely critical issues, kind of on the table, if you will, at the very beginning of the class so that we're not-- you know, this is not a class about abstraction. These are real issues that are extremely timely in the way in which society operates and functions.

And the US built a comparative advantage around its R&D system. It did not organize around manufacturing-led innovation like Japan, Germany, Korea, Taiwan, China, and so on. We missed that organizational step, and we are continuing to pay a price for that.

So we talked some two weeks ago about David Autor and some of his absolutely fascinating work on the decline of US manufacturing and the effect on the labor force. He has a new piece out that talks about the effect on innovation of the intense competition with China.

And essentially, what he finds is that firms that go up against head-on-head competition over a particular product line with China have to slash their innovation capability, their R&D capability, in order to stay in the game. But that kills them long term. So that this competition with Japan is not only having an effect on the labor market, but it's also having a big effect on the innovation capability of companies.

And then we talked about whether advanced manufacturing could bring a new paradigm to the table, where US developing countries can kind of get back in the game. So in effect, they would take innovation leadership, innovate in the production system process and technologies, and be able to compete better. That's the essential question.

And as I mentioned last week, MIT for example, there are 14 manufacturing institutes that

have been stood up in the last few years. MIT is participating in 9 of the 14, leading one particularly active and about three or four but also participant in the others. So this is something that's now ongoing on this campus in a pretty sizable way.

So that's the background on manufacturing. Now we go back to our whole project on innovation organization. That's today's class. And we're looking at that problem of how do you innovate at the institutional level? And in particular, we're looking at where did the US innovation system come from? How did it get created? How did it get put together? And that's the story that we'll weave today.

As a backdrop to that, we'll start with a framework that a scholar named David Hart put together. David teaches at George Mason University. He teaches innovation policy issues there. And he wrote this-- he got his PhD from MIT and STS. He was head service in the Office of Science and Technology Policy so he as practical as well as a lot of academic experience. Authored a big book on energy innovation policy.

David did this book back in 1998, and he essentially argued that what's the ideology of science and technology policy? If you put it into a political economy kind of context, what is the thinking process? How do you think about this? And he identified kind of five political strains in the US that, frankly-- you know, I worked in the Senate for over 15 years.

I saw them all. They were real. They were all still there. There were all ways of thinking about science and tech policy, R&D policy that I saw all the time. So these are very much alive and well. And David tried to put a frame around them, and they will help guide us on the politics of science support and technology support in this country.

So what are these five visions? I'm going to summarize them briefly. And which of our discussion leaders has David Hart? Chloe. OK. So the first was conservatism. Now, this perspective saw no need for the state, for the federal government, to do anything other than providing for defense, including military technological innovation. Get the state out of everything else. They've got to do defense, but leave the rest to the private sector. It's a very non-interventionist philosophy.

The budget that President Trump just sent to Capitol Hill for FY '18, largely written by the Heritage Foundation, argues exactly this theoretical line. It takes-- it eliminates ARPA-E. It takes \$2 billion out of the Applied Research Agencies and takes almost \$1 billion out of the basic research agencies at DOD. It's a gigantic cutback.

The theory is, let the private sector do it. They'll do this energy stuff. So this is right on us. This theoretical framework for approaching R&D support is right there. One of its leading proponents, and we'll run into him when we talk about Alfred Loomis, one of its leading proponents in this period that David is writing about in the '20s and '30s when these ideologies really got put together, was Frank Jewett.

Jewett was head of Bell Labs. And he was president of the National Academies. Had both slots. And he's head of Bell Labs. He's got one of the great basic research as well as applied research organizations in the history of the world. There is a phone monopoly. The last thing he wants is the federal government messing around in his turf. Naturally, he's a proponent of keeping the feds out, except in defense and military, which they've got to do. So we'll run into to Frank Jewett in a few minutes.

But this is a noninterventionist defense-only piece. Now, on the other side of that argument is what David calls associationalism. We would call this public-private partnership. That's the lingo that's more or less used at this time. But the meaning is roughly the same. David points out that the first big exponent of this was Herbert Hoover, who along with Jimmy Carter, was one of the only two engineers to ever be president of the United States. That doesn't mean there can't be more, just as a lesson to you all.

Hoover was an amazing engineer. He was a great organizer of the World War I war relief effort to save a staggering number of lives after World War I. He was a highly innovative Secretary of Commerce and made that at the time actually one of the most important Cabinet agencies.

His vision was that unlimited economic competition curtails firms' ability to be innovative. If you're frantic about your competitors, you're not going to have the energy, time, or resources to spend on longer-term, higher-risk R&D. So there ought to be a gathering between public and private sectors that would collaboratively undertake this. So this associationalism really occurs in the 1920s.

He is a great leader of NIST, the standards organization. That would be a classic public-private collaboration, where private industry under government auspices would come together and negotiate and work on standards for products to enable their much more efficient production. So that would be a classic Hoover kind of effort. Franklin Roosevelt adopts this associationalist attitude when he becomes president. There's a lot of public-private

connectivity during the Roosevelt regime.

So that gives us two models. Now, this next model, it's kind of disappeared. But it was a big movement in the 1930s. The basic theory was that cartels, monopolies, collusion amongst firms discouraged innovation. You build up market control. The last thing you want is a disruptive product. And we can see this, of course, in a sector like energy even now.

So the concept was reestablish markets by legislation or through example by anti-trust laws or regulation, and you would encourage innovation to break out, because you'd be breaking up the cartels that were limiting innovation. There's so much global competition now, and the competition is so intense that, frankly, there's just not that much going on in anti-trust practice or theory.

Firms may be curtailing innovation because of day-to-day competitive pressures, but that's not an underlying cartel problem, because the competition is there. So this one is less present now than it was back in that kind of '30s timetable. A fourth is Keynesianism. You remember John Maynard Keynes. Keynes develops some crucial fundamentals of economic theory. His principle contribution is that in periods of economic downturn, government spending can step up to the plate and substitute for the decline in private spending and, in effect, stabilize an economy.

So we just saw this whole theory played out in the Economic Stimulus Bill. That's what happened in the 2009 Stimulus Bill. Massive economic stimulus. I think the economists would say that it worked. It did stabilize the economy in fairly surprisingly short order, given the depth of the threat. We didn't slip into another great recession or Great Depression.

But the idea here, as applies to science and technology, is look out for the macro-investment levels. Have a governmental role in funding investment in research and development, because the public sector is not prone to take longer-term risks. And the government can substitute for that.

So this factor is still with us. That's why in the first class when I had you read the National Science Foundation Indicators that told us all about investment levels by government in R&D, that's Keynesian thinking. That's government substituting because the private sector has difficulty undertaking longer-term high-risk investment in research. So that's another ideological kind of element here.

The fifth is a little bit like conservatism, but it's much more far-reaching. This emerges in World War II and the Cold War. And this doctrine says, whatever it takes. National security is so critical, we will do anything. The government will be every bit as interventionist as it needs to be in order to get us national security. And that's still very much alive.

I'd say someone like Senator John McCain, who is currently the Chairman of the Armed Services Committee, Senator McCain is probably a conservative when it comes to federal R&D investment, generally speaking, but when it comes to defense, whatever it takes. So an organization like In-Q-Tel, which essentially buys a controlling share in companies so that the government can get the technologies at once, it's government becoming a venture capitalist on steroids.

McCain is a huge fan of that model. And In-Q-Tel works in the intelligence security kind of area. So this movement is around too. And this is essentially what Vannevar Bush puts together when he creates a connected science model-- we'll talk about it a little later-- during World War II. So these are the ideologies. I'll just summarize them very quickly again. Conservatism. Government can do military innovation, but that's it. Associationism. That's the public-private partnership model.

Reform liberalism, which is stamp out cartels. They limit innovation. And that's not really a powerful movement at this point because of international competition. Keynesianism, which is analyze science and technology based upon these macro-investment levels by the public sector, which serves to offset a market failure on the private sector side. And then the fifth one is this national security state.

So when I was working in the Senate, I saw all of these, sometimes all them in a single day in debate on the Senate floor, as people would take different perspectives and different political understandings of the role of R&D in science and technology. So these are all still very much with us and guide a lot of thinking to this day. The latest example, as I mentioned, was President Trump's FY '18 budget, and it's his position-- explicit position-- on supporting research, which a lot of it the private sector could just pick up. It's just straight out of the conservatism playbook.

CHLOE:

All right. Chloe, it's yours. OK. So first of all, obviously he gave a rundown-- very effective rundown-- of all the different--

WILLIAM

You didn't have to say that.

BONVILLIAN:

CHLOE:

Just curious why I wouldn't do that for a third time and for everyone. I think just in conclusion, tagging onto that, it's interesting to note, and Hart sort of makes the point, that an ever-changing political tide-- he says that it generates a lot of policy entrepreneurs. So people coming out and being like, oh, this is my new policy. It's brand new. It's never been seen before.

But in actuality, a lot of these theories were Frankenstein models from previous theories, and the successful ones often owed their success in part to the pre-existence of other institutions that they could-- and resources that already existed that they could repurpose for readjusting their mission for how they viewed how science and technology research funding should actually go about.

So in looking through a lot of your summaries and questions, it seemed like a lot of people wanted to discuss the relevance of these five visions to this particular time period, maybe what political climate we have now, like what combination, what hybrid of these five that we are living with today, and then also maybe what components of each are actually successful, in your own personal opinion or experience, for funding research.

So just to sort of hone in on-- I found associationalism and liberal-- reform liberalism the two most interesting ones. This is my first question for you all, sort of talk about a bit of a tragedy of the commons type situation. So the institutions desired by associationalism are intriguing. But does such an implementation have a degree of naivete outside of wartime?

Are all who utilized new information as a resource-- do all who utilize the information as a resource as an end to the mean of their final product, are they also invested in the idea of sharing that information with their competition, even on a national scale? I think that's something a lot of people would think is naive perspective.

MAX:

Well, it probably depends on the context. So if you were to look at Conant actually-- yeah, shameless plug, because that's my reading-- so if you look at that, in the time period in World War II, you had companies that were fighting this absolute evil that was going to just engulf the entire planet in a Nazi regime. So people were scared. So they thought, OK, you know what? We're going to do whatever it takes to fight this. And it was in everyone's best interest to fight it and share all the knowledge that they could.

Actually, in another class, I was reading a case study on-- your part of Lincoln Electric. It's this company, they make arc welders. They've been working for a few decades, and they actually shared a lot of their manufacturing secrets during World War II, just for that reason. So that's one way that it's not extremely naive, like they just need the right incentive. On the other hand, if you look at today when there isn't an overarching evil that everyone can unite behind and try to fight against, people have-- they don't have as much incentive to share all their secrets.

CHLOE: So you think that you specifically need that defense urge for that cooperation to exist.

MAX: That's what history has shown me, at least from what I can tell.

**WILLIAM
BONVILLIAN:** And I'm glad to tell you, Max, that next week we'll have a reading that's entitled, is war necessary for economic growth? So we will continue this discussion-- that's week two-- in addition to that.

CHLOE: Do you think that any institution that might benefit from this sort of like government-funded encyclopedia of information, do you think people wouldn't want to contribute to that or take from that in a time where there wasn't this pre-existing military need?

Because I mean, I think it's arguable that you would still benefit-- a lot of these theories, to me, one of the overarching things I noticed is that a lot of them-- a lot of people seem to feel the need for the government providing something that would facilitate communication between innovators and people who implemented them in industry. It would save a lot of time if the people who-- if everyone had the same terms and everyone knew what was possible and achievable. And I don't know if that's something that implementable, even outside of a time when the country all needs to work together.

MAX: It also could-- it could scare some people who went through the Great Recession, where you've got industries that are quote, unquote, "too big to fail," because then if you have all these industries that are all sharing all of their information, then in some sense, they kind of are becoming one big conglomerate then. At least, that's the way that the thinking could go. Now, there are flaws with that, like they don't share the same finances, the same employees, all sorts of stuff. But people could rationalize that it's a step toward that. And from there, that also could reduce competition. Then you have, hey, the anti-trust laws come back, and they're actually interesting again.

CHLOE: I think that's a good point.

MAX: Yeah.

WILLIAM BONVILLIAN: Right. So Chloe, you're pushing us to the point that, given your interest in the associationalist, model, the public-private partnership model, this is exactly what Hoover was driving at, that there may be a way of creating at least limited cooperation, particularly around fundamentals, what has become known as pre-competitive research. In other words, research before it reaches a competitive level that's key to your company. At least you could all cooperate on the pre-competitive stuff.

So that has been a conscious part of the design of some of these public-private partnership models in timetables like the Clinton administration, for example in particular. Could companies come together and share resources around this pre-competitive stuff, even if they were going to be at each other's throats the moment that work was completed? And how practical is that? Are they really going to be willing to do that?

So the manufacturing institutes that we mentioned last week, this is a critical issue for them, absolutely critical issue. How do you get all these manufacturing companies, small and large, into the same room to cooperate when they're all also in competition and the Manufacturing Institute model is actually pretty applied stuff. It's around production processes that for some companies may be their secret sauce. So how do you-- you've identified a really critical barrier in this associationalist model. How do you get this kind of collaboration, given the reality and importance of competition?

MARTHA: I'll come in [INAUDIBLE]. I've been here for a while. I'll throw in a thought, though, of course, which is I do think it's really specific areas and pre-competitive and then also enabling technologies, like looking today, and we're not seeing it really happen, but at the MIT Energy Initiative, companies are excited-- energy companies are excited about getting together on carbon capture and sequestration, partly to enable them to just keep running their companies. Now, we haven't seen anything formal really happen, and the technology is still way too expensive, et cetera. But I think in specific areas that it can happen.

WILLIAM BONVILLIAN: Yeah. It's an interesting idea, Martha. In other words, in areas that are outside your core competitive advantage, could you get together with others, because it might help everybody? Right?

MARTHA: And I was wondering that about the Manufacturing Bill. I am not that knowledgeable about it, but if you consider your secret sauce is something other than the actual manufacturing, if you're a conglomerate that has great product design, you know, I don't know, does that work? I don't know. I don't know where you were willing to open up your secrets to others.

WILLIAM BONVILLIAN: Right. One of the manufacturing institutes we talked about last week was around 3D printing. And it turns out that that is going to be an absolutely critical core technology and the way in which anybody ever makes another jet aircraft engine. I mean, maybe not this week, but certainly in the foreseeable future. It's going to be absolutely critical.

So there's frankly been a tremendous amount of nervousness when companies like Pratt and GE and Lockheed and Boeing and others come into the same room and who's ahead? And are they going to share their secret sauce? And how is this going to work? So working on the intellectual property agreements has been really complicated.

So this public-private partnership model, there's a lot of examples of it working brilliantly, but it is not an easy model to bring these highly-competitive firms together and try to get them to agree on an agenda that they might be able to work together on that would save them all resources and funding. And Martha runs into that, I'm sure, every day in MITii.

MARTHA: And we will more and more with these consortia that we're putting together.

CHLOE: I thought another-- to bring free-form liberalism into the discussion-- an interesting contrast that emerged between them, sort of off of that topic of trying to figure out who the other people-- like, what they're thinking-- other people in the room, what they're thinking, and what your competition is thinking, is that both these two policies seem to have very different-- two theories-- seem to have very different views on-- to get a philosophical-- the innate goodness or lack thereof in industry leaders.

And I think this was such fun. I forget the name of the reading. It was a later one about how there's a philosophical difference between basic research and applied research and the people who make a career out of each. At the end of the day, they have different end goals and different core values. So I thought it was interesting that that was sort of superimposed onto industry leaders, and I wasn't quite sure if that actually makes a wide base for an economic policy, is judgment calls about human nature. So I know that's a little touchy-feely, but I'd be interested to hear if anyone was like, good idea, bad idea.

AUDIENCE: I have the reading that you're talking about.

WILLIAM Stokes, right?

BONVILLIAN:

AUDIENCE: Yeah.

WILLIAM Right.

BONVILLIAN:

AUDIENCE: So I think a really important distinction that he makes is that whether to sort of base it off like a speculating, like speculating if these are the goals of the actors or like retroactively knowing. And I think that's a really important distinction, because if you're making policy, of course, you can't-- hindsight is 20/20, but you're not going to know retroactively.

But you have to have some degree of speculation, which is something that I think-- well, I wasn't quite sure what Stokes was trying to say there, but he was saying that the retroactive way of looking at it is actually better than speculating when looking at it. But I think it's a little bit impractical when you're thinking about policy decisions to kind of try to interpret what the goals of the certain actor are going to be based on something like human nature or like some other type of kind of non-tangible measurement.

CHLOE: Yeah.

WILLIAM Chloe, would you give us a closing thought on David Hart?

BONVILLIAN:

CHLOE: Sure. I guess, would a closing question be OK?

WILLIAM Sure.

BONVILLIAN:

CHLOE: Because still I haven't-- this is a central question for me, one that I haven't-- I feel like we haven't come to a conclusion on. How have these policies sort of survived the test of time, because they're all so built on each other and come from different periods, World War II, Cold War, the Korean War, and different manufacturing stages throughout our history? They're all sort of interwoven and interdependent. What does that leave us with today? What lessons have we learned and what theories are working for us right now? Yeah.

STEPH: I thought that an interesting concern about the Hart reading was that it sort of presumed that scientists and technologists were not a part of the policymaking process, that they're sort of receivers of the actions of policymakers.

So I thought it would be interesting to think about maybe today or in the next election cycle for midterms and congressional elections, what it would look like if there were more scientists and technologists running for Congress and as policymakers and maybe a scientist running for president someday or a technologist running for president, as they have speculated Mark Zuckerberg to be doing.

Because in that sense, it wouldn't so much be a conflict between ideologies, but a conflict between how much that person chose to put in money in the industry that they're a part of and how much they are motivated by what they know and what they don't know.

WILLIAM BONVILLIAN: You put your finger right on a pulse of a critical issue, Steph. The reason why I'm teaching this class is so that all of you will be citizen scientists and be ready to do great science and engineering and technology and run venture capital firms and start companies. I want you to do all that stuff, but in addition, if you would also hold public office, I'd appreciate it too.

No, in other words, the science-- this is an intensely interest-group-driven political system we've got. And if you don't show up, you don't get counted. And frankly, science and engineering doesn't show up very much. And there's a lot at stake, as we speak, now. And part of the story is that the community of scientists and engineers, they're not on the front lines. And in an interest-driven society, that's really problematic.

Now, it's storytelling time. I couldn't resist putting this in, because at least those of you from MIT, this is an important part of your history. But I think it's fun for everybody. This is Alfred Loomis and Luis Alvarez, the great physicist, called him the last of the great amateurs of science. And you know, he's a very good-looking guy, as you can tell. That's the Rad Lab in MIT, where the Stata Building is now. That's Rad Lab latest radar sets. This is an early army radar set. And that's the business of the Rad Lab that Loomis creates.

And what's this story? So if you go over to Stata and you wander around on the first floor, at the end of one of the student streets, you're going to see some of these radar sets. And there's plaques and stuff around there saying this was where the Rad Lab was. And from an MIT perspective, one statistic you need to know is that in the period from 1941 to 1945, MIT as an institution received 80 times more federal funding than it did in all its previous 80 years.

This is what creates the federally funded research university. And it starts big time at the Rad Lab. This is where MIT becomes a critical and big and important institution. It really scales up. It's not alone. This is happening at other research universities across the country. And it's the story of Loomis.

So let me just give you a little background on him. Born in 1887, died in 1975. He comes from this WASP blue-blood family in New York. And his father is a society doctor, serving those blue bloods. I'm not sure they ever had any health problems. But if they did, his father was there. Until his father runs off with his secretary. This is when Loomis's life changes. Loomis is now stuck with the responsibility for the family. He can't do what he wanted to do, study science. Instead, he has to go to become an investment banker, heaven forbid.

So he has technical training and a love for science and strong undergraduate science training. He goes into investment banking. He understands technology. He makes an absolute, one of the great fortunes of the 1920s, and his investment banking firm is setting up utility companies, utility holding companies. He understands the electricity technology, the transmission technology. He's able to capitalize on that and make a fortune for essentially electrifying large parts of the country.

He sees what's going to happen. And in 1928, he takes his entire fortune, every last dime and gets out of the market, because he saw the bubble. And then what's he do? So he's got the estate, Tudor house, pond, grounds, lawns, 40 acres' worth in a place called Tuxedo Park, which is about 40 miles north of New York City.

The tuxedo, you know, the funny penguin suit, comes from Tuxedo Park. That's the kind of clothing they used to wear there in the evening. Gives you an idea of what it's like. It's like the first kind of gated multi-acre suburb for this compound for staggeringly rich people from New York City, only 40 miles north of the city.

So he's got this Tudor mansion there. He loves science. He's walked out with his entire fortune intact. He creates probably the top private research laboratory in the 1930s. And he staffs it with an incredible group of scientists. But in particular, he specializes in what we now call the summer study. And he's there researching himself.

His field is physics, particularly microwave physics. And he invites in the who's who of physics from all over the world. So the famous European physicists are there for summer projects, and

they're rowing on the pond and playing lawn tennis on his lawn tennis courts, and doing little research, and talking away and having great meals. It's a nice setting. All right. You get the picture. He gets to know the whole who's who of physics, and he's doing important work himself in physics, in this microwave physics area.

Time passes. World War II breaks out in Europe. He's very sympathetic to the Brits. His mentor, his first cousin, is Henry Stimson, who's Secretary of War to Franklin Roosevelt. Stimson's a famous leader of the War Department during World War II and a very talented, highly respected figure in that era. He had been Secretary of State previously in the Coolidge administration.

Stimson trusts Loomis. They're first cousins, but in effect, Stimson is Loomis's father figure. So Loomis becomes a very critical advisor to Stimson in this pre-war period of '39 and '40. And in addition, Loomis is serving with Vannevar Bush, who we'll talk about in a minute, on the early defense research committees.

So let's switch to Britain. So Britain and the United States develop radar roughly about the same time. In the US context, it was developed by the Navy at the Naval Research Laboratory on the Anacostia River. And they were fiddling around with radio, and they were shooting radio beams across the Anacostia River. And they noticed this peculiar thing, which was a boat would come down the river and the radio beam would bounce back. Voila. Radar.

The British were doing experiments at roughly the same time period. And because the US wasn't 100 miles away from German bombers or 200 miles away from German bombers, there was no sense of emergency in developing radar in the United States. There was a considerable sense of emergency developing radar in Germany.

The going doctrine of the time, based upon what happened in Spanish Civil War, was that mass bombing was going to be a core offensive military strategy and that there was nothing you could do to stop the bombers from getting through. The bomber will always get through was the prevailing doctrine.

And people began to see the effect of mass bombing from sites like Guernica during the Spanish Civil War. And it was a nightmare. It was a complete nightmare. It dictated a lot of the politics of appeasement in the 1930s, because the assumption was that you couldn't defend against these bombing raids and that cities like London and others would just be leveled at huge slaughter.

So if your assumption is that you can't defend it against them, that you're going to lose, and that you're going to lose a huge part of your population, you can understand where a lot of appeasement doctrine comes from, where Chamberlain and Baldwin are coming from.

Meanwhile, Winston Churchill, always a maverick, is trying to figure out what to do about the situation, because it's untenable from his political perspective. So there's an interesting figure named Tizard, who is head of Imperial College in London, previously a professor.

But in World War I a pilot and in World War II, because his eyes weren't that good, he was relegated to being a test pilot in the RAF, the early version of the Royal Air Force, Royal Flying Corps. And he learned pilot talk and how to get along with pilots. He was one, and he knows the language, and he understands how it works.

And the radar idea comes to Tizard. As a noted academic, he's heading a scientific advisory group for the Royal Air Force. And he seizes on and realizes this could be the core of a defensive strategy against the bomber, that might be able to stop it from going through. And just because you have a technology idea, it doesn't mean anybody in the military is going to adopt it. It's got to become operational.

So he spends years working with the Royal Air Force trying to develop an operational doctrine to integrate these early long-range radar systems, the communications with the Spitfires and fighter planes and the Hurricanes, to communicate the results that they're seeing on their long-range radar screens, get those up to the fighter pilots, get them up in the air and get them to the right location at the right time. That's critical, because you can't maintain continuous cover with fighter planes. They're short distance, and they're under-fueled, and they have limited range.

So if you try to defend against 1,000 bombers, you can only have a certain number of fighter planes up in the air at any given moment. But if you know exactly when the bombers are coming in and exactly where they're going to be and what altitude they're going to be at, then you can mash your fighter planes and get them position ahead of time and then take them on. And that's what the British figured out with radar.

Now, their system was long-wave radar. So they had like a system all around the coast of like these big 60-foot telephone pole kinds of things. And then these big 30-foot things that looked like bed springs, only they were about 30 feet long. And it was called the Chain Home System.

It was all around the critical parts of the coast. And then they would get information from the radar sets to an operational center that would run the fighter plane cover system at air bases throughout the British Isles and scramble these jets-- scramble these fighter planes and get them up just in the nick of time.

It's very clumsy, because the pilots don't have the radar. They're talking to somebody who is in turn talking to somebody about what the radar says. You can imagine what the communication is like. I mean, you have to get it down to almost a separate language on how to communicate to people. So it's very tough. Wouldn't it be better if the radar was in the airplane? But if it's long-wave, how are you going to put this 30-foot bed spring into a fighter aircraft? That's the problem.

So the British work on-- frantically working on microwave radar. You know, 10-centimeter long radar beams with the idea that whoever figures this out is going to have a staggering tactical and strategic advantage in the war. And they put a team on it, and they come up with 12 early sets of microwave radar. Then what are they going to do?

Well, Britain has so allowed its industrial base to erode that the best thinking is we're going flat-out on building munitions, we don't have capacity to take up this whole new territory of effectively invent electronics. We can't do that. Who could do it? The Americans.

So Churchill, pressed by Tizard and others, comes up with the idea that they'll take microwave radar and a bunch of other secrets that they've got, because remember, British science is very strong, probably stronger than American science at this point. We wouldn't think so, but that's probably the case. And Tizard is nominated to lead a mission to the United States and essentially offer to put-- they offer to put their secrets on the table if we, the United States, will put our secrets on the table. We'll trade secrets. That would be the deal.

So a group of scientists and a couple of military officers get on a high-speed passenger ship. Now, passenger ships are moving at about 30 knots across the ocean. U-boats are much slower. On the surface, 14, 15 knots. Far slower if they're running below the surface. So nobody sank a passenger ship on the Atlantic route during World War II. They always got across. So they go into Halifax, and they have in a suitcase the most critical secret of the war, one of these 12 microwave radar sets.

Loomis flies over to Halifax. They take the train down. They go to Washington. There's a meeting at the Wardman Park Hotel. At the Wardman Park-- I bet you it's still there-- Loomis,

rich guy, consulting in Washington, trying to help the war effort, he's got a penthouse on the top of the Wardman Park Hotel. Fireplace. I'm sure it's nice. The meeting is at a private dining room downstairs, just off the big dining room at this fancy Wardman Park Hotel.

Loomis, as Stimson's unofficial advisor about science and technology, has convened the military leaders from the services, from the Army and Marine Corps and the Air Force, and this is apocryphal. It may not exactly be the case, but in essence, our uniformed military leaders have decided that they're not going to agree to exchange secrets. They're not going to do it. They think Britain is going down. Battle of Britain is hanging in the balance. They think Britain is going to lose. Anything that goes to Britain is going to fall in the hands of Germany.

So they hit on a strategy, is the apocryphal part, of pretending to get drunk at the meal and falling asleep in their soup dishes. This may be exaggerated. It was told to me by someone, but we'll see. So they do it, and the Brits have just come across the ocean. They've got the suitcase with them in the room. I mean, you know, the war is hanging in the balance. The war could be one if these yanks just pick up this technology and manufacture it. Lots at stake.

And they start throwing out hints, little hints about this cavity magnetron, as they call it, and it's microwave capabilities. And of course, sitting at the head of the table as Alfred Loomis, who happens to be America's leading microwave physicist at this point, author of numerous articles and so forth. No PhD, but for every intent and purpose, the leading scientist in the field. He gets the hints. He's been working on a much earlier stage of radar himself. He starts to understand what they're talking about.

Little more time passes, Loomis goes up to his penthouse with Tizard and the five others, sitting around the fireplace, and the story comes out. And then they in turn meet at, you guessed it, Tuxedo Park and the suitcase exchanges hands. And then Loomis, who probably unique in the country, is capable of understanding what this technology is, how powerful it is.

If you put a radar set in an airplane, the airplane can see. The airplane can fly at night, not just during the daytime. It can do its own interception and its own targeting. It doesn't have to rely on clumsy radar signals going through two or three people down on the ground.

It's a transformative technology. He understands what it is. He's been himself working on kind of a much earlier stage of this. What's he do? Well, never underestimate the power of WASP blue blood family connections. So he immediately gets on the phone with Henry Stimson, his

first cousin and mentor and surrogate father. Henry, got to move on this. Go see Roosevelt.

Henry, same day, goes to see Franklin Roosevelt. Franklin Roosevelt, the Dutch WASP blue blood family connections, yes, move on it, Henry. So literally within a day, the fundamental decisions are made. Tizard advises Loomis on the structure of the organization that the Brits have put together to do this. And the Americans decide to do the same thing.

So what did Tizard set up? This is not a bureaucratic military laboratory that is doing radar in Britain. This is civilian scientists, not in uniform, organized in a loose almost academic kind of flat, non-hierarchical way, where everybody pitches in with the best ideas. There's leadership, there's organization, but it's not a military bureaucracy.

Loomis doesn't have to be persuaded of this. He had worked in the war effort in World War I and found the military bureaucracy impregnable to ideas. It had been completely frustrating to him. Vannevar Bush had a similar experience, complete frustrated as well. So they latch onto the way in which the Brits have organized their scientific advisory groups and their organizational efforts for doing science in a wartime setting.

The microwave radar is a really important importation. The model of scientific organization is every bit as important. And Loomis runs with them both. Loomis immediately gets hold of one of his very best friends, who is Ernest Lawrence, who is the most prominent American physicist at the time. Loomis had been personally funding Ernest Lawrence's work to build the early cyclotrons up on Cyclotron Road up above the Berkeley campus, where Lawrence is a professor.

They're very close. Loomis has been paying for him and his projects. So he gets Lawrence on the phone and says get out here. I can't talk to you on the phone. Let's get out of here. Lawrence-- it's unusual at the time-- he doesn't take the train, he flies, meets within days with Loomis. The two of them get on the phone. Because of the summer studies at Tuxedo Park, Loomis knows everybody and Lawrence knows everybody else.

They get on the phone, and the conversations are like this-- hi. We're going to set up a new laboratory. We can't tell you anything about it. Arrive in Cambridge in two weeks and quit your job. They all come. So powerful are Loomis and Lawrence that they come. The who's who of physics in the United States shows up. Now, why are they going to show up in Cambridge? It's another critical part of our story.

Here's some of the characters that were in the room. So there's this defense advisory group that Vannevar Bush, here, is running. And Loomis is his advisor, here, on air defense microwave radar, all this radar stuff. And Vannevar Bush and Loomis call a meeting to make a decision about where to put this new Critical Defense Laboratory that's going to work on microwave radar.

Not in this picture is-- you remember Frank Jewett, the exponent of conservatism who is head of Bell Labs and the National Academies? Frank is in the room. Frank says, well, we're going to put it at Bell Labs. It's where it's going to go. Who would even think otherwise? There's a vote. So in the room is Karl Compton, president at MIT, and James Conant, president of Harvard. You may recall that those two schools are in Cambridge. Loomis and Vannevar Bush, and that's Lawrence, by the way.

And that's Arthur Compton, who is Karl's brother, also a quite prominent and important physicist at the time and involved in this defense work. But he's not in the room. And then Jewett. So it's one, two, three, four, plus Frank Jewett. Surprise. The vote is 4 to 1. It's going to go to MIT.

And all there is at MIT is industrial space all over the place. And it's just coming out of the depression. So there's a lot of space in MIT. So President Compton volunteers the space. And that's how come the Rad Lab gets stood up at MIT. And it is a remarkably successful organization. This is how-- this is the foundation of electronics in the United States. This is where most of that comes from.

Of the numerous scientists that worked at the Rad Lab, I think 10 win the Nobel Prize. The group of scientists that set up the Rad Lab do it on this radical, flat, non-hierarchical, collaborative stay-out-of-uniform organizational model. Loomis plays a critical role in scaling up the Manhattan Project, which had been kind of desultory.

So Loomis next jumps on that problem and gets that organized. Oppenheimer, who we will talk about in class 7, is designated to head that project. Oppenheimer doesn't know how to organize this stuff. Eleven people go from the Rad Lab to organize Los Alamos and work there.

And Il Rabi, who is from the Rad Lab, a great Columbia physicist and Nobel Prize winner, he becomes Oppenheimer's advisor on how to organize the Los Alamos project. So you begin to get the picture here. It's a great story about a particular technology, but frankly, more

important than that story is a story of scientific organization.

How are we going to organize science in this country? And what we call the FFRDC, Federally Funded Research and Development Center, that is going to be the model. And the first one of those is the Rad Lab at MIT. Then Los Alamos and then so on. Lincoln Laboratory up the street is organized on exactly these same kind of lines. So it's the organizational model in the end that's most critical. Rasheed.

RASHEED: This is a point-- I'm pretty sure that's a cyclotron drawing on the chalkboard in the picture?

WILLIAM That's very cool. I've always wondered what it was.

BONVILLIAN:

RASHEED: Yeah. You can tell by the two halves of the circle on the right. Yeah. And then I'm pretty sure that's a magnet drawing in the middle there on the left. I don't know what's going on above it.

WILLIAM This one?

BONVILLIAN:

RASHEED: Yeah. Random sinusoid.

WILLIAM Thank you. That's great. Now I know the picture better. If you could check that out, that would be fascinating.

BONVILLIAN:

RASHEED: Yeah. I mean, if Ernest Lawrence drew it, it's probably a cyclotron.

WILLIAM Yeah. I don't see the chalk in his hand, though. I don't know. But it's entirely possible. But I think they're all smiling and happy because they just rolled Bell Labs and Frank Jewett, I think is what's going on. All right. That's my storytelling.

BONVILLIAN:

RASHEED: Yeah.

WILLIAM What's that?

BONVILLIAN:

STEPH: Oh, I looked up the blueprint.

WILLIAM It is a cyclotron.

BONVILLIAN:

RASHEED: Cool.

WILLIAM Is it? Steph, it's a cyclotron? Great. Rasheed, good. That was good.
BONVILLIAN:

STEPH: Created by UC Berkeley.

WILLIAM Hey, that's Lawrence. This guy.
BONVILLIAN:

MAX: I had no idea that Karl Compton was both the guy who made Compton scattering and the president of MIT.

WILLIAM Oh, yeah.
BONVILLIAN:

MAX: I didn't-- awesome.

WILLIAM Yeah. This is an unbelievable collection in this room. That is hard to beat that collection of
BONVILLIAN: talent. And Janet Conant's grandfather is James Conant. So she's able to do the book on Loomis, because she's got all the family records, all the correspondence. So Rasheed, we have a scientific consensus that that's what it is?

RASHEED: Yeah.

WILLIAM All right. Great. Thank you. That will add a whole new piece to the evolution of the story over
BONVILLIAN: time.

STEPH: It's what social scientists are good for. We do research.

WILLIAM That's good. All right. Max, have you got this one?
BONVILLIAN:

MAX: Yep.

WILLIAM All right.
BONVILLIAN:

MAX: All right. So--

WILLIAM You could actually tell us what actually happened.

BONVILLIAN:

MAX: Yeah. Let's see. I don't know. You covered it in spades.

WILLIAM It's one of my favorite story tellings.

BONVILLIAN:

MAX: Yeah, I gathered.

WILLIAM One story time in most classes.

BONVILLIAN:

MAX: I did appreciate-- so something I gather when I was reading it, I thought this had taken place before the Manhattan Project was ever formed.

WILLIAM Oh, yes. Absolutely right.

BONVILLIAN:

MAX: OK, which I thought--

WILLIAM So this group may well be gathering around next stages of the Manhattan Project.

BONVILLIAN:

MAX: Yeah. OK. So--

WILLIAM Because these folks are central to everything that goes on in science in that World War II

BONVILLIAN: period.

MAX: So I thought that was one of the general, like that emphasized the genius of this group, because before then, there weren't really like giant gatherings of this sort. And it was before the Manhattan Project. So something of that scale had never been done before.

LUYAN: What is the Manhattan Project?

MAX: What?

LUYAN: What is the Manhattan Project?

MAX: Oh, to build the atomic bomb. Yeah.

WILLIAM So Luyan, that's the great crash project at Los Alamos up on a mesa surrounded by barbed

BONVILLIAN: wire, way off in the deserts of New Mexico that does most of the critical work on developing atomic weapons in the course of World War II. And the great fear, as Max suggested earlier, was that most of US physicists knew Heisenberg personally. And Heisenberg's in charge of the German atomic projects. They know how brilliant he is. They think he's going to get there first, that they're way behind. And the war would be over in an hour if Germany had gotten there first.

So that's why there's such a crash project to put together the Manhattan Project. But this Rad Lab stuff is equally important from a tactical point of view, in terms of the ability of aircraft to operate, the power of air power in World War II, the ability of radar systems to see incoming airstrikes, the ability of fleets to manage air attacks. All that stuff comes out of here, and those are critical day-to-day developments in World War II. So the atomic bomb may have been the final ending of the war, but the day-to-day warfare is dramatically changed by the microwave radar.

MAX: So the first question that I thought of when I was just looking at all this reading, I was thinking, well, they were able to take this design and build up the manufacturing facilities, build up the labs, scale everything, get all the people, and make these planes with this radar equipment really quickly, because the war is only like four years. So they had to do all of that in that time span. So what struck me was just how did they get it so quickly?

Was it only because they had, as I mentioned earlier, the Nazi Germany threat, that they had to actually face? And is there any way that we could do that today? Any way that we could, without some sort of evil that we have to fight, is there any way that we can increase the speed of, I don't know, research progress or at least the development side of R&D, if we can make it that quick?

CHLOE: I think, just tagging off of MIT history and then sort of tagging in modern MIT history or modern today and MIT, but my lab builds CubeSats that are oftentimes funded by the Air Force Research Laboratory. And it's an interesting experience seeing the extremely short developmental lifecycle of these spacecraft that are technically supposed to be student platform spacecraft, but still spacecraft, being churned out in two years, which is ridiculous. Like, that's really fast.

And I think that that development speed is achievable because of the combination of a lot of money from the government institution given to an academic institution where you have a lot

of young people who are very passionate to do good work, who haven't been sort of exhausted by the industry already, you know, their passion is still there. There's still light in their eyes. And then they have the resources to-- you know, everyone's in one building or, you know, everyone can easily connect to the resources they need. So it's a good utilization of funds given to people who know how to use them quickly. So I think that combination works.

AUDIENCE:

Yeah. And I think that the Nazi threat is definitely something that lit a fire under the research, leading to application and widespread use, but there were also a couple of really important factors in this particular instance. One is that Loomis bankrolled a lot of this stuff by himself at the very beginning. And so it took the government more years, I think, to get behind it financially and finally jump on board.

And then the other is that Loomis can do pretty much anything he wants, because his cousin Stimson just gives him this blank carte blanche, do what you have to do. But I think that-- I don't know. Luis Alvarez makes a really bold statement there, like the last of the amateurs of science. I don't know. What about Bill Gates, for example, is trying to do something, I think, sort of analogous at this point in time, with the newer nuclear reactors that he's trying to set up. And he's bankrolling that to a large degree and so--

WILLIAM

Yeah. And he's taking MIT online courses in physics at the same time.

BONVILLIAN:

AUDIENCE:

Is he really?

MAX:

That's great.

AUDIENCE:

And he's assembling billionaires for this fun.

AUDIENCE:

Exactly. It's another great group, I think. So I disagree with Luis Alvarez on this point.

WILLIAM

Stay tuned.

BONVILLIAN:

MAX:

Well maybe the emphasis in that statement is amateur, like maybe there's a difference between what he defines as an amateur and maybe Bill Gates. Of course, it depends on his specific definition. Other questions that I had included-- so someone had mentioned-- someone had asked the question, I think it was Stimson, who had said that he would prefer to just stay involved with his research, and he really didn't want to get involved with politics or any

sort of war effort.

So someone has-- well, should research-- should scientists usually just stay in that mindset and just mostly stay into their research or should they get involved in politics at all? And I see the advantages of both, because on one hand, if you have people that are only working on research, well, they have a unique perspective they can contribute to politics. But on the other hand, that's time they're spending away from their research when they could be producing something that could be helping people across the planet. So I was just curious if you guys had any thoughts on that.

RASHEED:

I think I'd probably argue that it's pretty myopic to say that your research has no effect on the outside world, in general, just because you're studying the outside world. That's going to exist outside of the lab, and it will have an effect no matter what you do scientifically, like say you have your academic research circle that's probably directly affected. And then if you look outside of that, if your research is impactful enough, you're just going to have, no matter what, there are political ramifications and everything like that to everything you do.

And so I think reminding scientists to be cognizant of those things and just asking them to actively take this into consideration while they do research is sort of necessary, just because that's just how the world works. You can't really put a drop in the bucket and not experience ripple effects outside. So I think this is probably one of the really solid examples of why that's probably a good thing, because you have these people very interested, obviously, in the academic--

WILLIAM

[INAUDIBLE]

BONVILLIAN:

RASHEED:

Yeah, like the academic implications of radar, sort of electromagnetism, but also war efforts.

MAX:

And I definitely think that like this current political climate definitely emphasizes why scientists should make their opinions more known and the consequences of not doing that. You had your hand up for a while.

AUDIENCE:

So I think scientists and engineers like to do their work as entirely objective, like what they're doing is like black and white, like this is the truth and this is not. And I think that misses out a lot on the social context in which they're doing their work and other, like the fact that both what they're studying and how they study it is affected by the world that they live in. And so saying

that they can isolate themselves from that doesn't seem very realistic or effective.

AUDIENCE: I want to go back to that point about Luis Alvarez. I think what he meant is there's a whole period time where it was like [INAUDIBLE] JJ Thomson, Tesla, where it was like people who just did experiments versus now it's more academic, more like worrying about theories and then you do some research, but there's people that only-- they did experiments and they came to conclusions, and then they built stuff off those conclusions about what they built.

So it's more of an organic process of innovation. The benefit too of Loomis was he came from Wall Street, so it was very fast cycle, always getting things done. Sometimes you step on some toes. And so there's some benefits to that kind of attitude, because he thought about it more like a business problem in terms of, like, oh, I'm going to get the best people. I can incentivize them to be here. And he pretty much made the first startup incubator. And then also, who do I need to know to get this thing done? Because he could have definitely just been like, I'm rich. I'm going to be in my penthouse and just like take some drinks. I don't know if he had a wife or not. But like--

WILLIAM I'll tell that story in a minute.

BONVILLIAN:

AUDIENCE: I mean, I assumed there was a story there, yeah. But it very interesting in terms of he's just somebody who had to get things done. But he just kind of had a side interest. And that's usually like some of the most interesting people, where they come from one domain and then they go interdisciplinary.

WILLIAM And that Cavendish Lab, by the way, is a fascinating great group in the 1920s. It's really

BONVILLIAN: fascinating.

AUDIENCE: Cavendish?

WILLIAM Yeah. At Cambridge.

BONVILLIAN:

AUDIENCE: Oh, yeah.

WILLIAM Where Thompson and some of the others were.

BONVILLIAN:

AUDIENCE: So there's this theory that we should think about science more like politics and that there's different schools of thought. And the problem with science right now is more that we only have one way of thinking about-- you learn science one way or you learn math one way, whereas if you got different perspectives, it would make it easier to solve problems. And that's a very powerful thing.

AUDIENCE: Yeah. So I think Loomis himself said that he thought the scientists were more productive focusing on their research and getting them off politics. I think maybe he almost saw himself as kind of the interface probably. So as a more amateur scientist. But he's kind of facilitating this and making it possible.

And yeah, I think it's important that scientists think about it. But I guess I do agree that they should be focused and maybe have other people interface. I'll be interested to hear what goes in it as working in the MIT DC office that's a large part of what you guys do, I imagine, just talking to the professors here and advocating on their behalf.

MAX: You had a comment?

AUDIENCE: Oh, yeah, I just had a similar point to Matt, just like if you have an organization with people serving different functions, like maybe scientists can stick to their realm of specialty and focus on their research if there are other people connecting them to politics and outside.

MAX: I see.

STEPH: To counter, I think about Ernie Moniz a lot.

AUDIENCE: Moniz.

STEPH: Moniz. Moniz.

AUDIENCE: That's all right. You've got it.

STEPH: I think of it in the French, yeah.

AUDIENCE: You're fine. You're fine.

STEPH: I greatly admire him. No, I've just been looking at this picture lovingly of him. He's one of my heroes.

AUDIENCE: Well, tell him that.

STEPH: Please.

WILLIAM He's next door to Martha, as I recall.

BONVILLIAN:

STEPH: Oh, my gosh. OK. Even better. I'm blushing. Oh. Yeah, so I totally have a crush on Moniz. And I think about him a lot. So I spent-- I took a gap semester to intern in Congress. And one of the things that came out of that was becoming good friends with the White House liaison to the Department of Energy. So he was one of the people who helped pick Moniz. And then he took me around the office and the top floor. I saw his office. That was amazing. I passed out nearly.

But the reason I think a lot of him, other than the fact that I have an enormous crush, is that he is one of those scientists who I almost feel like there's an immense trade off because he became a public servant, because he could be doing so much incredible work in his area of specialty and expertise. And at the same time, I trust no one but him to run the Department of Energy, right? And so there's that sense that he can do what he wants, because he is an adult expert. And at the same time, that is an immensely personal calculation that he has to make about where he feels he can make the best contribution.

So I feel like our jobs, not only as students taking this course and as stakeholders in the future of a civil society, as civil as it can be, is to really consider whether the bulk of our impact in the world is going to be in an arena like politics or if we are better off delegating that to someone else and providing our opinion to other people. Because it could be that in the coming years, as he reflects on his tenure as Secretary of Energy, he thinks, well, I feel like I made a measurable impact, but it was for naught, because of the ways in which this next administration is going to dissolve a lot of the efforts that he was able to create.

So I think it'll be interesting to hear him give personal feedback and maybe more intimate conversations with students like ourselves. And if you do, please let me know, because I guess I would hate for him personally to feel like he gave up so much of his life and his productive potential to something that is going to get destroyed ultimately.

MAX: I was actually, like, when you mentioned him, I kind of started thinking-- I hadn't researched Compton before outside of scattering, for anyone who doesn't know, it's like when photons will scatter off of electrons and atoms. Does anyone know this? I don't know. So I didn't realize before this class that he was actually the president of MIT. So it makes me think that I don't

really-- at that point, I have a parallel point--

WILLIAM And this guy is his brother.

BONVILLIAN:

MAX: I wonder-- wow, I didn't realize-- I didn't even know he had a brother. There's so much about him I don't know.

WILLIAM He was another famous physicist. One of the all-time great MIT presidents.

BONVILLIAN:

MAX: And it makes-- apparently, he was a really good president, so that's good.

WILLIAM He's credited, Max, with bringing great science to MIT.

BONVILLIAN:

MAX: OK.

WILLIAM So MIT had always had very good science, but Compton really embarks on an effort to both train and find truly fabulous science. So MIT is positioned, frankly, in that 1930s period when he's president, to be this unbelievably capable institution with these two sides of the story, engineering and science, kind of unified here.

MAX: So, Stephanie, I would bring up that as almost a counter to your point, saying that maybe it depends on the time period, not really the person, because I'm sure that Moniz is really awesome. I'm sure that he's very articulate, and he is certainly very convincing in his positions. But it probably just depends on the timing.

So when people try to advance whatever agenda that they believe is best for society as a whole. So with Compton, I feel that he had a lot of advantages at the time period, like, oh, the United States at the time realized that we needed science in addition to our development capabilities. So it definitely made it a lot easier to-- there was a gap that he recognized, that he knew that could be filled.

STEPH: I'm trying to think of a statistic about how to get a woman to run for office. I think it takes something like eight times to tell a woman to run for office, and she'll finally begin considering it in any sort of serious capacity. And when you ask a man, it has to be maybe one or two times before they seriously consider it. And I wonder what it would look like for scientists. To

what extent do you have to cultivate this paradigm of evaluating the impact of your work and weighing that against the impact of your perspective and how that can make a difference in the world? So I'm going to be quiet now, because Rasheed seems to have a point.

RASHEED:

Yeah. I'm going to argue for form here. So I think these guys are all sitting around the room. I don't know how often this happens. This might happen like totally independent of the war. These guys just get together just because they're so interconnected anyway. But the point is, I think there's an argument here that all it really takes is you just have to get six or seven of these people of this caliber in a room, and they'll naturally come to the conclusion or the idea that we need-- they'll sort of come up with a solution for a federally funded research institution. And what's really nice here, I think, is Karl Compton is president of MIT and then it's Bush. Is there a president between them?

WILLIAM

BONVILLIAN:

No. Bush is not president. So Bush is Dean of Engineering and then Vice President MIT. And then he goes off to the Carnegie Institute, which is then a leading scientific funding organization based in Washington. And he becomes Franklin Roosevelt's science advisor member during the war. So he has a rich past at MIT as a faculty member. We'll talk about it in a minute.

RASHEED:

But I think because they're both sort of at MIT and sort of interconnected anyway, you have an opportunity to kind of transform MIT from this great scientific institution under Karl Compton to sort of this federally funded research institution, which largely was an idea of Bush and like a proponent. I guess they sort of around the table came up with the idea, but Bush writes this paper about it. And then they kind of do it under the Rad Lab and everything.

So I think there's an argument here that all you really need is sort of seven people, six or-- I'm going to say a small group of people who have the connections and sort of ability to make transformations with connections with large research endowments, the top facilities, the top minds, and then have access to just the kind of drop-of-a-hat call, 70 scientists, and they'll all sort of show up the next day in a room and then say that this is the direction we're going to chart.

And so I'd probably argue that, in addition to timing, kind of access to human capital and form probably allows these people to just make decisions and kind of chart, really, kind of drastically new directions for large institutions or large universities and even like the future of science and technology funding in the US and kind of make this model up. Because it didn't really exist

before they got there, but just get seven of them in a room, and now you have the Rad Lab kind of in two weeks. And then a day later, you have funding from Stimson.

So I think maybe if we're looking in the future to do this, a group of these people of this magnitude or caliber could meet together semi-regularly and come up with ideas for how to do that. And then maybe they don't necessarily every single time have to do something, because it's not as imminent as like Manhattan Project or the Rad Lab, but they probably have some pretty good ideas on how to reorganize things.

**WILLIAM
BONVILLIAN:**

So we'll dive into a lot of this thinking when we talk about great group theory. So Martina, I'm going to ask you to hold your question. I want to wrap up Loomis, take a quick break, and then we'll come back. Let me just close out with Loomis. What happens after the war? He's created these amazing institutions. He's played an absolutely critical role in both getting the Rad Lab created and in getting Los Alamos and then the Manhattan projects accelerated. It's quite a story.

We just talked about the ideology. He's an adherent of conservatism. He doesn't believe in a strong federal-dominating kind of role in R&D. So he and Vannevar Bush literally, as soon as the war ends, dismantle the Rad Lab. So MIT is left with this weird wooden building. And a fair amount of that is Building 20. Loomis divorces his wife, marries the wife of his research director for Tuxedo Park. They live in a Mies van der Rohe-like glass house on the site of Tuxedo Park.

Loomis also happens to own Hilton Head. So they spent a nice time down there. He just quietly fades. He kind of quietly, kind of after this amazing activist role, and he faces a certain amount of social ostracism because of this divorce, which was considered pretty scandalous in that era, and it affects him. So he kind of withdraws from society in some interesting kind of ways. But he kind of does this fade out in places like his Mies van der Rohe glass house and on Hilton head.

And the Rad Lab itself, Building 20, becomes very famous at MIT, as those of you engineers will understand this, but this is anything-goes space. And it's an engineering dream. When the returning vets come back World War II, this is thrown into a dormitory. A lot of returning vets had learned gun play during the course of the war. There's an old shooting gallery in the basement. The stories are wild.

It was made from Hurricane of 1936 wood that crashed all over New Hampshire, and they just

trucked it down. This lasts for another more than a half century at MIT. And it's like the wide-open experiment. You can blow the place up, anything goes kind of space, which is great in science. I mean, you really want some of that space where you can do anything and don't worry about blowing the building up and knocking it down. So it becomes this great creative center at MIT, and lots of stuff happens there, not quite related to science, but Noam Chomsky invents the whole field of linguistics in Building 20.

AUDIENCE: Does he really?

WILLIAM Huh?

BONVILLIAN:

AUDIENCE: Does he really?

WILLIAM Yeah. Really. It's true. In this building. So it's wild wide-open space. So when Frank Gehry is

BONVILLIAN: brought in to tear this down and put the Stata Center in its place, he tries to preserve some of that funky wide-open cement open space culture to sponsor its experimentalist feeling, which I think he actually does fairly successfully. And indeed, in those wild CSAIL laboratories behind glass, pretty much anything goes up there. It's a pretty interesting space.