WILLIAM BONVILLIAN: Tonight is a deep dive into DARPA, which is a very different model. And we've talked around DARPA, but-- we talked about snippets of some of the stuff that we're going to be going through, but tonight will be kind of a deep dive into looking this model.

And the model is an important one because we've gotten so much out of DARPA. It's been a remarkable run. And so we'll look at the foundational period. And there are a lot of good MIT stories in here, which we'll get on the table. But we'll also try to frame really how this organization, institution functions.

So this is a JCR Licklider story-- one of MIT's all-time greats. And he doesn't get talked about that much at MIT, but he really is foundational. And he comes in at a time when computing is really completely different than it is today, and is able, with his team at DARPA, to really work through-- really in an intergenerational between DARPA program manager and office director generations-- really rework the entire sector.

So we've talked a little about Whirlwind and SAGE earlier. And just to remind you, this was one of the foundational early computer systems. This was MIT's at Whirlwind and SAGE. And this is Jay Forrester, and he's holding the magnetic core memory that he and one of his graduate students came up with, which enabled real-time computing, a really critical advance. And you start to see at Whirlwind, in the upper-left-- which merges into the SAGE system-- you start to see where computing is going.

So that picture of this computer operator, this lady sitting in front of a keyboard in front of a cathode ray screen. That's this, and there it is. There it is at late '40s, early '50s when this is going on. And how did this all evolve?

So on the lower left is George Valley, who was an MIT faculty member. He was part of the Rad Lab. And Valley is on a scientific advisory board to the Air Force, and he sounds the alarm, essentially alerting the Air Force and frankly, the military in general, that the Soviet Union has developed a long-range bombing aircraft. They have nuclear weapons, and they have, therefore, first strike capability.

There is absolutely nothing in their way of reaching the northern part of the United States. And that's a terrifying realization moment. So Valley gets the Air Force mobilized around an air defense mission. And computers are going to be at the heart of it.

And he goes to Jay Forrester, who was developing this Whirlwind system for the Navy. The Navy's is canceling Whirlwind, so Valley just moves Forrester and the team, and the Whirlwind system over to the SAGE project of developing an air defense and controllers.

And the picture here is what-- essentially what the picture looks like of all these different defense systems, from early missiles, and artillery, and fighter aircraft, all being linked into a communications and computer system. And then fascinatingly, the communications lines are across phone lines. So the computers are running a communication system that comes into them across phone lines.

The picture of those Air Force airmen sitting in front of cathode ray screens, they're holding this peculiar thing. It's a light gun. It's the mouse. So you start to see these iconic pictures of where all this stuff that we just live with-- where it all starts-- come from.

And a lot of it came from the Whirlwind and SAGE systems. MIT doesn't want to go into the business of manufacturing computers. They make the initial computers for the SAGE system, but they don't want to be in the computer manufacturing business. So the SAGE system becomes the IBM 700 series. This is how IBM really starts to scale up in mainframe computing, and starts building the computing systems for this air defense system.

So again, it's an example of long-term patient military support for technology development that we've seen before. And here it is again, only this time, it's sponsoring computing. So Mitch Waldrop's book *The Dream Machine* I think is really the classic on the evolution of the Information Age.

It is full of wonderful stories, and I feel that it's really well written, and I couldn't resist giving you a big dose of it. But there's a lot of great MIT stories in here, but lots of great stories about the foundation of where computing came from and how it arrived-- that fits with the material that we've been covering in class. So we'll go through some of this key stuff, and then stuff will lead us in some Q and A.

Computing before Licklider, it's non-interactive, it's not fun, it's not exciting. It's behind glass walls, and you'd be met by like these high priests and priestesses who are wearing white coats and gloves. And you'd carry your stack of punch cards in, and you'd hand over this offering of punch cards. Maybe the high priest or priestess paid attention to you, and took the cards from you in a few minutes. Maybe that happened.

And then they would tell you, fine. You're 479th on line, come back in a week and we'll have run this for you. This is not real-time computing. That's literally the way was like. It was just very forbidding. It was very distant. It's definitely not what we have now. And it's built for a number of processing bureaucracies, and initially in government, but then in business. And that's not to knock mainframes. They're a huge remarkable technical advance.

IBM starts to dominate that sector, and they incorporate the machinery that they're familiar with. They're building business machines that are punch card-run with batch processing, so they carry those technologies into the mainframe computers that they're building.

Now, look, you come back after a week, and you get handed a stack of this weird green and white paper with holes punched in the side. You probably don't have any memory of this, but I remember it deeply. And you'd get handed this stack of paper, and then you'd go back to your office and you sit down and start go through. And then you realize that, after page 39, it's all gibberish, because one of your punch cards was out of order or upside-down, or something like that.

So then you'd have to go back and stand in line with the high priest or priestess, and start the whole process all over again. That's really what it's like. And the emphasis was on bigger and bigger computers able to handle more and more large number batch processing.

So Licklider comes along more than a decade before personal computing and 30 years before the internet. And the word computer still has this kind of ominous tone of a intimidating device that's kind of hidden away in this over-lit, super air conditioned, noisy, fan-ridden space. And it's about as far away from this as we can imagine.

And this is really what that team at DARPA, that Licklider led, drives us to. So we'll talk about the film in a bit, but a lot of the leadership of that team goes on to do amazing stuff. So here's some Licklider photographs. This is DEC's PDP-8 that was the first mini-computer, an entire movement away from mainframe computing that really was key to spawning the revolution.

So Licklider is playing with this machine. It came from Digital Equipment Corporation, which is an MIT spin-off, Lincoln Labs spin-off. And they're trying to start to pursue a more personalized kind of computing with these mini-computers. On the lower left is the team that does the internet.

And the only book that Licklider wrote is called libraries of the future, and he essentially envisions digital storage of everything. It's amazing work. The librarians are trying to wonder what to do with computers, and he essentially gives them the entire picture. So a very innovative character.

He grows up in St. Louis. Both his parents are ministers, and he's an only child. Responsible kid, but his parents let him do things like assemble automobiles in the living room. For most of his life, he never spent more than $50 on a car because he would essentially by junkers and parts and put them together himself. That's the way the family drove around.

So he has a tremendous facility for-- remember we talked about mind in hand, when we talked about the Industrial Revolution. Licklider has got that tactile knowledge facility, and it's part of why he loves computers. He falls in love with psychology and becomes actually quite a significant early thinker about psychology, when that whole field is starting to change.

His World War II experience is spent up at Memorial Hall at Harvard in the basement with acoustics researchers, studying how pilots and the crew of airplanes-- how they react to this unbelievable noise that they've got in the pre-pressurized cabins in World War II aircraft with these huge propeller engines next to their ears, trying to hear radio signals.

And this is before there's any kind of navigation system. It's pre-LORAN. It's obviously long before GPS. So there's a terrible problem-- the communication systems and the way in which you navigate a lot-- typically, if you can't see the stars. So all kinds of plans get lost over oceans because the radio communications don't get picked up.

So he's working on how can we improve the man machine interface around acoustics in the cabin of airplanes? So he's thinking a lot about how people and machines work together. And that's what he contributes. He's not a computer-- early computer scientists really, although he loves computing. He plays with them all the time, knows them backwards and forwards.

He's bringing the ideas of a psychologist to computing, and that's what, in many ways, makes him a complete breakthrough thinker. He's able to take ideas from psychology and bring them into this early computing field, not just an engineering perspective-- although he has very good engineering and physics capability.

And he writes an absolutely seminal article in 1960 that essentially was still working through his agenda of what needs to happen. It's an amazing vision. He, for example, realizes that 85% of human time is spent getting into a position to be able to think about a problem-- arranging all the stuff, pulling all the material together, and then you can think.

Suppose you could reduce that 85% by having the computer do it for you, by having the computer arrange a lot of that stuff for you. That's part of his concept. The central concept he has is the hope is that human brains and computing machines will be coupled together very tightly, and that the resulting partnership will think as no human is ever thought and process data in a way not approached by the information handling machines we have today.

In other words, he has this vision of people and machines in this collaborative relationship. It's going to be a symbiotic kind of relationship. And let's think for a minute about how people thought about computing. Now, we're all sitting around wondering when the robots are going to take our jobs. That's the obsession of the moment.

Licklider's coming out of a time period where there was a similar obsession. And Norbert Wiener-- who pops up in this book, and is another one of MIT's absolute greats, a brilliant mathematician-- Wiener is the one who invents the term cybernetics. And Wiener has this vision saying, look out-- and he writes in the late '40s-- the computers are coming, and they're going to be smarter than we are.

It's a very dark picture of what computing is going to be. And you all seen *2001*-- the Hal computer in *2001, Space Odyssey?*

AUDIENCE: I know of it.

WILLIAM BONVILLIAN: You know of it. Well, shows my age. But that's the kind of vision, this dark vision of computers taking over our lives. And that was alive and well as early computing started to take off. And actually one of the conceptualizers who got people worrying about computing was Norbert Wiener. But along comes Licklider with his background in psychology, and he has a completely different vision. And his vision wins, thank heavens.

So he understands that things like human perception, and goal-setting, and judgment, and insight, and intuition-- machines can't do that, that these are incredible human attributes. And the machine's just not can be able to equal us on that stuff, that certain kind of feel we have about how things fall together and shape up in place.

So those qualities are going to surpass machines, but then machines are going to be able to do stuff that we can't do, that we're not particularly good at, and don't want to spend a lot of time on. So there's going to be a relationship. So Licklider is spending a lot of time at Lincoln Lab, playing on computers up at Lincoln Lab. He starts to master all those early machines, including the DEC PDP-8, the first mini-computer-- real mini-computer.

He leads the MIT Psychology Department, and joins BBN, which is the premier acoustics consulting firm. And they do things like design the acoustics for Lincoln Center, the famous acoustics firm. And they had this idea, gee, maybe we ought to get in the computing business. Maybe these computers are actually here to stay.

So Licklider is hired at BBN to bring them into the early computing age. And that's what he does. In this 1960 article, his key image is a fig tree pollinated by a wasp-- in other words, the relationship between the fig tree and the wasp is complete symbiotic, they're mutually supporting. And that's the vision that he has of what computing is going to be.

So it's this partnership between computers and people with a complementary set of functions. And the partnership has to be in real time. We have to get rid of all this batch processing stuff. That has to go. It's got to be real time. The computer is to relieve people of that drudgery factor, the 85% of the set up time we spend getting ready to think.

It is a time of what is being talked about as time-sharing. These big massive mainframes behind glass with the priests and priestesses dressed in white lab coats, people don't get access to them. It's like going to the Oracle of Delphi. You just can't get in there. And he has a vision of could we, in effect, take that computer back from the Oracle of Delphi and get people on this thing.

And how are we going to do that with mainframes? We're going to do it through what's called, at the time, time-sharing. Let people have access to a share of time. And he has a vision of central computers, and then-- as a competing center with interconnected users tying into that central computing system.

He understands things like building a network of people aided by computers that become in effect a network of thinking centers. And that network concept really is a core idea that is what his team at DARPA later establishes BBN as the internet. He understands that artificial intelligence isn't an analogous way of thinking about thinking-- that it can help us understand intellectual processes.

He has a vision of a computer being a desk console, not a mainframe-- in other words, radically smaller than even the mini-computer that he's starting to play with from DEC. He just has a series of these concepts in this 1960 article that lays out what's going to be happening.

He envisions interactive computing. He envisions programming is not just programming. He starts to understand that this is a science-- in a way, a linguistic kind of science in itself, and is deserving full respect-- that computer science is a true science like other sciences, that it's not just a new tool set. It's something organized around one of the fundamentals.

Information is one of the fundamentals, one of the foundational building blocks of the universe. And you're organizing a whole field around information. Computing is going to be a science, and he recognizes that very, very early on, long before others. It'll have practical applications, but it's worthy of a whole intellectual field on its own.

And he sees how closely tied computing can be to cognitive science. So he has a whole vision of what he calls dynamic modeling. In other words, this is an era where you get statistics on paper. But he has the vision that, if you put them on your computer, you can start to play with each element and begin to turn them into graphs and designs, and get a much more realistic feel for how the data is related, and start to play with the different variables.

And you're going to be able to feed hundreds and thousands of variables into your computer in a way that you can never even think of, when people are doing statistics by pencil and paper. So he has a whole vision of this whole-- what he calls dynamic modeling, the modeling role that computing effect plays in a very dramatic way now.

It's that computing can handle this problem of complexity because of its relentless ability to deal with thousands and now millions of variables. So economics is a profession that was organized around essentially you couldn't really handle much more than three, four, five variables operating at the same time. Computing would enable science to cope with a profound issue like climate change, and actually start to be able to model that.

So he understands all this stuff, and he starts to lay this out piece after piece after piece for people to think about. I mentioned earlier the electronic library. He understands that all information is going to be online and stored online, and is going to be accessible to everybody who can get onto one of these things.

And it will be the library and it will be the reference librarian for you. In other words, it's going to be able to organize your information in ways that could never have been previously anticipated. 15 years before the PC and 30 years before the internet, he's seeing graphics, computing graphics, computer rich personal workstations, human computer symbiosis, interactive computing, networks, the idea of an online community, a thinking community, online libraries, instant retrieval, data organization, computing language, programming that's elevated from a tool set to a true science-- a whole digital medium of expression.

So he is a visionary. He has this vision. He starts writing about the intergalactic network. It's the worldwide web decades before Tim Berners-Lee here pulls it off. He's a visionary, and of a remarkable degree. And there's no vision champion at the time, but then DARPA hires him out of BBN to lead essentially this whole development of personal computing. So it's a completely rare moment where a visionary becomes the vision enabler.

What happens here? So 1957 is the Shock of Sputnik. So DARPA gets created. DARPA's originally given the space mission, because. Eisenhower's angry at the services because they weren't talking to each other, and Russians got the first satellite up. And it wasn't about a nice blinking thing, it was about first strike ballistic missile capability. It was very serious.

But then he took space away and gave it to NASA, so DARPA had to start thinking of things to do. And they come up with a lot of creative stuff, but the big early DARPA set of accomplishments is really Licklider and his team. And so Jack Kennedy and Secretary of Defense Robert McNamara go through the Cuban Missile Crisis, which is a complete nightmare.

And we do things like get within 20 minutes of launching a first strike by mistake. And they realize that they've got something that they call a really serious command and control problem. And Jack Ruina, who's head of DARPA, who's another MIT faculty member, hires Licklider to come in and deal with the command and control problem. And Licklider's response to Jack Ruina is, wait a minute, I'm working on computing and making computing personal. What do I know about the command and control problem?

But they have a conversation, and Ruina starts to see that what Licklider is working on is exactly what he is concerned about the command and control problem. With all these capabilities that Licklider is thinking about, they need to tackle this command and control problem. So Licklider comes to DARPA, and because he's working on what DARPA calls a presidential-- in other words, this is the problem the president himself-- President Kennedy himself has posed-- fix the command and control problem.

He has resources. So it's a very powerful moment. Ruina hadn't really thought about the linkage between behavior and computing and the kind of way that Licklider has really thought through, but begins to see what the possibilities are. So this is really when DARPA becomes DARPA. Some of the culture of DARPA-- we've talked a bit about they-- I'll just summarize some of the rules sets-- hire great program managers that are world-class, give them an enormous amount of power.

They have the authority at DARPA. Unleash them. Let them develop a vision, and then find the best people in the country to do it, and they're on the problem. That is not the way R&D organizations are organized in the United States. That's not the way we do it. We have NSF. It has a program manager. The program manager is overseen by a peer review system.

They wait for applications to come in from the scientists saying what they'd like to do. They review the applications. The peer review team makes a decision on who gets funded. The project manager is essentially doing the arranging here, but peer review makes the core decision. It's a much more bureaucratic process, and it's a bottom-up system.

Look, there's a lot of strengths in that system. I don't want to knock that system. It's an important function to have for science so that scientists themselves can originate ideas. That's not what DARPA does. DARPA has strong program managers. They develop the ideas, and they go out and make them happen. And program managers don't last much more than four years at DARPA-- three to five years-- so they've got that period of time to get done with their project. They've got to get something happening within a relatively short period of time.

So that's going to affect what they try to do. They're not working on 25-year projects at DARPA. They're working on stuff that can be stood up in a relatively short period of time. And it's just a very different organizational model for a science organization-- no peer review, two sign offs. If you're a program manager, you figure out what you want to do, what direction you want to go to. You sell that to your office director and to the director, and that's it. And it could be funded that day. Can move very, very fast.

So they're organized around this whole grand challenge concept. DARPA doesn't want to do incremental advance. It only wants to do technology revolutionary breakthroughs that will change everything. So it doesn't want to waste-- let other people do the incremental stuff. That's not DARPA's job. And it's a right left model, so it wants to look at what's going to come out of the pipeline, what the technology is that it wants-- personal computing-- and then go back into the pipeline and figure out the research that's going to get this outcome, again, within the life of the program manager-- in this, say, four or five-year period.

So it's just it's really different. Licklider pulls together different teams. So the other thing about DARPA is that essentially the role of the program manager is to find great groups and to support them. And that's what Licklider does. So DARPA's different because it's working as an institutional entity in the R&D space. It's another institutional R&D entity, but it is also organized around personal face-to-face great group creation.

So it takes the two sides of the equation that we treated as two entirely different classes-- it is putting them together. That's why DARPA follows after our discussion in the great group class. That's who Licklider is finding. He's finding these amazing, great groups. And we've seen some of them before. These are all who's who of the history of computer science.

We talked about Doug Engelbart. There he is. He's someone that Lick finds and supports, even though the Stanford Research Institute doesn't know what he's doing. They don't know what he's doing there. He does this you know unbelievable demonstration with all this breakthrough material.

At MIT, project MAC is right there at Technology Square. You can kind of recognize that building on the lower left. So after Licklider leaves DARPA, he goes to join Project MAC. But meanwhile, he gets stood up by Ferdinand Corbato and Bob Fano. The history of what comes out of that place-- and it's all DARPA-funded-- is just breathtaking. So when you go buy that building at Technology Square-- I think it's 500 Technology Square-- take off your hat, because amazing stuff. Absolutely amazing stuff occurred there.

Within MIT, there's this fascinating conflict. There are two competing visions of computer science fighting for the same DARPA money. So Marvin Minsky, great roboticist-- great, one of the all-time greats-- just died recently. Amazing figure. Minsky had this vision that computer thinking would be superior to people's thinking.

So he has the Artificial Intelligence Laboratory and he's running that vision, and then Licklider and Corbato and Fano are running the computer is our friend, it's going to be a symbiotic relationship, we're going to do personal computing-- very different ideas battling each other for funding. It took years of negotiation to put together CSAIL because you had these two different visions that competed with each other.

Just like we talked about the competition over the genome last week in the great groups session, this was one of those great competitions. Each side is pushing each other. And Minsky and team would do unbelievable robotics stuff and they would say, top this. And the MAC team would come back with the responses. So it was a wonderful competition going on here.

But creating CSAIL required two co-directors at the outset-- somebody from computer science and somebody from artificial intelligence. It's kind of settled down. They're all on the same project now. But that was a 20-year or longer project to get there.

So it's a remarkable set of communities, including project MAC at MIT, but that's just one of them that gets created. So all the early computer science departments are funded by DARPA. DARPA really creates them. That's why there is computer science that's so deep and rich in the United States. Licklider's three research priorities at DARPA are time-sharing-- which is really interactive computing, and that's like personal computing-- and then graphics and modeling-- and another great name, Ivan Sullivan ran that piece, and that's early computing graphics-- and then networking.

And Bob Taylor, who we talked about last week, who goes to Xerox PARC, comes out of DARPA to do that. And that's where he gets his stuff. And he and Larry Roberts, as well, are the ones that lead the effort to stand up computing. And Luyao told us about him last week. We sent around his obit, because he just died this past week. It's a wonderful obit-- if you all get a chance to read it-- that appeared in *New York Times.*

The ARPANET gets created. So Licklider creates this thing at DARPA. He has two different stints at DARPA. He's there for several years in each stint. But he creates a community that keeps carrying on the projects. In other words, they agree on what the agenda is, and they keep working down the list together. So DARPA doesn't-- isn't always able to do this.

Often there's a real problem-- when a program manager leaves after their three to five-year stint, what happens to their work? Who does the follow one? But Licklider builds this community that keeps the project going for way over a decade and a half. And Taylor and Larry Roberts, out of Lincoln Lab, really lead the effort to do the internet. They hire BBN, Licklider's old firm, to actually carry it out.

And that's done right here in Cambridge. So the internet gets done just up the road. I won't go into the whole story about it, but it's-- two great things occur in 1969-- we go to the Moon and we do the internet. And in 1969, nobody had ever heard of the internet. But in the end, which is the more significant? And that's going on just up the street here.

It's a pretty breathtaking moment. These are some of the figures. This is Vint Cerf and Bob Kahn, who were-- lead the whole protocol effort-- TCPIP, the whole internet protocol system that's really at the heart of the internet system. Larry Roberts, who came out of Lincoln Lab, persuades the DARPA director to use email. So DARPA's like an early email user. So everybody in DARPA goes on email. They nurture this thing along.

They're one of the early user bases. It becomes this system of communication, because DARPA's using it for the entire ARPANET community. It's originally stood up with about five universities linked. The real problem they've got is that, because DARPA's trying to stand up these early computer science departments, and lots of breakthroughs are happening in computing, DARPA's having to spend a fortune buying new computers for each one of its university research centers.

And it's costing them a fortune. They're spending their money on the next generation of computer advance. And one of the reasons they have to do the internet is that, gee, if they only had to buy one computer and everybody could get on it, it would be a gigantic money saver, and they'd be able to do much more stuff. So they have this internal reason.

Now, the external reason for the internet is much more complicated. One of the reasons is that it's going to be a secure communications system at the time of terrible threats. Now, you all are-- probably don't have much recollection of 9/11, but sure enough, the only way we knew about each other on 9/11 was because the internet still worked. The phone system in New York City went down in a flash.

And it worked. It turned out to be an enormously important fallback communications system, because it is so resilient. In other word, you can establish those network lines essentially through any connection, not just a straight one-to-one connection like the phone system. So that was part of the rationale for developing the internet, but frankly, Licklider had a vision of what the internet was going to be, which is really why they did it.

And then they had this excuse of trying to reduce their computer budget for buying fancy computers for all their computer science departments around the country at universities. So it's an amazing story. We heard the story from Luyao last week about how personal computing comes about. A lot of that community is being-- came right out of Licklider's support of university research groups, and then moves over to carry it out at Xerox PARC.

And then, of course, Xerox PARC is unable to do it, as you explained. But luckily, Steve Jobs buys the rights to walk through and see the stuff. And he's a great implementer. All right. Steph, what did I miss?

STEPH: The one point that I really emphasize to set up the questions is something that Bill touched on, which is that there was a very significant dispute between the scientists and engineers who were trying to create the integrated system versus the isolated, siloed systems. And that ends up playing a big role in, I think, how many of you perceived the readings and the kinds of questions that you asked.

I think the place where I'd like to start our conversation is about the mechanisms by which the computing revolution was created. We have a question from Beth, and she poses, how much of a technological revolution is the scientific breakthrough versus the vision of how to use it? So I think that really gets at the heart of whether or not a technology can be disruptive or revolutionary versus whether the implementation is what actually matters, as you just noted with Steve Jobs.

AUDIENCE: Just to clarify, when you vision, do you mean JCR's vision, or do you mean the visionary who comes after, which is Steve Jobs? Which vision are you talking about?

STEPH: Well, I think we'll let Beth clarify.

BETH: I think it is kind of a cumulative process. The reason that Steve Jobs was able to have a vision was because JCR had his vision. So it's kind of like at each step in the road, the reason we were able to progress, was it because of the vision, or was it because of technology, or was it this guy was at the right place at the right time? How else could things have progressed, if it had been under different circumstance?

STEPH: So how much weight, I guess, are we giving the invention, the mechanism by which this was achieved on its own?

AUDIENCE: Well, I think a good way to think about it is-- you know ancient dynasties? There's one that was-- took a while and didn't do anything that was that significant. But they built up the infrastructure so that a dynasty comes in right after, and they end up doing all these awesome things, and everyone talks about that one. So I think it's kind of like Steve Jobs, Bill Gates have a voice because, again, billionaires-- everyone is like, oh.

There's a saying where, I wish I was rich so then people would listen to me. So people pay attention because they became wealthy. And they kind of popularized the idea of the engineer, and they gave a lot of respect to the engineer, because now they make money. But in the past, like in the '70s, there's a couple series and a couple of biographies where there's really, really brilliant people that might be-- I would even say more brilliant than some people today-- in those technologies.

But they just weren't valued economically or socially. In the '70s, being a nerd was being a nerd. Nowadays, being a nerd is like, damn, he might be a billionaire-- I'm going to be nice to him. Now it's cool to be a nerd-- to be smart.

STEPH: I think we'll hit that point again in a second, so keep that thought process.

RASHEED: I think Steve and Bill Gates had a vision socially like, we're going to make being a nerd cool again because we can't make a whole bunch of money. You think they had that vision when they were being nerds and making money, that we're going to make-- basically being a nerd cool because [INAUDIBLE]

AUDIENCE: What I'm saying is-- so it's the difference between-- like somebody gets a car-- somebody has a Rolls Royce and somebody has a Toyota. This first wave--

WILLIAM BONVILLIAN: Licklider would have a junker.

AUDIENCE: Hm?

WILLIAM BONVILLIAN: Licklider would have a junker--

AUDIENCE: Yeah.

WILLIAM BONVILLIAN: --that he personally would build.

AUDIENCE: Well, what I'm trying to say is that it's an item that doesn't touch the hands of a lot of people, so it's not valued, versus-- the personal computer was a big thing because everyone could interact with it and would be able to communicate with it. These big computers, it was only if you were in the institution, if you were probably a grad student or more brilliant-- that you've got to interact with the computer.

And what they really did was just make it common for everybody to use. Really that's what the entrepreneur-- the Gates, Ford-- is somebody who takes a complicated technology and simplifies it to the point where [INAUDIBLE] a large subset of consumers, if not all consumers. Any other questions?

STEPH: Sorry, I'm comfortable with silence, so I like it when people think.

AUDIENCE: What was interesting about this though, that I connected, is when you think about startups or any-- I guess vision organization, it always starts-- Reid Hoffman talks about this, which is you want to have a key insight about society. So not all people know this, but Mark Zuckerberg was a psychology major. He just happened to take computer science classes, and then would say that he studied computer science because it was easier when he got into the marketplace and he had to get investors.

JCR was psychology, so he had a main insight into how the human brain worked, and also a secret about human nature-- that it seemed that he used that to his advantage in order to create computer systems, which I thought was really, really interesting.

RASHEED: Yeah, I think I'm going to echo Martin's point here. I think a lot of this kind of visionary insight was built off of a lot of things that he did. So he had all this tactile knowledge, skill to build cars and things like that, but as you can see, probably used that psychology background a lot more when he was trying to figure out how are we going to put together and really move personal computing forward. And I think another understated point here was, in this great groups model, I think he and Licklider definitely not only underscores, but kind of creates that great groups model for DARPA.

But not only bringing it together and finding these people, but-- I think you mentioned like a lot of these projects weren't on the normal three to five years. But he took maybe four or five different collaborators and said, we're going to build out 10 years, 15 years, and then just stay working on the same projects and solve all these five different problems so he could move the whole field forward, instead of just focusing on one or two problems.

In this great groups model, he creates great groups within DARPA, and then that leadership, as well, and moves that forward. And so he's obviously using his psychology insight and research maybe a little bit more than his computing aptitude.

AUDIENCE: I think [INAUDIBLE] I remember in the reading we talked about the cool think about being an entrepreneur in DARPA is that you get to be an entrepreneur in multiple things at once. So if you're an entrepreneur, you only have your business and you can only really focus on that. But an entrepreneur in DARPA who leads a vision, gets to touch multiple products at once-- you can see that with JCR, where do you have the three sections.

But what I find is interesting is why isn't he more famous? Because this is the first time I heard of him, honestly. He probably should get more credit than a jobs or Bill Gates. And even in the papers about Bill Gates, he was like, well, yeah, we really did this revolution in the '70s. And it's like, you did the last 2%. So it was pretty interesting.

WILLIAM BONVILLIAN: He is an absolutely remarkable figure. And the core was this vision that he had, and that-- and this image of a crossover. Often, great technologies are crossover technologies. When Jobs creates the iPod, he's taking an MP3 player of high quality and certain new attributes, but he's joining with something entirely different-- a remarkable way of organizing the music industry.

And it's two pieces that get joined together, and that's at the heart, I think, of what Licklider is able to do. He's able to take his capability at computing and his engineering and computing capabilities, but he crosses it with his whole background and set of understandings that he comes to with psychology. And it's that crossover that makes him an absolutely unique figure.

So when you all are thinking about how to do great stuff, just keep that crossover idea. Bringing something from here and moving it over here is really at the heart of a tremendous amount of creativity. It may be the definition of creativity. It's what he pulls off. By the time Licklider finishes his second stint-- which he's not as happy at DARPA the second time around.

He has a director that doesn't get along with as well as he did with Jack Ruina and another famous DARPA director, named Heilmeier-- Heilmeier, excuse me. That team of has worked their way through those three critical projects. They've done the internet by '69. They've done desktop computing, carried out with some DARPA support by largely ex-DARPA funded folks at Xerox PARC.

And then they've done computer graphics, and a lot of that moves over to Xerox PARC, as well, and then from there to Apple. But they've worked their way through those pieces. Let me just mention the film here briefly. Rasheed, you can-- we'll get to this in a second, although we'll let Steph do a couple more questions. The film was this desperate attempt by these people who have created the internet.

And you saw them. This is an amazing collection. A lot of on the film. Larry Roberts is there; and Frank Hart, who led the BBN team; and Bob Kahn, who's on the BBN team, then goes to DARPA and works with Vint Cerf on the TCPIP internet protocol system; and Paul Baran who has a vision for what-- how to put together the whole packet switching system to begin with, that's at the core of the internet. All these famous internet pioneers are all put together on the film, and they're desperate.

They are desperate because the internet has only gone to about 50 different university computer science departments. And they realize that they've got this earth-shattering communication system, and they're trying to explain desperately to-- on a 16-millimeter film what they've accomplished. I think it's '72 they do the film. It's not until really 1993, 1994 that the internet completely scales up.

So it's a long hiatus. And NSF actually plays a very, very important role in the scale up, because it gets turned over by DARPA and NSF-- and NSF under Erich Bloch, who is the only NSF director who came out of industry. So Erich Bloch was one of the leaders of the IBM 360 project and was a winner of the president's Medal of Technology, but he was an IBM officially. Wasn't a scientist.

So he gets leadership of NSF, and he creates NSF's very strong computing organization, pulls one of the leaders of deck to run it. And they treat computing as a full science. That's another moment in Licklider's vision is when NSF starts to treat computer science as a true science, not just a nice tool for other scientists to play with. So that's a pretty critical moment too.

And Erich Bloch and some others understand the power of the internet as a communication system in science, and then they spread it to universities all over the place. That creates a fabulous early audience, and then it scales. So that's kind of what plays out there. But it's a remarkable-- another remarkable is part of the story here. Steph, let's go to a question or two more.

STEPH: Great. I have precisely two more questions, the first is with a sentiment that was shared in the last couple of pages in the reading. And I'll read a quote now. It says, "Technology isn't destiny, no matter how inexorable its evolution may seem. The way its capabilities are used is much a matter of cultural choice and historical accident as it's politics or fashion."

And I think back to maybe the first or the second course where we began to reflect about the role of culture and technology transfer and technology adoption, and in thinking about the sort of functionalist ethics that we might have-- this is by Amartya Sen-- he's a very famous economists who came out of India in developmental economics-- and thinking about the ways in which we're evaluating vision. And it seems-- and I have a couple of notes here from which I will read-- that many of the people who we have truly regarded as innovators and as transformative members of society have created things that are effectively public goods.

They've really approached technology and engineering from a utility ethics framework-- not so much about having that bias to commercialization, like maybe Steve Jobs did, but it was the Lickliders and the Edisons who really thought about how they could create a product that was going to transform society for everyone. It wasn't going to be a product that was going to be accessible only to the few.

And so in thinking about our ethics of commercialization today, perhaps I thought it would be worth it to become a little philosophical, in the sense that Licklider was very philosophical about the role that technology played in the development of society-- and ask ourselves about the ways in which our commercialization bias may be precluding us from creating the so-called disruptive inventions that we still wish to pursue.

RASHEED: So I think this is a cool question because you get to do both-- bring in, just like you said, a lot of the cultural aspects that might be overlooked when you have this commercialization bias. But I'm not sure-- or in this context, I think Licklider's got a head up because the field is wide open. And so nobody's really thought about personal computing before, so he gets to really define the field and make it general enough so that it can be applicable everywhere.

But in every sense, you get the idea that-- time-sharing and all these things-- he's trying to make computing a lot more accessible. And so I think that's a really cool and seminal thought to make things more accessible, and that'll give you the innovative, transformative, and really disruptive ideals-- to bring this technology from one and spread it around.

And so the spreading effect, I'm not sure how to quantify it like Licklider did, but I'm interested in what projects that DARPA might be running now or might be looking at, to see if there's still those people within the organization that are looking at bringing technologies that might exist now that might just be [INAUDIBLE] or high risk, and spreading that to a more normative phase, maybe like Licklider did.

WILLIAM BONVILLIAN: It's an interesting point, Steph, that you raise, and Rasheed, that you comment on, because there's a very strong culture in computing, including a very strong culture in MIT computing, that computing should be free, and computing should be in the commons, and computing should serve us all. And that's actually a very strong ethos that, in a way-- and I think it's your point-- comes out of the way in which Licklider and that remarkable team evolved this technology.

Because this is not a for-profit operation. They are essentially trying to spring a transformative technology onto the world by building it in the commons, and creating it into the commons, and making it universally accessible. That's a remarkable project. That's not normally the way we do stuff in a capitalist country.

You've identified an interesting kind of strand here that's a different kind of technology development story. And it's a different story than the Edison story. It enables, Martin, as you point out, Gates and Jobs to get extremely rich. But all the foundational stuff was thrown into the commons long ahead of them, which was an enabler for them, as well. So interesting. Thanks for bringing the philosophy in.

STEPH: [INAUDIBLE] for what it's worth, I find-- or I personally think that doing good is not exclusive mutually with doing well for yourself and for other people. I think that there's a lot of value in thinking about the ways in which you can reach a wide audience and transform society, and at the same time, do well for yourself, and then return that value onto society.

But to what extent? The kinds of people who are capitalizing on the gains of the Lickliders is-- I'm not sure that they may all share my vision for the world.

AUDIENCE: I'll add a point to that. I think a lot of it matters-- it matters how you define success. What channel's creating wealth? Because you'd say, yeah, these innovators didn't become wealthy, or the US did become wealthy, Bill Gates and Steve Jobs became wealthy, and their employees. But at the same time, how much the net worth was increased in the US based on the economic output, based on these people, and also their legacy?

Because most likely in the future, we'll re-look into this and say, actually, these were the pioneers. And they get even more traction. Bob Metcalfe, who made the internet, made the original one. And then other people implemented it and made it better and better and better, so he's like, oh, it's awesome, because I get credit for all these brilliant things people did after me. So I wouldn't have been able to do any of this, but I get all this credit.

And I think that another sector that I could compare to, which is space. So NASA went very fast and used a lot of resources in order to go into space, and they really set the framework. I listened to an astronaut talk about it, and he'd said, our job is to widen the horizons, because it's going to be a while until it's commercially viable for other people to do.

But that's our job, to really go into the places where a capitalist can't go. But there is a net positive on our society because of it. And that's kind of the job of government, to do the stuff that individuals can't do for the net positive of the society. So I think there is a lot of-- there is a wealth creation there. They create a wealth of knowledge. They get respected and honored much higher maybe. So I think it really depends how you define it.

WILLIAM BONVILLIAN: How about a closing thought, Steph?

STEPH: Yeah, that sets us up for the closing thought, which is really about Licklider's legacy. I really appreciated that Bill had us read the component in which the author is having his wife Linda reflect on Lick and the ways in which he passed away, and the reaction from the computing community about not only his legacy, but the ways in which he had been such an incredible not only mentor, but champion-- which was a word that you used today maybe for the first time.

The kinds of mentors that one can have-- there's the people who will help you learn things, and then there's the people who will advocate for you. And Lick was really the champion of computing, not only for so many people, but for the field itself, with ARPA being his champion. And I think there's a lot to be said for finding those mentors, and I hope that we in this room have found those mentors in our own lives, or on the path to finding those people, and that MIT and our institutions generally help to connect us with the people who will help us make the revolutions that we want to see happen.

WILLIAM BONVILLIAN: Great, thank you. Rasheed, a couple of thoughts about the film clip?

RASHEED: Yeah. So I think the film clip is really a little bit weird to watch, because it was really putting a face, and a name, and a voice to all these people that we just read about. And they came out about as I expected, especially because I think the audience of the film was really trying to make the internet ideas and computing a lot more palatable for the general audience.

And so it was nice to see them kind of struggle with translating what they do on the daily basis in their research, which is highly technical, to general population. And I think it's why be needed time to ask, are these sort of efforts that DARPA should be doing normally-- to take these high, very technical, research-based-- you might need a PhD to understand what's going on on a project. And then really translating it for that public good of disseminating scientific information.

AUDIENCE: So to rephrase the question, you're asking basically should DARPA be engaging in high risk, high rewards?

WILLIAM BONVILLIAN: Little bit louder.

AUDIENCE: I'm trying to figure out-- so I was thinking, is the question basically that, should DARPA be engaging in relatively technical projects that might have really huge impacts to society?

RASHEED: I think more so, should DARPA, with each project that they take on-- should they be advocating to the general public as much as possible?

STEPH: Should there be a communication component--

AUDIENCE: OK.

STEPH: --to the research that they do, much like they'll be doing this weekend at the Science March or the Climate March next weekend.

AUDIENCE: Is DARPA going to be there?

STEPH: I don't know. Will they?

WILLIAM BONVILLIAN: It's an interesting point about DARPA, because in the IT revolution, DARPA is working to stand up that revolution essentially in the civilian economy, not in a closed-system defense economy. And there were conscious decisions that DARPA made there. So the military is often pretty good at getting that first prototype out, the initial breakthrough prototype.

What it doesn't really have the resources to do are the relentless series of follow-on incremental advances. And clearly, the IT revolution that we've been living through for the last 25 years is predominantly incremental advances, some of which can be obviously quite significant.

But the basic framework was kind of in place more than 20 years ago, and we'd been in effect building on that. And DARPA understood that the military would never have the resources-- that it can really use this personal computing system and the internet system-- they would be incredible tools to create a whole new organizational set of mechanisms for the military.

But it was going to stand it up in the civilian sector, and then the military could buy it back from the civilian sector after the civilian sector did all the incremental work and flooded the sector with investment. So for a long period of time, the military was the principal spender on computing and on software. It dominated as the big buyer in that sector for actually a fairly significant period of time.

That era is long gone. The military's control of these sectors is infinitesimally small now, because they've been stood up, and the military buys them back. So that's a different kind of way for DARPA to operate. In the next piece we're going to do in a second, we're going to talk about DARPA developing technologies within the military and how it operated in that way, which is, in some ways, quite challenging. Closing thought on the film? Question?

AUDIENCE: Just very short-- I do think there is stuff for DARPA to do, at the very least, in today's society. I worked on a DARPA-funded project. People I generally talked to-- oh yeah, DARPA is just a money sinkhole. They just have a ton of money. A lot of people don't necessarily see how a defense agency's research comes down to benefit them.

RASHEED: So as a closing thought, I think DARPA has the advantage where they can do this high risk, high reward. Under the guise of defense, I think they kind of get away with not being able to say-- or being able to be like, it's classified, or we don't really have to tell you under defense parameters or protocols, or the reasons why we want to look into these projects can be a little more complicated.

But I do see-- and in cases like this, where to have ARPANET, which obviously has great potential, if you do that type transfer and move it into the civilian sector, and then you get this

IT revolution that follows on after. s so I think it's important for DARPA to keep initiatives like this for maybe bigger projects that are a lot easier to stand up in civilian sectors.

WILLIAM BONVILLIAN: Yeah, an important point, Rasheed. DARPA gets a lot of slack because it's under that national security mission banner. So it's able to take the kind of risks and aim for a set of breakthroughs that's harder for a civilian agency to undertake. This is wild stuff. Think about coming up with this stuff in the 1960s. It's pretty amazing. And that national security justification was pretty key.

All right, so now we're going to look at a different side of DARPA. This is a chapter from our textbook. And there's a couple of topics here. DARPA's a very unique institutional model that operates at both the personal and institutional levels, as we talked about. And then secondly, DARPA's organizational rule set.

DARPA also creates lots of the foundational technologies behind what is called the revolution in military affairs. So here's the setting. Harold Brown And Bill Perry are running Jimmy Carter's Defense Department. So Harold Brown's Secretary of Defense. He's president of Caltech. And Perry is out of Stanford.

They are great technologists. Imagine the top leadership of the Defense Department and the hands of really hot-shot technologists. Doesn't usually happen. And they've got a big problem on their hands. The problem is the American army, in particular, has been completely demoralized by the Vietnam War, and has largely disintegrated. It is not, in many ways, a truly functioning force.

And the Cold War is heating up, and there's a monster presence of Soviet and Allied armies in Eastern Europe. And the problem that Brown and Perry have got is that, because the US Army is weak and perceived as weak, there's kind of nothing in the way of that Soviet army having the capability of moving right into Western Europe.

And that's not a good situation because then you have to decide whether to use nuclear weapons very quickly. You don't have a lot of time. You've got to decide whether you use nuclear weapons, which essentially means ending the world. It's not the kind of decision you want to have to make. So Perry and Brown started thinking about what they call an offset strategy. Can we start to restore the strength of the American military?

They're not going to do it with a mass army, and a citizen army, and building thousands of tanks, and adding millions to the military. That's unaffordable and they're just not-- it's not going to happen. And they'd be putting those people into a broken institution anyway. So the Army's in the process of restoring itself through leadership of people like Colin Powell, and a whole generation of new army leaders that really restore that operation in a remarkable kind of way. It restores itself.

But that is an underway project at that time. And what Perry and Brown start is a whole set of thinking about can we get a significant on-the-ground conventional warfare superiority so that the Soviet Union won't even think about coming through the Fulda Gap and moving on Western Europe.

And that's what they embark on, and that becomes DARPA's job. So DARPA develops precision strike, they develop stealth, and they develop UAVs or drones as core. And then they're also doing a lot of stuff with submarines that nobody ever talks about. So I don't know what that is. Something's going on there with submarines, undersea warfare.

But we know about the other three. We have a pretty good idea what those look like. And these are not technologies that the services want. The Air Force is dominated by pilots. They don't want drones. That's like the last thing they want. The Air Force wants airplanes that can actually fly. They don't want these stealth things that, if the computers weren't continually adjusting the exact movement of the aircraft, it would fall like a rock.

It's completely maneuverable. It's like flying a bus. No, really. These are not maneuverable. Nobody can see them, but it's still a bus. So the Air Force doesn't want this thing. But DARPA, in the case of stealth, working with Lockheed-- which does some fascinating translation of actually Russian scientific literature and develops the concept of stealth-- and then DARPA jumps on it with Lockheed and his Skunk Works-- works on developing stealth. It's working on precision strike.

Completely changes warfare. Completely changes warfare. In the Gulf War-- which was where all this stuff got shown and demonstrated, and that's where this phrase revolution in military affairs comes from-- the Russians saw this stuff. Revolution in military affairs is their term for what we pulled off.

They were stunned that these technologies created a whole new kind of capability on the ground for the US military that was just-- they had no defense against. We were essentially up against a Soviet-trained and style army in the Gulf War in Iraq, and their air defense system fell apart in a very short period of time.

They could not deal with stealth. They could not deal with drones. This continuous battlefield awareness, the airline battle doctrine that was enabled by these different technologies created an entirely different kind of military force. So DARPA's doing this stuff, but this stuff gets stood up within the military. It's hard to think what the civilian application-- we're now working away in civilian applications on drones, so that's transferring over.

That could be pretty interesting. It's already starting to get interesting. GPS is another part of that story that's already long since transferred over, that DARPA works on helping to nurture. Maybe stealth will transfer over, because if we do something resembling autonomous vehicles, you really want inverse stealth so that the radar systems can really see your vehicle, that they're not going to miss you in the noise. Cars could look a lot weirder if they go stealth, but-- inverse stealth, but that may be what we need to do in order to make these autonomous vehicle systems really operable.

AUDIENCE: Oh, you're saying autonomous cars.

WILLIAM BONVILLIAN: Yeah, autonomous cars. I'm sorry. Not drones-- cars.

AUDIENCE: I was thinking, why would you want your drones to be visible?

WILLIAM BONVILLIAN: Yeah, so you'd have a car that looks like the other side of stealth. The military is a legacy sector. It's been around since 1787. It's extremely resistant to change. It has its routines and doctrines, and it has its doctrines that govern how it fights and so forth. And then here are Perry, and Brown, and DARPA landing in the middle, forcing them to adopt completely disruptive technologies from what they've been doing.

So DARPA's operating here as a change agent within a legacy sector military. It's able to do the change agent role because of the island bridge model. Remember, we talked about this at Xerox PARC. DARPA's got a bridge to the Secretary of Defense, and the Secretary of Defense is able to make it stick with the military.

And that's a really key DARPA capability. That's also a lesson about how to bring change into legacy sectors. So DARPA rules of small and flexible, and it's a flat organization, and it allows a lot of freedom and autonomy outside bureaucratic impediments to its program managers.

And it has an incredibly talented technical staff, and it works off the challenge model. The end of a project is the end of the project. DARPA shuts things down. It just doesn't persevere forever. If it doesn't seem to be working, it'll close it down. It's got all those rules sets, but in addition, it's got some other capabilities that I think are important to understanding how to bring change into legacy sectors.

Form critical innovation institutions-- that's a lesson that we're seeing here. The Defense Department would not have done the revolution in military affairs if there wasn't a critical innovation system there to do the innovation, ie DARPA. So that's kind of a new rule for us to think about right. What's going to be that critical innovation system that can operate as a change agent? Because most of the economy is legacy sectors.

Point two, use this island-bridge model. It's really critical. Remember, Edison can call up JP Morgan and get JP Morgan to fund the electrical system. That island-bridge thing is absolutely critical. Get your innovation team on a protected island, make sure they have a route back. And DARPA's a great example of that.

Build a thinking community-- that's what the Licklider story is about. This is not just Licklider having a vision. He's able to create a whole thought community within DARPA and then by all the people that are working for DARPA, like Doug angle and that whole world out there. So they're able to scale up this community so that it reaches a certain kind of mass, and it can get a lot of stuff done and a lot of thinking done, and develop a lot and nurture a lot of ideas.

So when you're thinking about implementing your technologies, think about building that thinking community. And then another key piece here is linking the technologies to the operators. So if the technologists are only talking to themselves and they are not talking to the people actually operating the system, nothing will happen. So you've got to make sure, as you're working on bringing your technologies about, that you've got linkages to the actual operators.

So these are a few more lessons from DARPA. And look, I'm going to go ahead and do Glenn Fong here, because he's got some pieces about. And Max, you've got Glenn, right? OK. Glenn just kind of tells us what we talked about earlier, that so much of the IT revolution really came out of this DARPA-supported thinking community.

So Glenn argues that the Apple Lisa, and then its Mac really came from Xerox PARC's Alto, as we've discussed. And then in turn, Microsoft's Windows really came from Apple's Mac, which in turn, came back from Xerox PARC, which in turn came from this DARPA community. So the key staff from PARC go to Apple and to Microsoft as implementers on the pieces of the vision that they're pursuing.

And it's all Lick's kids. It's this community that Licklider starts to establish. So Bob Taylor, as Luyao led us through last week, he leads PARC. The DARPA-funded university research centers at places like MIT, and Berkeley, and Stanford, and Carnegie Mellon, and SRI, and Utah, they support and train a lot of that Xerox PARC staff. So by creating the early computer science departments and funding them, DARPA's training a whole thinking community here.

And this is just a list of famous computer scientists that got their support from DARPA. And this is who did it. This is that community. And it's a stunning set of realizations as to just how central DARPA is at the whole follow-on to the IT revolution. So Glenn Fong brings us to this set of realizations. Things like Ed Catmull at Lucas Films, it's a remarkable-- you see where the strands lead in off and all kinds of directions.

So you get a sense for just how-- what a great foundational institution this is. So let's do some questions and points about these two readings. And Rasheed, do you want to lead us off on that chapter from the textbook? And then we'll go to Max.

RASHEED: Yeah. So I think it's really important in this chapter. as it highlights the elements of DARPA that make it so fantastic and able to do all the things it's been able to do. So we talked about the island-bridge, and things like that-- but really the success of also the program managers acting as sort of mini change agents to be able to be that small and flexible. I think somebody asked actually a pretty good question, which was, is there a way to bring these elements of DARPA-- giving these dynamic program managers the freedom to do the things that they want to do-- is there a way to do that outside of this DARPA model, maybe enterprise to this model? I think Martin asked that question.

AUDIENCE: [INAUDIBLE]

RASHEED: Yeah.

AUDIENCE: OK. Yeah, I just thought it was really interesting, because in business, a lot of time, you talk about focusing on the alphas and getting rid of your betas. The stuff that you're really, really good at, focus on doing that, so if you're not good don't do it. So you try to outsource everything.

So it'd be an interesting model because you might move faster. And so that's what I was thinking about-- a way of people with good ideas that are very capable being linked with resources in a much better manner to maybe being able to do more.

AUDIENCE: So I just want to interject. I think you just described the MIT faculty. We can hire postdocs and students to do things that might not want to do, and they specialize.

AUDIENCE: The way I would think about it differently is-- that would mean they're very book smart. And like what kind of person would come into-- through MIT faculty versus like people that might be-- look at the people-- Apple-- Steve Jobs-- drop out-- Bill Gates-- drop out. You're definitely right, but I guess the way I was trying to say it is more--

AUDIENCE: You don't have to say that.

AUDIENCE: Well, you are right. I meant more of a spectrum of personality types.

AUDIENCE: Yeah, it's interesting, because I don't know-- you guys see the research that they do. A lot of these faculty that I come across have spin out companies. And no, they're not all becoming Bill Gates overnight, but they're all very much into their own specialty.

AUDIENCE: Well, the research model is a very productive model because you have somebody who has key insights but didn't have the time to go and make a company off of it, so you get somebody from-- who's one of their grad students.

AUDIENCE: Exactly.

AUDIENCE: [INAUDIBLE] the professor [INAUDIBLE]

AUDIENCE: Oh god, which one? Langer?

[INTERPOSING VOICES]

AUDIENCE: But also, research institutions try to focus more on-- they're called frog innovations, [INAUDIBLE] you move forward. And so I was thinking more of hawk innovations, which is a completely different field that no one's looking into, because it doesn't seem to be that productive.

AUDIENCE: OK. There are a bunch of frogs, is that what you're saying?

AUDIENCE: No, no. Just in general the type of research that goes through. Or it has to have a purpose that's already defined based on a current paradigm.

AUDIENCE: Yeah.

AUDIENCE: So it's very risky to do anything different because you're kind of ruining your thesis [INAUDIBLE]

AUDIENCE: Yeah. And have to get money from somebody to do it, and convince them it's going somewhere. Yeah, OK. Got it.

RASHEED: How does that fit in with what you were saying with the alphas and betas, where focus in on what you're good at [INAUDIBLE]

AUDIENCE: [INAUDIBLE]

AUDIENCE: I think the idea that he's trying to get at is that you focus on what you're good at, but in order to figure out what you're good at, you have to have some failures along the way. So you have to try and do different things, and then maybe you find out, oh, this idea's really amazing he. It'll make lots of money, and it-- and we happen to know what we're doing. But you might have a dozen things that don't go so well before that, so it's a process.

AUDIENCE: You might come up with the key insight, in terms of manufacturing, but you haven't done large-scale manufacturing. So how are you going to connect to a firm that already understands that really well so that you can execute quickly?

So you try to figure it out. But it'll take you a lot of capital, will take you two to three years and you're screwed. I was thinking more of like-- in software, you have Amazon Web Services, you have databases that are already huge that you can just go port and you're through. And it'd be interesting for that for like manufacturing or biology. Because also biology's super difficult, because say you want to do a startup [INAUDIBLE]

RASHEED: Yeah. So I guess obviously it's going to vary along different fields, but I think manufacturing might be an interesting one, or even biology and the intricacies there. But I think this brings up a larger point, which is an even bigger abstract question, but follows along with this idea of creating these change agents.

And so somebody asked, is there this sense that these change agents, like the ones that are found in DARPA-- is it important to create these change agents by giving you a whole bunch of money and letting do you do-- use your own discretion, as product managers, to figure out what you want to do, or to find these people that are doing these fringe research opportunities that might be high risk, and they might not just have the funding, and go from there? And so it's the do you want to create these change agents by giving it to the people that you think are capable, or do you want to go out and find these people who may not be proven in their field yet, or might not have research that is still at this high risk phase?

STEPH: Could I ask clarifying question? Do you mean in the context of DARPA, or do you mean generally?

RASHEED: I would say generally, because DARPA-- you have a model that's established already.

STEPH: OK. The reason I ask that is because in one of the readings for today-- I believe it was the-- it might have been the Fong reading actually-- there's a quote from that technologist who said that he wanted the government to not touch engineers at all. He said, "If the US wants [INAUDIBLE] jobs, better lives, and a stronger economy, the best thing lobbyist bureaucrats and politicians can do is leave us alone."

And so that's why I thought it was interesting if you were asking in the context of DARPA or in the context of an organization that was funded by government dollars. Because maybe what the implication is, in this instance, for this person who is rejecting the notion that government played any role in the computational revolution of all, is we don't want government help. But maybe the model that they actually want is give us the money, and then don't mess with us at all.

AUDIENCE: [INAUDIBLE]

STEPH: Yeah, exactly.

AUDIENCE: [INAUDIBLE]

WILLIAM BONVILLIAN: That's spoken like someone who's done some annual reports before.

AUDIENCE: Is that the VC? Is that from [INAUDIBLE]?

STEPH: This is from Draper.

AUDIENCE: Draper?

STEPH: It's on page 234, if you're interested in looking at it.

AUDIENCE: [INAUDIBLE] I think what he meant is more like please don't put more regulations that I have to deal with, when I'm trying to innovate. It keeps getting in the way.

STEPH: I like Beth's point too, though. I'm so sorry, Lily. I like your point too. Could you maybe elaborate in your experience what it's like to innovate under the auspices of working in a team with other people?

AUDIENCE: Yeah, so I've had experience with National Science Foundation grants, huge NASA Astrobiology Institute grants. And their reporting requirements are intensive. Usually they're on an annual basis. And I think that the intention behind the annual reporting is necessary and good.

But often, they're extremely cumbersome, and the first year, really people are just making-- including stuff that they've already done, or even inflating what they've accomplished to look good on the report. So those are the only experiences that I've had is with those two funding agencies.

AUDIENCE: Yeah, I think DARPA has the more [INAUDIBLE]

WILLIAM BONVILLIAN: Yeah, DARPA runs a different kind of opposite operation. There's there's, frankly, less paper, but the DARPA program manager-- at a traditional science agency, the whole decision process is organized around whether or not you get the grant award. And it's getting that award out the door.

That's how these organizations are really put together. That's what the peer review process and selection process is built around. In DARPA, and in the-- excuse me-- in some of the DARPA clones, the program manager, that's like just getting the grant award. That's only stage one.

Then the program manager moves into your home. They're going to be on you all the time. Someone has described DARPA as a set of geniuses connected by a travel agent. They spend at least 1/3 of their time on the road out meeting with their teams, bringing ideas around. They'll typically have a number of teams standing up working on a portfolio of related projects, and they're going to bring ideas from those different teams.

They're going to put those teams together to talk to each other, and sometimes that's not easy, because they don't want to really share their secrets. So it's a complicated process to manage, but the DARPA manager is there to help the awardee get through the process and work in any way possible to expedite things and move things along.

So it's a completely different organizational model for the program manager compared to most science agencies. And the DARPA clones, like ARPA-E or IARPA do the same thing. And we'll talk about ARPA-E at a later class. But it's just a different model.

And there's another capability-- we talked earlier about DARPA capability of working at both the institutional level and at the great groups level. The purpose of this reading about its work within the defense legacy sector is to say that DARPA has been able to do innovation within a legacy sector to a remarkable degree, as well as do innovation, like in IT, on a complete technology frontier.

That's a remarkable set of capabilities to build into one place, and yet its model has been able to fit both. So it's another interesting achievement that it's been able to pull off.

AUDIENCE: Just to go back to-- I know in your book, you talk about how, when you go to a legacy sector, it's like you're parachuting down and they're shooting at you. I think, for the military [INAUDIBLE] fields and new areas, it might easier, and so it's easier to form. But I think for energy, how do you make that distinction? Because I feel other legacy sectors would be a lot harder to come in.

WILLIAM BONVILLIAN: Well, the military's a hard legacy sector, because remember, the services have the money. They have their procurement budgets. And if they don't want to do it, it's really hard to get them to do it. So that's why that island-bridge model's so crucial for DARPA being able to rely on the Secretary of Defense to kind of override some of the directions where the services want to go.

It's still not easy, but all of these three technology areas-- position strike, UAVs, and stealth-- were all stood up despite some pretty significant service opposition. Not uniformly-- different parts of the services saw what the possibilities were, so I want to make that uniform. But it was still tough. These were not easy to implement.

AUDIENCE: So [INAUDIBLE] so would you say that ARPA-E has been as successful [INAUDIBLE] DARPA did, or how do you think about it?

WILLIAM BONVILLIAN: Yeah, we'll go through ARPA-E when we get to that class, but I think ARPA-E is very young. It's still very new.

AUDIENCE: Like 2009, right?

WILLIAM BONVILLIAN: Yeah, it's like seven years old. So in some ways, the jury's still out, and we'll see if they survive. But looks very interesting. So Max, let's do some of Glenn Fong's work.

AUDIENCE: Yeah, sure. OK, so basically, Fong was talking a lot about PARC, and Xerox, and how a lot of the concepts that were developed at PARC were eventually taken by Microsoft and Apple, and basically created the PC revolution that we all know and love. Yeah, Apple fanboy.

So one of the questions that I had while I was reading the Fong reading-- maybe someone can clarify this-- I didn't really understand-- so PARC's-- a lot of their ideas were taken-- virtually everything-- so how did PARC not respond in any sort of legal way? They could have sued. Was it possibly because they didn't patent anything?

WILLIAM BONVILLIAN: [INAUDIBLE] Jobs bought the rights--

AUDIENCE: OK.

WILLIAM BONVILLIAN: --to go through. So he acquired those rights.

AUDIENCE: I thought it was just the rights to visit, or the--

WILLIAM BONVILLIAN: Do you know? Go ahead.

AUDIENCE: So I don't know if it was he bought the rights. I know he funded them somehow. And part of that funding was that, oh, I get to check out your projects, and I get to see [INAUDIBLE] and take those ideas how it is.

AUDIENCE: OK.

AUDIENCE: I'll clarify this later. But he can do that because of the rights. And then Gates could do it because-- Jobs and Gates were friends, so Jobs showed Gates. He's like, look at this cool thing I'm doing. Because they're kind of in a different sectors.

So he's like, check it out. What do you think? Because he's trying to impress his friend, who's also very successful. And Gates is like, whoa, this is pretty great. And then he goes and he copies, and he [INAUDIBLE]

WILLIAM BONVILLIAN: You're talking about Windows?

AUDIENCE: Yeah. So he comes out with-- was it Windows DOS? He comes out with that right before [INAUDIBLE]-- because this is going to be part of the new Apple Computer. And he comes out with it right before, so it looks like Gates did it, instead of Jobs.

And then Jobs literally goes like, we're done. This is the reason Apple became so secretive. They used to be a lot more open. They became secretive because after that, he's like-- any little thing can go out if somebody finds out, we're not doing this anymore. So it completely switched the hierarchy.

So that's the rights part, in terms of the-- Jobs does it, and then when Gates copies-- when Jobs takes Gates to court, Gates can say, actually, we both stole it, but it's OK for him, so it's OK for me. [INAUDIBLE]

RASHEED: But like you just said, Jobs didn't steal it. He paid for it. He paid for the rights.

AUDIENCE: When they go to court, Gates says, yeah, we both just took it from the same source. It's not like it was originally Apple's. So that's the element of the legality. The second part-- the reason PARC wouldn't do it is because you have this thing called the innovator's dilemma, which is, in any company, you can have a product line-- a great example's Kodak.

They came out with the first digital camera, but you're already making so much money, and you probably have a large revenue streams doing-- taking pictures of Polaroids. So this is like, if you're an Ethernet company-- if you're 3Com, and you have these-- you're putting wires everywhere. It's not the next day it's like, yo, we got to take all this out.

Another thing too is, once you become a public company, every single quarter matters. So you can never go back. You can know you're wrong but you have to go forward, or else you're just going to get kicked off or you're going to get sued by shareholders. It's a very complicated thing to do.

RASHEED: Also, I wonder how much PARC staff or Xerox staff there is left to really benefit after all the staff is actually just going to Apple and Microsoft afterward. It's not that I didn't benefit because I was at Xerox and I missed the wave. It's like. I just left Xerox to go work at Apple or Microsoft, and then got the benefits later.

WILLIAM BONVILLIAN: So Luyao, you led us through Xerox PARC very nicely last week. Do you have thoughts on how come Xerox didn't get the Alto, and personal computing, and graphic interfaces, and the mouse out the door?

AUDIENCE: Yeah. [INAUDIBLE] last week that they were so much into the research they're doing, and so passion that they didn't consider commercializing or bringing it to the general audience at all. One of the investors came to their meeting room and sat on a beanbag saying, OK, I'm the person [INAUDIBLE] Can you guys show me more, and I will commercialize all this by making it possible? But they didn't. They totally ignored him. So I think they had a chance, but they just didn't the awareness, I think.

WILLIAM BONVILLIAN: Right. It was a really serious island-bridge connection problem.

AUDIENCE: They were probably missing one or two [INAUDIBLE] in their group that were able to connect with the market with their innovations.

WILLIAM BONVILLIAN: Part of this is something Martin mentioned earlier, which is Xerox was focused on its core competency, which is photocopies. And they didn't understand this explosion of personal computing capability, because it didn't fit their core competency model. So they missed a staggering opportunity. But fortunately, Jobs saved us, so it all worked out.

AUDIENCE: [INAUDIBLE] call it the beanbag effect.

WILLIAM BONVILLIAN: Too much time in those beanbags, is that your point, Lily? Right, that was a great photograph of the crowd on beanbags. You had a great picture for us, Luyao. How about a closing thought, Max, on Fong-- or a couple of closing thoughts?

AUDIENCE: Let's see. How about a closing question?

WILLIAM BONVILLIAN: Sure.

AUDIENCE: All right.

WILLIAM BONVILLIAN: That's fine. I only let you get one out, so it's all yours.

AUDIENCE: So one of the questions that someone had previously mentioned this basically there's this stereotype that government bureaucracy is kind of slow, and sluggish, and full of red tape, and it's horrible. And is it possible that you could use-- like DARPA as an example-- to say that maybe this is unfounded, maybe it's not as realistic as people think?

And to continue that-- there's a couple of parts to this question-- is it unfounded, and should the government try to redo its image, for lack of a better phrase? And is there really any advantage they can get from it?

AUDIENCE: [INAUDIBLE]

AUDIENCE: What? Can you repeat [INAUDIBLE]

AUDIENCE: Yeah, sorry. I know, I started rambling a little bit. Is the government slow?

AUDIENCE: Is DARPA an instance where it's not slow?

AUDIENCE: Yes. And does the government get any advantage from making itself not look slow?

WILLIAM BONVILLIAN: That's a really important point, Max. DARPA is stuck in one of the most notorious bureaucracies on the planet, that dates back to like 1787. So how is it able to do this stuff? It has some very interesting rules and some capabilities that it got. One of its key rules is that-- it's called other transactions authority.

And DARPA's argument is, look, we're not buying jet aircraft. We're just buying what's in people's heads to do research. So we don't have to go through all that elaborate procurement process to protect the taxpayers. We're out of the Federal Procurement System. So DARPA's able to contract with enormous freedom, and very quickly.

And then another DARPA power that they've acquired is that they can-- they have what's called IPA authority-- Intergovernmental Personnel Act authority. So ironically, at one point, DARPA was attempting to hire me. And I don't know why they wanted me, because I'm not a technologist, but they seemed to want this. And I wasn't going to go to DARPA, because I'm not a technologist. I wouldn't be at the heart of the operation. So it wasn't going to work.

But they said they can hire me in a day. They said, you want to do this? You're on tomorrow. This Intergovernmental Personnel Act enables them essentially to enter into a contract with whatever organization you're working for-- maybe it's a university. They buy your services using this other transactions authority, so that they can put you on the job almost immediately.

The use this IPA authority with companies or with universities to be able to hire in lickety-split time. Corporations can't hire this fast, much less universities, much less government. So they're outside of two of the traditional banes of governmental bureaucratic nightmares. They got themselves free of that authority. And interestingly, both ARPA-E and IARPA, the DARPA clones, have the same capabilities. So that's part of why they're able to act like this. I don't think it's the whole story, but that's a couple of pieces that they've been able to acquire.

RASHEED: I would answer and say, I guess, traditionally, you would say government and bureaucracy [INAUDIBLE] the highlight of what's slow is. When you think of something slow, you think of a congressional session or something like that.

STEPH: It's deliberate.

RASHEED: And it's deliberately so, and then there's an entire argument that can get into. Is government supposed to move slow because they need to be this giant political machine that shouldn't be swayed by just one charismatic convincing guy? But anyway, I think it's important to understand that DARPA doesn't move slow because often it doesn't have this opportunity to, for national security reasons.

It's just not prudent for us to move slow to develop, I guess, like drones and things like that, which might have been drastic-- things would look drastically different if someone else had done it first, or if we went into the Gulf War without these capabilities.

STEPH: Transferring that example to something like genomics, could you imagine the kinds of implications that it could have on just fundamental human ethics if they were able to create genetic engineering overnight in the way that DARPA's been able to create things?

WILLIAM BONVILLIAN: Well, I do want to tell you that DARPA now has a very strong biological technologies office. So look out, Steph.

AUDIENCE: [INAUDIBLE]

STEPH: Oh, yeah. Well, it was interesting, because at the AAAS meeting in February, one of the sessions that I went to-- actually with Chuck Weiss, who is Bill's co-author for many of the things-- was a panel that included this guy named Gary Marchant who's a lawyer ethicist and genomics engineer, who was talking about the necessity to include a participatory ethics in decision making for genomics.

And one of his primary arguments, as I recall it, is truly that, if we want to have-- if we want to do a good job of implementing this at scale, nationally or globally, we also have to do a good job of listening meaningfully to populations and then, at the same time, meaningfully incorporating their feedback into their creation process. And that's very difficult. And I think that's precisely why government is slow-- or the word that I choose use-- deliberate-- but it obviously does make innovation. So to Max's point, I don't think that DARPA applies, unfortunately, to most things.