[SQUEAKING]

[RUSTLING]

[CLICKING]

DAVID KAISER: So today, we're really kind of making a transition in the material from the overtly war time focus we've had for a couple of classes in a row. And now we'll start moving toward the last part of the semester, asking about some implications or some effects from that very dramatic series of developments during the Second World War on the course of physics, and physicists, and research, and teaching in the decades after the Second World War. So today's lecture is a kind of pivot to help us think about moving from the focus squarely on the wartime projects toward a longer Cold War situation.

So for the main lecture today, as almost always, it has three main parts. The first part is to talk about this phrase that got used pretty early on after the Second World War, and historians and scientists still tend to use it, this phrase called big science.

So the first part, we're going to talk about what have people meant by this phrase big science. What are the characteristics that we can identify from our readings? And we already saw hints from our focus on the wartime projects. And that will be the first main chunk. I think that will be the longest part of our discussion today.

And then two shorter follow-ups that begin to help us think through, what were some of the implications of this period of post Second World War big-science for the study of physics largely in the United States so we can talk about some comparative or examples from other parts of the world? But most of my own research has been focused on developments within the United States.

That's what I know best, and that's what I'll mostly focus on for the rest of today. So who was thinking about this question of a term that was also very widely used at the time, "scientific manpower," with its to our I think many of our eyes today this kind of inescapable gendered notion of a manpower, a certain kind of person that was being sought for to be trained up during the Cold War?

And then lastly, the part that I wrote about for one of the assigned readings for today, can we trace through intellectual or pedagogical aftershocks or implications of these very significant changes in priorities and infrastructure? What does it mean for people entering the field of physics during this new and kind of fast changing scene? So that's our plan for today.

So one of the classic and still kind of worth reading articles on this topic of big science is published now more than 30 years ago by my colleague Paul Forman in this journal that used to be called *Historical Studies in the Physical and Biological Sciences.* Now the journal is called *Historical Studies in the Natural Sciences.* So Forman's article is very, very long. The article is nearly 90 journal pages. It's practically a little monograph unto itself.

What Forman was tracking was basically anything he could count. Much like me, Paul likes to count stuff. He's very good at counting things. As a way to characterize change over time for the US based physics community. And this is just a remarkably rich article. I think very creative use of lots of different sources to put together what is otherwise a pretty consistent picture that Paul was really among the earliest to paint in some kind of comprehensiveness. So I've taken this plot from his article.

Anything we might want to count, the membership in the American Physical Society, the number of papers presented at the meetings or something I've done. Look at, say, the number of articles published in the main journals. Any of these things we can count starts growing exponentially during and especially after the Second World War. And the one that Paul really focuses on and what often gets associated with the term big science was budgets, was funding rates. And that's this third, that's his curve labeled number three in this plot of his.

The funding for what at the time was often called basic research, meaning it wasn't to build specific devices. It wasn't tied to very specific so called mission oriented programs. It was for open ended, often academic or university based research in the physical sciences or in physics even narrowly.

And one of the amazing things that Paul identifies is that over just a 15 year period from just on the cusp of the Second World War to just really not even a whole decade after, that 15 year period, the budget just in the United States for funding the so called curiosity driven or basic research in physics, the budget grew by a factor of 25.

Not a 25% increase. That already would seem generous these days. It grew by a multiplicative factor of 25. That still boggles my mind. And that includes the extraordinary escalation during the war and then as we see, it never slows down or doesn't slow down for quite some time, even after the war. So the first thing that Paul helps us identify is this enormous change in scale for things like funding for research in physics.

The next thing he does, this is really where the article gets especially detailed, is not just the scale of funding, but also the source. That was also changing quite dramatically during these immediate years after the Second World War in the United States. So four years after the war in 1949, 96% of the funds that represent this kind of curve here for academic basic physics research were coming not just from the US federal government. That was already new. But coming from the defense or military related branches.

That included the Atomic Energy Commission, a nominally civilian agency that nonetheless was responsible for the nuclear weapons complex. So if you include AEC as defense related as well as what became called the Department of Defense, 96% of this exponentially growing amount of money was coming from now a unique source.

And what Paul finds, I find this even more interesting, five years later in 1954 as the total budget has grown exponentially, the proportion coming from the defense related agencies of the US federal government, those had actually grown to an even higher proportion from 96% to 98%. So why is that especially noteworthy? Because in between these two dates starting in 1950, the United States established for the first time the fully civilian National Science Foundation.

So even after there was a different non-defense oriented funding mechanism to support basic research in areas like physics and the life sciences and eventually some social sciences, even after there was a thoroughly civilian mechanism, nonetheless both the amount and the proportion of funds coming from defense related agencies was growing exponentially during this period.

So often when people use the term big science, even back in this time period itself, let alone by historians since, they often mean big budgets. Kind of unprecedented expenditures often from federal, from nation state budgets, not just from local universities or associated industries. But enormous rise, fast rising budgets to support basic research across the sciences. That was big science kind of type one.

And indeed here at MIT, that was absolutely the story of MIT in particular as a kind of microcosm for the broader trends. Here I'm showing the annual operating budget for MIT in inflation adjusted constant dollars. You can compare like with like. Here's the very end of spending on the radiation laboratory, the radar project here on campus. There was a brief demobilization after the war and then a kind of exponential rise, again, for the better part of two decades. That was by no means unique to MIT. That's reflective of these broader trends that Forman had identified in that long ago article.

Now, what were people doing with that money? I love this photograph. This is from MIT's own once quite beloved synchrotron only recently decommissioned. And this is on the facility on Vassar Street that will soon be torn down to make room for the new building for the new College of Computing. But starting in the early and mid '50s, MIT had its own particle accelerator, this particular kind of device called a synchrotron.

Here's a grad student working on it with his bow tie. I just love this. You're going to do your work with your sports jacket and bow tie. I don't know if that was because this person knew a photographer was coming or just how you dressed as a grad student in the '50s. I think it might have been the latter.

Anyway, so we start getting larger and larger equipment. There's no longer benchtop experimentation. This is beginning to grow rapidly. It's about to engulf at least this one graduate student.

And of course, that became just a pale, pale imitation of the kinds of machines that were also becoming more common in other parts of the country also supported by the federal government, almost overwhelmingly by the Atomic Energy Commission. This was, for a time at least, the largest in the world and certainly in the United States called the bevatron, or the billion electron volt accelerator at Berkeley. And here again, you see an individual working on it with necktie in the lab.

This is a complete circle. This is a ring of magnets to accelerate the particles in a circle. So you can get a sense of the scale. They're really filling a kind of factory sized building.

So what happens when the United States built many, many of these from the kind of modest sized MIT synchrotron up to these behemoths? Physicists were able to accelerate nuclear particles to unprecedented energies, rev them up by those electromagnets, basically, smash them together, and very soon they began finding an exponentially rising number of new particles.

So with these larger and larger machines, physicists could probe energies, excuse me, interactions among particles at higher and higher energies. And not quite every single time they tried, but with remarkable rapidity, they began finding more and more new or previously unidentified particles zooming out of these collisions among nuclear particles.

And so even by the early '50s, the joke was that basically every single month the community had discovered some new particle. And that's borne out by the data that a different colleague of mine compiled some years ago showing the number of published discoveries of seemingly new or elementary particles growing, again, exponentially over time.

So the second meaning that's often attributed to this very common phrase of big science is big machines. Kind of conducting research on a scale of instrumentation that, again, had very few, if any precedents before the Second World War. So big funding, big budgets, and also big machines.

There's a third kind of element of big science that really I've been most interested in, most curious about for a long time. I've written a lot about this in my historical work. And the third aspect of big science that I think often got overlooked in the early days was actually big enrollments, an enormous training mission to train lots and lots and lots of specialists in the sciences often in physics or the closely allied physical sciences in particular.

So here's, again, a photo kind of close to home. Here's Tony French, who I think only very recently passed away. He lived to just about age 100. He lived a very, very long life. He actually had worked on the Manhattan Project originally himself and then made a very long, distinguished career teaching physics here at MIT teaching in a room that I'm sure many of very well, 26100. This is when this kind of picture for undergraduate physics classes no longer becomes unthinkable but in fact becomes the norm. Again, really across the country, not only at MIT.

So the third kind of aspect of big science that I found most interesting, most wanted to dig into, was big enrollments. This kind of training mission. I want to take a few minutes now-- oh, excuse me. The last point here again to bring it back home to MIT. Again, MIT's enrollments grow exponentially during this period exactly consistent with the broader nationwide pattern.

This is also the first period in the decades soon after the Second World War where the number of full time graduate students catches up and actually comes to exceed the number of undergraduates at MIT. Right now they're just about 50/50, but it had been a very small proportion in MIT's earlier history. Again, consistent with many places across the country. So you have a very rapid expansion in this mission to train lots and lots of new specialists, often in the physical sciences and related engineering fields.

OK. So this plot I look at all the time. I stare at this picture really often. And I think most if not all of our teaching assistants have stared at this graph also, because I can't get away from it. So this is a plot of the number of physics PhDs granted in the United States per year starting in 1900. And this version of the plot goes through 1980.

We can read a lot of information off the kind of structure of this plot. We can see that the enrollments fell very quickly during the Second World War, which makes sense. People were drafted. Many people who were drafted but were in physics left school to go work on either the radar project, the Manhattan Project, or dozens of other related military projects. During the war, enrollments plummet.

Then they start growing very rapidly as soon as the war is over. They kind of reach a kind of what felt to many people at the time a saturation or a plateau. But then they come take off yet again exponentially very soon after the launch of Sputnik, the first artificial satellite launched by the Soviet Union late in 1957. So we have these periods of exponential growth and expansion.

In fact, during the second wave here starting in the late 1950s between this take off and the peak, the number of university departments within the United States, physics departments, that began offering a PhD degree doubled. So it wasn't just that the same departments got bigger. They really did. But even the capacity across the United States to make more and more trained specialists doubled as well. The number of PhD granting physics departments doubled between 1958 and 1960 in the US.

So many, many fields of study were growing rapidly after the war. This was a period of the so called GI Bill, which gave support at least to some returning veterans. Or now historians have identified some really quite extraordinary racial disparities and frankly just biases in the way the GI Bill was implemented. It was originally designed to help all US veterans of the wartime Armed Services get extensive support for things like going back to college, including also going back for advanced degrees beyond undergraduate degrees.

So every field of study in the academy across the United States grew exponentially after the war. But look at these exponents. The rate of growth for physics was actually twice as high as for all other fields combined. So to make it easy to spot, I've taken all PhDs, that's this green line, and I've just normalized it to the same value as physics PhDs in 1945, just so we can see rates of change. I could have made a log plot.

But anyway, this is just a way to say what's the steepness of this curve compared to that curve. So physics was growing twice as fast during a period of general exponential growth. Likewise, all the fields went through some contraction. None fell nearly as sharply or as dramatically as physics following this peak.

This was not simply demographics. This wasn't just more kind of college aged people in the community. In fact, those trends were exactly out of phase. So this purple line shows the proportion of the US population in what we think of as a kind of common age range for graduate school. The bulk of PhD students would have been in the range ages 25 to 29 at the time. That proportion was actually falling during this exponential rise and then rising during the crash.

So this wasn't just more baby boomers entering higher education. That's actually out of phase. The baby boomers enter higher education later. This is growing like gangbusters. It points instead to some very intentional shifts in policies and incentives, as we'll talk a bit more about today.

But before we go on to say what was happening at universities and congressional funding and all that, I want to pause a bit more on this photograph and say, who is filling those seats? If we see, it's not only Professor French up front in these familiar multistage blackboards, which I personally dearly miss. I wish I could get back to those blackboards.

But look at the folks filling the room. And even from a quick glance, we can see they mostly come from one demographic type. And this was, as I'll say in the next few slides that we'll talk about, that was not unique to MIT. It was not unique to physics, but it became especially accentuated during this period of exponential growth in the study of physics.

More and more, the field narrowed in the enrollment trends around basically white males, whereas that had been not always the trend before and not exclusively the trend now. But in this intermediate period, it was really, really a period of real consolidation or contraction of the range of the types of people who were otherwise flooding into physics.

A couple of examples that I wrote about some years ago in an article that I wrote up. And other historians have found similar things in their own archive work. Here's an example that I found of a letter of recommendation being written by a Harvard physics professor on behalf of his recent PhD student looking for a job at one of the new national laboratories. It's a letter from 1954.

The student had a traditional sounding Japanese last name. The letter goes on to explain this was a child of Japanese immigrants. The student was actually born in the US in Honolulu. But nonetheless, the letter writer felt the need to emphasize that despite this kind of Japanese sounding name, the applicant for this job actually was not just a US citizen but a kind of honorable US veteran who had fought in the US Armed Forces during the Second World War. It had to spell that out because of a kind of anticipation of a kind of anti Japanese sentiment so soon after the war.

There are other notes I found in Berkeley's physics department archives from 1950, a kind of way to try to figure out who should be hired as the new faculty, some new faculty members in the physics department. And these are these kind of informal handwritten notes that mostly emphasize personal appearances using some pretty kind of flagrant stereotypes. They often talk in these notes about Jewish noses, Jewish features, Jewish type, whether the person who looked Jewish was or was not too pushy.

And that got much more space in these notes than what the actual topic of research was or what kind of teacher the person might be in the classroom. We saw some of that even in some lectures ago about a kind of widespread anti-Semitism in many, many US universities before the war. It certainly didn't vanish after the war.

Here's another example also it turns out from Berkeley. This was also not unique. The department head was answering a kind of query from I think in this case a Dean or someone like that. I can't remember. And the department head for Berkeley's physics department says, we have in the department head's words, no minority group problems in the department. So far as race is concerned, we have never yet had a Negro grad student in the department, hence that particular problem, so called problem, has never arisen.

I just find this extraordinary. Berkeley, as I think you know, was then, is now a public University in a very dense metropolitan area that also was at this point the country's largest graduate program in physics. They had well over 100 grad students at a time enrolled and that number kept rising during this period. And yet the department head could state, I don't want to say proudly, but could state matter-of-factly that they've never had a so called minority problem because they haven't had any minorities, at least no African Americans in the department. I just find that extraordinary.

One last example. In case these were a little too subtle, I don't think they're very subtle, here's another one I found in the archives some years ago. A job ad for a new physics instructor at the US Naval Academy in Annapolis. And it actually said in the ad things that people literally can't write anymore. It said literally don't even apply unless you are quote "white male and an American citizen." So this was a kind of assumption throughout all of these notes that the people entering the field either as grad students or as young faculty would be male, they'd be men, but of a particular kind of background.

And then we can ask what about people who weren't male? And here again, we had the reading from Professor Evelyn Fox Keller. And I just want to share, put that in a bit more context as well. Also again, research I've done in many other stories by now have found very similar things. So throughout the 1930s in the US physics community, women had accounted for about 1/6 of the students who earned undergraduate degrees in physics each year during the '30s, so before the Second World War. Not 1/2, but about 1/6 were getting undergraduate degrees.

And that proportion fell by a factor of 4 during the postwar decades. So instead of 16% each year, it fell to 4% very, very rapidly after the end of the Second World War. Meanwhile, the proportion in the US of women who had earned PhDs before the war, PhDs in physics, had never been larger. It had only been about average about 4%, held pretty steady throughout the '30s at 4%, 1 out of 25. And that already small proportion fell in half again during that postwar decade. So from a trickle to barely even measurable practically in terms of just the throughput through these now large, booming departments.

There's also this part I found even more interesting, troubling. There was a kind of ambivalence among many physicists about what to do about women in or near or around physics. If there weren't very many women who were students formally in physics, what about the male students who were assumed to be heterosexual and having relationships with women? What about the women who were dating or married to the male physicists, either young grad students or young faculty?

And again, there's a kind of back and forth. Some people say it's great that so many male physics students are married because it gives them stability. Now they can focus. I don't know where they get that idea from. That was one idea. On the other hand, you see these things like the women are cast as being too demanding. The wives want the husbands home early.

I wrote a lot about these advertisements for physics positions in the main kind of trade journal of the time, *Physics Today,* which were mostly advertising about the kind of great kind of suburban comfortable middle class lifestyle rather than we have the best equipment. We study really interesting problems. It was all about the male physicist so called breadwinner can make a nice life for the family while the wife presumably stays home with the kids. That was how you would advertise to get the best physicists.

This photograph I love. This is from, again, Berkeley's archives. This was a get acquainted tea for the wives of new physicists who had joined the Lawrence Radiation Laboratory. And what I love is that the greeting line here are the wives standing in rank order of their husbands at the lab. So I can never remember if it's here or here. But one of the endpoints was the wife of the director of the laboratory at the time and this was like the wife of the associate director and so on.

Meanwhile, John Slater, who was for a very long time the department head of physics here at MIT, really helped build the modern department and did many things that he set in motion that we now kind of take for granted. On the other hand, he was also capable of writing things like this in the late '60s in *Physics Today* where he's not so excited about the presence of so many wives just seemingly distracting the male physics grad students.

So he writes that present students, by which he means male physics grad students, find it harder to settle down to work. Wives and babies take up a lot of time that my generation put into physics. There's very little evidence of actually there was marriage rates very different at that point, but this is his nostalgia.

The wives, it is true, helped to type their husbands thesis. But in the older days, the necessity of doing this ourselves made us learn typing. I mean, that one just kind of makes me laugh. It's not even a benefit to have secretarial support. The men should learn how to type on their own. He's just frazzled by the fact that women are not studying physics but they're also not somehow leaving the male physics grad students alone. It's really remarkable.

So I find that helpful context, then, when we come to this reading that we had for today from my colleague, Professor Evelyn Fox Keller, which is a remarkable scholar who's gone on to win more awards than I could list. She retired from MIT a couple of years ago. So she wrote this reminiscence kind of later in her own career of her experiences as a grad student in theoretical physics at Harvard starting in the late 1950s. I think she entered around 1958, give or take, if I remember correctly.

So at that time, again, this is based on my own archival research, Harvard's department also by that point had grown to have about 100 PhD students enrolled total. Not 100 new ones per year but across the approximately five years of grad school, they had about 100 at a time. And during the 1950s, the number of women PhD students in physics at Harvard ranged between three and seven. So it was fewer than 10% at any given year.

And this is something that Evelyn writes about in her piece and I'd found independently in the archives. The women who entered grad school to study for a PhD in physics at Harvard would apply to Harvard's department of physics. If they were accepted, then their files were transferred to a make believe shell game called the Radcliffe Graduate School. At the time, Radcliffe was, of course, a separate all women undergraduate institution that did not have its own physics department and did not grant PhDs in physics.

So the women applicants, like the male applicants, would apply to Harvard's department of physics for graduate study. And then their papers, their files, would literally be segregated to be shipped off to be physically housed in a different building. And as Evelyn recounts, that actually could matter. It wasn't just that they were treated differently. That could mean they wouldn't get certain announcements for when the final exam would be for a graduate level course, as Evelyn recounts in her piece.

Of course, she talks about many other microaggressions, what we might even call mezzo aggressions. Some of them weren't so micro. And the kind of climate or atmospherics in the department at the time. So I just want to give background she was she was not alone and other archival materials from the time suggest similar kinds of experiences for other people in the field.

The American Institute of Physics compiled data in the early '60s and they found, again, maybe not surprising what we now know today, that the few women who did pursue careers in physics were paid much, much, much less than men in the field who had achieved the same level of education.

And here are the other things that I found in some of my own archival work that still frankly kind of haunts me. There was a tradition in many departments, at Harvard's department and many around the country, including University of Illinois Urbana-Champaign, of having these informal skits. Sometimes the grad students do a skit to have a gentle roast to tease their own faculty. Sometimes the junior faculty do a skit to tease their senior faculty, which is not a good idea if you're tenure track.

Anyway, so I found these transcripts of these faculty skits from physics from Urbana in the early '60s. And they were joking one year, seemingly innocently, just kind of blowing off steam, about how they should handle admissions to the PhD program. And they said the male applicants should submit their credit ratings. Do you have a strong FICO score? And the women applicants, excuse me, the girls, as written there, should submit photos of themselves in bathing suits and give their critical measurements.

And again, that wasn't unique. You see the other ads from physics today for a kind of optics manufacturer that has really good polarizing lenses. And so instead of just saying we have great equipment and cool research projects, they put a so called pinup girl woman in basically sort of like a bathing suit to show off the optics of the company.

So again, this was obviously not unique to physics departments in the United States at the time. It's reflective of but also kind of amplifying some very pervasive kind of cultural stereotypes or notions to which universities were hardly immune. And we see a kind of, I think, a kind of amplification of these things in physics departments during exactly this period. I find Evelyn's piece especially evocative of that, though hardly unique.

So my last thing to say on this first part about big science meaning big enrollments. So it's really critical to keep in mind these were enrollments of a certain kind. Enrollments were growing exponentially faster in physics than in any other field across the American academy. And yet it was really as the numbers were growing, the demographics were actually winnowing very, very, very quickly and looked nothing like either what had come before or indeed what has slowly come to open up since.

So let me pause there and ask for some questions and then we'll go to the next part. Any questions on any of that? DAS. Why is there a sawtooth pattern of the number of papers released? That might be referring to that early plot from Paul Forman about the number of papers at the meetings.

I think there were a lot of disruptions for things like the Second World War and the Korean War. I'd have to go back to the exact dates, but I think there were other kind of worldly events that would affect the kind of annual attendance. But I can go back and double check on that.

OK, other comments. Benjamin asks, did I just say that physics were expected to be in relationships? It's really interesting. So sometimes department heads would celebrate that more and more of their, again, overwhelmingly male grad students were getting married to women. And they thought this was adorable. They'd have little parties. The Berkeley department head called it an outbreak of marriage-itis, as if it was like a disease, but he was cute about it. We should have a party, we'll have cake.

So sometimes they eyed this very as a cause for celebration. And other times they say no, no, no. Our students are too distracted. Back in my day, all we did as a kind of monkish thing, they would say, we were kind of celibate monks and only studied physics, which is not borne out by the existing data at all, but it was how it was often cast by not all but by some physicists who were then later in their careers thinking back to their own experiences from the '30s.

Fisher, you say on the graph physics PhDs over time, what caused a massive drop? Yeah, we'll get to that. I would think Vietnam is correct. That is a large, large, large part of the answer. But we will talk about that actually a little bit later in today's class. It's a great, great question. Any other questions about that?

I mean, one of the main takeaways is when people talk about big science, that might mean big money. And remember, it's not just money, budgets, but actually the source. And the US had really collapsed to these defense agencies at the federal level. Big machines. And I love those pictures of these enormous cyclotrons and synchrotrons swallowing up the people wearing their neckties.

And then big enrollments. That enrollments part I really think we have to keep the asterisk in mind. Some people wanted lots and lots and lots of people to study physics. Let's ask a little bit more about who. Lots of what kinds of people?

And so I should say that last part I meant to queue up. I have some ideas about why it was so narrowing demographically or stereotypically during that period, and that does have to do with the part we'll talk about next in class. So I don't think it was either accidental or unexplainable why that demographic narrowing happened amidst the exponential growth. And that's also I think at least in part a kind of Cold War story.

DA says that it might have been Slater's own lifestyle. I read somewhere that he was not very social. That absolutely could be. There's a lot of personal idiosyncrasy to bear in mind with these reminiscences. But as I say, sometimes it's just fake nostalgia or real nostalgia even though it was never quite that way at the time. We can now independently check. And also it is the mixture of the usual kind of play of personalities and who's recalling what, when, and in what context. I absolutely agree with that, DA. I think that's exactly right.

OK, let me press on to the next part. I did not assign a reading squarely in this next part, but I have written quite a lot about it. So if people are curious, I'd be glad to point you to some more readings if this just kind of gets you interested.

So let's talk about this term manpower. And by now the man part might not be so surprising. And I do think that's relevant for this next phase to help us account for this really extraordinary, unprecedented growth in the numbers of people entering the field. Manpower was originally a military term. It really meant things like so called boots on the ground, the number of, say, members of the armed forces who could mount an invasion such like that.

And this kind of military force term, manpower, was very rapidly taken over to describe what seemed like a very pressing at times kind of hysterical concern in the United States to make lots and lots of new scientists. That almost always meant physicists. Make lots and lots of new scientists so you could expand the ranks of so called scientific manpower.

Here's one example. Many examples beyond these. Henry Barton, shown here, was at the time the president of the American Institute of Physics very soon after the war. He was saying in a time of national emergency, including this period after the war, this unsteady developing Cold War with the Soviet Union, this country would think nothing of spending $1 million to survey developing-- conserve a short commodity like natural rubber or tin.

In other places, people compared physics grad students to pigs and cattle, which I thought was telling. Highly trained and able human resources viewed as a commodity are far more important. So he was urging the federal government to spend even more money or at least help the AIP do very extensive surveys of the kind of capacity within the country to make lots and lots of newly trained physicists very quickly.

Likewise, Henry Dewolf Smyth, department head of physics at Princeton and an accomplished nuclear physicist, was by this point a leading member of the Atomic Energy Commission as well. And he gave a series of speeches and lectures on this topic to Congress, to public lectures, to civic groups. Again, saying things like scientific manpower was a war commodity, a tool of war, a major war asset, and hence had to be stockpiled and rationed, again, like rubber, tin, or gasoline during the recent war. And here he's referring to scientific manpower by which he then goes on to say things like PhD students in physics.

So we come back to this plot that I was showing earlier, the PhDs in physics over time. Fisher already noted there's this very precipitous fall around 1970, '71 after it peaks. Let's look first, though, at this unbelievable rise. And I showed you earlier this is rising faster than any other field in terms of rates of growth across the American academy.

The feature that this curve looks most like, at least to my eye, is actually something economists call a speculative bubble. It looks like a stock market crash. This exponential rise that is clearly not sustainable followed by an exponential crash. We see this happen on admittedly shorter time scales but with the same kind of characteristic curve all the time.

So that got me thinking about what do economists or economic historians or sociologists, how have they described these financial or speculative bubbles? And is there anything we can learn from their treatment of these kind of characteristic patterns of prices of commodities that might help us make sense of this very unusual, very stark pattern in the training of young physicists in this country?

So there are some lovely books. There's a book that I quite enjoyed by Robert Shiller, who's an economist at Yale. Actually received the Nobel Prize in economics a number of years ago. There's also a very cool book by my colleague Donald McKenzie, who's a sociologist of science and technology. Many others we could point to beyond these that look at these kind of characteristic financial or speculative anomalies. Although unfortunately, they're not so anomalous. They happen kind of often.

Where the price of some object gets bid up kind of all out of proportion to any kind of valuations that otherwise might have made sense until the price becomes simply unsustainable and followed by a crash. Not just a market readjustment, but a catastrophic fall. And these have been identified at least as early as the 17th century.

The South Sea bubble, about which my friend, two of my colleagues are writing actually. Both Will Derringer in our SGS program and also Tom Levinson in our comparative media studies and writing programs are each writing really fascinating books about this. This one nearly bankrupted Isaac Newton. More recent examples, not just in the US, but around the world. We see this happen sometimes with great frequency.

What Shiller has written about, is there a way to make sense of this recurring pattern of this exponential rise followed by an exponential fall as opposed to anything else that might be kind of tiptoeing around some supposed market equilibrium? This is clearly non-equilibrium behavior here. And Shiller identifies three stages that he thinks are consistent across each of these examples and others. A role of hype, of amplification, and then feedback. And I found that really helpful. I think that helps us make sense also of that plot of PhDs in physics as well.

So let's talk first about what gets this first phase of hype. During the early years of the Cold War, soon after the end of the Second World War, there were a series of efforts to assess just how many physicists and engineers were being trained each year in the Soviet Union after the Soviet Union had emerged as the number one rival to the United States, at least as seen by many political and military leaders in the US.

So there were these three kind of classic studies undertaken in rapid order. You can see they're years. Not much time between them. Written by the lead authors were Nicholas DeWitt and then separately Alexander Korol. DeWitt and Korol actually shared a lot in common. They were both Soviet ex-pats themselves. They'd both been trained in engineering in the early years of the Soviet Union before they left and moved to the United States. They both settled in Cambridge, Massachusetts.

So DeWitt was hired at the brand new Russian Research Center at Harvard, founded in around 1948 or so. Meanwhile, Alexander Korol was hired at MIT in the Center for International Studies, just set up after the war, part of the political science department. We now know both of these efforts were secretly bankrolled in part by the CIA. Although the documents they produced were fully open, not classified and so on. They were widely covered. And in fact, these reports were covered in the news. They were reviewed in widespread magazines and newspapers.

What they both wanted to do was figure out how many scientists and engineers mostly in the physical sciences were being trained each year in the Soviet Union. And then you could ask is there a kind of concern about a gap, a kind of scientific preparedness gap between these now two rival countries.

So both DeWitt and Korol, who had direct experience of the early years of the Soviet training system, had a lot of caveats. They were very explicit if you read these thick books that let's not get lost in what I think it was Korol called the numbers game. Let's not just look at tabulations of enrollments or degrees conferred each year. Because the systems were very different, they argued. The educational systems themselves were actually kind of a bit like apples and oranges. So just comparing numbers might be misleading was at least their conclusion.

So for example, they both emphasize there were a large fraction of Soviet engineers who stayed in the Soviet Union who worked in administration or bureaucracy, not actively in research and development. So just because someone got a degree in metallurgy, if they're not actually an applied metallurgist, does that change the kind of preparedness for either country? They both identified what they considered an extreme specialization in the courses of study. And I'll say more about that in a moment.

They each also independently suggested, the evidence here is a bit more spotty, but they put forward in their reports that when the targets for the numbers of degrees in a given field per five year period look like they might be falling below the quotas, below the expected production quotas in the Soviet Union, that standards would be lowered. So you could force more kind of mediocre students through, which of course, we would never do at MIT, of course.

But the allegation was made in each of these studies that the quality of instruction is quite uneven because the Soviets were more concerned about these famous five year plans, how many tons of pig iron produced per five years, how many undergraduates majoring in physics or mathematics per five years.

And likewise, something became increasingly important in these later reports. The number of students in the Soviet system who were full time students was actually the proportion was falling. And that in fact, the number of students enrolled included as many as 1/3 in the early years greater than 1/2 by 1960 of part time students who held full time production jobs in a factory or some office and were doing basically correspondence courses to get their degrees at night. Which at least the authors argued would be quite a different kind of training than full time students in their chemistry labs day and night.

In fact, many of these correspondence students had a ratio of 80 to 1, 80 students to 1 professor, including for seemingly hands on courses like organic chemistry. So not only were they never in a laboratory, but they had one overworked instructor grading 80 problem sets at a time. Which at least the assumption was this would maybe not be the best kind of education.

For the specialization, they both point out things like at the undergraduate level at this time in the Soviet Union, a student would not major in the field of metallurgy. But actually in 1 of 11 subfields of the subfield of non-ferrous metals, metallurgy. So they argued there was this enormous specialization, nothing like a kind of GIRs or let alone a liberal arts model that was becoming so common in the United States. My colleague Lauren Graham has written a bit about this as well, more recently looking back, but even at the time both DeWitt and Korol emphasize these caveats.

So with all those in mind, Korol, the MIT based author, refused to even tabulate enrollment data side by side. He puts numbers in his very lengthy report. It was published by the MIT Press, in fact. So his book is full of data, but he always put them on-- he never put them side by side because he wanted to avoid what he called unwarranted implications.

Nicholas DeWitt did have tables kind of side by side, but he also always emphasized all these caveats about the kind of apples and oranges. What DeWitt found was consistent with Korol's numbers as well. I've now simplified it to make it just ratios. He actually has full kind of tabulations of enrollments and degrees conferred.

If you compare the number of photon university students pursuing undergraduate degrees or more advanced degrees, in the US, the Soviet Union, there were actually three times more full time students in higher education in the US than the Soviet Union at the time he wrote these reports. If you add in all those extension and correspondence students, there were still more students in US higher education than the Soviet Union. Now the gap closes from 3 to 1 to 4 to 3.

But there was this remarkable imbalance in the distribution of topics of the students were studying. And in the Soviet Union, it was nearly 3/4 of the students who were studying something in science and engineering versus only 1/4 of the time in the American system. So when you combine these last two, include all those extension and correspondence school students, and look at this high imbalance in the distribution of topics, it looked like the Soviet Union was graduating two to three times more students per year in engineering and the applied sciences than the United States.

So they were both saying, it's not the same education. The fields of study don't map onto it the same way. There's all these part time students. But if you ignore all those caveats and just run the numbers, it looks like you have this gap of two to three times per year.

Now, that's where we find Robert Shiller the economist's example of hype. That ratio, two to three times, gets pulled entirely out of context and repeated literally ad nauseam in briefings by members of the CIA, in congressional testimony, both by and to members of the Pentagon in various congressional committees, the AEC. You see it everywhere. It's repeated in mainstream media up and down. It was just everywhere.

And again, I've written about this in a separate essay. And that was a dominant story even before the launch of the Soviet satellite Sputnik in early October of '57. So this notion of two to three times was put forward even with the first of DeWitt's studies in 1955. And that just gets even more hyped up much more dramatically after Sputnik.

So let's look at that second step in Shiller's model. You go from hype to amplification. Again, I found these examples in various archives. And I found them just fascinating. Looking at physicists trying to respond to them the surprise launch of Sputnik. So now this is people mobilizing literally in the closing weeks of 1957, just days and weeks after the launch of the Soviet satellite.

We now know there was at the time a private briefing in the Oval Office for President Dwight Eisenhower by his brand new still rather informal group that came to be called the Presidential Science Advisory Committee or PSAC. It was actually formalized by President Eisenhower soon after the launch of Sputnik.

And one of the leads of that was Nobel Laureate Isidor Rabi. Rabi actually knew Eisenhower. Eisenhower, as you may know, before becoming President of the United States was actually president of Columbia University and that was where Rabi was a member of the faculty for many, many, many years. So Rabi knew Eisenhower personally. And we now know from notes of this meeting that were much later released, Rabi was pushing Eisenhower to use Sputnik as a pretext, as an excuse to close what they already began calling the manpower gap.

The notes make clear that Rabi didn't think, or some of Rabi's colleagues, didn't think there was actually a gap to worry about in the numbers of scientists and engineers trained. But that nonetheless, you could use this politically as cover to make a whole different kind of investment in making young physicists and engineers in the US.

Likewise, right around the same time, another set of notes I found in the archives. Elmer Hutchinson, who by this point had become director of the American Institute of Physics, was chairing many committees. Kind of outreach and public policy and lobbying committees. And he writes in memos to his committee mates that the launch of Sputnik presents the physicists an almost unprecedented opportunity to influence public opinion greatly with the same argument as Rabi was making, that we should be investing in training many, many more physicists now.

Here's actually my favorite example I found in Hans Bethe's papers at Cornell. Bethe would go on to win the Nobel Prize. He was already a very prominent physicist in the United States context. He was an emigre originally from Germany in the '30s. But by the '50s, was an outspoken leading member of the community in the US. He was already by this point a past president of the American Physics Society.

So he gave a series of radio addresses to reflect on what the launch of Sputnik might mean. And I found the typescript from which he would read. And in pencil in his handwriting in the margins are all these questions about where this ratio of two to three times had come from. He said, what is it based on? How is it computed? But he never took it out of his address. He was like, I should look that up, and it appears he never did.

So that kind of amplifying of the initial hype of two to three times is getting-- we see it unfolding in very concrete places and then, again, picked up broadly by journalists and policymakers. My colleague John Rudolph writes nicely about this in his book and I write about it in my own recent book as well.

Now we come to the last step in that kind of three stage model from the economist Robert Shiller's I think really lovely book. And that's feedback. So again, it's actually kind of hard to believe, given our recent experience of US Congress these days and how slow it is to get anything to move, US Congress moved unbelievably quickly in response to Sputnik and passed major, we might rightly say, landmark legislation less than one year after the launch of Sputnik.

It became known as the National Defense Education Act or often just called the NDEA. It was signed into law by President Eisenhower in September of 1958. It made available $1 billion from the federal government. If you adjust for inflation, that's around $9 billion today over a short few years window. Concentrated kind of infusion of a lot of cash.

And part of what's so interesting to me about this historically is that that was literally the first federal aid to higher education in the United States in about 100 years. There had become a long standing tradition between the 1860s with the Morrill Land Grant Act, which actually helped found MIT and many of the public universities throughout the United States around the time of the Civil War, between the 1860s and this post-Sputnik bill of the NDEA.

The tradition had kind of coalesced that education, including higher education, was a local affair. It was for local communities and states to handle, not the federal government. This was often seen as a so called third rail. The federal government had no business in higher education. And that was just shattered. That tradition was overridden very rapidly in response to the launch of Sputnik with this billion dollar investment in higher education.

That had some predictable and very rapid effects. So in the first four years after the bill was enacted, it basically doubled the number of graduate fellowships, fully funded graduate fellowships for the sciences in the United States. So 7,000 isn't very many until you compare it to the number of federal fellowships before then, which had been practically none.

It also subsidized the training of half a million undergraduates as long as they were studying the physical sciences, engineering, or mathematics. It also offered block grants to states and to individual institutions. The whole point was to get more and more students in to address the so called manpower gap of so called scientific manpower.

Since that time, several political and legislative historians have gone back to the passage of this landmark bill, the National Defense Education Act, and they've really picked apart the kind of backroom lobbying and the sometimes, let's say, shadings of the truth that were done by proponents to kind of ram this thing through so quickly.

And as the historian Barbara Clouse has concluded from a really just fascinating kind of moment-by-moment legislative history of this bill, in her own conclusions I'm quoting here, that the "proponents were willing to strain the evidence to establish a new policy," that it was made to seem that the manpower gap was more important and more dire than indeed it seemed to be.

And again, what explains these plots here is that all the aid from the NDEA was earmarked for so called defense fields in the language of the time, fields that were seen to be relevant for the National Defense. Science, math, engineering and area studies. Some parts of humanities and social sciences, like we need more people who are experts in the Soviet language or know about the culture of Ukrainians. So that would get some support as well.

Now here's, again, something I've written about. I'd be glad to share the piece. If you go back to those reports from DeWitt and Korol, remember, these CIA funded, thoroughly unclassified, easy to find books still in our libraries today. If you go back and look at even just their data, let alone all their caveats about the different kind of educational institutions in the two countries, the numbers themselves really deserved a closer look.

The columns in DeWitt's very densely formatted data that are in his book that were put together to form this ratio of so called two to three times were labeled in the press engineering and applied sciences. It actually meant engineering, agriculture, and health. And it explicitly did not include the natural sciences. It left out physics, chemistry, mathematics, and biology.

And so what it was actually counting was a lot of experts in agriculture and agronomics, as it was called at the time, including this what I think most would conclude devastating tradition in the Soviet Union of so called lysenkoism, kind of an idiosyncratic non-genetics notion about how plants change, which is at least blamed by some historians as having led to just failures of many collective farms and widespread starvation.

So yes, there were many people graduating with degrees in agriculture but not in physics or math. Likewise, it Included health professionals, including nurses and dentists and so on, which is clearly an important measure of overall society labor force but was not the kinds of defense oriented experts that it was characterized to have been.

If you go back to the thoroughly accessible non-classified data in these books and you drop agriculture and health, keep engineering and add back in science natural sciences and mathematics, then actually the enrollment numbers come out to be basically a tie. The Soviet lead falls by a factor of 10. And that's still including all those extension school students and correspondence school students. So the so called manpower gap was really a kind of chimera.

And that's still under the assumption that the nation's safety depended on having lots and lots of theoretical physicists around, which itself was an assumption that one might have questioned at the time. Just playing the numbers game was actually really kind of twisted or misread and used very strategically during the kind of fever years of the early Cold War.

So we come back to this plot. Now anticipating Fisher's question from earlier, the same plot I showed before. We now have a better sense of what was driving this up so high. And it was kind of a series of political decisions and priorities often at the federal government level. We can question them, but we can at least understand why they were made when they were.

What about this really quite precipitous fall? And that was very much as Fisher had anticipated, a lot of that had to do with other changes in the United States and around the world by the late '60s compared to the early '50s. A lot of that was the escalation of fighting in the Vietnam War, which included things like the removal of draft deferments for full time students.

So full time students, both undergraduate and graduate students, got deferments from being drafted to go serve in Vietnam up until the late '60s. And then because the war wasn't going very well for the United States, there was a kind of recalculation on the number of people needed for the kind of military manpower. So people who had gotten deferments for being full time students were now no longer eligible for them starting in the late '60s.

There was also a lot of concern on campus and actually even within the Pentagon about why the defense agencies were funding all of non-mission oriented, open ended, basic research. The argument came to be among the Department of Defense's own auditors, their own accountants. If we want to get better kind of military ready instrumentation or weapons or defense systems, shouldn't we invest in projects that will make better defense systems as opposed to spending money on seemingly open ended topics and basic physics that sometimes have a kind of trickle on effect and lead to new devices but often do not.

So the Pentagon itself began this internal audit that they concluded in 1969 saying that it's been a terrible policy, terrible return on investment for the Pentagon itself to be paying all these students to pursue PhDs in physics if they're not doing defense related work. And at the same time, really because of the new kind of immediacy of the fighting in Vietnam, there were many, many critics of the Pentagon on college campuses, students, faculty, staff alike who were very upset at the domination of spending on university campuses by the Pentagon.

So both from within the Defense Department and from without really heightened by the course of the Vietnam War. This whole kind of framework for funding research and research students in the science is especially in the physical sciences gets in for some really quite severe Reassessment All kind of cresting around the same time? A third factor that starts to impact things is huge federal deficits and cuts to both military and education spending at the federal level. So there's less money to go around and then fights over how to allocate that money.

These photos are both taken, by the way, from MIT. You can see instrumentation laboratory. That's what's now Draper laboratory. There were these marches in November of 1969 throughout the month of basically fights not just anti Vietnam War. But also anti-military spending on MIT's campus. And that led to fighting.

You can see this was also-- I think this is right down the street from that where it led to clashes with the local Cambridge police. Really, I mean, students and staff being clubbed and some police injured as well. It was a melee. It was a fight in the streets. And that became emblematic of the kind of deep questioning of the arrangement that had led to this extraordinary 20 year rate of growth.

And as you also see it in a very dramatic contraction in the field itself. So here's other information I found in the AIP archives. The AIP is to run a placement service to match up young PhDs finishing their degrees in the field with employers who are eager to hire young physicists. So we would help arrange interviews for jobs at the annual meetings, for example. And the AIP kept statistics on how many people, how many students registered with the service and how many employers were coming with jobs on offer.

And you can see even after 15 years of exponential growth in the field. there was still more jobs on offer than students seeking jobs at, least through the AIP right into the early and mid 1960s. I find that mind boggling. But then you see the fortunes begin to reverse quite dramatically.

So the bottom falls out. And then by 1971, there were more than 1,000 students registered with only 53 jobs on offer. And that wasn't just in universities. That was across government laboratories, private sector industrial laboratories, and universities. So you have this unbelievable collapse of the physics job market as a coalescence of these forces, political, economic, cultural, institutional. So that together helps account for. I think this very sudden and dramatic fall. Which again, every field of study went through some fall, but none so steeply or so quickly as physics.

So we pause there and ask for some questions and we'll talk briefly about the last part. Oh, good. DA asks in the chat, how did the government cultivate interest in physics? Great question. Again, some wonderful things written about that. I've written some. Many colleagues written about it.

One of the things that I find really astonishing about this time period is that physicists, if you can believe it, it will your imagination, became kind of rock stars. They were featured on the covers of *Time Magazine.* The word that was often used at the time was they were lionized, which is to say they were treated like celebrities speaking to League of Women Voters Luncheons, speaking to civic groups paraded into school auditoriums or assemblies for L23 students.

In 1961 I think it was '61, *Time Magazine* named a dozen physicists, all white male physicists, people of the year. Time magazine for years ran the Person of the Year Award. And in '61, it was physics as embodied by 12 faces of renowned physicists as the people of the year. That same year, maybe 1963, in Gallup polls of the American population with public opinion polls, people were asked to rate the three most favorable professions. Three professions that are seen as most highly respected among the general American public.

Number one was Supreme Court Justice. I don't know if that would be the same way today. But 1963, that was seen as above the fray in the most kind of laudatory role someone could aspire to. Number two is medical physician. Number three was nuclear physicist.

So it was a combination of a sense that nuclear physicists had single-handedly built the bomb and, therefore, won the war, each part of which I think we've seen in this class was maybe a bit of an oversimplification, combined with this really kind of hero kind of treatment of young physicists, even those who had had no role whatsoever in the war. So there was a moment in time when physicists in the United States were treated really as kind of celebrities and also a very, very valuable resource in all too short of supply.

So you have a lot of these cultural reinforcements that then get overlaid upon this kind of more politically driven post Sputnik cry to get lots and lots of people in defense, because the Soviets are somehow ahead of the United States in military technology. So that was at least the allegation.

Fisher says it looks like the number of PhDs over time to the 1980s-- yeah. So I'll show later on what the graph looks like later on. That's right. And Jade perhaps rightly or wistfully says, a brief shining moment. You may recall from the very first lecture for this class, literally the first day, I shared with you a quotation from *Harper's Magazine* in 1946 or '48. Late '40s.

And the columnists had said that physicists are in vogue these days. No dinner party is a success without at least one physicist. I always say, boy, you know something's changed if that was what people thought in the late '40s about physicists as the ideal dining companions.

So a lot was reinforcing. There were overt incentive structures for kind of political decisions. And they were often in tandem with or reinforcing a kind of broader, maybe more diffuse kind of cultural notion that physicists, in particular nuclear physicists, were somehow at the top.

Let me go on to the last part today. This is the part that's closest to the reading. And so I'll go quickly through it, but I do think it helps make sense of some of the trends we've been thinking about in the class so far.

And that's to ask what happens to the study of physics when the number of people pursuing it grows exponentially? How does the world of ideas begin to react or change, not just the kind of enrollment patterns or degrees conferred? I love this photo. I always show this photo. This is actually Nobel laureate Val Fitch, a particle physicist, who about to be crushed to death by the *Physical Review.* It's like death by journal. Many of us think that scientific journals are deadly boring, but this is actual a weapon.

So this is now stacked by decade. The journal began in the 1890s, 1900s, '10s, '20s, '30s, '40s, '50s, '60s, '70s. By the 1970s, there were 30,000 pages published per year across all the phys rev journals as opposed to a tiny fraction at that point. And can see they're tracing out basically that same exponential curve as the number of new PhD students. Of course, they went together. All those PhD students had to write a thesis on something and publish it.

So I wanted to ask what's it like to be a physics student under these two very different regimes? So let's try to look at apples to apples as best we can. Here's one instructor who happens to be a rather popular one named Richard Feynman, teaching at Caltech. The first one here with his necktie on in 1962, much like those photos of Tony French here at MIT. Very large stadium tiered auditorium seating with hundreds of students.

And here just 13 years later after that bust teaching admittedly a different course but what he used to call his physics ex course with about a dozen students. Much has changed. He's lost his necktie, his collar, and his pants cuffs are both much more wide. Feynman's own hair is longer. I don't know what's going on with these feet on the desk. Things have changed, again, culturally as well as kind of numerically.

What's it like to become a young physicist under these two quite starkly different sets of conditions? And I wanted to track that by looking at the training of first year grad students in a particular subject that's near, I think, to many of our hearts, quantum mechanics. This is exactly the period in time when at least one course in quantum mechanics became required of all grad students in physics in the US. It was an elective in many places until soon after the wall. So we have one kind of way to, again, compare like with like.

And so this is a topic that, again, as we've seen in our own class, had been the subject of or treated to overtly philosophical discussion by many, many leading physicists who helped invent quantum theory in the 1920s and '30s. We looked a bit at things like the Einstein board debate. It turns out they filled their own textbooks-- they didn't fill, but that philosophical mode of interpretation shows up in their explicitly pedagogical materials as well.

Now, they would disagree about which philosophical tradition they found most helpful. Some turn to the German idealism of Immanuel Kant. Several turned, like Einstein himself had done to Ernst Mach, all kinds of favorite philosophical or interpretive traditions. But they all agree that this was part of what it meant to be a physicist and part of what it meant in particular to learn about quantum theory.

Before the Second World War, this part actually surprised me, that was also true in the United States. If you look at all the leading instructors in the new quantum theory in the US, the early textbooks, their own lecture notes that survived, student reminiscences, homework problems, and so on, that there also was an explicitly kind of interpretive or philosophical approach to quantum theory.

Again, they would often appeal to different philosophical traditions than many of the continental Europeans. Rarely would they appeal to Immanuel Kant. But there was a notion, an explicit one, set in the opening prefaces of the famous books by these authors and throughout the lecture notes from Oppenheimer's very famous course at Berkeley that part of the job of reckoning with quantum theory is adopting an explicitly philosophical mode to try to understand these kind of puzzles, like things like Schrodinger's cat or the restriction to probabilities and determinism, things that we at least talk briefly about in our own class here.

Well, my favorite source of that is a series of notebooks that are preserved in Caltech's archives. They span from 1929 to 1969. Unbroken 30 year period. These were communal notebooks passed down among the physics PhD students and then finally grabbed and preserved 30 years later. When the students would narrate how they prepared for their general exam was an oral exam only for the PhD students. No written exam.

So the students would say I studied this and this and here are the questions I was asked so that you could pass it along and help your friends who would come to the exam a little later. So read these accounts of the exam at Caltech for your PhD qualifying exam in physics. And in the '30s, indeed, they were pressed to calculate certain things but also pressed to give kind of interpretive answers to some of these quandaries of quantum theory. And likewise, if you look at lecture notes, textbooks, book reviews in the '30s, you see that rapidly as well.

What happens very quickly after the end of the war is that that just vanishes in the United States. I mean, it just vanishes very quickly. Again, many ways to measure this. One of my favorites, a statement from MIT's own Herman Feshbach, very, very prominent nuclear physicist, helped found our Center for Theoretical Physics, very prominent. And he wrote in '62, in his words, enough with this musty atavistic to do about position and momentum. He's like, enough fretting over the kind of philosophical implications of the uncertainty principle. We have all this stuff to calculate.

And a kind of common phrase that was uttered kind of half jokingly at the time was the phrase shut up and calculate. Meaning don't stay up all night wondering about the grand mysteries of quantum theory. Learn how to do your problem sets, which now were winnowing to be almost exclusively calculation and not kind of verbal or essay like response.

So again, we can go back to those notebooks from Caltech, these kind of continuous record of the qualifying exam for the Caltech physics students. And you see some of these entries in the early '50s of students who are kind of shocked. They actually feel cheated because they'd studied years and years of these older accounts of the types of questions they would likely be asked on the exam.

And then they didn't show up when those students in the early '50s actually sat for their exams. They write things like the effort invested in analysis of paradoxes and queer logical points was of no use in the exam. The student was only asked to solve certain problems. Another student says, the best advice is to memorize and rehearse the stock problems or give what he calls the usual spiel. They will not ask you to wax poetic about the fall of determinism, whereas they had really been asking that question only a generation or two earlier.

Likewise, the newer textbooks now get praised in the published book reviews for, quote, "avoiding philosophical discussion or omitting this distracting philosophically tainted questions." You see this very stark shift also in other written exams and problem sets across the country. So between the '30s and the '50s, something really stark has changed.

How might we account for that shift? We might say, well, these were open questions in the '30s, but maybe they were just solved by the '50s. I mean, I chuckle because, of course, we know as many of us have been discussing in office hours they haven't been solved to this day. These are still open questions subject now to very active research even in the United States and even then in the '50s were being pursued actively by some physicists outside the United States. So it wasn't that the philosophical challenges had gone away.

Another answer that I find much more compelling, much more interesting, is written by two of my own mentors. So I guess I have to like it, but I do. Both Sam Schuaver and Peter Galison, among others, have written that maybe it was what happened between the '30s and '50s that could account for the change. In particular, this really massive, unprecedented effort by physicists and engineers and chemists and so on during the Second World War.

If you're working in the midst of factory sized kind of time sensitive, mission oriented projects like isotope separation, like radar, you don't have time for kind of philosophical niceties. And both Sam and Peter find just really compelling examples of physicists saying stop doing what we used to do. There's a war on. Get the numbers out was a phrase that Sam in particular would use to characterize this period.

And yet I found from going through the archives and collecting unpublished lecture notes from physicists at a range of institutions during the '50s, not just during the '30s, that that wouldn't account for the shift. The folks I was looking at were often highly placed veterans from the wartime projects, people who had very extensive roles at Oak Ridge, Los Alamos, and the Rad Lab during the war, and yet went back to their home institutions after the war and taught quantum mechanics in this comparable kind of overtly philosophical vein like in the '30s. So it can't be that somehow the war was this unique explanation, even though I do think there's a lot of compelling evidence about a shift.

Maybe we might say after the war, it was a change in who wanted to hire these physicists. After all, 1953 was the first year in US history in which more physics PhDs were hired in industrial jobs than academic ones. So the industrial research in physics was really growing rapidly in the United States in the '50s.

And again, maybe if you're working at Westinghouse or Bell Telephone or many, many others, maybe you don't need to worry about the kind of epistemological philosophical questions about Schrodinger's cat. And yet because of the Cold War fears about where the physicists were and how they could be tracked, we have an enormous amount of information about this generation, where they were trained, where they got their jobs, who moved to what position when.

So you can actually do a statistical sample and find there was literally no correlation. In fact, some strong anti-correlations between where an individual got jobs, got a position after PhD, and the type of course they had for quantum mechanics during their graduate work. This was not a kind of job market driven shift either.

So what it led me to, and this is where you'll read in the piece, was to ask about is this a kind of legacy of bloated classrooms that were changing very, very rapidly? So if so, how might we assess that? And here I'll boil down a lot that's written a bit more in the essay. One thing I did was collect all of the lecture notes I could find, mostly unpublished lecture notes, from the 1950s from, again, this same kind of course. First year graduate level quantum mechanics to try to compare apples with apples.

Then separately assess the enrollment of those courses. Sometimes it was actually in the archives. I could see Hans Bethe's grade sheet. I know which of his students got a B+ and I know how many students were in the class. Other times the University registrar's still have records or other ways to get this. So you can look for correlations between class size and the nature of discussion of the quantum theory in those classes.

And again, to boil down a lot, to be kind of overly precise, these numbers should be read with grains of salt. But a shift in a factor of 3 in the average enrollment size, a larger class by a factor of 3, was accompanied by a fall by something like a factor of 5, by a very dramatic, very noticeable shift, in the number of, say, pages of the lecture notes devoted to anything we might generously consider philosophical, speculative, or interpretive.

So that's one moment in time over the '50s. Then I also wanted to look across time, what historians would call diachronically. So with the help of several grad student research assistants, I looked at all of the graduate level textbooks on quantum mechanics that were aimed at that same audience, first year grad quantum mechanics courses. All of them that were published in English during this 30 year period after the war.

And then we looked at all of the homework problems that appear in those books. There's 6,000 homework problems. Actually there's, of course, many redundancies. The same problems get repeated over and over again. And again, we can look at trends and how many of those homework problems in the textbooks asked for any kind of verbal or interpretive answer. How many of them were short essay? Write out an answer in reasoning in sentences and paragraphs versus calculation and circling your answer at the end.

So what I'm showing here is the proportion of the questions that asked for an essay like a brief short answer, paragraph structured response. And it hovers at around 10% while the enrollments grow rapidly. And then look at this transition to an absolutely explosion to reaching almost 50% where kind of essay or discussion questions in these books published soon after that bubble had burst.

A couple of more kind of archival examples. Are these general trends kind of playing out on the ground? And here's just two more that I'll share with you before I wrap up. There's one example I found again in the archives at Berkeley in the early or mid 1950s. This was a young assistant professor, tenure track professor named Roland Good, who was interested in some of the kind of foundational aspects of quantum mechanics, including quantum field theory. He was ultimately not promoted. He was denied tenure. And that always leaves a large paper trail. It's actually hard to deny someone tenure because you write a lot of reports, then as now.

And so if you look at the reports of why was Roland Good not promoted, they say that his choice of research topic was pedagogically inappropriate. He wasn't generating doable homework problems for his students, especially for his PhD students. And because Berkeley had such a huge population of overwhelmingly white male PhD students in physics, that every faculty member had to be a kind of engine, a factory producing dissertations. And Roland Good's choice of topic by being more overtly interpretive or philosophical, it was a poor fit. And so he wouldn't be promoted even though it was a productive researcher and an effective teacher, according to reports.

Last example of that. I looked just nearby Berkeley at Stanford. Their department was always much smaller than Berkeley's. But in relative terms went through, again, its own kind of boom and bust cycle. It grew much larger compared to its older kind of capacity very rapidly. And again, you see this remarkable correlation in time as incoming cohorts grow from 10's to 20's in the earlier period up to nearly 50 a year, high 40's per year.

The faculty get overwhelmed doing all these qualifying exams. They switch to a true false general exam. For a period of almost 20 years, you can get your PhD in physics at Stanford on the basis of a true false qualifying exam. I'll let that sink in.

Meanwhile, just as it did everywhere, enrollments crashed, fell by more than half in about two years. And as the numbers fall, then kind of coincidentally, or at least correlated in time, the faculty say, no, actually our students should be better prepared to discuss these things in words. So 40% of the revised general exams become essay questions. Literally write out interpretive answers as opposed to true false and so on. So I find those trends really quite striking.

So this is now my final set of slides. I'll wrap up very, very quickly. Going back to that early article that I mentioned by Paul Forman, Forman was convinced that this overwhelming military sponsorship of funding for basic research had changed what it meant to do physics. In his famous words, physicists pretended a fundamental character to their work that it scarcely had. They become merely instrumental to their military patrons. He says they sold out.

This really is a kind of one dimensional analysis that somehow money from the Pentagon changed the world of ideas. It certainly had an impact. But it seems to me that what's missing from those kinds of explanations is the way the money gets used, the decisions about what one does with this suddenly very fast changing infrastructure. How do people learn in institutions as opposed to react to budget lines?

And then we start asking questions about enrollment patterns, about the nature of the instrumentation, and about the kind of arrangement of the world of ideas. Then I think we can start asking about did the character of research and teaching start to shift during this Cold War period?

So let me pause there. I'm sorry again for running long. I'd be glad to stay longer and chat if people would like. Any other questions?

So if there are no questions, I see Jade has helpfully shared a link to a chapter I'd written that Peter Fisher shared with our department back in August. I'd be happy to share more. I wrote a lot about those reports on the Soviet training. And if that's of interest, I can share that too.

Fisher asked, was there any renewed interest in experimental physics? Yes. So it's actually this period. Very good question. It's this period in the United States that theoretical physics really begins to break off into its own subfield, considerably later than we'd seen it happen in other parts of especially in Europe.

Nonetheless, it was still a kind of predominant assumption during this time period that physics meant experimental physics, like we saw much earlier, and that maybe there should be a role for theory. The theorists were often considered so called house theorists. So you do theoretical physics by being kind of a closely coupled consultant to your immediate local experimental group. And that kind of shifts and gets renegotiated over this time period.

Another example I cut for time but I think it speaks to this point. I found advice letters from Berkeley physicists to people who wanted to enter the field during this kind of heyday in the '50s. And in one letter, a presumably very well intentioned Berkeley physicist wrote back to a student. The student explained that he was in a wheelchair permanently, had some reason to be in a wheelchair permanently.

And the physicist said, you can't have a career in physics, because he'd never heard of Stephen Hawking, who hadn't become prominent yet and hadn't been in a wheelchair yet. But also because the presumption was still physics meant experimental physics, which meant getting around these factories like at Berkeley like the bevatron. I mean, it was a kind of collapse of who's legitimate to even consider a role for themselves in physics, which is assumption both about we might now call it ableism.

There was a clear kind of bias against people with a non-standard physical situation. But also I mean, that came coupled with in this person's mind the sense that physics equals experimental physics which equals somehow lumbering around kind of large equipment like in a factory. So again, that's one data point, but I think it's illustrative of this kind of collapse in many physicists' minds at the time about what a proper role, a proper career in physics would be like. And of course, that changes. That changed over time, both about the kind of demographics, but also about theory as an autonomous thing.

MIT Center for Theoretical Physics was among the earliest founded as a standalone center for theory. And it was founded in 1967 or '68, I can't remember which. Maybe Julian might remember. But late '60s. So that's a kind of marker point for when this was seen as, oh, this is a separate thing. It needs its own space on campus. So anyway, so that's a great question.

OK, I've gone for a long time. Sorry to keep you all long. Stay well. Hang in there. And I'll see you all on Monday. Stay well.