[SQUEAKING]

[RUSTLING]

[CLICKING]

DAVID KAISER: So today we're going to continue the story that we were looking at in on Monday's class, some of these early developments after the end of the Second World War to grapple with quantum field theory, trying to piece together a quantized treatment of things like the Maxwell field or electrons and positrons. And we saw last time there were a lot of puzzles, some of which had been identified as puzzles well before the war, going all the way back to the 1920s and '30s, like virtual particles that could borrow energy for temporary periods of time, which led to these infinite quantum corrections that when people tried to take into account quantitatively the impact of those virtual particles, their equations would blow up. They would encounter these divergences.

And those were a big, big puzzle really for decades. And it was only very soon after the end of the Second World War when a newer generation of physicists came back to those older questions but now with some new skills, sometimes literally new instruments, and new ideas about how to organize their calculations. So near the end of last class, we looked at some of Julian Schwinger's efforts in that, and today we'll look at some efforts by his contemporary, Richard Feynman and Feynman's circle.

So today's class draws on a book I wrote 15 years ago. It's actually hard to believe it's been out for 15 years. And so we'll talk about the book and how it fits into the kind of series of developments we've been talking about throughout the semester. And so as usual, the class has three main sections for today. The first is how did these new tools, in this case Feynman's particular approach to quantum field theory, how did these new tools enter into circulation? How do they become kind of second nature or a standard technique ultimately for essentially the entire profession?

So how did these new tools begin to get picked up by people other than Feynman himself? And we'll spend a good chunk of time on that today. And then we'll see that once some people did start using them, they would often deploy these new tools, these paper techniques, pencil and paper techniques often in quite different ways than what Feynman himself had had in mind.

So it's not just that new people had to start using the tools. New people began adapting the tools toward new or different ends. And what did that mean for the continuing conceptual puzzles they were wrestling with as they tried to continue to make sense of high-energy physics phenomena?

And then finally, we'll ask just very briefly near the end why did this particular set of tools actually stick? Why was it that these tools really became the standard way to this day that physicists all around the world try to make sense of nature when it wasn't so clear they were going to take off at all? So why did this tool come to seem like such a natural or winning strategy? And that's our kind of three-part scope for today.

So some of you, many of you, by now might recognize these things. You might have already had them in some classes. If not, you've probably seen them around, these things that we now call Feynman diagrams. They are really unavoidable today. They are literally ubiquitous.

In fact, not only are they all over the place, they've become automatic or automated. So there are now computer routines that will both evaluate tens of thousands of them now even on not very fancy laptops. So you have these algorithms to compute very large numbers of Feynman diagrams quantitatively and also routines to draw them. They literally have-- the way they look has now become automated or subject to computer routine as well. They're all over the place in virtually every subfield of the discipline or many, many subfields anyway.

And yet, of course, they have a history. It wasn't always that way. Our kind of present day saturation with these things is of relatively recent vintage. And when they were first introduced in the years through and after the Second World War, some people thought they didn't make any sense. Others thought they must be all wrong, and a small number of people began using a couple of them at a time.

So it was a publishable feat to evaluate five of them by hand, not 10,000 on your laptop. You could get your PhD dissertation by evaluating a few dozen at most by hand into the early 1950s. So these things weren't these kind of automated tools from the beginning. In fact, they were kind of suspect, at least to some people, for quite a while.

And as I mentioned already, they're kind of single-use. The fact that now you can train computer algorithms to evaluate these things unambiguously that, again, took quite a while to settle into place. And in fact, for two decades or more, even arguably about close to three decades, what the diagrams were used for, how they would be turned into elements of calculations and quantified, that was actually pretty malleable for, to me, what seemed at least to be a surprisingly long period of time. They were adapted as well as adopted.

And so before I started working on this kind of series of puzzles, how the diagram spread, why did they come to be used, and so on, very little had been written about the spread of the diagrams. I used to tease that there had been more written at that point about the search for Richard Feynman's actual van. This is Feynman and his then very young children with his van that he used to drive around Southern California and with larger than life Feynman diagrams on the side. There was more written about his van than about the diagrams that bedecked the van.

And so how was it that this was even something you'd paint on your van in the first place? Not only because it was the groovy '70s, and you're in California. Why would these become something to paint on your personal vehicle? And so that's what I was really kind of curious to try to learn more about.

So to make sense of that, I wound up thinking a lot about this word dispersion. I think it's-- I still think it's helpful to organize the kind of different layers of this study. So disperse means many different things, the verb to disperse, where there are two main clusters of meanings. If you go look up, for example in the Oxford English Dictionary or your favorite Merriam-Webster or whatever you like, there are kind of two basic sets of meanings for the verb to disperse.

One of them means to put into circulation. Think about dispersing things to people. And that's really the first part for today's class that we'll focus on. But disperse also means to cause to become basically more and more different. Think about optical dispersion, taking light and dispersing it into the distinct colors of the rainbow.

And I think that also captures a lot of what was going on with these new theoretical tools, not just for a month or a year, but really over decades even after Feynman had introduced them. So that's what set up these three questions that I mentioned earlier. How do they spread so fast? What do people actually use them for? And then given that variety, why did they stick?

So let's talk about how they start entering circulation in the first place. Let's dig into that a bit more. These were introduced at the second of this three sets of annual meetings that we talked a little bit about in the previous class. You might remember on Monday, I talked a bit about the June 1947 Shelter Island meeting. In fact, this photograph was from that Shelter Island one, from the first of them held just on a tiny island near Long Island at the Ram's Head Inn.

I've never been there. I don't know if it still exists. You got to go to the actual bed and breakfast where they all stayed. And that was the meeting at which 24 kind of personally selected participants, selected by Robert Oppenheimer, got together for a week to talk about theoretical physics, including these puzzles of quantum field theory. That's where Willis Lamb first presented his results on what would come to be called the Lamb shift. Isidor Rabi talked about the new experimental measurements on the anomalous magnetic moment of the electron.

And there were three in a row, so every annual meeting. The second of them was held not in Long Island but in the Pocono area, so due north of Philadelphia, due west of Manhattan, a short bus ride out of Manhattan, again, organized by Oppenheimer, very similar format, roughly 24 or 25 kind of hand-selected participants, many of whom had come to the previous one.

This is the first meeting of its kind after the experimental results for things that seemed to suggest at least to some people that virtual particles might actually have some heft to them. They might actually affect measurable quantities. So it was at this meeting, the second in the series, where Feynman introduced his new techniques. He was one of the informal speakers.

This is actually from his first published example. We know from notes that were taken, he was scribbling very similar things on the blackboard, and he was trying to motivate for his colleagues how they could try to understand at a quantum mechanical level why it is that, for example, electrons repel each other. We all learned in high school that like charges repel and opposite charges attract.

He said, well, why is it if we think at this quantum mechanical level that charges with the same sign of their electric charge would push each other apart, would repel each other? And so here's the kind of story that we could imagine unfolding. There would be some electron coming in here from the right hand side. It would have some likelihood to travel from here to here.

It has some separate but calculable likelihood to emit a virtual photon at this point in space. You can't turn off. In certain principle, the vacuum is roiling with virtual particles. By this time, that had become kind of familiar set of ideas. So he wanted to have a way to calculate these effects.

A little while later, that virtual photon would have zoomed across space, and it could smack into this other electron. The second electron, which had been otherwise traveling all by its lonesome unsuspected, going from, say, 0.1 to 0.5, all of a sudden it will get smacked by this virtual photon that is carrying some energy and some momentum. It's absorbing that momentum.

So after it absorbs that virtual photon, it will be knocked off its course. Imagine getting hit with a dodgeball. Not that I want to trigger bad memories of elementary school, but anyway, you get knocked off your course. You get hit with some object carrying momentum. So its direction of travel will change.

Meanwhile, the original electron has shot out that virtual photon. It recoils like a hunter firing a rifle. It's going to have some equal and opposite kickback, so its direction of travel will change as well. So Feynman says here's the kind of fundamental process or the reason quantum mechanically why like charges repel because they're trading back and forth these virtual force-carrying particles, in this case, the photon, the virtual photon.

Now he could use this kind of map of that interaction and relate every single part of this little squiggle, this line drawing to particular parts of the integrals. And so he would map this, and he tried to explain this at the blackboard. The electron has some likelihood to travel unimpeded from 0.2 to 0.6. Here it is. The other electron, electron A, let's say, has some likelihood to travel from 0.1 to 0.5.

They have a likelihood to both emit and absorb a photon, each of which is proportional to the charge of the electron. The photon itself has a likelihood to travel from 0.6 to 0.5. You just piece it all together kind of LEGO bricks.

So now you have a kind of road map to the corresponding equations to map out this series of steps between electrons and their traded virtual particle. And Feynman said this is going to be important, not just because this is one thing that could happen, but because all kinds of things could happen when we start thinking about virtual particles. It had already become clear well before the Second World War that these virtual particles could borrow any amount of energy and momentum, consistent with paying it back sufficiently quickly given the uncertainty principle.

But it was even more complicated. The electrons could trade more than one virtual particle. So instead of just saying the amount of energy that this one virtual particle borrowed was uncertain and in principle could be infinite, what if the electrons had traded two photons, two virtual photons, each of which had temporarily borrowed energy and momentum from the vacuum? Well, now it starts getting pretty tricky because there are actually several topologically distinct ways in which two incoming electrons could trade two photons and leave only two electrons coming out.

So instead of only one storyline that you have to map into a line of algebra, you actually have all these different ways in which two electrons come in, two electrons go out, and in between, they've traded two virtual photons. And so the diagrams were literally a form of bookkeeping. And again, you could walk through the same series of steps to map every element of these slightly more complicated Feynman diagrams to the corresponding algebra.

And you can do the same with three virtual photons or four. In principle, there could be an infinite number of virtual photons, each of which could carry, in principle, an infinite, albeit short-lived energy momentum. So you see the bookkeeping becomes like mind boggling, right? So this is a kind of way to march through the corresponding equations.

Make sure you don't omit any terms. Make sure you don't double count certain terms, which have been bedeviling these calculations throughout the '30s. And so now you have a map to make sure you at least have the equations, the correct set of equations to worry about. And then by this point in late spring or early summer of 1948, Feynman could show that if you have the right bookkeeping, you see some of these infinities actually exactly cancel each other.

So some of the infinities that had cropped up in these earlier calculations, not all, but some of them would exactly cancel, even before you got fancy with renormalization just because you have to remember that for every diagram like this, there's a corresponding one like that and so on. And some of these have exact cancellations. So the challenge of taming those infinities would become a bit more manageable if you could make sure you had the right set of equations to work with from the start.

So how did that go? That sounds awesome. You have this amazing, kind of intuitive, very simple looking technique. This used to occupy literally 30 and 40-page journal articles. When people tried to do this without the diagrams throughout the 1930s, they'd always find these infinities. You think, oh, our hero's in town. Finally, help us on the way.

It didn't play out that way for our poor MIT alumnus Richard Feynman. The first thing to keep in mind is that his presentation followed an eight-hour lecture by Julian Schwinger. I want to let that settle in for a second, an eight-hour lecture. That's like unlawful imprisonment. Now, I often run along with my lectures here, and I feel bad. I go like three minutes late, and you're all busy. But you know, anyway, there are limits.

So why was Schwinger given so much space? In between the Shelter Island meeting and this follow up one in Pocono, it was Schwinger who had first found a way to tame these infinities. He first calculated with his new techniques that did not involve any of these diagrammatic methods. He not only found a way in principle to respect the symmetries of relativity and find finite answers with his renormalization program, he'd also found right in between, six months between these meetings. In December '47, he published the first kind of successful demonstration that renormalization could yield finite results that actually match unbelievably closely to these latest high precision experiments.

Schwinger was the person of the hour, or rather, the eight hours. So it was after that they had a brief break for lunch, and they came back, and Schwinger talked for another four hours afterwards. And then Feynman had 30 minutes at the blackboard at the end of that day. All right, so it's not like great setup.

Moreover, he was completely unprepared. He did not have a well-polished presentation in hand. He just wanted to wing it, which is not a good thing to do at the end of a busy day. It was so-- it seemed at least to some of his listeners to be so scattershot and so disorganized, that not only was he repeated all the time. At one point, Niels Bohr, who had been invited to participate, one of the few older European physicists, Bohr got out of his chair, walked up to the blackboard and took the chalk out of Feynman's hand and said you don't understand quantum physics. Not great. God, I wish I'd been able to see that.

So Bohr says not just this is like a disorganized presentation. He says your whole framework violates the uncertainty principle. What does it mean to draw a picture of an electron moving from here to here? Bohr says, in effect, you've assumed that the electron has a well-defined trajectory. But no quantum object has a well-defined trajectory because to do so, it would need to have arbitrarily precise position and momentum at the same time. But we can't have that because it's [INAUDIBLE]. Bohr is like this is a nonstarter from the beginning.

Feynman's a little flustered. It's not literally trajectory. Oh, let me go on. OK. Paul Dirac was there. Dirac says, well, when you calculate the probabilities with these new diagrams, do they obey unitarity? To which Feynman's response was, what's unitarity? Not good. OK, this was not a great presentation for Feynman.

Unitarity is, again, some of you might know, you might have found in other classes, it's really a fancy way of asking, does the sum of all these probabilities add up to 1. There's a 100% chance that something will happen, and it might be 35% chance of that and 42% chance of that. Do you have to add all these up? Does the sum of all these probabilities obey the conservation of probability? Does it add up to 1? That's the fancy way that's the simple version of what Dirac was asking.

And Feynman hadn't even thought about the question. He certainly had no answer ready yet. Edward Teller was there. He says what about when you have a pair of electrons in these intermediate states? Two virtual electrons, let's say, do they obey the Pauli exclusion principle? Another kind of cornerstone of quantum physics. And Feynman's response was that's a really interesting question. I'll go think about it. Again, he had no answer.

So in general, what this very esteemed but small kind of elite audience kept asking during this scattered presentation of Feynman's was what rules govern their use. It really looked like he was kind of making it up as it went along, and he was not able, at least at that moment, to articulate the kind of rule-bound techniques that would perhaps yield as a comparable set of calculating techniques to Schwinger's very pristine, very precise algebraic approach. So by many, many accounts, Feynman left this meeting disappointed, maybe even depressed. He really-- was not a great start for these new techniques, and he was really down.

Now, one might have thought that all he had to do was kind of collect himself, write up some clear articles. Just say point by point first do this, then do that. Here's where this comes from, give a derivation, give a demonstration. He could have cleared this up, one might have thought, by writing some kind of clear articles.

Well, he did write some clear articles, and yet what, again, I found really fascinating was that kind of confusion about the diagrams lingered for years afterwards, not just for days, weeks, or months, even years after his articles had been in print. And so I won't go through all of these examples, just some examples I write about in more detail in the book. But you have people either writing directly to Feynman saying, I can't do this calculation. Will you do it for me with your trick and tell me what you get? And then they thank him in the acknowledgments.

There's some interesting correspondence circles with people not including Feynman from very, very elite, very well-trained experts in the field who three of them try to do the same calculation with the same diagrams, and they keep getting different answers from each other. So you have this wonderful series of letters because it was pre-email, and they were, in fact, in different countries. They weren't going to make long distance telephone calls.

So you have these letters of them trying to figure out what keeps going wrong when they each try to do the same calculation. That's as late as 1950. A few years later, my favorite example actually comes from a letter of recommendation for young physicists who was finishing his PhD at Stanford, and the student's advisor, Leonard Schiff, wrote to recommend the students, saying you should hire my student for the new professorship because he actually does understand Feynman diagrams and actually use them in his thesis. That was five years after this Pocono conference, roughly four years after Feynman's attempt to clarify the techniques with his articles.

So if four or five years later, your advisor says my student is unusual because he does understand them, that seems to suggest these were not universally clear to everyone. So they were clearly not so obvious or automatic, even after Feynman did have a chance to try to clarify. And yet, you can see evidence that they're taking off like gangbusters. If you go through the *Physical Review,* and in the book I talk about similar searches in other journals throughout many parts of the world, in fact, not just in the United States, you see an exponential rise that these diagram techniques are getting picked up. The doubling time I've forgotten, something like two and a half years, or three years, where they're really growing literally exponentially over the first half decade or so.

So you have the one hand, you have people saying, I don't get it. I can't do it. And yet you find this growing evidence that at least some people have become early adopters of the techniques. You can then do the usual trick and not just say how many articles use the diagrams, but who wrote those articles? Where were they? What were they up to? And you start seeing some pretty interesting traits, some common features among the people who were beginning to use these techniques in their own papers.

So first hand, they were all young in their careers, which is to say they were all either graduate students or postdocs, the vast majority. A few of them were kind of assistant professors. They were all still fairly early in their training or careers. They were all theoretical physicists in this early period.

Nowadays, these diagrams are just as ubiquitous among experimental physicists as theorists. But in this early phase, this early kind of exponential rise, it was young theoretical physicists. And the last part was that they were really in contact with each other. You can do a network study and trace it through by co-authorship, by acknowledgments, by unpublished correspondence that I was able to find and piece together, that they knew each other.

It was a network that had come together to learn these techniques, and then they began to spread out. So something pedagogical is going on. The majority of these early users were still in the formal phase of their training. They were literally still being taught to be physicists.

And yet it wasn't a story of write a textbook, teach some classes, it'll spread. All of the data points shown on this early plot represent articles that came out before the very first English language textbooks had been published that incorporated the new techniques. So the first textbook treatments of how to calculate with these new Feynman diagrams weren't published until 1955, by which time, there was already evidence of this fast-growing exponential rise.

So how does that happen? A lot of it comes down to this gentleman, Freeman Dyson. Dyson, you might know his name, he just passed away this past March, just before quarantine in fact. He died in very early March at the age of 96, quite a remarkable life and career.

At this point, he was quite a young person. He was, in fact, still in graduate school. He was originally from Britain. He got a very competitive fellowship very soon after the end of the Second World War called a Commonwealth fellowship reserved at the time for British students to study abroad, mostly in Commonwealth countries, like Canada or Australia, but also the United States. So he got a fellowship.

He came to Cornell University because he really wanted to study with Hans Bethe, whose work even in the '30s had seemed super exciting. And this young grad student said I want to go study with Bethe. So he gets the fellowship, comes to Cornell. He starts working with Bethe but also very quickly meets Feynman, who at this point was a very young professor whom Bethe had recruited as soon as they were done at Los Alamos for the war.

So Bethe and Feynman had interacted at Los Alamos. Bethe as the senior theoretical physicist at Cornell hired Feynman. This even younger physicist Dyson meets Feynman there. The summer after his first year of grad school, Dyson and Feynman drive cross-country together. Dyson wanted to just see the country. He was from Britain. He was sending these really kind of delicious letters back to his family, often writing to his mother on a manual typewriter, a small portable manual typewriter, often more than once a week.

And he just wanted to explore the country because no member of his immediate family had ever visited the United States. He was very curious. And Feynman wanted to go back to New Mexico. He had a girlfriend out there. He maybe was going to do some consulting at Los Alamos.

So they basically drove cross country together that summer of '48, which is to say just weeks after the Pocono meeting. So Dyson had not been at Pocono. He'd been hearing about these things in Ithaca from his new friend, but now he had a several days long drive to just pick his brain.

Meanwhile, once they get to New Mexico, then Dyson took the bus to go to Ann Arbor, Michigan. Again, he was like being a tourist. He then spent about six weeks at University of Michigan attending summer school lectures by none other than Julian Schwinger, where, again, he had a chance to talk directly with Schwinger at some length and some detail, where Schwinger was lecturing on his own approach to quantum field theory and renormalization. So at this point, by the end of the summer of 1948, Freeman Dyson was the only person in the universe who had spent extensive close informal personal time with both Richard Feynman and Julian Schwinger, just at the time they were each working on their separate and very different looking approaches to quantum field theory and renormalization.

Then he takes this now famous bus ride back from Michigan, back to the East Coast where he was going to start for the second year of his fellowship at the Institute for Advanced Study in Princeton. By the way, he never finished his PhD. To the day that he died, he was Mr. Dyson. And he thought PhDs were a horrible waste of time. He might be right. I don't know.

Anyway, so he spent one year as a grad student at Cornell and one year on fellowship at the institute and then was hired as a full professor at Cornell like four years later, this guy. Anyway, on the bus ride, he has this amazing epiphany that he starts seeing how the two very different looking approaches might fit together, Schwinger's very formal looking approach to a kind of algebraic, almost axiomatic approach to quantum field theory and renormalization and Feynman's diagram-based, bookkeeping, more pictorial, somewhat more intuitive approach.

The two of them, meaning Feynman and Schwinger, couldn't understand what each other was doing yet. And so Dyson is the one who actually puts it together, a good chunk of it literally on the bus heading back east. Once he gets to the institute, he then submits two articles that autumn. He sends both of them to the *Physical Review.* These were published before Feynman's own articles had come out.

Feynman was actually quite slow to write up his articles. They came out a little bit later, also in 1949. In the early years, Dyson's papers were cited more often than Feynman's. They were getting a lot of attention, in part because he was so methodical in laying out a kind of point by point almost recipe book, a rule book for how to use them, exactly the stuff that seemed to be missing from Feynman's presentation at least at Pocono.

Dyson was able to ground the diagrams in more first principles derivations and really go through worked examples. And it was in these papers where Dyson demonstrates the mathematical equivalence of the three separate methods that by this point were on offer, those by Feynman, Schwinger, and then separately by Shin'ichiro Tomonaga, whom I mentioned briefly last time. Tomonaga was still in Tokyo. Japan was under US occupation after the war.

Tomonaga had worked out a method remarkably similar to what Schwinger later came up with. They were working totally independent of each other, partly because of the war. Tomonaga had gotten there to many parts, not all the things Schwinger did, but very similar framework and many of the same insights along the way. Tomonaga had done this starting in 1943, but the results were not known outside of Japan, partly because of the Naval blockade and so on.

One of the first packages sent overseas out of Japan after the end of the war was actually a package of manuscripts and reprints that Tomonaga sent to Robert Oppenheimer. And it was actually ferried in official kind of US delivery was from US occupied Tokyo back to the United States because Tomonaga had read in one of the new libraries in a new subscription to *Newsweek* magazine about the Lamb shift because the magazine had covered everything about Robert Oppenheimer. He was amazingly famous in the United States after the war.

The fact that he was talking about the Shelter Island conference became newsworthy. He mentioned the Lamb shift. This gets all the way back to Tokyo through this kind of crazy convoluted system. Tomonaga reads about it and says, hey, wow, and digs back through some of his old wartime manuscripts and reprints and sends materials to Oppenheimer, who then had them effectively mimeographed and shared with all the participants who'd been at Shelter Island.

So the US-based and some of the Western European physicists who had heard about the Lamb shift and Rabi's group's measurements and so on, they learned in the days after Shelter Island about some of Tomonaga's work. Then they helped arrange for English language publications. They were very, very eager in fact to try to get Tomonaga credit, partly because it was remarkable, partly, I think, because there was a sense that this was a dramatic development coming from the city that had just been subjected to relentless firebombing during the war. But nonetheless this work somehow got done in the midst of horrible conditions.

So Oppenheimer worked hard to get Tomonaga's work into circulation. So Dyson learned of it. And Dyson was able to show in this pair of articles from 1949 that these three distinct approaches to quantum electrodynamics were, in fact, mathematically equivalent very, very much like what had happened 20 years earlier between Heisenberg's and Schrodinger's approaches to quantum theory.

So Oppenheimer at this point had become director of the Institute for Advanced Study. He was now basically, in effect, Dyson's supervisor for the second year of the fellowship. Oppenheimer was not that impressed with the Feynman diagrams at first. In fact, he used to come to Dyson's informal presentations and interrupt and basically show that he was still the smartest person in the room.

So the postdocs at the institute asked Dyson to repeat each of his many hour seminars in private, and they just wouldn't tell Oppenheimer. So Dyson would give these announced seminars. Oppenheimer would come and essentially kind of heckle and disrupt, and they'd have Dyson give the exact same talk the next day in secret. And finally, after Hans Bethe intervened, Oppenheimer said, OK, OK, I relent. In fact, he literally wrote "nolo contendere," like I don't contest the charge. I give up. And by a few weeks into Dyson's fellowship and after that, the diagrams really began to take off at the institute.

So to understand what's going on in this very specific setting at the Institute for Advanced Study and all these new postdocs, again, it's important to step back and understand this is happening in a very particular institutional setting. And again, I found this really kind of fun to dig into when I was working on the book. The idea that young scientists will typically do one or two or now, not infrequently, three separate postdoctoral appointments after their PhD, but before they take on either a faculty job or a research job in industry or anything else, that's now pretty routine today across the sciences, in fact, even in more and more of the humanities and social sciences.

The postdoc stage is pretty kind of self-evident, or we've gotten used to it by and large. But again, that has a history. And it's not a very, very long history. The idea of the postdoctoral fellowship really dates to the early years of the 20th century, very soon after the end of the First World War, but they were still pretty rare. They were very elite.

It was a big deal if you got this fancy postdoctoral fellowship after finishing a PhD. And in fact, even after the Second World War, only roughly 16% of the US-based PhDs actually did a postdoc, just did one postdoc, let alone two or three. But the numbers began to grow very rapidly after the war. They became more and more common.

The idea going right back to the 1918 proposals and 1920s and so on was these postdoc positions were designed to foster a different kind of training compared to the PhD. That was the hope, the ambition that they should foster what historians and sociologists often call tacit knowledge, not explicit kind of book learning, but actually stuff that you know by doing. Even for theoretical physicists who aren't like building apparatus or glass blowing or eventually learning to solder, there's a kind of knowledge that comes at a kind of intuitive level by practice, which is why we work on problem sets all the time, rather than only reading textbooks and dealing with kind of formal levels or explicit levels of instruction.

So the postdoc was meant to complement the kind of more formal, more book learning explicit training of the PhD with this more informal tacit knowledge. Moreover, especially in the United States, they were often funded by private foundations and, again, a kind of patriotic ambition to improve and build up US domestic scientific talent and to share the wealth.

So you'd have these young people getting PhDs at one school. They would get a fellowship to become a postdoc at a second school. They could bring what they've learned to the new place. They'll learn new things at the new place themselves. Then they'll hopefully get a job at a third place. So you start building in an emphasis on circulation. So that it was hoped would help knit together the US scientific community even more tightly and build in this kind of sharing of skills, especially this tacit knowledge stuff.

So after the Second World War, this became especially pertinent for training young physicists in theoretical physics in the United States, which it's still been, it was felt at the time, kind of lacking and not nearly so developed as in other parts of the world. Mostly they had their eyes on Western Europe. So the most important place where this begins to really change soon after the war is this place where Dyson had wound up, the Institute for Advanced Study in Princeton, New Jersey. It's near Princeton University, though it's independent from the university.

Oppenheimer moved there just soon after the end of the war. He became the new director for the institute starting in 1947. Partly so he'd be closer to Washington DC, he was still doing a lot of consulting for the government and so on. So one of the first things he did when he moved was jack up the number of theory postdocs by 60% in one year. He really said I'm in charge now. And rather than support other things that he might have done with the institute's budget, he said the most important thing we can do for the scientific community is really kind of rapidly expand the number of slots for young postdocs in theoretical physics, not coincidentally his own field.

A lot of people at the institute didn't like that at all. But that's what he did. And he did that year after year. And he was-- I love this letter he wrote to Wolfgang Pauli, who at this point was back in Europe. Pauli had spent a good chunk of the war years at the institute, in fact, visiting and avoiding some of the worst parts of fighting in Europe. By this point, he was back in Europe, and Oppenheimer explained that the institute by this point is not a school in the sense that even the younger people are not listening to lectures or working for doctor's degrees, but it is a school in the sense that everyone who comes learns parts of physics which are new to him.

And it turns out that was especially happening once Dyson arrived among the very first of these new cohorts, the very first of these rapidly expanded groups of 8 to 10 to 12 theory postdocs at a time, as opposed to only one or two as before. So by the time Dyson and his first cohort arrived as the new postdocs, although Dyson was actually not a postdoc, he never did a doc, but he was joining these postdocs. The new building that would house him actually was still under construction, so they all shared one office.

In fact, they shared Oppenheimer's office. The director's office was big enough for 12 postdocs. So some things don't change. Oppenheimer was in Europe for much of that fall. So they were literally kind of sharing desk space. They were completely kind of elbow to elbow informally sharing space.

And then the postdocs would circulate through what Oppenheimer loved to call his intellectual hotel. You don't move there. You stay there for a visit, and then you go somewhere else. They would tend to stay for two-year stays. And while they were there in these early years, Dyson would coordinate these kind of little teams or groups of calculations because he was coaching them in these new techniques for the Feynman diagrams. And again, the word of that began to spread, so Pauli writes back from Europe, what are Dyson and the rest of the Feynman school working on? It was already becoming clear that they were working very hard on that.

So when you go back to that curve of these exponential rise of who's using the diagrams, it starts to make a little more sense. It really was a social network. The overwhelming majority of those early users were either direct members of these postdoc cohorts at the institute, who learned and practiced the techniques directly from Freeman Dyson, just as he was working them out himself, or they were students or immediate colleagues of those postdocs, once they took up their first teaching jobs and moved to other universities around the country. The remaining 20%, I mean, not much more than a rounding error of those early articles that used the diagrams had come from Feynman or from Feynman's immediate circle. So we really have kind of two lightly connected networks that dominate the early adopters.

And so you get these airline flight maps. This is not literally the Feynman diagram case. I just took it from Google Images. It is a map of domestic air travel route. But it was very similar how the diagrams themselves spread out. You can trace through who's publishing with the diagrams.

We have their articles. We can see where they were from their kind of byline. Then look up where were, when did they interact with, say, Freeman Dyson, when did they attend this lengthy set of summer school lectures, and so on. And you start getting this kind of connected graph of how the diagrams spread throughout the United States.

Let me say a little bit more about the spread, and then we'll pause for some discussion. So that works-- I think that helps account for how these techniques spread so kind of anomalously quickly within the United States. Go to the *Physical Review.* At that point, most articles in the*Physical Review* were contributed by physicists in the United States. That's certainly not the case now, but back then, that was a pretty good reflection of US-based physics. We get a sense of how the US-based users of the diagrams began to fan out.

But very quickly, the diagrams were showing up in the journals from other countries as well. So it turns out in the United Kingdom, we have a replay of the same story. The rules of that fellowship that had allowed Dyson to come over to Cornell and to Princeton and the institute required that the recipients return to Britain for a couple of years. So you could go for two or three years on the fellowship, but you had to return to Britain, and you had to stay in Britain for at least two years. Those were the rules.

So when Dyson's fellowship expired, he went back to Britain. He began teaching first at Cambridge, then eventually at Birmingham, again, no PhD. He was a professor with no PhD. And when you look at who was using the diagrams in Britain, there were people who learned from Dyson once Dyson went back to Britain. So like no mystery there. It's kind of the same story.

This next part I found really interesting. I didn't know about this until my friend Kenji Ito helped me a lot with this, because he's originally from Tokyo. He knows the language, and he's also an expert in the history of quantum physics. We read some of Kenji's stuff I think for the paper 2 assignment. So Kenji helped me a lot with this. I really learned working with him.

So what happens with the spread of the diagrams in Japan after the Second World War? Here's Tomonaga, whose work I was describing earlier. It turns out Tomonaga was doing, I think, just extraordinary, kind of superhuman efforts, I think, superhuman efforts to try to rebuild the Japanese community in theoretical physics after the war. The university where he was hired had been leveled. Most of the buildings had been destroyed in the saturation firebombing of Tokyo.

So he and his little group of students and advisees would meet in his home, but his home had been bombed out. So they were meeting in his Quonset hut. He literally had temporary shelter that was not meant to outlive the war. And soon after the war, amid shortages of food and paper and clothing and much else, he was working to get his own students back on track, and they would meet together in his residence, which itself was a temporary literally kind of a shell.

And they returned to questions they'd been thinking about together, even during the war, things like quantum electrodynamics, virtual particles, and infinities. And they had developed soon after the war their own kinds of pencil and paper diagram bookkeepers, which Kenji found for me, and we wrote about them together, that these were in some of the kind of mimeographed early postwar publications that circulated in Japan. And these were in momentum space. They weren't space time diagrams.

But they were trying to do similar things. You have all these ways that virtual particles could be involved. Here are examples of a single electron that's interacting with two separate virtual photons. How do you keep track of it?

So Tomonaga and his group had invented their own techniques. Then they began getting news back from the United States after the war about Dyson's new work, not only from reading *Newsweek* magazine in the new occupation libraries but also because the more senior Japanese physicists, Hideki Yukawa, was actually invited to spend two years at the Institute for Advanced Study invited at the personal invitation of Robert Oppenheimer.

While Yukawa was visiting is when he won the Nobel Prize for his work on nuclear physics, and he was unable to send preprints and news back to his younger colleagues in Japan. So Yukawa was learning-- he was among the people who could learn directly from Freeman Dyson and this very active group at the institute and is able to share news back with this incredible, tight knit, but somewhat separated group in Tokyo. And they basically, after a few months, kind of ditched their own momentum space diagrams and began adopting the Feynman diagrams because they were already kind of primed for it. They were already immersed in a lot of these details. They already recognized that some kind of diagrammatic bookkeeping would be important, and then they were kind of quick to go.

So now you have a group in Tokyo that's really getting very intensively into this stuff. How do you spread it throughout the rest of Japan? Again, remarkable kind of coincidence. Under US occupation, the general headquarters, which is what the occupying force was called, they issued a decree right at this time to try to weaken the hold of the traditional, the so-called imperial university system throughout Japan.

So the US ally occupying authorities kind of by Fiat said that every prefect, kind of like roughly speaking every state within Japan will have at least one new university. Much like in the United States, most states have at least two public universities, Michigan State and University of Michigan and so on. They wanted to do a similar thing in Japan partly to help rebuild the country and expand higher education, partly as a kind of political move to break the hold of the elites at these imperial universities.

So suddenly, you have a tenfold increase in the number of universities that need to hire young physicists because they suddenly have to teach young students physics. So basically, Tomonaga's students disperse all through Japan because they're now getting hired in these newly created physics professorships. And again, you can chart when do the diagrams show up in Osaka. Oh, that's because this person just moved from Tokyo to Osaka or Kyoto and so on.

Last example. What about in the Soviet Union? This is now the-- Feynman introduces these diagrams, and Dyson really helps make sense of them exactly the time that the kind of broader Cold War rivalry begins to really harden into a kind of standoff that would come to dominate the next roughly 20 years or more. So this is exactly the time. In other words, when it was very hard for journals to get back and forth and even much more difficult for individuals to travel back and forth.

In fact, in this earliest period between 1948 until the death of Stalin-- he died in March of '53, and about a year later, there was the first kind of tentative person to person exchanges again between scientists. There was no possibility for the kinds of personal contact that had been so important for spreading the diagrams in the United States and Britain. And maybe therefore, it helps us make sense of the fact that no diagrams were published in any of the Soviet physics journals for several years.

There was a few months delay in Japan, which makes sense. And we saw how that was kind of overcome or what changed. It was actually multiple years in the case of the Soviet Union. When they did start showing up, there were just a trickle. There were literally 12 articles over the span of two years at the time when there were more than 100 in the US journal. And it turns out six of those 12 were submitted by one physicist.

This was not an exponential rise in the same way at the same time. That one person who did submit them, Aleksei Galanin, had very particular reasons to learn how to calculate radiative corrections to Compton scattering because he was working on the top secret H-bomb project and had to worry about shielding for that high radiation pressure inside various H-bomb designs. He had, let's just say, special incentive to learn from Dyson's articles.

When he did-- here's an example of one of Galanin's his early papers he stuck and this is not to fault him he did exactly what Dyson's articles had prepped him to do, but he didn't do any of the-- neither Galanin nor any of his immediate colleagues in the Soviet Union did any of the kind of broader more, let's just say, improvisational or more adaptive uses of the diagrams that were already becoming quite common in the United States, in Britain, and even in Japan. So you have a kind of transmission by text, it takes a lot longer, and then and the range of applications remains rather narrow.

OK, let me pause. It was a long chunk, but let me pause there. I see some questions popping up in the chat. Lucas tells me the Ram's Head Inn is still in business. So once any of us can travel, we should all arrange a field trip. I'd love to see that place with my own eyes. Thank you, Lucas, for confirming.

Alex says, "It's like when you get a speeding ticket, and you have to take the class." I'm not sure what that was about, but yes. Fisher says, I'd finally received [INAUDIBLE]. Oh, very good. So Feynman was, indeed, you know, [INAUDIBLE].

So Feynman was a professor at Cornell. He had finished his PhD. It's actually a little funny. He had finished the work for his PhD before leaving Princeton. He was in grad school at Princeton before leaving for Los Alamos before the war, but he hadn't formally written up his thesis yet. And then he got really busy working in the theory group at Los Alamos.

So he didn't formally file his PhD dissertation until basically a few weeks after the end of the war, very soon afterwards. So he officially got his PhD, but everyone knew it was all but done. Remember, postdoc stages were not all that common. It was not that unusual to be hired straight into a faculty position from one's PhD. And everyone knew at that point that Feynman's PhD was essentially done.

Moreover, he had really impressed people like Hans Bethe and Robert Oppenheimer during the war. And so he was actually being multiply recruited. I found letters-- he was made multiple faculty offers. So Oppenheimer first went back to Berkeley. He tried to hire Feynman. Bethe basically a better offer. So he was in high demand as many, many of these young physicists were.

So he was starting a formally as a member of the faculty. He was still rather young. And he was relatively close in age to Freeman Dyson because Dyson had also had his studies kind of interrupted by the war. He'd worked for a couple of years in the British kind of military, basically part of the Royal Air Force doing statistical analysis of bombing runs. So they hired a lot of mathematically gifted young people to say what's the most effective way to use bombers, what's the highest kind of kill ratio, so to speak, and what yields the least losses to the British planes.

So Dyson was doing a lot of statistical stuff during the war. So his own studies were delayed. So they were pretty close in age, even though they were at kind of different career stages. And Alex confirms that you have to take a class-- would be like having to sit through Schwinger's eight hour lecture. That's right. I've seen the lecture notes that I think it was John Wheeler took on Schwinger's lecture there, and they really they were-- they were beautiful, polished. Like Schwinger had thought this through.

He wasn't hemming and hawing for eight hours. So the contrast with Feynman just like winging it for 30 minutes was, I think, all the more stark. But any other questions on any of that material, the early postdoc cascade or anything like that?

OK, I will press on. But as usual, please jump in if other questions come up. I'm going to talk next about what were people actually doing with these diagrams, and this will connect with one of the articles that I assigned for today's reading. So remember that second meaning of dispersion is to get more and more distinct from each other, like the single beam of light separating into the distinct colors of the rainbow.

So this picture of how they spread doesn't really capture what people were doing with them. And in fact, as we'll talk about in this part, people got actually pretty creative in putting the diagrams to a range of different uses. And so that had a few different seeds, what was driving the kind of distinctiveness of what people did with the diagrams. There were a couple ingredients.

One of them was that Feynman and Dyson themselves actually held pretty different ideas about what the diagrams really represented or how they should properly be interpreted. So Feynman always talked about them as intuitive pictures, and I think that's fair. We can see where it's coming from. This is a story that he would often act out as I tried to do in my little Zoom share. The electron spits out a photon. It recoils here.

They were very kind of animated tales of things unfolding through space and time. Dyson, whose original training was actually in pure mathematics, not in physics, and throughout his career was always much more formal, much more a mathematical physicist than Feynman, Dyson cautions in his very first article on the new techniques, that these are merely graphs on paper. That's the phrase he uses.

These are not pictures. They're not Minkowski diagrams. These are not literal depictions of events unfolding in space and time. What's relevant is their kind of topology that some two lines connect here in a vertex. Two other lines connect over here, and there are only so many ways to arrange various lines and vertices. It was for him more like a kind of mathematician's graph theory.

So from the beginning, you're getting kind of mixed signals about what these diagrams are all about. Even more important-- and this is what we'll talk more about in this next part-- was that the questions that seemed most pressing toward which many, many people tried to put these diagrams for use actually were not the problem area for which they'd first been introduced. So these were introduced in the context of quantum electrodynamics, meaning electrons, positrons, photons, every interaction of which is governed by the electric charge.

When I say that the electron has a certain likelihood to emit a virtual photon, that likelihood quantitatively is governed by what we call its coupling constant, which is just to say its electric charge. So what that means pictorially is every one of these red dots where, say, electron lines meet a photon line, every time these lines literally connect in a vertex, the corresponding algebraic expression is multiplied by one factor of that coefficient, the electric charge. And then the lines meet again here.

So there's a second factor. This is in e squared diagram, we say our second order diagram. This one is a more complicated diagram. There are four spots where electron lines hit photon lines, or vice versa, four red circles. That's an e to the fourth diagram. The algebra in front of the long-- excuse me, the coefficient, I should say, in front of the long algebra would have four factors of this constant number e.

Here is an example of a tenth order diagram, where you're trading five virtual photons as an e to the 10th. Now, it turns out that in natural units, the charge of the electron is small. Electricity is a weakly coupled force. In particular, e squared is about 1 over 137 in these kind of natural units.

So it's smaller than 1 over 100. So this diagram is multiplied by a number that's 100 times smaller than this diagram. No matter what the algebra is, its coefficient is parametrically smaller, which means this diagram is exponentially smaller still. So that's why these things became so useful in QED. You didn't have to calculate to infinity.

These two electrons could have traded an infinite number of virtual particles, but the more complicated the interaction, the more places where blue electron lines would connect with these kind of neon green photon lines, the more powers of that small number would come out in front. So as a practical matter, you could stop. This is a perturbation theory calculation, kind of tailor expanding, and each additional contribution weighs less quantitatively.

OK, so that's great. It's a great bookkeeper. None of that works. None of that works when you apply these to strongly interacting particles. And guess what most physicists in the United States applied them to. Strongly interacting particles.

It makes no sense at all. They applied it to the scattering of, say, protons off of pi mesons or neutrons off of other particles coming out of the new accelerators. The equivalent, the analog of the electric charge for those nuclear forces was strong in the same natural units. In fact, it's larger than 10, which means that this diagram counts exponentially more than this diagram, and there's an infinite number of them.

So they're not doing perturbation theory. And yet they're still using diagrams. That is pretty strange. So that leads to this kind of real kind of explosion in how people adapted the diagrams, often even just the pictorial form of the diagrams themselves because they weren't just doing one cookie cutter thing. They weren't all doing Feynman-Dyson perturbation theory for electromagnetism.

They were mostly applying it to nuclear forces. You can't just do the same trick for nuclear forces because the coupling constant is so large, but people still found useful things. So the diagrams are taking off exponentially, even though they're applying them to new and different kinds of applications. So this looks kind of scattershot. It's meant to be scattershot.

We begin to get some order once we come back to asking about who knew whom. In particular, who was learning from whom? Who was studying with whom? So I call these kind of family resemblances. In each of these colored rectangles, the diagram that's a little elevated on the left comes from the PhD advisor of the person whose diagram appears on the right.

These are literally mentor-student relationships. I used to try to play this kind of parlor game when I could go to parlors, like choose any random Feynman diagram from the *Physical Review,* at least up through the mid '50s. And with reasonable accuracy I at least would claim I could tell you who drew it or at least what PhD program they came from. It was pretty good. Now, there may be more useful life skills to train oneself on, but that was my way into this.

So you could literally see oh, that's a Richard Feynman student, but this over here, that's actually a Norman Crowell student, or this one over here, that person was trained by Robert Marshak and so on. You can see these local adaptations because these different departments or members of these different departments were actually doing different things with the diagrams, often set by local demands or priorities.

So for example, at Rochester in upstate New York, not too, too far from Ithaca, but far enough, they had just received, much like MIT had done, funding to build their own small particle accelerator, much like MIT synchrotron. So the theorists there, their main task was to help make sense of all the new kinds of particles that kept shooting out every time they turned that machine on.

They weren't worrying about these very fancy virtual particle corrections. There were no single virtual particle or not these so-called loop corrections that led to all the trouble. It was really a way of classifying what goes with what. When these two things smash into each other, what kinds of detritus comes out? It's classification, rather than kind of dynamics.

These people were applying the diagrams not to high energy physics but actually to many body theory and what would become known as condensed matter physics. What happens when two things are near each other and the same two particles keep trading force carrying particles over and over again? How do you add up that effect? That might be something like a kind of bound state or some stable system, different kind of challenge they were working toward. So you can start seeing the kind of local variation in who's using the diagrams toward what ends.

And now let me talk briefly about one of the even further kind of excursions or even more creative reinterpretations that I wrote about in one of the articles for today a little bit later in time. And this focuses on the work from Geoffrey Chew. He's the one shown here at the blackboard. He was the kind of main theoretical high-energy physicist at the University of California Berkeley over the '50s and into the '60s, most of the '60s.

So he was, like many of his colleagues, really interested in nuclear forces, not electrodynamics, but all these many particles coming out of these big accelerators and all different ways they could interact with each other. He knew as well as all of them did that faced with a large coupling constant, you can't do this kind of perturbation theory. And yet there's all this other things that one might try to do to make sense of all the new empirical riches coming from the new machines.

So he actually becomes so frustrated with quantum field theory, precisely because it seems not up to the task of handling these strong nuclear forces. He says quantum field theory is dead. In fact, he says, it's sterile and destined not to die but just to fade away. And yet the diagrams, he says, hold a real key to moving forward. So he tries to lift the diagrams out of quantum field theory and toss away what had been used actually to derive them in the first place. That's a pretty interesting move.

In its place, he starts talking about something he called-- he and his students called a nuclear democracy and eventually the bootstrap. And the idea was to treat all of these nuclear particles-- by this point, more than 100 of them have been identified-- treat them all as being on an equal footing. Why was that so radical?

According to ordinary quantum field theory, you start out with a very different picture. You have certain so-called elementary particles, and some of them might stick together and make bound states or composites. But some are more special than others. He actually called that an aristocracy or a kind of elitism or hierarchy.

And he said maybe with 100 plus nuclear particles, let's not try to them into which ones are really special and which ones aren't. They all seem to fly out together. Let's assume they're all equally special, treat them all on the same footing, a democracy.

Now, this was unfolding, as I wrote about in the essay, at a time when he was very concerned about if other forms of democracy, a slightly more literal form involving people and fair treatment of people, amid the early stages of domestic anticommunism, the so-called McCarthy era, which we talked about some weeks ago in this class. In fact, he was the first professor to resign from the physics department and perhaps the first one from the entire University of California Berkeley campus to resign in 1949 over a kind of loyalty oath controversy.

He got clearance during the war. There was no question that he had any kind of communist association in his past, but he thought on principle, it was simply un-American in his estimation to demand that people swear a kind of political allegiance. That was a violation of people's free political consciences. So he resigned because the university started requiring these anti-communist loyalty oaths, and he said, forget it.

He was immediately hired at the University of Urbana-Champaign, University of Illinois, Urbana-Champaign. And he got more and more involved with political efforts on behalf of similar physics groups. He testified before the US Congress and so on.

And what's interesting is if you go through his testimony and his op eds and his other publications from this period, you see that same language recur about a kind of Democratic treatment. No one and no nuclear particle deserves special status. They should all be treated as equal partners under the law. And I write more about that in the essay, but that's the kind of gist. He's using this language of treat everyone the same in a lot of domains around the same time.

So why does he think these Feynman diagrams might be the way forward? What does he want to do with them in this new kind of so-called democracy? Well, he says that the diagrams seem to hold more content than quantum field theory. So let's see where the diagrams themselves will lead us. In this orientation of a simple-looking Feynman diagram, this rho meson, which had actually just been identified by some of his experimentalist colleagues not long before, this shows two pions exchanging aromas on. So the rho is a kind of force carrier, much like say a virtual photon would be a force carrier in a QED diagram.

But if you just rotate that diagram, literally turn it on its side, then you tell a very different story. You see two pi mesons brought together. They were attracted because they felt a force. Once they approached each other, they actually can create a temporary bound state of the rho meson.

It's an unstable particle, so the rho meson later will then decay into a pair of pions. And that just comes from rotating the same diagram. Meanwhile, once you create some rho mesons this way, they can be involved in all kinds of other interactions where they would appear as a so-called elementary particle. So the status of the particle type seems to have changed just by rotating these very simple looking line drawings.

And so Geoffrey Chew said basically let's take the diagrams at face value. If they make no distinction between these different kinds of roles or hierarchies, what's truly elementary versus composite, then why should our equations? So he wanted to build a new approach to a quantitative approach to the nuclear forces following these kind of diagram-based symmetries, rather than quantum field theory.

And they went even further, saying what if every single particle was a kind of self-consistent state? They would each pull itself up by its own bootstraps, using a well known kind of American idiomatic expression. They would all be kind of self-made people. No one was born being more elementary or more elitist than others was the idea.

So what if every particle like that rho meson would generate the forces that would give rise to its own production? That's pretty cool. Could there be one unique self-consistent solution to all these equations? Going back to those kind of rotated diagrams, in this example, you have some expression, some quantitative expression for the force that this rho meson exerts, an attractive force between these pi mesons.

That expression will depend on the mass of the particle that's exchanged and this coupling constant, the factor that appears at every vertex. Rotate your piece of paper by 90 degrees. Now you see that once these two pions have attracted each other, thanks to that rho meson, they've formed this bound state. The odds for that to happen also has some distinct expression still only depends on two quantities, the mass of the bound state they produce and this coupling constant.

So now you have two equations for two unknowns. So you can look for self-consistent solutions. And in fact, he and his students were able to publish the first compelling theoretical account of why these nuclear particles had the quantitative properties they did at a time when no one else could come even close. This is far outside the regime of perturbation theory, and here was this kind of very clever self-consistent way of using their so-called bootstrap to try to account for why the rho meson was particularly heavy and why it had this particular interaction strength with pions. So let me pause there. Any questions on that?

It's just too awesome. You're stunned. You're just shocked into silence. I mean, I get that way, too. It's OK.

What I mostly want to emphasize with that part-- hopefully it was clear-- was that even after Dyson did such a kind of celebrated job of clarifying exactly what Feynman diagrams should mean, exactly how they should be used in a calculation, in the years after that, that led to more different uses, rather than collapsing to only one. I found that really pretty cool actually, pretty neat.

And then some people got even more ambitious with them, like the more extreme example from the Berkeley group. But even at Rochester or Urbana or in Cambridge, England, people were being very kind of-- they treated the diagrams as pretty malleable for a long time. And that was neat. It's hard to get computers to do that.

Computers do one thing with these diagrams now. But for much of their history, the diagrams meant a variety of things that could be used in a variety of ways. Yeah, good, Fisher. So Fisher asks when we're talking about perturbation theory, they're referring to everything beyond the second order, beyond e squared, or everything including e squared.

I guess technically, we would even include the e squared. The first term would be one, which is that nothing happens, or I should say nothing changes. So your first term in what's called the s-matrix expansion is 1 plus everything else. And so the next term, the first term to enter would then be e squared. That would be the first correction to nothing happening is you could add two electrons scatter. Then they could have scattered in more complicated ways.

So it is really like Taylor-- you can think of it like Taylor expanding. And it is remarkable because it works because you have a controlled parameter. You can really do a controlled perturbation expansion. So even without calculating an arbitrarily complicated diagram, you can estimate how much would that matter quantitatively. What would be your error budget if you left it out, so to speak. And you can't do any of that using these techniques for the nuclear force.

All right, let me press on. I'll talk about this last part. It'll be pretty quick. I'll try not to go for eight hours. No promises. But this last part is so why do they stick. If they're being used in so many different ways, why do people stick with them instead of design other techniques along the way?

So within perturbation theory, when doing these electrodynamic calculations, I think it's pretty clear, and people spoke of it even at the time that these were just an extraordinarily useful tool. Dyson remarked years later, the calculation he first did for Hans Bethe as a grad student before learning about the diagrams took him several months of work and several hundred sheets of paper. I've been there. Dick Feynman, Richard Feynman could get the same answer calculated on the blackboard in half an hour, and that was indeed what became more and more common.

Julian Schwinger said a bit more snarkily-- Schwinger and Feynman were kind of like frenemies. He said Feynman diagrams brought computation to the masses, which you could almost see Schwinger sniffing, like my students learn really how to calculate. But any old slob could calculate using Feynman's techniques. That's the spirit in which I take that remark, that these really were effective, and many, many people could calculate, even people who weren't blessed to have had Julian Schwinger as their PhD advisor.

This cartoon here ran in *Physics Today* many years ago, basically saying if you're going to go through these dangerous thickets of perturbation theory as if you were a kind of an explorer in the Amazonian jungle, it helps to have a reliable map. And so the Feynman diagrams would be a reliable map to this complicated, seemingly dangerous, or forbidding terrain.

So I think that makes sense, except as I was just emphasizing, that can't hardly be the whole story because most physicists weren't using them in the early days for what they were most good at, for what the diagrams were most efficacious for. So sure, that would explain why people use them in weakly coupled situations, but what about all these other applications?

So to make sense of that, I found it very helpful actually, very fun, to go back to some classic studies in art history. These are, after all, a kind of visual representation scheme, and art historians have been arguing for a long, long, long time about why various styles in art, modes of depiction have come to seem natural or have a kind of staying power and what happens when they change.

So the art historian Ernst Gombrich wrote this really lovely, very influential book many years ago, where he says that basically, his catch phrase was that painters-- how does it go? They see what they paint. They don't paint what they see. They have an idea of what they want to convey, and that structure is how they even take in the world.

He kind of flips the story. You don't just look out your window and say, oh, I'll paint a landscape. You learn techniques, and then you come to see the world through the lens of those techniques, not unlike what, say, the historian of science Thomas Kuhn had argued around the same time. So these are examples of the kinds of things that Gombrich talks about.

These were from training manuals in the early modern period to learn how to draw a human face. You break it down, and you literally practice drawing an eyeball over and over and over again until you see actual people through this kind of schema of how you draw an eyeball, or if you're employed by the local church, which most of them were, here's how you draw the adorable little cherubs and little baby angels.

You practice drawing these chubby-cheeked faces, and you break it down and you practice and practice, like doing problem sets before you go paint the kind of latest picture to decorate the local church. And so it seemed a similar thing was happening with these scientific images, that the physicists' kind of prior habits helped them see in a new way just as if you stare-- if you practice this technique over and over again, you structure your newer experiences with a kind of scheme that you've already worked hard to master.

And so these pictorial conventions can actually help us map a kind of pedagogical lineage, not necessarily a conceptual one. They were often kind of irrelevant to the new uses, but they were more a kind of social or pedagogical value. They helped you get up to speed or helped you make sense of the newer things you then wanted to do.

The most significant, at least as it makes sense to me, was that people were slotting these things into a very well established tradition by that point these Minkowski diagrams, which, of course, many of you had already seen probably before this term. We spent some time looking at them together early in this semester, and we talked about special relativity, literally space time diagrams.

Feynman was not shy about adopting the exact same conventions when he began he began drawing his little doodles at first kind of private sketches, not just that you have one dimension of space and one of time. He oriented space along the horizontal. There's no law that says you have to do that. He tacitly starts scaling the speed of light to be 1, so that light travels along 45 degree diagonals. He's not literally drawing space time diagrams.

These have been completely kind of pedagogical second nature for generations of physicists in many, many parts of the world, not just the United States before Feynman and Dyson introduced their work. This was a kind of context in which students could then encounter Feynman diagrams. These are examples from Feynman's own work. This one and this one from his early publications, this is my favorite.

This comes from one of his lecture notes at Cornell when he was lecturing in 1949 in the department. What I love is that he actually gets his own Feynman diagrams wrong. He's so enamored of this kind of storytelling space time structure that he draws diagrams that literally could not be calculated in QED. They have the wrong number of legs entering each vertex, but he got caught up trying to tell his students about how you could have pair production in QED and trying to narrate that story through space and time, and we can forgive him for forgetting that in the theory for which he'd eventually win the Nobel Prize, only two straight legs and one curvy leg can meet at each point.

Oops, because there's so much bound up, I think, for Feynman in this kind of space time narrative and the trajectories of thinking moving through space and time. It wasn't only Feynman. If you look at the earliest textbooks, they keep repeating the same scheme even when they move into momentum space for which there's absolutely no meaning, no relevance whatsoever to the kind of 45-degree diagonal.

That only means something if you're actually working in space time and scaling the speed of light to be one. It means nothing if you're in momentum space, and yet in these examples and many of these, physicists working explicitly in momentum space nonetheless kept using the space time Minkowski conventions. And I argue that was kind of what they were used to, and it also probably helped some of the students get kind of comfortable with them as well.

There was another visual feature that, again, in terms of which these physicists were immersed at the time, and that comes from this explosion of a very large and rapid production of particle accelerators after the war funded by the Atomic Energy Commission, where many, many universities, like MIT, had their own local atom smasher. So what became common was to draw these freehand reconstructions based on the bubble chamber photographs.

This was not meant to be a Feynman diagram, but it became clear because it was actually fairly expensive to keep trying to reprint the photographs. You would just reprint these often hand drawn line sketch reconstructions, which consist, like these Feynman diagrams, of propagation lines and vertices. So there was, in some sense, a kind of reinforcement of a kind of realism that had nothing to do with Freeman Dyson's very careful kind of derivation or anything about perturbation theory.

So there was a kind of realism. And you see that in this quotation for example, some years later in a very influential textbook that these Feynman diagrams, Richard Mattuck wrote, are so vividly physical looking, that it seems a bit extreme to completely reject any physical interpretation whatsoever. So he does this funny kind of cheat. We will therefore talk about the diagrams as if they were physical, but remember they're not.

So he can't help himself but describe them as real things moving through space and time. Other textbooks at the time reinforced that visually. If we could look, if we could really kind of zoom in with our own eye and watch these elementary processes, we would see Feynman diagrams. We wouldn't see cathode rays scattering on a scintillation screen. We would literally see nature as Feynman diagrams if we had a big microscope.

OK, so let me wrap up. So scientists then and as now always have to practice using their tools. Our tools don't come for free. That's why it takes many years of training, many problem sets, many late nights, many lab reports. And the tools aren't automatic. The tools themselves can change and can be put to new uses.

How do they spread, how these new techniques spread, writing a really great textbook or nowadays maybe making a great viral YouTube video, that can certainly help. But at least in these early days, it took much more than only kind of text-based means of propagation. It really took very specific social institutions, like postdocs and their dispersal, to help move these techniques around.

Moreover, even the kind of fanciest new techniques will be incorporated, will be made sense of in the context of what generations had already become comfortable with. So I think we can make sense of the particular rhythms of the history of these diagrams by thinking about pedagogically what had previous generations already become used to. And so in that sense, you can see the diagrams and their users kind of being forged together, that the early users of the techniques were themselves being molded in these formal training stages of their career, and they're often molding the diagrams at the same time. So I will stop there.