[SQUEAKING]

[RUSTLING]

[CLICKING]

DAVID KAISER: Today, we're now sort of in the middle of our unit of our last main unit on a kind of quarks to the cosmos, trends in high energy physics, astrophysics, and gravitation and cosmology. And so today, we'll be picking up part of the kind of threads that we were looking at before the Thanksgiving break, and we'll be looking in particular how was it and over what kind of time scale was it that most physicists came to be convinced that many, many types of matter are actually formed of quarks, that there's a constituent elementary unit within many kinds of matter. How did people come to be convinced of that and over what kind of time scale? So that's what we'll look at for part of today.

So the three parts-- we'll revisit some of the trends that we already began looking at briefly, again, in the class sessions before the Thanksgiving break, in particular this what seemed to be a really unexpected and at times almost overwhelming proliferation of the number of particles that seem to come out practically every time that the physicists turned on their often brand new particle accelerators.

We saw there was a huge spate, a kind of building boom coming out of the Second World War of making larger and larger more and more powerful accelerators. MIT had our own synchrotron in operation for many years. Many, many individual universities had them, and of course, there were larger national laboratory facilities as well, all a kind of carryover into the post-war period of the wartime Manhattan Project. These were Atomic Energy Commission facilities by and large.

So what were some of the responses to this proliferation of new kinds of particles that seem to be popping up all over the place? We'll see that that was really on many people's minds when they began thinking about what we now call quarks. The idea about quarks itself has a really, I think, fascinating kind of up-and-down history where not everyone was on board at the same time for the same reasons. There was a kind of long ambivalence or confusion or-- what are they? Are they real? Are they not? And so we'll look at that for a good chunk of today's class session.

And the last part will be a kind of what has become called the so-called standard model. We'll look at a key part of that called quantum chromodynamics, or QCD, which you've probably heard of, at least. You probably recognize the name. We'll talk a little bit about where that comes from and how that fits into the kind of larger picture today, and it's drawing on this many decades coming before it that we'll look at for today. So that's what we're heading to.

Quick reminder-- as I just mentioned, after the war, pictures like this were becoming more and more common. So by 1955, Berkeley's Bevatron, a billion-electron-volt accelerator, was filling kind of factory room floors. Again, here's a human operator to give you a sense of the scale-- absolutely enormous, certainly for its time.

And as I mentioned, that was driving this exponential rise in the numbers of seemingly new, seemingly elementary nuclear particles. When physicists could achieve higher and higher interaction rates, they found not just higher energy stuff flying out but new kinds of stuff flying out, new kinds of particles among the decay chains and all that. So that was often called the particle zoo.

We saw in the previous class session one of the first challenges, kind of intellectual challenges to try to make sense of this very large number of nuclear particles is that they interacted strongly with each other. In fact, we now call this the strong nuclear force. It's called strong because the analogy to the electric charge, which we often abbreviate just by the letter G, the coupling constant, is very big.

So whereas the electric charge is small in kind of appropriate units, it's roughly 1/137, so you could do a perturbation expansion, kind of Taylor expand, and more and more complicated contributions, just countless because they have more and more factors of a small number in front.

The opposite seemed to happen for these strongly interacting nuclear particles, where an arbitrarily complicated interaction, a Feynman diagram with many, many vertices, would actually weigh more in your final answer than the simple ones, and there was, in principle, an infinite number of complicated ones. It really didn't make any sense for how to calculate in this strong nuclear force regime.

There was a second challenge, which was maybe a bit more meta, which was really this question of, could all these hundred or soon 200 or almost 300 seemingly elementary particles-- are they all really elementary? Are they all kind of to be understood on the same footing? Is there any pattern or any underlying order that one might be able to bring to what otherwise looked like really just a kind of scattershot display of soon hundreds, many hundreds, of seemingly elementary particles? How do you make sense of all that stuff?

So we saw, just a brief recap for last time, one response to this, very creative response, which was really dominant for a better part of 15 years or so, was associated with the particle physicist Geoffrey Chew, and we talked a bit about him and read about some of his work last time. He introduced the idea of nuclear democracy and then, with some of his students, the idea of the bootstrap, and they were really being very creative with these simple Feynman diagrams, where if you simply rotate the diagram, turn it on its head by 90 degrees, then the roles that these particles seem to play could swap.

And so maybe, Chew and his very active group-- maybe, they wondered, that all these many hundreds of particles are actually, in a sense, equally elementary because they're all bound states of each other. They're equally composite and equally elementary and that the labels we apply to them really depend on which orientation of a kind of scattering diagram or Feynman diagram we happen to consider.

So it was all about-- Geoffrey Chew's program was all about dynamics. Is there one self-consistent set of forces with which they could make sense of all these many types of particles and their scatterings and interactions? So one response to the strongly coupled particle zoo was to look for a single, kind of self-consistent bootstrap that might encompass more and more examples of this nuclear domain.

So what we'll mostly talk about today is actually a distinct approach, a kind of not even rival but a complementary approach that started to gather more and more attention around the same time period, during the 1950s and 1960s, and that was to basically ignore or kind of bracket for the time being questions of forces or dynamics and just say that's beyond us right now but focus instead on classification.

Were there patterns among those dozens and soon hundreds of nuclear particles that came out of the accelerators? Were some more like others, more like each other and distinct from others? Are there family groups? Are there symmetries? Are there ways to group this plethora in a kind of ordered fashion, so to classify?

So in some sense, that approach was really also not very new. As we'll see, it actually reached back to some ideas from the earliest days of nuclear physics. In the early 1930s, one idea in particular I'll talk about now was introduced by Werner Heisenberg as early as 1932. Heisenberg-- it was a series of articles under the title "On the structure of atomic nuclei," the first of which was received in June of 1932, you may remember, though it was several weeks ago.

James Chadwick presented the first really kind of compelling evidence that the neutron exists, that atomic nuclei might have more than one kind of particle within them, both protons and neutrons. He presented that evidence very early in 1932, January, Februaryish, pretty early in the year.

Within not even half a year later, Heisenberg had really picked up that idea and run with it and introduced this idea called isospin. So what was the idea that Heisenberg had? The early experiments with both protons and neutrons in these early months after Chadwick had kind of isolated evidence for the neutron itself. The early follow-on experiments suggested that protons and neutrons were kind of interchangeable when it came to nuclear reactions, that they seem to have, for example, the same scattering strength, that what would later be called the coupling constant g, the analog to the electric charge.

It looked to be the same, so you get very similar patterns, reaction rates, and so on whether you considering proton-proton scattering, proton-neutron scattering, or neutron-neutron scattering. It looked like it was just one kind of particle, not two very different-seeming particles. There was a kind of symmetry in the way these particles were experiencing the nuclear force.

So Heisenberg suggested in this series of papers back in 1932 that maybe it's actually not two separate particles after all. Maybe it's not protons and separately neutrons. Maybe there's one kind of nuclear particle inside the atomic nuclei, but it had different internal states.

And so it would be one particle that could be in one kind of condition or another but not actually different kinds of particles altogether and that the symmetry, one particle that could show up as either proton or neutron-- that symmetry would be broken. We would notice the difference when we put those particles in external electromagnetic fields. Then we'd see quite obviously, for example, that the proton has one unit of positive electric charge, whereas the neutron is electrically neutral.

But we only notice that if we're measuring electromagnetic effects, if we're putting these particles in an external field. So Heisenberg said, that could be just like an electron, which could be either spin up or spin down. Its internal angular momentum could be spin up or spin down along some direction of space, but we'd only notice that if we put that electron in some external magnetic field like a Stern-Gerlach device.

So we don't say there's two separate electrons. We say there's one electron that could be one of two internal states. Heisenberg was saying maybe these nuclear particles were similar. So maybe it's not protons versus neutrons. Maybe there's one kind of particle.

After the war, it became common to call that single type of particle the nucleon, which could manifest as either a proton or a neutron, depending on its internal state, but one could imagine them as not being literally separate or distinct particles. So this is a kind of internal symmetry that would be distinguished by a new quantum number, not the same set that people had been thinking about through the '20s, an additional way to characterize that state called isospin. And the proton we would assign to, say, plus 1 unit of isospin, and the neutron would be isospin down. But maybe they're just internal states of a single nuclear particle.

So that was an idea that Heisenberg suggested very creatively in the earliest days of nuclear physics, and after the Second World War, a number of theorists kind of went back to that and said, let's look at that again. Let's maybe take that a bit more seriously because there were now many, many more particles to try to sift through and make sense of.

So one of the first patterns that a few physicists began to notice soon after the Second World War was that certain of these new particles-- they were often dubbed "strange" because they were unexpected or unfamiliar. These strange particles didn't show up one at a time. They seemed to come in pairs. They seemed to come together.

This became known as associated production. They were produced in particle scatterings in association with each other, so fancy way of saying you didn't get only one of these strange particles. They seemed to come often in pairs.

And so the idea then was could one assign to these, again, yet another internal quantum number, not isospin but a whole new kind of charge or quantum quantity? And so independently, Murray Gell-Mann, who was a very young particle theorist at the time, and Abraham Pais, a little bit older, still pretty young in his career-- they separately came up with the idea that maybe to make sense of these patterns that these kaons and hyperon particles, for example, were always produced together or, in a similar way, the sigma particles were produced with kaons but not separate from these kind of other strange particles.

Maybe there's a kind of charge called strangeness, and some particles were strangeness neutral, like the familiar proton, neutron, and pion, whereas other particles carried 1 or minus 1 units of strangeness. And strangeness had to be conserved. And so it was out of the way to make sense of these patterns.

And so a few years after that, the better part of a decade after that, Gell-Mann and then, again, separately in this case, a theorist based in Israel named Yuval Ne'eman-- in 1960, they separately introduced a new way to try to account for these kind of patterns. Remember that no one yet is talking about forces or decay rates. They're really just trying to say, is there a kind of pattern to this very large number of new particles? Are there new ways to make sense of why some particles seem to appear with others in these large experiments, for example?

So they introduced something that came to be called hypercharge. So take a couple of these as yet entirely hypothetical internal quantum numbers. They just keep inventing new ones that help us sort what goes with what.

One of them they called baryon number, and that applied to many familiar particles like protons and neutrons. It would also apply to many of these new strange particles. So one kind of charge would be baryon number or baryon charge.

Some of those particles would also carry this so-called strangeness charge. So the hyperon particles or the sigma particles would have one unit of baryon charge and one unit of strangeness charge, whereas the more familiar, the not strange baryons would have 1 unit of baryon charge but 0 units of strange, that kind of thing. So it's a new way to try to out this large kind of Sudoku puzzle of new particles.

And then, again, Gell-Mann and Ne'eman showed in 1960 if you pick up on Heisenberg's notion of isospin, this internal state that could distinguish, for example, a proton from a neutron, and then add in 1/2 of the hypercharge, this new one they had just basically made up, then you could, again, start making kind of sense of these patterns of these particles.

So if you go back to the familiar nucleons, the proton, according to Heisenberg, we could assign as this isospin-up state, so its isospin is plus 1/2. It has one unit of baryon charge but is not strange, so its hypercharge is plus 1. Plug in over here-- you see, well, then you'd expect any appropriate units. It should have 1 positive unit of electric charge.

Conversely, the other state of that nucleon, the neutron, would be an isospin-down state. It also has 1 unit of baryon charge but 0 strangeness, and so you would expect it to be electrically neutral. And then you can play that game dozens and dozens of more times to see, is there a kind of single, self-consistent pattern with which you could start grouping like with like among the even less-familiar particles, not just protons and neutrons?

And so in 1960, Gell-Mann presented these groupings, these group theory structures, that seem to make sense of families of particles, and he could array them by mapping them along two axes of hypercharge, this new internal quantum number he'd invented, he and Ne'eman invented, so hypercharge versus isospin as opposed to other ways one might try to group particles, electric charge, and mass and like that.

He was finding order by placing them in these abstract kind of mathematical spaces, grouping them based on their hypercharge and isospin, and he found these very distinct patterns. Some of them were eightfold patterns. Others were tenfold or decuplets.

So he labeled this first one the eightfold way. He was being very playful. Actually, he was just kind of showing off. Gell-Mann loved to the very end of his life-- he only died a few years ago. He lived quite a long life.

He loved showing off his knowledge of many languages, many literatures and cultures of the world. We'll see more examples of that even today. So he very playfully borrowed the term for this from the Buddhists' eight-step path to achieving Nirvana, which, for a long, long time, had been known in English translation at least as the eightfold way. Gell-Mann said that's just as important or just as evocative a term for these eightfold particle groupings he was finding when he mapped some of these nuclear particles in hypercharge isospin space.

So here are the familiar neutron and proton. They have 0 strangeness charge. They're not strange because they've been known for a while.

Here are the sigma particles. Here's the neutral hyperon. Here are other particles with 2 units of strangeness charge instead of 1 and so on. So we call that the eightfold way.

He went on to show there are other groupings, other very specific group theoretic structures, again, in this abstract hypercharge isospin plane, which no one in their right mind otherwise would have thought to use. But there were some times not just eightfold patterns but tenfold, decuplets, again, of particles that seemed to be kind of associated with each other based on their varying hypercharge and isospin. And this involves, again, some of the both unstrange and strange particles.

So the delta particles seem to have 0 strangeness. That meant they weren't subject to that associated production. You didn't have to make a separate strange particle with them. So they seemed to not carry conserved strangeness charge. Here are the sigmas, the cascades, and so on.

What he pointed out this time was that there seemed to be such a pristine geometrical pattern and yet a gap. There was a missing, an as yet unknown particle that Gell-Mann suggested maybe really is out there after all. To fill out this very clear pattern, he thought maybe there should be a particle with minus 2 units of hypercharge but 0 units of isospin, that red circle here, the kind of gap in his otherwise very clear mapping.

Moreover, for this decuplet, he went further and found that there was a kind of pattern in the masses between these rows as you went down in hypercharge. So the delta particles all had a mass of roughly 1,200 million electron volts, whereas the sigma particles had around 1,380, a gap of roughly 150 MeV.

The cascade particles were another 150 MeV heavier still. So Gell-Mann went even further, and in fact, very dramatically, in the middle of a conference with a number of experimental physicists in 1962, he basically stood up and challenged them. He said, I bet if you look hard enough, you'll find a particle with exactly these properties, with hypercharge minus 2, isospin 0. And he could then work out it should have 1 unit of minus electric charge.

And he could even give a mass estimate. He said it would probably be about 150 MeV heavier than the cascades, and sure enough, about not quite two years later, experimentalists came back and announced that they had actually found evidence for exactly that particle, almost exactly with the properties that Gell-Mann had very dramatically predicted.

The mass was extraordinarily close to Gell-Mann's kind of back-of-the-envelope estimate. It had indeed 1 unit of negative electric charge. It seemed to be consistent with a strangeness of minus 3 and so on, the so-called omega minus particle.

That was a big, big deal. In fact, it was such a big deal that Gell-Mann was awarded the Nobel Prize only five years later for all of these efforts to bring a kind of classification order to what it seemed like an orderless, just this kind of sludge of new particles. This kind of development impressed many of his colleagues very, very quickly in real time. So let me pause there and ask if you have any questions on that classification approach.

I really find it astonishing. I don't know if any fans of puzzles like Sudoku or other highly constrained, "you can go here but not there" kinds of pencil and paper puzzles like a crossword puzzle, but he's doing that now with 300 seemingly elementary nuclear particles and finding, I think, these very abstract or at least not the most obvious properties to focus on. But he just sort of rearranged things then began finding order where few had found it before, so it's pretty cool.

I should say Gell-Mann did his PhD at MIT, so we should be proud of him for that, I guess, so a local fella. He had been in graduate school-- I think he finished in 1951, so this is some work he was doing pretty soon after his PhD.

Any other questions on it? If not, I'm happy to press on. Oh, here's a question from Tiffany. Oh, yes, right. So that's true. Some of you might know Sabine Hossenfelder is a colleague of mine.

I know Sabine pretty well. She wrote a really interesting popular book about a year or two ago, not too long ago, called *Lost in Math,* and she's become a really very trenchant and outspoken critic of developments like string theory in high-energy physics.

And so her argument in brief is that sometimes, in more recent years, physicists have maybe become too enamored of these kinds of symmetry arguments, that just because there's a pattern that nature must have taken advantage of what we consider a beautiful symmetrical pattern, and could we have a little more input than that? And I should say, back in Gell-Mann's day, the early part of his career, he could make an announcement in 1962 with real confidence that his colleagues could actually try to find empirical evidence that was consistent with it within a short while, let alone 50 years later.

And so the kind of symmetry argument was getting a lot of feedback, let's say, positive reinforcement in the '60s because these experiments were feasible, and many, many places could do it. And it actually wasn't based only on mathematical beauty but a kind of pattern that maybe one could then have a kind of empirical dialogue about.

And I think Sabine's point, quite well taken, is that, more recently, the energy scales involved in the hypothetical string theories and so on are so far removed from earthbound experiments that we've lost that kind of back-and-forth kind of conversation, so to speak, between theory and experiment in a lot of these ideas so that what seems to some people like beautiful mathematics seems to others like groundless speculation. And that seems to be a different situation than in the mid '60s. So she's a very good writer, and I think she's a great kind of popular science writer anyway, let alone that she has, I think, really interesting insights into some of the sociology of more recent high-energy theory.

Yeah. Good. Any other questions or comments? If not, I'll jump in to the next part. OK. Well, let's see what people do with all this crazy symmetry stuff.

So I mentioned that in 1962, Gell-Mann made this kind of very dramatic prediction at a conference, saying, go look for the omega minus right where I told you to look for it, essentially. Not long after that, very early in calendar year 1964, Gell-Mann and then, again, independently, another much younger theorist, George Zweig, separately suggested, proposed a new hypothetical way to bring order to these kind of hypercharged isospin groupings that Gell-Mann by this point had been doing for quite a while.

So Gell-Mann's article was received at the journal, you can see here, received on the 4th of January 1964. It was published three and a half weeks later. It was pretty quick to get through peer review. George Zweig's preprint from CERN-- he was at that point a very new postdoc at CERN. His preprint was dated the 17th of January 1964.

So they were working separately, and not even two whole weeks elapsed between each of them committing this to paper. And they came up with a remarkably similar set of ideas that maybe actually the order that Gell-Mann in Yuval Ne'eman and others have been finding for a couple of years, eightfold way and the decuplets and so on-- maybe that was due to an even more deeper underlying symmetry involving actually a very small number of truly elementary particles or constituents.

Gell-Mann very famously called them quarks. You see them even in quotation marks here on the first page of his-- it was only a two-page article. Zweig had called them aces. These were really very, very similar ideas, and it is that all the patterns on these hypercharged isospin plots among the then-known nuclear particles could be reproduced under a very simple-sounding assumption that there existed only three types of elementary constituents, quarks or aces. And if you were very careful in assigning them very specific values for hypercharge, isospin, strangeness, and all the rest that you could reproduce those group theory patterns of eightfold patterns and tenfold patterns.

So Gell-Mann, as I mentioned earlier, loved to show off his wide-ranging knowledge of languages and literature and so on. So among this very short list-- this is the entire reference list, eight references only for this entire two-page article, number six of which is to James Joyce's very famous and famously obtuse novel *Finnegans Wake,* where this line many of you, by now, might recognize-- "Three quarks for Muster Mark!"

If that doesn't make any sense to you, good. It's all nonsensical. The Irish novelist James Joyce loved basically making up his own words and not bothering to, for example, define them, and "quarks" was one of these kind of made-up names. He kind of liked the sound of it.

Well, Gell-Mann liked the sound of that, so he borrowed it to label this new hypothetical constituent. Zweig stuck with "aces," which may be a little more familiar but still kind of a nonsensical word.

So they could go through this exercise and assign baryon charge, strangeness charge, and therefore, those two together would give you hypercharge. Likewise, just kind of intuit or posit that the two of those quarks would be an isospin doublet.

So one would be spin up. One would be isospin down. The third one would be what's called an isospin singlet, which means it has 0 isospin, and if you do that, then again, you can reproduce the electric charge assignments.

So these became known as the up quark, the down quark, and the strange quark. And the idea was that any of those so-called strange nuclear particles, the kaons, the hyperons, the sigmas, and all that, must include at least one constituent strange quark. That's what gave it its strangeness charge. So it has strangeness charge minus 1, whereas particles that were made only from up and down quarks would have 0 strangeness charge like protons, neutrons, and pions and so on.

So with those assignments, they could go through the known particles, even the more exotic or more recently found particles like the omega minus, and make these self-consistent assignments always on either triplets of these constituent quarks if they were baryons, like protons, neutrons, or the omega minus, or a quark-antiquark pair, bound states of only two quarks, if they were mesons, like pions or kaons or others.

So it was, again, even more a kind of-- it went from 2D Sudoku to 3D Sudoku, basically, even more tightly constrained assignments for these three constituent hypothetical entities, and they could reproduce all the kind of grouping structure in these abstract spaces like hypercharge, isospin. And there seemed to be a unique assignment for every one of those particles.

And so this looked like a remarkable kind of efficiency. Instead of having 100, let alone 300, seemingly elementary nuclear particles, you maybe only had to worry about three of them and then all these combinations. It looked more like the periodic table from chemistry where there are on the order of 100 chemical elements, but based on developments from the early 20th century, it looked like only a small number of elementary constituents whose combinations would yield 100 or more distinct chemical elements. It was that kind of move in the classify and simplify.

So with these two papers written early in 1964, the idea was to put forward these very specific assignments for three constituent pieces within those nuclear particles. Now, this raised new questions, and it was not unfamiliar even to Gell-Mann and Zweig themselves. The first one and maybe the most obvious is over here in this orange column, which maybe gave some of you pause already. If not, I'll draw your attention to it.

The suggestion seemed to be that these new particles of nature, the up, down, and strange quarks, would carry fractional electric charge. The up quark would have plus 2/3 unit of charge in the unit of, say, the electron. Both the down and the strange quarks would have minus 1/3 units of electric charge, and that rightly should give people pause. It certainly gave the authors of these ideas pause.

There had been no compelling experimental evidence for half a century at that point for fractional electric charges. There have been early, kind of ambiguous evidence in the early years of the 1900s and 1910s. It triggered, actually, some famously bitter fights among physicists in the early 1910s about whether labs had found evidence for fractional electric charges or not.

By the 1960s, that had seemed to be very well settled, and the answer was no. There was no compelling evidence, despite half a century of earnest experimental searching, no evidence for fractional charges, and now they were saying that all of nature, all these nuclear particles, every constituent inside even very humdrum atoms like hydrogen and helium, is somehow teeming with particles of fractional electric charge.

That seems hard to square with the evidence. So they knew that. They wrote about that, as I'll mention more in a moment.

There's a little more subtlety if you look at this great triumph of Gell-Mann's earlier symmetry-driven approach, the omega minus particle. That was consistent on this new scheme with being a bound state of three strange quarks, s, s, and s.

But that now raises a little more subtle question for quantum theory based on the Pauli exclusion principle, which we looked at together in this class a few weeks ago. The idea is that if you have spin-1/2 particles-- and that these quarks were assumed to be spin 1/2 in the angular momentum kind of spin-- then Pauli's exclusion principle forbids having any two spin-1/2 particles in the identical quantum state at the same time.

So if you're assigning all these internal quantum numbers to these particles, how could you possibly have three strange quarks bound together? That means at least two of them must have all these assignments plus also either be two of them spin up or two of them spin down. The last charge you have to assign would be the actual angular momentum spin. That could also either be only plus 1/2 or minus 1/2. If you have three of them bound together, two of them are going to overlap with spin.

That seems to violate the exclusion principle. That's a bit more abstract than no evidence for fractional charge, but the exclusion principle had been awfully well tested by the 1960s. That seemed like a pretty significant conceptual challenge.

And then third, as I mentioned earlier, in neither Gell-Mann's nor Zweig's scheme was there any discussion of forces of dynamics. Why do these objects interact with each other? Why do some have very large reaction rates, others have very small reaction rates, and so on? So there's still no idea of the kind of forces that might interact either between these constituent quarks or as a result of them.

And so as a result of that, Gell-Mann, who was no dummy, he hedged. You might have noticed on the very title of his article that I showed in the previous set of slides was called "A schematic model of baryons and mesons." You may remember back to very early this term we talked about when Einstein introduced the idea of light quanta, he called it a heuristic suggestion, and that's very much like what Gell-Mann is doing here. It's a schematic model, he announces even in the title.

He goes on to say-- near the very end, the last main paragraph of this brief article, Gell-Mann writes, "It's fun to speculate about the way quarks would behave if they were physical particles instead of purely mathematical entities." That's my italics. A search for stable quarks at the highest energy accelerators would help to reassure us that they don't exist at all, reassure us of the nonexistence of real quarks, precisely because of things like no one's ever found fractional electric charges, these subtleties about the exclusion principle.

And so Gell-Mann, in his very first article from 1964, was not saying, eureka, I found them. He was saying, it's helpful to think about as if protons, neutrons, and omega minuses and everything else were made up of these constituent parts, but it's really just a kind of mathematical shell game. These are mathematical classifiers rather than parts of nature. That's, I think, a fair reading of what Gell-Mann is saying in this 1964 article.

Meanwhile, George Zweig, a couple years younger than Gell-Mann, didn't even get the benefit of the doubt. So Gell-Mann's paper was rushed into print after three weeks. Gell-Mann and he wrote two papers within a few days of each other in January of 1964, both of which were rejected for publication. Neither of them even made it through peer review. He was a very young postdoc at that point, and they said, this doesn't make any sense at all, no fractional charge, exclusion principle, and so on.

Plus, everyone seemed to have confidence by this point-- many people did-- that actually Geoffrey Chew's approach, the kind of single self-consistent bootstrap, was going to save the day. That had made enormous progress experimentally by the late '50s, and this seemed to go exactly counter to that single self-consistent bootstrap idea. This was reintroducing or seemed to be reintroducing the idea of a small set of special particles breaking that so-called nuclear democracy that had been so central to Geoffrey Chew's otherwise very successful program.

So the young postdoc George Zweig can't even get his papers published. Gell-Mann is maybe a little wiser, a little bit more long in his career, and he knows to bracket this as kind of schematic hypothetical mathematical [INAUDIBLE].

So this is now in early 1964. A few years later, some very dramatic new experiments were conducted. Depending on who you ask, they're either called the SLAC-MIT experiments or the MIT-SLAC experiments. I'll let you guess which coast prefers which version.

These were some of the first experiments conducted at what was then a brand new accelerator-- you can see it above ground here-- that now we simply call SLAC. That stands for the Stanford Linear Accelerator Laboratory.

It is a linear accelerator. You see that straight line. It's 3.2 kilometers, basically 2 miles long, and it's a series of electrostatic voltage gaps along which you can accelerate electrons to very, very high energies, basically up to a significant fraction of the speed of light. You just keep shoving electrons with very clever electric fields down a 2-mile track and smash them into stationary targets.

And so one of the first applications of this new device built for the same reasons we've talked about before as part of the Atomic Energy Commission effort to get lots and lots of physicists kind of trained and at the ready-- not that this would help make weapons for defense, but it would make people well trained who could be mobilized in a new Manhattan Project if needed. This is a huge version of that post-war policy of making very, very expensive machinery available for nonmilitary purposes to keep communities well trained, even though this device itself is strictly for kind of peacetime questions.

So SLAC came online in 1966, and one of the very first sets of experiments to go in there was actually directed and designed by two members of the MIT physics department, one of whom is still with us. He's emeritus Professor Jerry Friedman. You might have met Professor Friedman. He comes to colloquia and so on-- and then Henry Kendall, who passed away some years ago. The two of them were partners in these experiments.

The idea was to accelerate these electrons to very high energies and smash them into very proton-rich targets, so you could actually study very high-energy electron-proton scattering. In the detector bay at the end of that line, you have this unbelievable equipment-- you can see this is like train tracking, and here's a person for a sense of scale-- where you could actually change the angle at which you measure the detritus that comes out of these very high-energy scatterings.

This is basically a redo of Rutherford scattering but now across miles as opposed to across a single desktop. You may remember back from in the early-- several weeks ago in our class, we were looking at Rutherford scattering from around 1909, 1911, firing alpha particles at very thin gold foil and then surrounding it with scintillating screen, so you could get information for the number of scatterings per angle as a function of scattering angle.

And Rutherford was so shocked when he found a significant number of backscatter events, where the incoming projectile scattered almost all the way backwards, which was consistent only with a very, very small, very massive inner core to the atom. That became known as the atomic nucleus.

Now they're doing the same thing but not with a thin metal foil but literally moving these enormous, enormous detectors around in a semicircle on these kind of inbuilt train tracks, so they could get things like scattering rates as a function of angle. I just find that astonishing, that in that case, well, roughly 50 years or 60 years, maybe, they'd gone from the same basic concept but now taking miles and train tracks as opposed to a grad student sitting in the dark.

Anyway, what they found, the SLAC-MIT experiments, was again very similar pattern to what Rutherford and his team had found about atoms. When they were scattering off of protons, they found evidence for an internal structure within protons. Protons seem to have internal hard scattering sites just like atoms in that gold foil seem to have internal hard scattering sites that we now call the nucleus.

The particle physicist by this point were using a bit funny variables. It wasn't just scattering angle, but this angle q depends both on the energy but also on the scattering angle. It's a larger q here. q squared corresponds to larger scattering angle.

And so if there were just kind of equally diffuse smush inside a proton, you'd expect a very rapid falloff with angle. You'd expect very few large backscatter events, but instead, they were finding on this logarithmic scale quite substantial numbers of large-angle scatter. So again, the argument was really identical conceptually to the Rutherford scattering.

So how do you make sense of the quantitative details, not just the fact that there seems to be a structure inside these proton targets? But could you actually reproduce those curves with the real specific structure inside the proton?

So a number of theorists who were following the SLAC-MIT experiments closely-- they jumped in, one of whom is Richard Feynman, some of whose work we already looked at together in this class. The other was, at the time, a much younger theoretical physicist named James Bjorken. He often just goes by BJ for the start of his last name. They were able to make sense of the scattering data quantitatively, not just kind of qualitatively. There's a scattering site.

But they could actually reproduce these very specific curves, those scaling laws, by introducing what they called partons. This was Feynman's kind of joke. So he was absolutely clear not to call these quarks, partly because, as we'll see, they were conceptually quite different, partly because, by this point, both Feynman and Gell-Mann were together at Caltech in the same department of physics, and they were kind of friendly rivals. Feynman had a lot of these relationships throughout his life.

So by this point, he traded Julian Schwinger as his main rival for Murray Gell-Mann. Their offices were just down the hall. And so Feynman was as much tweaking his nose at Gell-Mann as being kind of maybe appropriately skeptical by very intentionally not calling his new theoretical particles quarks. They were called partons, meaning they're some parts inside of protons. I thought it was a cute name. These are parts inside protons.

So the idea was, why would this help? How would you make theoretical sense of these experiments? And this, again, I think was kind of classic Feynman in his use of intuition.

At low energies inside a nucleus, a proton would actually be a big mess. If it has internal structure, whether they're quarks or anything else, if they're parts within a proton, then they're going to be subject to very strong internal forces. After all, protons don't fall apart, so whatever internal parts it might have must be stuck together very, very tightly, very strong nuclear forces. So it would be some jumble of moving parts with a big dynamical mess.

And people had no idea how to calculate that. Remember, perturbation theory failed and all that. So at low energies, a proton would basically be extremely difficult to characterize quantitatively.

Luckily for Feynman and Bjorken and their colleagues, that was not the situation at hand. To make sense of the SLAC-MIT experiments, they were going to, in a sense, the opposite regime. The protons were stationary targets, but what mattered was the view from the speeding electron.

And the electron had just as much license as the person riding on Einstein's imaginary trains to pretend that the electron was fully at rest, and it saw the target racing toward it at very high speed. So if you imagine sitting on the electron, you could say you're sitting at rest while the proton target races toward you at a very high fraction of the speed of light. So from your point of view as the electron, you see the proton undergo length contraction, become a pancake along its direction of motion.

And its internal clocks will be slowed. It will undergo time dilation, at least as far as you're concerned. So all of the time scales for these very big, complicated nuclear forces will be slowed down as if they were kind of frozen in time.

What the electron would see in this target of the proton would basically be a kind of frozen set of free, non-interacting stationary targets. All the stuff that people had no possibility to calculate, the strong, fast changing nuclear forces become irrelevant in the limits of very high energies as relevant for these SLAC-MIT experiments. So you can go for these internal parts called partons and analyze all this very specific scattering data based on free partons because in the energy regimes of interest, the dynamical forces should basically be irrelevant or subdominant.

So you might say, was this a victory for quarks? Well, not right away. As I've mentioned, Feynman went out of his way not to call these partons quarks.

The two sets of ideas really were answering different kinds of questions. So Gell-Mann's or even more importantly George Zweig's notion of quarks from early 1964-- that was really a kind of constituent model. What are the little pieces that are bound tightly inside protons, neutrons, hyperons, and all the rest? It's about the kind of internal parts and eventually the forces that must keep them bound together so that we don't see things like free fractional charge.

And Feynman's partons were really answering just an entirely separate set of questions, very high energy behavior. When you basically ignore the internal forces, you have effectively free scatterers as opposed to bound strongly interacting particles.

And again, you don't just take my word for it that this wasn't seen in its day as direct evidence for quarks. Gell-Mann himself, three years later, three and four years later, mentioned at a very well-attended conference for high-energy physics that quarks were still fictitious, his word, and that his own favored theoretical approach was actually equivalent to the ongoing work from that Berkeley tradition on the bootstrap, which disavows a set of literally elementary particles and talks about the kind of self-consistent composites one can make from the nuclear particles. So Gell-Mann himself didn't leap on the SLAC-MIT results and say, now we found quarks, let alone Feynman. So this was, again, a kind of slow drift.

So let me pause there and see if there are any questions on SLAC-MIT or any of that kind of stuff. I know I've said it before, but I just love that contrast from the Rutherford scattering-- and the whole apparatus was maybe a meter across-- to 2 miles with that train track to change scattering angle of your detectors. I just think that's pretty-- that right there is the story of high-energy physics in the 20th century, that transition. You're asking basically the same question, getting remarkably consistent results, but you had to do a lot of work in between and convince a lot more people to pay the money to let you do it.

Any questions on that? OK, I will press on. If any questions come up, of course, please chime in.

Let's go to that last part for today and talk about quantum chromodynamics. So right around the time when Gell-Mann stood up at the conference in Florida and said, quarks, as we all know, are fictitious, he was actually working on a whole new theoretical approach, sometimes with coauthors and others working independently. Another key architect of this was a younger theorist named Harald Fritzsch. They were developing what came to be called quantum chromodynamics, or QCD.

The idea now was, in some sense, to finally ask this question about dynamics, about forces, which Gell-Mann himself had very successfully kind of bracketed or left aside from the '50s and '60s with his focus on classification. The idea now is to try to look more squarely at forces and try to find a quantitative way of making sense of these nuclear constituents.

So they're working in very explicit analogy to QED, to quantum electrodynamics, the work that we saw in previous class sessions that really had kind of come together soon after the Second World War in the late 1940s, though the roots of it went back to the early days of quantum theory. So there really was a kind of step-by-step analogy.

In the new work, in place of electrons and positrons, the new work would focus on quarks. Those were the kind of elementary particles that would be subjecting each other to forces. So just like electrons could repel electrons, quarks could repel or attract other quarks, and the quarks would interact in the new scheme by exchanging a new kind of force-carrying particle called the gluon. This would be the glue, the nuclear glue, that would keep, say, an up-down and down quark bound within a proton or within, in this case, a neutron.

So the gluons, which were maybe not the most creative name-- they were meant to be nuclear glue. They would play the analogous role to the photons. Remember, we saw electrons on the idea from quantum field theory would repel each other by firing force-carrying photons at each other back and forth, virtual photons, and now you have a similar structure, at least hypothetically, about quarks interacting by the exchange of gluons.

So there are some new ideas, again, to try to make sense of all the newer classifications and symmetries and internal charges and stuff that have been introduced for the nuclear particles over the previous 20 years. The main idea the main new ingredient conceptually for this newer QCD was to introduce yet another internal quantum number, or charge, again, strictly hypothetical at this point, and they called it color.

The idea was that each quark carries yet another internal quantum number. This one could have one of three values. So instead of spin or isospin, which were usually one of two values, spin up or spin down, the color charge could come in one of three values, and rather than call it minus 1, 0, or plus 1, they called them-- they gave them names like colors, the primary colors, red, green, and blue.

And they did that because they posited several features of this color charge, that the color charge overall is conserved-- you can neither create nor destroy, say, red quark charge-- that the free particles you could ever measure or experience in nature-- bound protons, neutrons, pions, or anything else-- have to have an exact balance among the color charges. So if you have one unit of red color charge, you must have exactly one unit of both green and blue, and vice versa.

And that's why they appealed to the primary colors. If you mix red light, green light, and blue light in equal intensity, together, they will make white light, or all the color will vanish, basically. You'll have perfect color balance.

So that's what they were positing among the color charges on these quarks, that baryon, a bound state of three quarks, must have exactly one of each color to balance out. The mesons, which were like a pion, which is one quark and a bound antiquark, would have to have one red and one anti-red quark, so together, the color charge would exactly balance, or one blue and anti-blue and so on. So that's the first two assumptions. Color charge is conserved, and you have to have an exact balance.

And the last one is that this new force law has to be symmetric with respect to random permutations of the color charge, and that's, again, a lot like the symmetries of isospin, that when you consider a lot of these nuclear interactions about, say, proton-proton scattering or proton-neutron or neutron-neutron, it didn't matter. Any random permutation gave the same results, any random permutation of isospin.

So the nuclear reactions were isospin symmetric, and the idea was that maybe that same kind of symmetry held among this as yet unobserved, maybe even unobservable charge on these elementary constituents, the quarks. And they jumbled up in any old way as long as the total balance remained. So you'd have no free color charge leaking out because you'd have an exact balance. So those were the assumptions.

Then you could go back and resolve some of the puzzles like the omega minus. So now maybe the omega minus would not violate the Pauli exclusion principle because it was not three literally identical spin-1/2 particles in a balanced state. They differed on one more set of internal quantum numbers that had not yet been taken into account. The omega minus would have to be a bound state of one red, one green, and one blue strange quark, so no two of them would be in the same quantum state, even if they had the same values for all the other quantum numbers, including spin.

So now you can have bound states like the omega minus. This also suggested at least why it might be possible to avoid having fractional electric charge. If you always have to have this exact balancing, maybe the electric charge always has to come in integer units and never see, say, a plus 2/3 charge that wasn't appropriately balanced by the color charge-preserving quarks that, as a consequence, would also get you back to only integer values for electric charge. That was at least a hypothesis.

So as I mentioned even just in that brief discussion a moment ago, really central to the idea of quantum chromodynamics, or QCD, is the idea of symmetry, that some property of the system remains invariant or unchanged even when certain parts are changed to undergo what we call transformations. We saw how important this was in the study of relativity. It plays an increasing role in quantum theory, and by this point, by the early and mid-1970s, the theorists like Gell-Mann and Fritzsch and their colleagues were elevating this to a kind of guiding principle to be driven by what has to remain unchanged even among symmetry transformations.

So again, we're all familiar with symmetries. You can imagine, for example, a sphere. It will appear unchanged. Its appearance will remain invariant even if you rotate the sphere by an arbitrary angle around an arbitrary axis.

That's an example of a continuous symmetry. The transformations can be by any angle, 7.1 degrees, 7.1003 degrees, any continuously variable transformation. Likewise, you could rotate the axis itself by any amount.

We're also familiar with things like discrete symmetries. Imagine a square, a simple two-dimensional square. Its appearance will remain invariant if you rotate it by any integer number of times 90 degrees. Rotated by 90 degrees, you can't tell the difference by 180, by 270.

If you rotate a square by 37 degrees, you can tell the difference. It no longer looks unchanged. So that's an example of an object that remains invariant under discrete symmetries, a more kind of constrained set of transformations that will nonetheless leave the appearance unchanged. So we're familiar with these kind of everyday examples from geometry.

Then we're going to start applying this to these more abstract, internal mathematical spaces of these things like color charge. But what if there were symmetrical rotations, in this case discrete ones, because only three values of the color charge, red, green, or blue? So that's more like the square.

You can rotate a red quark by kind of 1 unit in color space or 2 units or 3, but not by 1.2 units. That would be like the square being rotated by something other than 90 degrees. So it's a discrete symmetry, and not that you're literally picking up a quark and rotating it, but you're changing its description in this abstract mathematical space of color space.

So you could perform rotations on your quantum field that represents a given quark. You could transform it by rotating it by some angle in color space. The angle has some constraints. Like I mentioned, it's a discrete symmetry.

Moreover, in this case, you could be changing the angle of rotation at different times and places. This is what's called a local transformation. So it's not just that you take a red color quark and you rotate it once and you're done. The excitation of the quanta of your quark field could be rotated by 2 clicks of the turnover here but by minus 1 click over here, and you could change your mind over time.

So these are called local transformations, and the idea, which was really just a hypothesis that was driving people like Gell-Mann and Fritzsch and others, was that what would it take to leave any observable features of such a theory invariant even under that set of quite wide-spanning transformations.

So one thing you could do is make sure that the observable features of the system only depend on the kind of length of that quantum field. That would be like a radial symmetry, if you think back to the sphere. If you rotate your quantum field by some fancy phase here, if the observable only depend on the absolute square, then the rotations will cancel out. Any local transformation would fall out.

But remember, they're now trying to handle dynamics. They're trying to explain change over time and not just a static configuration, and that's when it gets much trickier. And this is really where the real work had to come in.

If you try to describe the behavior of that quantum quark field over time quantitatively, it obeys an equation kind of like the Schrodinger equation, a little different, but it has time dependence. It has spatial gradients. It has derivatives.

Well, now if you perform these local transformations where your rotation angle itself can, in principle, depend on both space and time, you get in trouble with the kinetic energy, with the change of that quantum field. If the field itself undergoes a rotation-- so psi goes to psi prime-- now we're taking the time change of this slightly more complicated object, and we'll have a chain rule. There'll be psi times the derivative of theta plus theta times the derivative of psi.

It will not be such that the phase terms will just cancel out. If we go back to this term for the kinetic energy, if the derivative of the rotated quark field had just a phase pulled out front, then it would cancel. We'd have one term times its complex conjugate.

It would be just like the radial symmetry here, but we don't have that anymore because of the chain rule. We have to involve a more complicated set of dynamics to ensure that overall symmetry is preserved.

And so that's what then Gell-Mann, Fritzsch, and their colleagues do next. To maintain that symmetry under local rotations in that color space-- red, green, and blue quark color-- they add in these force-carrying particles with very specific properties that the gluons have to behave a certain way as well. In fact, the gluons in some sense, are added for the sole purpose of enforcing that symmetry.

So the quark field could undergo these very fancy rotations, all the permutations of color charge, and to leave all the observables, including the dynamics of forces over time. To leave all those unchanged, these other kinds of matter, the gluon fields, have to have very specific properties.

And this goes is a little technical. Don't worry if this goes by too fast. There's no quiz. But what they do is they construct what's called a covariant derivative, which combines the change in space and time. This is just shorthand for taking the time derivative and the spatial gradient. This is just the derivative of the quark field in time and space.

But instead of only worrying about its change that way, you say it also coupled. It also interacts with these gluon fields. And if the gluon field itself has a very specific transformation-- so if you rotate a quark color, you also have to do a transformation of the gluon field at the same time.

If you choose that transformation very cleverly, then your covariant derivative, this combination of changes in space and time plus the very particular way that the quark field interacts with the gluon field, that will leave this souped-up derivative, the covariant derivative, such that you actually get this overall phase factor out front, and now the kinetic energy remains unchanged. Now your e to the plus i theta is exactly canceled by its complex conjugate, so now your souped-up kinetic term remains totally invariant even under these more elaborate color charge symmetries.

So you can have quarks that run around the world with different colors-- red, green, blue. The color assignment to a given quark could change over time and across space, and as long as you have an additional kind of matter running around with them, these gluon fields, and if you specify very quantitatively how the gluons have to behave much like the quarks, together, the whole assembly will remain symmetric under that symmetry. That's, again, a bit more abstract, but the idea conceptually is, I think, really pretty astonishing.

Basically, Gell-Mann and Fritzsch and their colleagues have to dream up, have to hypothesize a whole new kind of matter, the gluon fields, strictly to enforce a symmetry which was just of their own thinking in the first place. So because they're so enamored by the symmetry properties that made sense of all the kind of classifications of the nuclear particles-- hypercharge, isospin, and all those eightfold ways and tenfold ways and so on, they're really smitten with this idea of an underlying symmetry.

But when they really, really want to describe the forces that would obey that symmetry, remain consistent with that symmetry, they can't do it with quarks alone. They have to have quarks interacting in a very specific way within a whole separate family of matter, the gluon field, and in this kind of coordinated dance, and then the whole collection, this kind of rickety collection would protect or enforce that symmetry. I find that pretty astonishing.

So the symmetry of quantum chromodynamics is more complicated than that of other examples of these kinds of models, like quantum electrodynamics, and therefore, the properties of the gluons are actually more complicated than those of the photon. In particular, the color charge is a discrete symmetry, whereas the phase freedom for the electric charge is a continuous symmetry.

So the gluons really have much more kind of articulated mathematical structure, and one consequence of that is that the gluons can actually interact with other gluons. Or at least, they're all still hypothetical at this stage, but in order for all that symmetry preservation stuff to work, for the math to work out, the gluons have to be able to scatter off of other gluons or attract other gluons, whereas photons don't do that. Photons don't have electric charge, so photons only scatter off objects with electric charge, at least according to QED.

So photons will scatter off of electrons or positrons or protons. Photons won't scatter off of neutrons, at least not directly, and they certainly won't scatter off each other, whereas gluons can scatter off each other as well as off of the quarks. Another way to say that a bit more succinctly is that gluons also carry color charge much like quarks do, whereas photons do not carry electric charge.

So the fact that gluons can attract each other actually changes the kind of force law between quarks. The force between quarks will actually grow with distance because the distance between them is filled with gluons, and the gluons themselves can interact with each other. So it starts looking a bit actually like a kind of stretched rubber band. There's a kind of elastic restoring force at least as predicted by quantum chromodynamics which has no analog in, say, the force between an electron and a positron.

Remember, the attractive force between opposite electric charges falls off with the distance. That's a famous Coulomb's law. In fact, the force falls off like the square of the distance. So they could still exert an attraction across space, but if you double the distance between them, the strength of the attraction falls by a factor of 4.

With quarks, attracting each other by this highly symmetric, strong nuclear force, it seems to go the other way, that the force actually seems to get stronger with distance, and the explanation of why it's so different is because the gluons that are mediating that force can actually kind of glue on to each other, so to speak. They can attract each other.

So if you imagine trying to stretch to a bound state of a quark and an antiquark apart-- this could be, for example, a pion, some sort of meson, and you could instill external energy by pulling on these two particles to try to pry them apart by adding external energy to the system. At some point, you will have to add so much external energy, you'll be tugging so hard, adding so much external energy, that you'll actually create a whole new quark-antiquark pair.

In a sense, these were already around. They were virtual quark-antiquark particles, but they can now borrow the energy you've given to that system to become real particles. They now can pay off their debt to the vacuum. If they were virtual particles, they now can pay off the energy they borrowed as soon as you give to the system enough energy to cover their masses.

So in fact, when you try to pull a quark and an antiquark apart within this scheme, at least hypothetically, you'll never get a free quark isolated. You'll never see a fractional charge. In fact, what you do is you create two bound states of quark-antiquark pairs where before you only had one. And so that, again, comes because the gluons can interact with gluons directly.

So that was a prediction from QCD by the later 1970s that was worked out a few years after this symmetry-preserving scheme, and that, again, gave experimental colleagues something to look for, some specific kind of thing to look for in the high-energy scatterings of these nuclear particles. And the first evidence in concert with that idea was presented by an experimental team in Europe. I think this was a group in Germany in the early 1980s.

These came to be called jets. The idea would have these jets streaming off where the original beam might have gone in one direction and is not quite torn-apart but rapidly reassembled pair of mesons would jet off in opposite directions or nearly opposite directions. So you can have a very specific pattern of the debris coming out of the very high-energy collisions of these particles because in the effort to crack apart a quark and an antiquark pair, for example, you wind up making more kinds of particles that will jet off away from each other.

So these QCD or quark-gluon jets were actually a prediction of this new theory based, again, on the idea of preserving symmetry and having a certain way in which the force-carrying particles interact with the quarks, and that was at least assistant with some of the new experimental data within just a few years. It was at that point by the late '70s, early 80s, not in 1964 or 1967 or any other steps along the way-- it was really at that point that the community began to coalesce around the idea that quarks might actually be real particles in nature and not just a mathematical abstraction.

So let me wrap up here. One of the responses to this particle zoo after the end of the Second World War, as we saw, was to double down on dynamics, like Geoffrey Chew, to say all these particles are, in some deep sense, equally elementary because they're all composites of each other.

But as we've been tracking in today's class, there's a kind of parallel complementary approach really kind of led by Murray Gell-Mann, though many other people were making contributions along the way, which was to bracket the question of forces or dynamics for a while, really, for 20 years, and just to try to figure out, are there patterns in the kind of detritus that comes out of those new accelerators to emphasize classification and internal abstract symmetries?

So Gell-Mann and others-- but Gell-Mann, again, really took the lead at this-- began grouping these particles into families based on these kind of invented new quantities that did not have an obvious relation to measurable properties. So he invents hypercharge. He makes use of Heisenberg's older notion of isospin, and he starts grouping these particles in these very abstract, mathematical, highly symmetric spaces of hypercharge versus isospin.

That enables him to find kind of gaps where he predicts very boldly that there should be actual particles out there. Famously, this works for the omega minus, and then he suggests very kind of schematically, heuristically early in 1964 that maybe those patterns were consistent with a small set of something more fundamental that might not actually be real bits of matter. So the quarks or what Zweig calls aces are very kind of tentatively suggested as a kind of way of accounting for symmetries rather than saying these things are out there part of the world, in part because there was such steep conceptual challenges to thinking of them as part of the world-- fractional electric charge, exclusion principle, and all the rest.

The SLAC-MIT experiments or, I should say, the MIT-SLAC experiments from the later '60s produce suggestive compelling evidence that protons and similar nuclear particles had internal structure, but it was not at all clear to the community, even to people like Gell-Mann, let alone to friendly rivals like Richard Feynman-- it was not at all clear right away that those internal scattering sites were the same as these quarks.

In fact, even years later, Gell-Mann would say that quarks are fictitious. There might be parton structure within protons, but that need not be the same as quarks. We saw that was in hindsight taken to be very, very compelling evidence that quarks exist.

Jerry Friedman won the Nobel Prize for leading these experiments. And now we say this is in hindsight one of the really important touchstones for why we think quarks are physical particles, but again, it's important to see this didn't tie everything up with a bow right away. It was really the combination of that plus follow-on experiments like the jets from 15 years later plus a much more articulated theoretical structure which reintroduces quantum fields, doubles down on these internal symmetries that becomes known as quantum chromodynamics.

This combination of theoretical and experimental ideas and contributions come together over a kind of 20-year period, by the end of which most physicists then would say, the world is made of quarks. They obey a very specific kind of force, and even though quarks themselves are physical particles with fractional charge, the particles we ever can measure in our experiments only have integer units of charge.

So I'll stop there. We have time for some more questions or discussion if people would like. Stay well. I'll see you on Wednesday. Take care, everyone.