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**DAVID KAISER:** So today we're going to be talking about the kind of invention or the hybridization of a whole new subfield within physics that now is often called particle cosmology. It happens to be the field where I spend a lot of my time. I think it's just fantastic. But it's actually a fairly new invention.

This whole field of study came together fairly recently in historical terms. And so it gives us an opportunity to ask both about the ideas that help drive this kind of scientific change, but also, as we've been doing really throughout the whole semester, the kinds of factors or ingredients beyond only cool ideas or new experiments that also can play a very substantial role. So we'll talk about institutions, and politics, and some broader shifts in the field, out of which come this new now quite flourishing subfield. So that's our job for today.

So particle cosmology today is, objectively speaking, just the coolest part of physics. That's obvious. I don't have to belabor that.

It is actually very cool, and it's also flourishing by any measure. It studies the smallest units of matter, the fundamental forces and elementary constituents of matter. And it asks about what role they might have in shaping, really, the fate of the entire universe. So it's this melding of the very, very tiny elements of matter and the role they might play on literally cosmological scales, from the Big Bang to today.

Here's an example. We'll talk more about images like this actually in the next class session on next Monday. This is a series of images of what's called the Cosmic Microwave Background radiation, or the CMB, and we'll talk more about that. It's one of the best examples we have of how we now use our tools about quantum theory and elementary particle physics to try to make sense of and, in turn, be constrained by measurements of characteristics of the entire observable universe, this fascinating interplay between the very large and the very small.

The field is doing pretty well these days by other measures. Its annual budget just within the United States is on the order of $1 billion a year, roughly. That's if you combine spending in this subfield from the National Science Foundation, the Department of Energy, NASA, and other federal grants. I think a more telling measure of its kind of state of health these days is that there are on average two new physics papers, two new preprints posted to the central preprint server archive.org every hour of every day just in this subfield.

So during this class session, there will be, on average, three new preprints posted just in this subfield. And that doesn't take a break for nights and weekends. That's averaging over 24 hours a day, seven days a week. So it is really a booming, booming subject of study.

And I find that all the more astonishing since this field literally didn't even exist 45 years ago. So this is a $1 billion area of study with devoted colleagues all over the world, not just here in the United States. And yet, it's a fairly recent vintage in terms of the kinds of time scales we've been looking at this whole semester. So how did this come to be?

There's a very compelling story and the story that we will take some time to look at today that this new field really emerged in the mid-1970s because of changes in ideas, because of new research insights coming mostly from high-energy physics or particle physics and that it somehow this set of ideas that bubbled up in the mid-1970s of that we'll look at today-- that that compelled researchers to change their whole field of study and in the process to invent this new subfield, that it was somehow natural to start asking questions at the interface between particle physics and cosmology.

And so there's a lot going for that explanation. It's not flagrantly false, but it's also, I think, really, really quite incomplete. And so if we start using the kinds of tools that we've been working on together this whole semester, I think we can dig in a bit more and try to uncover some of the additional factors that really were at play in especially in the early years of this field.

So of the sort that we've been looking at throughout the term, we'll be talking about some changes in institutions, in training, in broader politics, and the support for science, and that these shifts were deeply consequential. And they weren't important to the exclusion of ideas from particle theory, but the ideas from particle theory, as cool and awesome as they are, I think, really were not the whole story. We were pretty insufficient to get us to where we are today. So we're going to, as usual, try to keep all these different kinds of facets in mind.

So our three steps for today's class-- the first is we want to set up what were the kinds of questions that were occupying physicists in various branches of the field before this merger took place, before the emergence of what we would now recognize as particle cosmology? And so as we'll see, that really comes around to one clever, one helpful way to think about that is to focus on the question of mass.

How did physicists in different subfields in the 1950s and '60s think about this very common seeming notion of mass? Why do objects, say, resist changes in their motion? The m that's in Newton's second law of F equals ma.

Was there a deeper origin or way to make sense of this very common notion of mass? And as we'll see, two different communities or sub communities were really focusing on that in quite different ways in the 1950s and '60s. And that helps us trace the precursors for this overlap field of particle cosmology.

Then we'll take a look at some of the broader institutional shifts that were also coming to be very dramatic, and some of them quite unexpected, by the late '60s and early 1970s that also helped propel this new merger of fields. And then we'll zoom back in to see what were some examples of the kinds of research questions that now seemed obvious or natural to ask for members of this new hybrid area in the wake both of the intellectual changes that we'll look at here, as well as some of these broader institutional shifts. So that's our three steps for today.

So as I mentioned, I find it helpful to think about the precursors for particle cosmology by asking about this very humdrum topic of mass. Why do objects have mass? Why do objects resist changes in their motion? And this actually became a very live question, a very challenging question, but in pretty distinct ways during the middle decades of the 20th century.

So in one of these subfields, a pretty tiny subfield at the time-- experts in gravitation and cosmology-- the question about the origin or the impact of mass was often framed in terms of what was called Mach's principle. That's the name for that same Ernst Mach whose work we looked at way back in the early part of this semester, the 19th century polymath, whose work actually helped very directly inspire the young Albert Einstein. It was actually Einstein who named this what we'll talk about in a moment.

It was Einstein who named it Mach's principle because Einstein was inspired by some of Mach's writings on this. So Mach himself didn't call it Mach's principle. But it was attributed to Mach by Albert Einstein as early as 1918-- so from the early days of the study of general relativity.

So the idea was-- it was often we could frame it as a question. Do the local effects that we attribute to mass-- do the local inertial effects resistances to changes in motion for everyday objects-- do those local inertial effects actually arise from very distant gravitational interactions? Do we have to think about the distribution of matter throughout the whole universe in order to make sense for why this block slides down this inclined plane at a certain rate? Local inertial effects, are they somehow tied to cosmological distributions of all the other stuff? After all, gravity is a long-range force, Both. Newton's gravity and even in Einstein's framework.

And so the idea was should we be constantly paying attention to the global distribution of matter and energy when we try to make sense of local phenomena associated with mass? So that was a very challenging question, and we'll say more about it soon. But that was one of the ways that the problem of mass took form, was given a concrete form for these specialists who were pursuing things like Einstein's general theory of relativity in the 1950s and '60s.

There was quite a different set of conversations happening around the same time. But now among the community that was focused on nuclear and particle physics, what we'd soon come to call simply high-energy physics. This had nothing to do, at least on the surface, with Mach's principle or even with Einstein's general theory of relativity. These experts were focused on a very different set of puzzles or challenges, and this picks up more directly from the material we were talking about just at the very end of Monday's class.

Already by the late 1950s and with greater acceleration throughout the 1960s, a number of high-energy theorists were trying to put together these highly symmetric models to account for things like the nuclear forces. We looked at one example of that at the end of class last time, Quantum Chromodynamics, or QCD, which was really coming together in the early to mid 1970s. There were other instances of that, cousin models, similar kinds of models that were getting a lot of attention throughout the 1960s for the other main nuclear force, for what's called the weak force rather than the strong force. That is to say the force that causes things like radioactive decay.

There was a lot of ideas and early suggestive experimental evidence already by the 1960s that the weak nuclear force was somehow mediated was what arose from particles exchanging certain kinds of force-carrying particles-- again, analogies to the photon-- in this case, what we would now call the W and Z bosons. The point is there were new kinds of matter, at least hypothesized. And then when particles tossed these back and forth, that would give rise to things like radioactive decay.

But the challenge became very clear very quickly. These nuclear forces are self-evidently of short range, unlike gravitation or electromagnetism, which, in principle, can extend arbitrarily long distances, the nuclear forces really exert themselves across nuclear dimensions, very tiny fraction of the size even of a single atom, let alone macroscopic scales.

And one of the most obvious ways to account for that at the time was to assume that the force-carrying particles that were responsible for those interactions were very massive. It would be unlikely for them to travel very far as virtual particles because, after all, they have to pay back the energy they've borrowed from the vacuum. So if they can only travel so far-- I should say, if they have a large mass, they have to borrow a substantial amount of energy from the vacuum as virtual particles. So they have to have a correspondingly short delta t, over which they pay it back. They can only travel so far.

So the idea was could have finite range nuclear forces if these force-carrying particles had a very large mass. That will make it very unlikely for that force to be felt across a very large distance because of the whole set of ideas about virtual particles and the uncertainty principle. All well and good, but the problem was with these new fancy highly symmetric models of the nuclear forces, like the weak force and also true as we saw in the strong force of quantum chromodynamics, the symmetries of these new particles are meant to enforce is violated if you give those particles a mass.

So you can have one or the other. You could have a short-range nuclear force that does not have any of those fancy symmetries. So that broke half of the motivation for it. Or we should keep all those fancy symmetries, but break this notion of a short-range force.

So this was a pretty substantial challenge. It got lots of theorists very exercised over the 1950s and especially 1960s, which is really like, why do these particles have mass at all? Can we account for mass of these elementary particles in a self-consistent way? Because there's other set of puzzles that didn't seem to fit together. So the question of mass turns out to have been on many specialists minds in the 1950s and '60s, but as embedded in quite different-sounding conversations.

So let's look a bit more at some of the proposed solutions to this question of mass from within these two quite separated communities. So right around the same time, often in the same year, often, in fact, published in the same journals, members of these two quite different communities-- the gravitation and cosmology crowd on one side and the high-energy particle physics community on the other side-- they were proposing solutions, or at least hypothetical solutions to these problems of mass-- but, again, within their own idiom. So even though the ideas were bubbling up around the same time and often published in the same journals, they still were embedded in quite different research traditions and conversations.

So on the gravity side, one of the most significant elements of research in this topic was put forward in 1961 by, at the time, a very young Carl Brans. This is part of Carl's PhD thesis that he worked on with his PhD advisor, Robert Dicke. So this became known as the Brans-Dicke theory of gravity, named for Carl Brans and Robert Dicke. And they published it in *the. Physical Review* in 1961.

Their idea was actually to try to go back to this notion of Mach's principle and more thoroughly account for that within a quantitative theory of gravity. So they wanted to modify Einstein's general theory of relativity in a very specific way to try to address this question of mass as it had been articulated around Mach's principle.

So their idea was to introduce a whole new kind of matter, a new kind of particle in nature. They labeled it with the Greek letter phi. And now the idea was that instead of having a single constant unit strength of gravity labeled by Newton's constant capital G-- that's the G that's in like Newton's force law. F equals G M1 M2 over R squared-- that unit strength of gravity.

You may remember that in Einstein's general theory of relativity, there's an exact same constant that gets carried over by design because Einstein wanted predictions from his theory to match smoothly with the Newtonian predictions in the appropriate regime. So even in Einstein's theory, the unit strength of gravity is set by some universal constant, the same constant G. The same here and on Andromeda, the same today as it was a billion years ago.

So what Brans and Dicke were wondering was, what if that unit strength of gravity is actually not a constant? What if the strength of gravity could vary across time and space? So one way to represent that variation was to say that this unit strength of gravity, Newton's so-called constant actually could vary because it was actually a dynamical field. It was some field extended across space that could vary over time. And whatever its value happens to be here and now is what we interpret as this local strength of gravity. So it was an attempt to modify Einstein's theory of general relativity actually in response to this challenge of Mach's principle.

So what were they doing? If you look slightly more quantitatively, in Einstein's version, you can represent Einstein's general theory of relativity by an action, in a sense, by writing down a Lagrangian. And I won't go through all the details, of course, right now. I'd be glad to chat more.

But what the relevant term in Einstein's equations looks like this. This R is one of those geometrical objects that he had to learn about from his friend Marcel Grossmann because Einstein had cut too many of his math courses in school. We talked about that.

So this is the geometer's tool of quantifying the warping of space and time. It's called the Ricci curvature scalar. And multiplying that is this constant, this unit strength of gravity in Einstein's theory. So what Brans and Dicke do is say, well, instead of this unit constant, let's replace G by 1 over phi. So now 1 over G becomes phi in the Lagrangian or in the action for gravitation, where in principle phi, the local strength of gravity could change across time and space.

So if this field phi could vary, if it, in principle, it could be changing eta over space or over time, then they also knew they had to take into account within this total energy budget, the Lagrangian, they had to take into account the kinetic energy associated with every time that field changes either gradients in space or changes over time. So the second term they had to add in-- this part here in red-- is basically the kinetic energy associated with variations in that new dynamical field. And they put in very cleverly this extra dimensionless constant, a fudge factor, that they labeled by the Greek letter omega.

And this was basically to control how much their version would depart from the ordinary behavior from general relativity. The idea is that if omega is just a number, just some real number, a positive integer-- or not integer, positive number-- if omega is a very small number of order 1 or a fraction of 1, then, in some sense, it doesn't cost very much. The kinetic energy is multiplied by a small number. So the field varying across space and time wouldn't cost very much of the whole system's energy balance, so it would not, in principle, be difficult for the field to actually be really quite wobbly, that it could vary quite significantly across space and time.

That would be like saying you can have quite dramatic differences in the local strength of gravity because this field phi could be wobbling all over the place because it wouldn't cost much from the energy balance. On the other hand, as you tune up that parameter, the simple number omega, as omega gets more and more large, then it costs more and more to have the field vary at all. And so, in some sense, you're stiffening up the trampoline.

So as omega becomes larger and larger, the field is much less likely to vary either over space or time. And in the limit that omega becomes arbitrarily large, then phi, in a sense, can't afford to vary at all. The kinetic energy cost is too high.

And so, on average, the field doesn't change at all. If the field doesn't change at all, it acts like a constant. So you get back to the Einstein-like limit.

So they had this very clever fudge factor, a coupling constant, so that, in principle, the local strength of gravity could be changing all the time. But the amount of that variation could be controlled by this one new parameter in the theory. And as omega becomes large, the behavior reduces to the kind of constant strength of gravity of the original Einstein theory.

Now, how does that help with Mach's principle? The idea is that this new form of matter, this new field phi, is extended throughout all of the universe through every nook and cranny of space. All of matter interacts with phi. It's almost like a new ether, you might say. It's everywhere.

And then the fact that phi is extended everywhere and is interacting universally is what they thought would take into account Mach's idea that the local inertial effects, the effects of this strength of local strength of gravity right here right now, really is attuned to or sensitive to the broader cosmic distribution of matter. So matter in the furthest away galaxies could be affecting the behavior of phi. And meanwhile, phi very locally affects local inertial effects. So this would be a way to incorporate Mach's principle going beyond even just Einstein's version.

OK, so the idea was there's a problem of mass. Why do local objects behave with the mass that we observe? Why do we attribute certain inertial effects to local objects? And the suggestion here by, at the time, a very young Carl Brans and his advisor Robert Dicke was a new form of matter. They'll label it by the Greek letter phi. It extends to all of space, and everything else interacts with it. OK, that's hypothetical proposal number 1.

Quite separately but right around the same time, there was a different conversation going on among people who were specialists in nuclear and particle physics and, to some degree, in solid-state physics. And that was this question about, why do certain elementary particles have a short range if we can't accommodate a mass while keeping this symmetry? This is now they're back in this question of things like the nuclear forces. Could you have nuclear forces mediated by the exchange of particles?

Could those particles themselves be very massive? So they don't go very far-- so a short-range force-- and yet, still be respecting the very symmetries for which people had invented those particles in the first place. So here was, again, a very clever suggestion coming first actually from Jeffrey Goldstone. Some of you might know Professor Goldstone. He's an emeritus professor of physics here at MIT. He's still very active-- and then independently a few years later by people like Peter Higgs and, in fact, many other theorists along the way.

All this work was bubbling up between 1961 and '64, again, often being published in that same journal as the Brans-Dicke work but embedded in quite a separate conversation. So their approach was not to tinker with the kinetic energy like Brans and Dicke were doing, but actually to study a new form of the potential energy. So they, again, introduce a new hypothetical form of matter. They, again, consider a scalar field-- let's say, a field with zero spin, zero intrinsic angular momentum. They again label it by the same Greek letter phi.

But now they focus on the potential energy stored that might be stored in this hypothetical field. And as they showed-- in fact, Goldstone was really among the very first to show this-- if you adopt a certain kind of characteristic shape for that potential energy function, then you could accomplish what they came to call spontaneous symmetry breaking. So this picture is actually taken from Goldstone's very first article in 1961.

It doesn't look so special. You've probably seen similar things yourselves. It looks like basically a double-well potential.

So the idea was, at the level of the governing equations for these nuclear forces, you would continue to assume that these force-carrying particles really were genuinely massless. They had zero mass, just like the photon from electromagnetism. So that would preserve all the symmetries.

The equations for the governing nuclear forces could retain all the fancy symmetries for which these new particles were introduced to reinforce that symmetry at each point in space and time. And you wouldn't put in any symmetry-breaking terms by hand. You'd leave those particles massless.

However, you'd add in a new additional form of matter even beyond those force-carrying particles. It's what's now called the Higgs field. Though, really, many people were working on similar ideas.

That separate field isn't responsible for the nuclear forces. It's responsible for giving everything else the masses that we measure, including those force-carrying particles. So the idea is this.

Here's an example of that double-well potential. The equations governing the full system-- the nuclear forces plus this coupled scalar field phi-- retain all the symmetries that seem to be so important for the nuclear forces. In this case, as a toy model, it'd be like a left-right symmetry.

So the governing equations don't care whether the field ultimately lands up in this local minimum of the potential or this local minimum. The equations are perfectly symmetrical, in this case, by a simple left-right symmetry. You can make that more fancy with a more involved symmetries of the nuclear forces. So the equations respect the symmetries.

However, this is a dynamical field. At some point, it will settle into a local minimum. The system will minimize its energy. That's how it will seek its equilibrium.

And so the system will settle in either to this local minimum on the right-hand side or this equivalent local minimum on the left-hand side-- same energy, so neither one is intrinsically preferable. And there's a 50/50 chance. So the idea is that the solutions to this dynamical system will break the symmetries that govern the equations themselves. That's why this became called spontaneous symmetry breaking.

The governing equations maintain the symmetry, but the symmetry is broken spontaneously when the system relaxes to some lowest energy state. Now, why would that help? Because I assume, much like Brans and Dicke assumed, that this new hypothetical state of matter, this new field phi, stretches through all of space, through every nook and cranny of universe. And all the rest of matter interacts with it.

So now once this scalar field, once this what we now call Higgs field gets anchored or gets stuck in one of its local minima, it now acts like it has a molasses. It actually is stuck at a nonzero value of its field. Instead of being stuck at the origin, it's stuck at some nonzero value. If these other nuclear force-carrying particles, for example, interact with phi, then, all of a sudden, they have drag this field around. They start acting as if they have a large mass.

At the level of the equations alone, they have zero mass. They should be exactly as massless as the photon. But unlike the photon, these newer force-carrying particles interact with this new scalar field. When the scalar field gets anchored to some energy-minimizing value, that changes the interactions of the effective dynamics of all the fields that coupled to it.

And now they number across space as if they have a very large mass. It's an induced mass coming from this spontaneous symmetry breaking. So now, again, you can have your symmetries and your short range. You can have your cake and eat it too-- very fascinating, a lovely idea being introduced right around the same time as Brans-Dicke also as one way to try to get to this question of why do objects have mass.

So these two communities saw very different things even though they use the same Greek letter phi. They were embedded in different kinds of conversations. To people like Brans and Dicke and their colleagues in gravitation and cosmology, this new field, the Brans-Dicke field phi, seemed very exciting. It got a lot of attention, as I'll say more about in a moment, because it offered the first really concrete quantitative alternative to Einstein's general theory of relativity in nearly half a century.

And so that helped to spur high-precision tests. Now there was not just Einstein versus Newton, which had really been put to bed already by earlier in the century, but is Einstein's the only possible relativistic generalization of Newton's gravity? Now it seemed very clear it was not the only possible one.

And in fact, depending on the value of that parameter omega, there might be really measurable astrophysical effects between whether it's really what Einstein said or this other seemingly self-consistent alternative. So this seemed to be really exciting to learn more about the behavior of gravity on a fine scale experimentally. So lots of people paid attention to Brans-Dicke gravity very quickly.

On the other side of these discussions, two theorists focusing on nuclear and particle physics-- what we now call the Higgs field, the separate field phi-- was tremendously exciting for the reasons I was emphasizing a moment ago. It finally seemed to offer a way forward to be able to maintain these very fancy, very abstract mathematical symmetries of the nuclear forces and keep them short-range, that the force-carrying particles could become massive due to their interactions with this all-pervasive field even though they had no intrinsic mass on their own. So that was answering a separate set of puzzles and quandaries.

No one suggested, at least in print, that these two scalar fields with the same Greek letter might be similar or even worth considering side by side for nearly 20 years, until the later 1970s. The sets of ideas were published to very wide acclaim in their own separate fields as early as 1961. But it took more than 15 years-- nearly 20 years-- until people began to say, hey, these two scalar fields are meant to pervade all of nature.

All of other forms of matter interact with them. They give rise to what we measure as local inertial effects. What if they're actually similar to each other? That question simply wasn't asked for a long, long time.

And as I mentioned a few times it's not because these were obscure papers. So I mentioned some time ago that particle physicists, in particular, love to count citations. We love to count our own citations. We love to lord it over our colleagues when we have more citations than them. It's the way we bully each other or assess inherent worth, either which is inappropriate.

Anyway, the idea is we have these very strict categories for how we sort citations. And the highest category we could think of is called renowned. It's not quite Beyonce, but that's the best we can reach for.

And so a paper is considered technically renowned if it accumulates at least 500 citations in the scientific literature. And that's the highest category we invented. So there's famous, very well-known, but renowned is the highest one.

So each of these papers-- the Brans-Dicke paper and the Higgs papers-- became technically renowned within fewer than 20 years. These were getting a lot of attention very quickly. So on the right-hand-- excuse me-- on the left-hand side in red are the worldwide citations to the 1961 Brans-Dicke paper. That was the gravity-type paper. It crosses 500 accumulated citations in fewer than 20 years.

And in blue are citations just to Peter Higgs's papers, not even counting the comparable number that went to Geoffrey Goldstone's early work. So in blue, I'm plotting just cumulative citations to Peter Higgs's work specifically on what we now call the Higgs mechanism. And again, they cross 500 within fewer than 20 years.

These papers were setting their own fields on fire. These were very highly prominent contributions. And yet, there's almost no overlap. So if you do more than just count and start saying, well, who wrote those papers that are adding up to these kind of dots on each of these histograms, you see it really is like oil and water. These two communities were quite separated during this 20-year span.

So you can see more than 500 each. In fact, it's 1,083 distinct papers doing the citing if you add up all the ones between these two plots. And yet, only six of those-- so less than 1%-- cited both the Brans-Dicke paper and the Higgs papers in the same article during this whole 20-year period. So both articles are getting lots of attention, but within really quite separated communities.

The earliest paper that co-cited-- the earliest article that cited both Brans-Dicke and Higgs in the same article came fully 11 years later in , say, 1972 after their original publications. And then most of the rest of them out of only these six came after 1975. So it was really in this late period when a very small handful-- still less than 1%-- started citing the papers together. And so that's by article reference lists. You can do play the same game and look at authorship.

So there are 990 distinct authors represented by all these papers doing all the citing, roughly equal numbers between the gravitation and the particle theory side. And yet, only 21 of them-- so a little over 2% of the author pool-- cited both Brans-Dicke and Higgs usually in separate papers, but actually cited them in any of their work, again, over that first 20-year period. These are just ways to say that these two subfields really were not strongly interacting.

You can have very attention-grabbing research, literally renowned research, bubbling up from the two separate communities at the same time, often in the same journals, asking similar kinds of questions introducing similar kinds of responses and new field labeled finds on and, yet, have almost no kind of crosstalk between them. That's how separate of those fields were.

So why was there such a sharp divide? Why was there no overlap? Were these fields just different from each other? Am I just asking a silly question? Why would anyone have ever thought that the Brans-Dicke field had anything to do with the Higgs field?

Well, no. Actually, as we'll see in the third part for today, in 1979, two separate theorists working independently of each other actually suggested that the two fields might be literally the same, not just comparable or worth considering side by side. But they developed a very cool model which depended on these two fields being literally the same field-- only one new field of nature, not two. So it's not that they're somehow intrinsically totally separate or different from each other.

So instead, their status, really, is historical. How people assess them or what they thought they were good for was changing over time. And so what had changed? That's what we'll pick up in the next part.

Let me pause here and ask for any questions. I see something come up in the chat. So Alex asks, was it an accident that both parties chose phi? Not really. I make a lot of that because I just think it's visually so striking they literally chose the same Greek letter. It wasn't too surprising, to be honest.

It was not a rule but a pretty widespread convention by that point that a field that had no spin-- so a scalar field-- would often be labeled by the Greek letter phi. The Greek letter psi was often reserved by this point for spin 1/2 particles. So an electron field would often be written with a psi then as now-- or eventually quark fields, spin 1/2 fields.

And fields that have one whole unit of spin-- vector fields would often be labeled with a Latin letter, like capital A or capital B. So it wasn't super, super shocking that both groups reached for the same Greek letter. Nonetheless, it is amazing to flip through *Physical Review* and see the same thing popping up and yet being seemingly in mutual oblivion.

So the notation isn't super surprising that it was so similar. But the rest of it that is a new hypothetical state of matter, it pervades all of nature, everything else interacts with it, that's what gives rise to mass-- it was more than just the letter that they chose. There was a lot of what we might have considered similarities. And yet, the two sets of ideas really were treated so separately. This is a good question. Other questions on that?

OK, I will charge on. But as usual, please don't be shy. Feel free to jump in with questions anytime. Let's look at what might have changed to make it possible, let alone feasible or likely for those theorists in the later '70s to consider these two scalar fields in a new light.

So we might wonder, well, was it changes in data? Did experiments force a new evaluation? No, not really. Let's look at the gravity side first.

So I mentioned that part of what got the gravitation community so excited about Brans-Dicke gravity was it now gave them something very tangible, very specific to try to test for to look for what could be subtle deviations from the predictions of Einstein's theory, not the larger-scale differences between Newton's universal gravity and Einstein's. And especially, in the so-called space age, by the later '60s and throughout the '70s, astrophysicists had all kinds of new tools with which to try to look for these effects, including things like human-built artificial satellites that can be sent throughout the solar system. Now you can do very precision monitoring of very weak or subtle gravitational effects, not just in the space of one laboratory, but literally on a solar system scale.

So these things were both, as I say, predictions both from Einstein's general theory of relativity and from Brans-Dicke gravity, we're subjected to some very, very clever and increasingly high-precision experimental tests starting, really, in the mid 1960s, some of them invented by Robert Dicke himself.

Dicke's quite astonishing, I think. He was one of the, I think, maybe one of the last physicists of the 20th century who was really, really active and gifted both in theory and experiment. Enrico Fermi was like that. But as we've seen throughout this term, there had been a real professional division going back even to the late 19th century between theory and experiment. And Robert Dicke was one of these really quite nimble physicists who was quite accomplished in both.

So he actually began testing his own theory with his group, his students, to do things like very sensitive measurements of the shape of the Sun, what was called solar oblateness. If the Sun is measurably different from an actual sphere, then that could actually have an impact on things like the orbit of the planet Mercury. If you have a more of an oblate spheroid from the Sun, then some effects that had been attributed to Einstein's general relativity might actually be different than assumed if you don't assume the Sun is perfectly spherical and, in fact, might be more consistent with this modified gravity Brans-Dicke. So Robert Dicke and his group started conducting high-precision visual measures of the shape of the Sun with the implication being that if that same mass had a different distribution, could you test between them?

And again, I'd say, to Dicke's credit, even though he was testing his own beloved theory, his group found basically no evidence for a significant departure from sphericity. So this was not a reason in favor of Brans-Dicke. So Dicke himself found some compelling evidence against Brans-Dicke theory, again, I'd say, to his credit.

Likewise, by the later '70s, there were these very cool efforts to do long range distance measurements between not just reflectors on the moon, but even to moving objects like the Viking Mars spacecraft and so on. So these were being done throughout the '60s and '70s. By the end of the '70s, things did not look very good for Brans-Dicke gravity experimentally.

That is all the tests were easily consistent with the predictions from Einstein's theory within experimental errors. And yet, to make the Brans-Dicke, the modified gravity version consistent, you had to crank up that one free parameter, that dimensionless number omega, to be very large. Remember, that's the parameter that lets-- that would tell you how easy or difficult it would be for that new field phi to vary. It's almost like its stiffness.

You had to tune that new field to be very stiff, so it would behave practically like a constant, which is what brings you back to the Einstein-like behavior. So to match all of these increasingly precise new experimental measurements, Brans-Dicke theory was not highly favored. In fact, Einstein's looked really good. So it was not that somehow experiments were in favor of Brans-Dicke gravity, and that's why everyone else started paying attention. So that's on the gravity side.

Meanwhile, as most of you probably know, there was literally zero experimental evidence, a big fat goose egg nothing in favor of the Higgs-Boson until July of 2012, or if you're extra generous, maybe December of 2011, the first hints experimentally-- well past the period we're talking about. Meaning it was not that experiments found conclusive evidence of the Higgs-Boson, and that's what got the gravitation experts to pay attention.

So each of these sets of ideas were inspiring intense efforts to do new experiments, but it wasn't that new experimental results had changed people's minds on the time scale that we're talking about for people very eventually to start taking these two sets of ideas consider them side by side. So it wasn't a new experiment. The main story that's mostly given-- I alluded to this in the very beginning of today's class-- is actually hearkens to changes in ideas and, in particular, on the particle physics or particle theory side.

And these are brilliant and beautiful ideas. These ideas are well worth appreciating. I just don't think of the whole story.

And two of these sets of ideas, in particular, are usually pointed to-- and they came in rapid fire in 1973 and 1974. The first of them is called asymptotic freedom. And actually, it's the reason why our friend and colleague here at MIT, Frank Wilczek, received the Nobel Prize. So he wasn't at MIT at the time, but he's now at MIT.

So this was work introduced by Frank and his then advisor David Gross, and independently by a different very young grad student at the time, David Politzer. His grad students doing work for which they won the Nobel prizes-- amazing.

And what they found was that the strength of the strong nuclear force, that QCD force that we talked about quite a bit at the end of last class session, the interaction between quarks and gluons, that the strength of that force actually decreases with the energy scale. If you rev up quarks to more and more energy, they actually interact less strongly with each other rather than more strongly. And that was the opposite from how the other known forces behave, both the strength of the electric force-- if you think about the unit strength the unit charge of the electron-- and also this other weak nuclear force, the radioactive decay force.

Those forces that literally the coupling constants, the effective charge, they get stronger as you go to higher and higher energies, which is like saying probing shorter and shorter distance scales. And yet, the strong nuclear force, the quark gluon force had the opposite behavior. So this is showing the interaction strength as you change the average energy of the particles involved in any given scattering situation.

And so the flow of the average charge for the strong force has the opposite sign to what was then known and was assumed to be universal. So this was called asymptotic freedom. As you go to asymptotically large energies, the quarks would become ultimately free. They would feel no force at all.

The effective force would go to 0. That's why it was called asymptotic freedom. They would be free from these forces in the arbitrarily high-energy limit or arbitrarily short distance limit-- same limit.

So that was introduced in 1973. That suggests that the behavior of quarks and gluons might be really different at very high energies compared to energies that might be probed even in particle accelerators on Earth. You can see the energy scale here to give it a sense for that. This is measured in units of billion electron volts, or GeV.

To give you a sense of scale, the present-day experiments at the Large Hadron Collider are here. So the highest energy particle accelerator on the planet, the Large Hadron Collider, is at about 1,000 GeV, or maybe now getting close to 10 TeV. So maybe it's up here. I'll give them one more notch because they've tried hard. They're here.

The effects that Frank Wilczek, and David Gross, and David Politzer were talking about would be noticeable exponentially higher energies, right? Well beyond anything that can be achieved even today, let alone in the 1970s. Slack was basically around here at the time. So it's suggested that if one could ever get to really super crazy high energies, you might see some very qualitatively different behavior among nuclear particles-- really cool, a very interesting set of ideas.

Right on the heels of that, there was the introduction of what were called GUTs, Grand Unified theories. My friend Alan Guth likes to call these actually GUTHs because theory should be spelled TH, not-- coincidentally that's his last name. Anyway, we won't give him that. He's not here right now. We'll call them GUTs.

So grand unified theories sprung from this idea from asymptotic freedom. Going back to this chart here, if you look at the average strength of the three main forces of nature, setting gravity aside, this first one is basically the strength of electromagnetism, the kind of QED force, photon scattering off electrons. This one is basically the weak nuclear force, radioactive decay, the W Boson stuff. And then this is the quark gluon force.

The force strengths look like they might converge. It's not just that these two get stronger with high energy, and this one gets weaker. They actually might overlap at a single value. If you squint and take these error bars somewhat generously, it looks like that at around 10 to the 16, give or take, roughly 10 to the 16 billion electron volts, if you could scatter particles with that average energy, maybe all these three separate forces would have the same unit strength.

If they have the same unit strength, maybe they're the same force. So the idea was that maybe all of these highly symmetry mediated forces that we see as very different at these low energies-- they have very different behaviors and characteristic strengths-- maybe they're actually all signs of a single force-- the grand unified theory, which would unite these three forces of nature into a single one modulated by a single force strength, a single effective charge. That was called GUTs. And you can see that only makes sense in the light of asymptotic freedom to get this strength to come down so that it can meet the very gently rising strengths of the two forces.

Those are awesome ideas. They're very cool. You can probably get a sense for where this is heading. It starts to become a natural question to ask about conditions when particles could have interacted with literally astronomically high energies, cosmologically high energies. In particular-- and we'll talk more about this in the next class session-- if one takes the Big Bang model seriously, cosmologically, if the whole universe began in a very, very hot, dense state, an unbelievably high-energy state, then at very early moments in cosmic history, the average energy of anything in equilibrium would have been really, really high, that you could have maybe gotten to a time in early cosmic history when the average energy of, say, quarks scattering off each other would have been more like 10 to the 16 GeV, rather than 10 to the 3 GeV.

So this became a natural reason-- this is the main argument-- for particle theorists to ask about things like a very high-energy cosmology, very high-energy early universe. And the phrase that was often used at the time, a gendered term, was a cosmology would provide the so-called poor man's accelerator, the poor person's accelerator, that it's really expensive to build Slack, and Fermilab, and the LHC. And yet, all they're doing is probing down here based on technological limitations.

So instead of spending another billion plus dollars, turn to cosmology. Let's use astrophysics and cosmology to probe this energy regime of interest. And so this becomes the main reason that's usually given, the main kind of cause for why these two previously quite separated fields of study-- gravitation and particle theory-- were somehow merged into this new field of particle cosmology as an ideas-driven hybrid.

And that has a lot going for it. It's not a dumb explanation. I just think it's a radically incomplete one.

OK, so this is a rhetorical question. Is it the whole story? Of course, you know by now I think it's not the whole story. It's a large part of ingredients, but it doesn't quite add up. If we go back to things that we can investigate empirically, it doesn't quite make sense of the time scales or some of the other phenomena that we can try to ask about physics work at that time.

So here, I'm plotting just the publications on cosmology worldwide, as indexed by these kind of worldwide physics literature searches, physics abstracts, and that kind of thing. You can see that there was a very steep rise, a decided shift, a inflection in the publication rate well before 1973, well before asymptotic freedom, and GUTs, and this so-called reason, this whole theory-driven argument to ask about the particle physics of the early universe. And in fact, the rate goes from roughly six or six and one half papers per year worldwide to around 21 on average. And not only that, you can see that the inflection point really comes quite a few years earlier than this purported cause from asymptotic freedom.

And GUTs-- I think even more important is that GUTs became really, really hot years later. So many particle theorists got very excited about that notion of a Grand Unified Theory, but not in 1974, more like in 1980. So if we want to account for changes in 1974-- and even the particle theorists weren't paying much attention till a half a decade later.

Again, the timing doesn't quite make sense. The ideas were published in 1974. That's true. But as we've seen a few times in this course, just publishing an article doesn't guarantee that people will pay attention to it right away. And that was certainly the case with that work in particle theory.

So even though it offered a very interesting reason to ask new questions, empirically speaking, most people weren't asking those questions at that time. That certainly doesn't seem to be the main driver.

So what else was going on? And we've seen this plot a number of times. This is now the familiar plot of a number of PhDs in physics granted in the US over time. We spent quite a while looking at both the reason why it grew so rapidly, both after the Second World War and after the launch of Sputnik, but also this really quite precipitous crash.

And a whole jumble of reasons combine, Some. Of them economic, a bunch of them more about policy shifts in geopolitics even that really combined-- that lined up in time that was a perfect storm. The result of which was a very, very sudden collapse in the funding and job prospects and enrollments for physics, which fell, as we've seen, a few times faster than any other field in the Academy.

And so physicists, starting the late '60s and really accelerating in the early '70s, saw a really dramatic change in the kind of infrastructure for the discipline. It turns out, again, we can ask a bit more-- that's very coarse-grained. That's looking at the whole field at once.

We can dig in a bit more and look at subfield by subfield. And by these kinds of measures, the single hardest hit subfield, the specialty within physics that was affected most dramatically even as all fields felt it, was particle physics. The field that had, so to speak, the most to lose or that lost the most during this reversal of fortune was actually high-energy particle physics.

The US budget for that subfield fell in half in just four years. It wasn't a 20% reduction. It literally fell by 50% in four years.

That's a very sudden drop. And that was combined with the drop in the job market demand and all the rest. So even as the whole field starts going through some pretty dramatic changes, particle physics feels the brunt of it in the most extreme way.

So that leads to an interesting set of internal migrations within the discipline. This plot comes from a series of studies that were produced very soon after the crash by the physics community. This one, I think, was from a National Academy of Sciences study. And so what you see is this is now looking only at the years between 1968 and 1970.

The plot was made around 1972 or '73 and is charting inflows and outflows among the recognized subfields of the discipline during the time when, really, the bottom falls out. So when particle physics is getting hit harder than any other field, you have twice as many people leaving that field than joining it. And that's where the people who stay within physics at all that you have a net outflow by a factor of 2 just within the field of people fleeing particle physics, partly because its budget was cut most dramatically. And the job scene was most hard hit, and experiments looked like they'd be on hold and so on.

And so in some of these reports that tried to make sense of the crash, like this 1972 report commissioned by the National Academy of Sciences. It was actually a huge three or four-volume thick government report-- 2,500 pages full of tables and charts, lots of fun stuff to geek out on. And yet, this blue-ribbon panel with several leading MIT colleagues who served on it, plus members from all across the country, were trying to survey the US-based physics profession and what had gone wrong during this period of very rapid transition. And they single out particle theory, in particular, for special kind of concern or critique.

They say it's not coincidental that these young theoretical physicists in particle theory had the hardest time when the trouble came because this report claimed, at least, they'd been poorly trained. It's not the young student's fault, per se. It's because the programs that were producing many, many, many highly trained particle theorists were training them much too narrowly. They were too narrowly specialized only in the esoteric of these symmetry arguments about nuclear forces.

They weren't being exposed to or responsible to the many, many other fields of physics. And so when trouble came, they were least adaptable. Now, I don't know if that's really the case or not, but I think it's quite telling that was the kind of explanation that this blue-ribbon panel of leading educators, including many leading particle physicists themselves-- that was their explanation for why that one subfield fared so especially poorly, even as the whole field had trouble.

And so they make recommendations. Their job, after all, is to say, how do you avoid this in the future? It was a very official high-profile blue-ribbon committee. So they make all these recommendations, some of which actually get taken up pretty quickly by departments across the country.

And one of the recommendations is to forcibly broaden the training of young physicists in particle theory. They have recommendations for other fields too, of course. But partly because they singled out particle theory, they say, we have to start changing how we train PhD students in this particular subfield. And so the idea is to actually formalize their exposure to more and more parts of the discipline, including explicitly more focus on gravitation and cosmology.

So that means that more and more departments, including very elite trend-setting departments across the country, start rushing to offer new graduate courses in general relativity, many of which had offered zero courses in the field until then. Or it was only an elective. Now became a requirement or was only offered in astronomy. And that was also offered in physics.

They're rushing to get more and more coursework on things like gravitation and cosmology. And likewise, questions on that field, really for the first time in the United States, start showing up regularly on the general exams for physicists across all fields, all specialties, not just those who wanted to study relativity, and gravitation, and cosmology. So now you have more and more students in their PhDs responsible to have learned something in neighboring subfields, whereas before then, the general exams had not emphasized or required that.

And you see a market response as well. You see a flood of new graduate-level textbooks on general relativity on gravitation and cosmology-- twice as many published in the 1970s versus the 1960s. And in fact, even of those 1970s books, the vast majority of those came really in the later '70s, in the wake of these pedagogical reforms. So remember that big report comes out in 1972. You start seeing curricular changes as early as '73, '74.

And by '75, '76, '77, you start seeing, in some sense, the market respond with many textbooks being really rushed into print. Some of these textbooks were basically mimeographed lecture notes. It was seen as such a rush, such a demand to get new pedagogical materials because more and more places wanted to offer courses on this where they hadn't before, that you had some informal kind of lecture note transmutes the transmutations into textbooks. And now there's very, very fancy books published in a more typical way. You see really a rush to get more books on that subfield in particular.

So let me pause there. Any questions on that? I see, again, some things in the chat. Yeah, so Alex says, quite rightly, if we can convince Alan Guth to clean his office, then we'll work on nomenclature change. Some of you might know that Alan-- I can't talk about Alan enough. He was my PhD advisor, so I've been teasing Alan for more than half of my life, to his great chagrin.

So you might know that some years ago, before there was a construction to build a new Center for Theoretical Physics, we all had to move out of our old offices. And it was around that time that *The Boston Globe* ran a contest for the entire Boston metro area for the area's messiest office-- not messiest academic's office-- the messiest office in the Boston metro area. And I give it away. Guess who won.

Single-- I mean, he's won a number of awards, gold prizes and that big award-winning fella. But the award that I take most pleasure in is the fact that he won Boston's Messiest Office. So yes, we'll work on that.

Let's see. Fisher says, on the chart of interactions with the field strengths, are those groups? Yes. Fisher, thank you. That's right.

So I was avoiding the nomenclature, but you're quite right. On that chart that's associated with asymptotic freedom, what I was referring to as the strength of electromagnetism-- it was labeled on the chart U1. And that is the fancy way of labeling the symmetry group that is associated with electromagnetism. It's a continuous unitary symmetry, which is like saying you could rotate the electron field by any continuum amount, and the equations remain unchanged.

And so that calls for certain properties of the force-carrying field. The photon only has to mop up a relatively simple symmetry, the U1 gauge symmetry. Whereas SU2 was what I was pointing to when I was referring to the weak nuclear force.

And you're right. That's a discrete symmetry. It's a more complicated symmetry structure.

And that's the symmetry group that these force-carrying particles-- the W and the Z particles-- are invented to enforce. So that has different implications for their properties, like we saw last time. And then SU3, that refers to three different color charges of quantum chromodynamics. You have a discrete three-way permutation.

And so therefore, the gluons have still a different set of properties, so that's right. So that chart was labeling the symmetry groups associated with the different kinds of forces. And so what's really at the heart of GUTs, or GUTHs-- I'll throw him a bone-- is that maybe you can play that game one more time and say, each of those three symmetry groups are actually sub-- you think about it. You can represent any of them as matrices. Is there one single larger set of matrices that would include the U1, SU2, and SU3 as submatrices? And there is the smallest group that includes those three as subgroups is an SU5.

So what if there were five-fold symmetry with still different kinds of requirements on its force carrying particles? So the first GUT was actually an SU5 model put forward by a then very young Howard Georgi and Sheldon Glashow. They were very young theorists at Harvard at the time.

And it was to do exactly that, to try to build on that symmetry structure and find the next simplest, most minimalist symmetry structure-- basically a matrix representation-- into which you could fit those three known symmetry groups and just a little bit else. It's a little bit bigger. So that led to new phenomena that were predicted that turned out not to be measured, but that was the idea at least.

And that maybe the photon, the W, Z, and the gluons actually are all instances of this one unified force carrier. And at very high energies, they would all be indistinguishable. But as the symmetry gets broken at lower energies, they take on different features. It's another example of spontaneous symmetry breaks. Their low-energy features would be different than their high-energy features. That's right.

And that's super cool and fun and lots more to be said about that. But that is, indeed, where that nomenclature came from. Great. Any other questions on that? OK. Anyone else want to share stories of Alan's messy office? No, going once? OK.

Then let me press on for the last-- the last part is pretty short, so the last little part, and we'll have time for more questions and discussion after that.

OK, so I mentioned very briefly that in 1979, two separate theorists-- in fact, they were working independently at the time-- Anthony Zee and Lee Smolin separately introduced a whole new model where they didn't only cite Brans-Dicke and Higgs, they didn't only say these two Greek letter phis might be similar, they literally united them. They proposed-- it was a hypothesis. It was a new model in which these two fields were literally the same. They glued them together.

And I found that really compelling to say it wasn't that the fields were in principle separable. Here were some clever folks coming along almost 20 years later to say, maybe they're actually the same. Maybe there's one field phi pervading all of nature, all of space, with which other matter interacts.

So again, if you look at the key parts of the Lagrangian that they put forward-- you can think of it as like the energy balance-- they combine the features from Brans-Dicke for the gravity side. So there's a direct coupling between this new hypothetical field and this geometrical structure, the local curvature of space and time. And so this is really, again, playing the role of the varying unit strength of gravity. That's in place of Newton's constant G.

If that field can vary, then you better include its kinetic energy with, again, some fudge factor. That's all the Brans-Dicke stuff. And give that field its own potential energy with that very specific shape, that symmetric double-well type shape from Goldstone, Higgs, and all the rest. Why would you do that?

They wanted to ask why gravity appears to be so weak compared to all the other forces. By this point, they knew about things like the strong force. Quarks and gluons interact very strongly. Even the electromagnetic force is exponentially stronger than the gravitational force. The force between an electron and a positron when they're close together is exponentially higher due to their Coulomb attraction compared to their gravitational attraction, even just classically.

So why is there such a strange hierarchy? Why such a huge divide in the average strength of gravity compared to these other seemingly elementary forces? So the idea was that this local strength of gravity, Newton's constant, which goes 1 over this phi squared, would get anchored to a very small value when phi gets stuck at a relatively large value.

So when this dynamical field, this field phi-- which, they also use the Greek letter phi-- which skitters around the universe, it eventually reaches some kind of lowest equilibrium state at the bottom of one of these symmetric minima of that potential. And so now it gets anchored at some large nonzero value instead of being at a local unstable equilibrium point of phi, roughly 0.

So if phi becomes stuck at some large nonzero value, either plus or minus, then the square of that will be some large number, some large positive number. And so that could be setting the inverse gravitational field strength. So why is gravity so weak? They suggested maybe it's because it's arising from some broken symmetry. Much like the Higgs-Goldstone mechanism, the field is dynamical, but it's getting stuck. And only in the broken-symmetry phase do we experience a phenomena that we are used to.

So gravity gets stuck being weak because its local strength is arising through the Brans-Dicke field getting accurate in a symmetry-breaking potential. It's lovely. It's a very cool idea.

So how did these two individuals come to that? Here's a photo of Tony Zee a few years ago. He wandered into this field really accidentally. He finished his PhD at Harvard in 1970.

He was a grad student in the late '60s. His work was squarely in particle physics, the fancy new symmetries and new nuclear forces. That's what he studied for his thesis.

He finished his PhD in 1970. He then had a sabbatical early on in his faculty career, and he happened to go to Paris. And he swapped apartments with a Parisian physicist who was about to come to the United States. So they basically swapped apartments for several months.

And as Tony recalled, the physicist with whom he again kind of accidentally wound up swapping apartments happens to have been immersed in gravitation and cosmology, more so than in Tony's main field. And as Tony recalls, he found these stacks of preprints all around the apartment that looked interesting. He was basically in a stranger's apartment, reading the mail, just reading papers on the coffee table.

And these looked really interesting. And in fact, they sparked. He had a little extra preparation. He'd been an undergraduate at Princeton, Tony had.

And there, he studied a little bit of gravity with John Wheeler for his undergraduate thesis. So he knew a little bit about Einstein's theory of gravitation, had worked with a renowned expert in the field as an undergraduate, but really focused on quite different topics throughout his own PhD and post-PhD training. So he accidentally stumbles back into the topic of gravitation and cosmology.

And that rekindles an interest. He actually gets back in touch with John Wheeler. He's not doing this on his own after his sabbatical is over.

So by the mid to late '70s, he's now asking questions at this interface between his formal training in high-energy particle theory and this new hobby interest in gravitation and cosmology. And it's in the midst of that new set of studies that he writes his version of his broken-symmetric gravity to suggest very creatively but tentatively that the Brans-Dicke field and the Higgs field might be identical. OK, that's his route in-- a kind of accidental.

For Lee Smolin, it was really not very accidental at all. He was the other person who independently introduced that broken-symmetric theory in 1979-- same year as Tony. So Lee actually was still in graduate school when he wrote that paper. He entered grad school in '75. So he entered roughly 10 years after Zee had. He was roughly 10 years younger. Both at Harvard, as it turns out, both at the same school, but 10 years apart, in the midst of which some pretty significant curricular changes had begun to take place there and elsewhere.

So unlike Zee, who focused pretty exclusively on particle theory as a graduate student, Smolin was actually, from the start, combining the two fields, both in the courses he took and eventually with his advising team for his thesis and for his dissertation itself. So from his first semester as he was taking courses with experts in gravitation and cosmology, like Stanley Deser and Steven Weinberg, as well as experts in particle theory and nuclear forces, like Sidney Coleman, Howard Georgi, I who introduced that GUT I mentioned, and a visiting professor at the time, Gerard Hooft.

So he was really being schooled, formally and informally, from the start to work at that boundary. And his paper that introduced this unification of the two scalar fields-- also published in '79-- that was part of his formal graduate training. That was part of his thesis.

It was actually that second version that becomes more and more common. So unlike this accident of trading apartments in Paris and reading a few preprints, more and more members of Lee Smolin's generation were going through a training more and more like his, partly by design, in the wake of the National Academies report and similar curricular reforms. So Lee was hardly alone from this generation. People like Michael Turner Edward-- he goes by Rocky-- Kolb or Paul Steinhardt-- all of whom finished their PhDs around the same time as Smolin in different universities.

They were trained in very similar ways that they likely had entered with an interest in particle theory. They learned a lot of particle theory, but both formally and informally, coursework general exams and advising for theses, they were working from the start with a combined set of ideas and advisors, quite different from what had been typical for say Tony Zee's generation, or, for that matter, Alan Gates.

And each of them, not just Lee Smolin, each of the folks here like, Turner Kolb Steinhardt and many of their colleagues, also took up this interesting idea and, in the years soon after 1979 throughout the early mid '80s, began pursuing other studies of their own in which they physically united the Brans-Dicke field and the Higgs field in new kinds of models to try to understand exotic phenomena. So that becomes the norm instead of the exception.

So I find this really interesting because there's a tradition for trying to think about how physics changes over time. We, often think about whole-scale theories, and one theory replaces another. And I think there's just that misses lot of what people do all the time. It certainly misses what people like Lee Smolin, or Tony Zee, or Mike Turner were doing in these instances. So very few people, very few physicists today, even including Carl Brans, think that Brans-Dicke theory of gravity best describes our. Universe

It's highly constrained by solar system tests. Any deviations would be pretty modest compared to pure Einstein gravity. And yet, this theoretical object that theorists still think about, the Brans-Dicke scalar field, hardly died off by his experiments. In fact, arguably, interest in the field grew even as it was getting experimentally less and less favored. So this new generation of theorists-- people like Lee Smolin, and Rocky Kolb, and Mike Turner, and Paul Steinhardt, and others-- they were trained to work at this interface.

And they found that this Brans-Dicke-like feature kept popping up all over the place. When you try to study the self-consistent quantum mechanics of these scalar fields in a warping space time, you are actually forced into these kinds of Brans-Dicke couplings. That wasn't Brans' and Dicke's motivation, but through this new set of questions, more particle theory inspired.

Then these terms come up unavoidably. They became more and more common in other efforts to unify the fundamental forces, like in string theory. They had all kinds of potential roles to play in new work that we'll look at together next time on inflation.

So even though experimental evidence for or against Brans-Dicke gravity was tilting more and more against, at that same moment, people were finding more and more creative reasons to pay attention to the scalar field, even though the full-blown theory was falling out of favor. And you see that, again, in this countables. If you reset the clock to zero and start counting citations starting in 1981 just to the Brans-Dicke paper, it becomes renowned all over again. It gets another 500, in face, almost 700 citations just in the 15 or 16 years after 1981, when it was already most disfavored by the astrophysical observations.

So the idea that we're picking single theories and that they replace each other, I think, just misses this fine structure, that while Brans-Dicke, as a description of gravity in our local solar system was under most pressure, was when people were actually doing the most things with the actual scalar field Brans-Dicke phi. I find that really curious, and it's still very much alive today. In fact, one of the leading contenders for our understanding of the very early universe does exactly the kinds of things that Smolin and Zee had been doing, trying to unify the Brans-Dicke and Higgs field.

So I want to emphasize that neither kind of new experiments nor new theories alone are really going to help us account for either the specific instance of people doing creative things with these two scalar fields or the broader instance of working at this new hybrid or boundary area of particle theory. And in fact, there were a bunch of concrete changes-- some of them geopolitical, the ramping up the Vietnam War, worldwide economic crisis, huge changes in policy priorities within the United States, shifts with the University departments-- all these things on a huge range of scales, from individual departments up to Cold War geopolitics, these things are helping to mold what's going to seem natural or, quote/unquote natural, for younger people to consider doing because it all helps to shape what counts as their formal and even their informal training.

And so the training of people like Lee Smolin and his generation just was importantly different from the very excellent training that people like Tony Zee had had even just a mere 10 years earlier. And in turn, these new folks, especially people like Mike Turner and Rocky Kolb, went on to become real institution builders in their own right. So in fact, they were accelerates. Not only had they been trained to think carefully at this new interface, they helped really accelerate the trend.

Turner and Kolb became the directors of the very first institutional center devoted to particle cosmology. It was called the Center for Particle Astrophysics at Fermilab when they were still relatively young in their careers. And then they wrote the very first textbook on the field first published in 1990.

I actually own three copies of that book. I love that book. And so they were actively working to perpetuate this new hybrid area, not only because of the force of lovely ideas like asymptotic freedom and GUTs.

So I'm going to pause there. We have time for-- oh, OK. Sorry, last little bit here. This is really a setup, then, for the next class session. We'll look more squarely at inflation in cosmology.

We'll have more opportunities to make fun of Alan Guth, which is what's in it for me. But we'll look at an instantiation of work at this new hybrid area, really of what becomes the poster example of particle cosmology. And we'll look squarely at that in the next time.

And as I say, within that work, including my own students and many people now around the world, it's now just totally bizarre not to consider the Brans-Dicke field and the Higgs field is somehow relatable or maybe even identical. So it's gone from really, really just never done for nearly 20 years to now remarkable if people even question it. So it goes from who even thought of it to who wouldn't even try that?

What counts as natural can shift in a pretty short time scale. And as we've been seeing throughout the whole term and including today, those shifts can be driven as much by things well outside of the physicist's control, geopolitics, and national scale budgets, and blue-ribbon committees as by the force of new ideas and experiments. So that's where I'll actually pause.

And we have time for a few more questions or comments. Any other thoughts on that? With my last moment here, I'll say, before the construction for the new Center for Theoretical Physics, I had an office just down the hall from Alan's. And by a quirk of the old building 6, we had the same key.

A single key would open the whole hallway. I guess, we all trusted each other then. Couldn't get rid of it now.

So my key would open Alan's office. And one time, my parents were visiting. And I basically broke into Alan's office. They couldn't believe me when I described what it was like to try to work with this person.

So I actually broke into his office to show them the safety violation, fire code violation, horror show that was the den of entropy known as Alan's office. So that's a true story. I no longer have a key to his office, so his mess is safe from me. Any questions about messy offices, or group theory, or geopolitics, or anything else?

If not, I will say good luck with paper 3. Please, please get back to it. Don't leave it to last minute. Please get in touch with your teaching assistant.

Think about the optional paper 1 rewrite if you have time and inclination for that. And stay well, and I look forward to seeing you on Monday. See you soon, everyone.