[SQUEAKING]

[CLICKING]

[RUSTLING]

[CLICKING]

DAVID KAISER: Welcome to our final class session for 8.225 STS042. Kind of hard to believe-- hard for me to believe at least. But we've made it to the end of the semester, this very strange and unusual semester.

So I hope you're all doing OK. It's I know it's crunch time, even more than usual, I think, with lots of final projects and end of term stuff. So I hope you're all hanging in there, doing OK.

There's a little while left on your paper three assignment for this class, and hopefully the other assignments for your other things are well in hand as well.

Any final logistical questions about the assignments, or really anything like that? If not, that's OK. I will jump in.

I do have one set of lecture slides for today and a kind of wrap-up. We won't wrap up every last question in all of physics, I don't think. But we'll explore some ideas that kind of build on, or follow the story forward in time, compared to some of the recent things we've been looking at, especially in this relatively recent overlap space-- conceptual overlap space-- between very high-energy theory, particle physics, and studies of cosmology, this area that we now call particle cosmology.

And so for today, I want to talk about some things that you might well have heard about. Some of you might have already had a chance to read up on this in some detail. But there are things that are often described in popular culture, there are ideas that are still clearly speculative.

These are not proven by any possible measure, but they are, at least to some members of the community, well-motivated. There's reasons why lots of people think about these things and take them seriously, even though we, the community, certainly don't have any kind of consensus yet, let alone a lot of reliable, empirical or observational information to help us sift through competing ideas.

So we're in this kind of awkward state where lots of folks are pursuing interesting and sometimes very strange-sounding questions, so they get actually stranger and stranger sounding, I think. And so I wanted to end our semester this term with this taste of some small set of the open questions that members of the community are still really wrestling with and grappling with today.

So I'll focus on ideas about string theory-- some ideas about string theory-- and the multiverse. And there's lots and lots and lots written about this, often for a non-technical audience.

And part of also what I wanted to do with this class was just give you some pointers for some books that I really like, some accessible, really very well-written popular books, by physicists with a range of opinions about this stuff. And so some of them are written by arch rivals conceptually, and they, together, I think can help us get a reasonable sense of at least what seems to be at stake, what are some of our colleagues so exercised about, and why do they have competing visions for next big steps forward on this intellectual journey we've been marching along together this term.

Not all of it is in the *Complete Idiot's Guide,* though I do actually like this book by my colleague, George Musser. He's an award-winning science writer and a PhD in astrophysics. He actually is, himself, not an idiot, but he wrote this fun book for the *Complete Idiot's Guide* series on string theory, and of course, other books as well.

So partly what I'll do with these slides is give a little shout-outs with some book covers for some of these books that I enjoy, you might enjoy, after the dust has settled on this crazy semester, if you're looking for some virtual beach reading for IAP. These might be of interest.

So as usual, of course, three main parts for the discussion today. I want to turn the clock back and look at this really 100-year long quest, 100-year long challenge, to try to find a quantum mechanical description of gravity. It's still an elusive challenge. And we'll see what were some of the earliest ideas about that and why has this approach taken such a different turn, compared to other efforts to describe fundamental forces of nature quantum mechanically. So we'll look at, in some sense, where string theory comes from, and why some people have been so excited about it, compared to other approaches.

Then in the second part, we'll look again, just very briefly, at some of the collisions, some of the possible implications of thinking about these string theory ideas in the context of cosmology, getting a little closer to work that I've been involved with, or some of my colleagues have been more involved with. So when we take this kind of particle cosmology view, and take on board, or try to take seriously, some of the ideas or possible implications of the high energy theory part-- the string theory part-- what does that lead us, at least lead some people, to wonder about, in this more cosmological setting. That's part two.

And then part three, just very briefly, we'll zoom out and again, remind ourselves that like all the work we've looked at together over this entire semester, none of these ideas are unfolding in a vacuum, that I think we can make sense of some of the rhythms of change, if not the particular ideas that come forward, by asking about who's doing this work in what settings, and some elements of a larger context that might help us make sense of the twists and turns.

OK. So as I mentioned this century-long challenge, a kind of grand challenge for the field, has been to find some internally, self-consistent description of gravity that would be in the framework of quantum theory.

We've seen, at least in some we looked at in some detail, others we had kind of hints at, that over the mostly second half of the 20th century especially, each of the other three main known physical forces of nature-- electromagnetism, the strong nuclear force, and the weak nuclear force-- each of those has been given a quantitative treatment in terms of quantum theory, in terms in particular of quantum field theory. We looked a little bit at quantum electrodynamics, the quantum mechanical version of electromagnetism, associated with people like Tomonaga and Julian Schwinger and Feynman and others.

Likewise, we looked briefly at quantum chromodynamics. There's been this remarkable success, often in very close dialogue, between experimental inputs and theoretical advances. And yet gravity, the fourth known force of nature, has been this stubborn holdout.

Turns out this has been going on for-- the effort has been going on for a long, long time. One of the earliest people to try to build a quantum theory of gravity is this young person shown here, Matvei Bronstein, who was working in the Soviet Union in the mid 1930s. He was quite young, as you can see in the photo.

Bronstein was writing papers, mostly in Russian in some of the Russian journals-- a few of them in German. Not very well known outside of his immediate circle at the time, but he was doing what, in hindsight, we see was really pretty advanced stuff and that has held up, I think, quite well over time, where he recognized that if one tried to make a quantum mechanical treatment of Einstein's general theory of relativity, the reigning description of gravity, then you could try to formulate it on a model of exchange of virtual particles.

The virtual particles, in this case, would have a little more complicated mathematical structure. That might not surprise us, given how much we saw even Einstein's own version of general relativity was so mathematically involved, this warping spacetime.

Bronstein showed that you could actually reproduce the basic structure of general relativity as arising from the exchange of a certain kind of particle, a certain kind of force-carrying particle. Instead of the photon being the force-carrying particle for a quantum mechanical version of electromagnetism-- for quantum electrodynamics-- there would be a hypothetical graviton, a particle force-carrier for gravity, and it would be kind of analogous to the photon.

It would have zero mass, just as the photon has zero mass. But it would have two units of spin, whereas the photon has one unit of spin, and most ordinary matter has half integer unit of spin, like electrons and quarks, or even protons and neutrons.

So one could try to reformulate Einstein's version of gravity as arising from the exchange of a particular kind of force-carrying particle, a massless spin-2 particle, dubbed the graviton. That was done mostly in obscurity.

And quite tragically, Bronstein was murdered about a year later. He never had a chance to explore this, or frankly anything else. He fell afoul, as many, many people did, of Joseph Stalin's purges in the Soviet Union. He had some political ideas that suddenly put him on the outs with the reigning authorities, and he was rounded up, found guilty on a so-called show trial, and executed the very same day. There's no chance for an appeal.

His quest, in many ways, ended tragically, tragically early. Many, many years later, other colleagues, including many in Western Europe and North America, kind of rediscovered insights that Bronstein had put together way back in the 1930s.

Much more famously, much, much better known, especially in the US and Europe, Richard Feynman followed along very similar thought paths in the early 1960s, when he himself got interested in gravity, not only particle physics. And Feynman also gave these now famous lectures that were published many years later, his lectures on gravitation, where he also taught his students that one could capture the mathematical structure of general relativity as arising from the exchange of these particular kinds of force-carrying particles. It's just classic.

Now you might be saying, well, don't we already know that gravitons exist? After all, our very good friends at LIGO have found gravitational radiation. They certainly have, using these enormous detectors in Hanford, Washington, in Louisiana, now similar devices throughout Europe, and others under construction elsewhere.

But this is really finding classical gravitational radiation. It is not identifying individual gravitons. It's like the difference between finding classical Maxwell waves, like radial waves, electromagnetic waves that are continuous and classical and extended through space and time. It's a similar gulf between that and finding evidence of individual photons, individual quantized force-carriers of a quantum mechanical force.

So whereas now we have-- thank goodness-- really quite compelling evidence about classical, extended, wave-like features from gravity, this still is not evidence of individual gravitons. A challenge on that score still elusive.

Now even though there's not compelling experimental inputs about the behavior of gravitons, one could still-- and people have over the century-- try to build a kind of quantum mechanical treatment of these hypothetical force-carriers-- these gravitons, massless spin-2 particles. And that's the kind of work that Bronstein began all the way back in the mid 1930s, and people like Feynman and many of his students tried to do head on throughout the 1960s, and so on.

So you can basically say there's a kind of gravitational field, a kind of warping spacetime of the sort that Einstein had described. You can imagine the little perturbations around some average value. You can try to treat that as some quantum mechanical object, all in direct analogy to the treatment of, say, the Maxwell field as composed of photons.

But unfortunately, as people like Bronstein were finding early on, this does not lead to a clear way forward. This approach leads to infinities, when you try to account quantitatively for the behavior of virtual gravitons, much as one would do with, say, the exchange of virtual photons. But unlike the case of QED, or as people now know, even unlike the case of quantum chromodynamics or the others, there's no way to get rid of or to self-consistently absorb these infinities.

And we can get a sense for that by going all the way back to one of the very first lectures this term. Even classically, we can get a sense, at least, for why there might be a disanalogy between, say, electromagnetism and gravity, when we try to treat them each quantum mechanically.

We go back to things that some of our favorite wranglers-- my favorite wranglers were super excited about back in the 1840s and 50s, people like William Thompson and James Clerk Maxwell. They were working out this electromagnetic-- or sorry-- this mechanical worldview, where they found these mathematical analogies between, say, electrostatics and Newtonian gravity. They can each be written in terms of some potential, some function that extends throughout space and changes over time.

In the electromagnetic case, the electromagnetic potential could be understood as responding to, as we'd say, sourced by, where the electric charges are. So we could look at the variation, the gradient squared, of the electric potential as arising from the charge density, the charge per volume.

And if we just do some dimensional analysis-- just some quick dimensions without trying to really calculate things carefully-- we can see that the charge density goes like the electric charge per volume, so it's like some charge, whether it's the charge electron or some larger collection of charges, divided by some volume.

Also remember, as the wranglers certainly well knew, that this all important quantity-- the wave number-- goes inversely like some characteristic length. So wave number is inverse to wavelength.

And so the charge density in appropriate units goes like the wave number cubed. As I mentioned at least briefly some lectures ago, the electric charge, if we choose to measure it in so-called natural or appropriate units, the electric charge is dimensionless. In fact, it's the charge squared goes like 1 over 137. It has a very particular-- just a number. It's not a dimensionful quantity. So dimensionally, the charge density to which the electrostatic potential is sensitive scales dimensionally like the cube of the wave number.

Now let's try to follow that analogy for the gravitational side. Even for Newtonian gravity, the very similar scaling holds even for Einstein's more complicated version. Remember that the gravitational potential is sourced not by the electric charges, but by masses, or eventually in Einstein's version, by mass energy.

So we have to worry about the dimensions of this source over here, the mass density. Well, that goes by some characteristic mass divided by volume. It's a density. The volume factor we just convinced ourselves scales like the cube of some wave number, but the mass, unlike the electric charge, the mass is also dimensionful.

And so something that was not known to either William Thompson or James Clark Maxwell, but has become really clear to theorists over the course of the 20th century, is that dimensionful quantity mass actually matters quite a lot. Thinking just from relativity, we saw, thanks to Einstein, that energy, mass, and momentum are all basically interchangeable. The famous expression E equals MC squared, we saw briefly, is really more generally a relation between energy, mass, and momentum, and they're all interchangeable with some kind of change of units, given by the speed of light.

But if we measure these quantities in appropriate units, they're all dimensionful-- energy, mass, and momentum-- and in fact, they're all really kind of interchangeable. There's not a sharp conceptual divide anymore, in relativity, between mass, energy, and momentum. That's the relativity side.

Then from quantum theory, we saw, thanks to people like Louis de Broglie, and built into the Schrodinger equation too much else, that momentum and wave number themselves are directly related. In fact, they're related by a different constant of nature-- Planck's constant.

So if we go back and find a scaling-- even classically for Newtonian gravity, that the source for these gravitational disturbances, which we might choose to quantize-- if they're sourced by a mass density, that actually has a different scaling with wave number, and therefore with momentum, than in the electrostatic case. There's a disanalogy between, say, quantum electrodynamics and a putative quantum theory of gravity, that has everything to do with insights from relativity and quantum theory-- E equals MC squared and the de Broglie wavelength.

And so what that means, when you try to just follow an analogy to these otherwise very successful quantum field theories, is that those divergences become more severe in quantum gravity than any of the analogous ones for the other forces. Because gravity is sourced by mass, this dimensionful quantity, which itself is akin to a kind of momentum, the integrals-- when we try to count up the effects of virtual gravitons-- the integrals diverge with momentum more violently than even the ones do in for the other forces.

And it really comes down to these very elementary notions from both relativity and quantum theory. The scaling is different.

What does that mean in more in slightly more practical terms? We saw-- again, going back to these insights from Sin-Itiro Tomonaga, during the war in Tokyo and then independently rediscovered soon after the Second World War by people like Julian Schwinger and Richard Feynman-- that for quantum electrodynamics, they could always arrange their equations so that they never had a bare infinity on its own.

They could always organize things as these input output relations-- this was the heart of what became known as renormalization-- and that you could always have these combinations which remained finite, even though each of these, on their own, would diverge usually logarithmically with momentum. So each of these, if you try to calculate them on their own, would be formally infinite, but their sum was well-defined and finite.

And what people like Freeman Dyson and others showed is it that trick holds, order by order, in perturbation theory. You can make arbitrarily precise calculations and these calculations hold all the way through.

What it means for quantum gravity, to have these more violent divergences, the stronger growth with momentum for these virtual graviton processes is that you need actually an infinite set of these absorbing coefficients, that you can't arrange things just in the input output way. You never have a finite number of these absorbing infinities. So you can never absorb or cancel the infinite divergences, at least from this first approach to quantum gravity.

So this leads to a hard problem. And one of the first books that I'll recommend today is this really lovely book that came out 20 years ago by physicist Lee Smolin. It's, I think, a very accessible, very nice book. He is a leading theoretical physicist. He's also a very gifted author.

This book is called *The Three Roads to Quantum Gravity.* And he really goes through some of these early arguments as to why the analogies failed, time and time again over decades, for generations when people tried to apply insights from something like quantum electrodynamics to try to just do the same thing for quantum gravity.

So what now we can call, somewhat tongue-in-cheek, old school, quantum gravity is not subject to renormalization. You can't get any sensible, finite answers from trying to deal with the zigzagging of virtual gravitons, because there's stronger dependence of gravity on things like mass and momentum.

OK. So read Lee's book. He'll explain it better than I do. But I think that's, in brief, some of what's going on was going on over, really, decades in this first wave of effort to combine gravitation and quantum theory.

While all that was unfolding and becoming clarified-- the dead end was becoming clarified-- in a quite different branch of physics, now going back to ideas about the strong nuclear force, high energy nuclear physics and particle physics, in the late 1960s, a number of theorists-- some of them actually, at the time, based at MIT-- were trying to make sense of this.

One of these main approaches we looked at, from the middle decades of the 20th century, for the strong force-- Geoffrey Chew's so-called S-matrix force. He was flipping these Feynman diagrams around, finding these self-consistent, dynamical solutions where basically nuclear particles could make the force-carrying particles that would bring the particles together to make those force-carrying particles. Could you find one self-consistent set of relationships among the particle zoo?

And so younger theorists were trying to play with that self-consistent swapping structure, and they kept finding these extended objects-- geometrical objects-- in space, where a lot of the force among these particles would be spread out, splayed out in a cylinder or tube, a kind of a string-like, flux tube. And that really was, as far as I'm concerned, what might explain the nuclear force that keeps things like protons and neutrons bound within atomic nuclei.

That was being published starting in 1967, '68 by a small little circle of specialists. It was curious. It was interesting. And it was very quickly overshadowed for the reasons that I think we can make sense of, given what we saw together, the kind of gathering force, the kind of choppy road toward the quark model and ultimately quantum chromodynamics.

So we saw, although the idea of quarks had been introduced as early as 1964, it took really a solid decade, into the mid 1970s, before even the quark model proponents themselves, like Murray Gell-Man, were saying that these are physical entities in the world. And it was really only after the successes of this very specific quantum mechanical field theory-- quantum chromodynamics, or QCD-- where the various pieces came together and also got some helpful, bolstered by some new experimental inputs.

So by the mid 1970s, it looked like people had this strong nuclear force well in hand. And all these kind of weird, funky, geometrical, flux tube things from the Geoffrey Chew-inspired work, that kind of faded away. It wasn't proven to be wrong, it just seemed like it was no longer the best road forward.

The quark model finally seemed to be scoring success after success, and that was built, as we saw, in much closer analogy to quantum electrodynamics. You imagine elementary point particles with certain kinds of charges-- in this case, quarks and gluons-- and you trace the virtual particles among them, and all that in a framework, much more like quantum electrodynamics.

So the early string ideas really kind of fell away, not because it was just proven, but because they seemed like maybe not the best way forward to understand the strong force.

Before people stopped paying attention though, a few of this small circle recognized something curious in their equations for the Geoffrey Chew-like for the string approach to the nuclear force. If you look at the low energy limit of these complicated, self-consistent, highly nonlinear equations, it looks like, in low energies, this seemingly nuclear type interaction includes the exchange of a massless spin-2 particle.

And some people said, wait a minute. I remember hearing something about that. A massless spin-2 particle maybe comes up in other contexts, like in gravity.

And so a few groups who knew about the kind of strong force, stringy work said, maybe these insights about strings extended 1D structures, maybe it has nothing particularly to do with the strong force, but maybe that was a theory about gravity and not about nuclear particles.

If so, then the typical length scale for these things would not be the size of a single nuclear particle like a proton, it wouldn't be femtometers-- 10 to the -15 meter-- it might be the Planck length, the smallest possible length longer than which one should be able to ignore quantum gravitational corrections. Maybe the actual fabric of Einstein's warping spacetime consists at these unimaginably tiny scales of a play of these extended one dimensional objects. Maybe the strings are what give rise to the effects of gravity, not that bound quarks within a proton, for example.

So because the low energy behavior of these stringy structures gave rise to just the kind of particle needed to make sense of gravity, in terms of particle exchange, the ideas about string theory became very interesting to a small circle as a possible road toward quantum gravity-- quite different from the kind of particle scattering QED type model.

In fact, it looked even more promising, again, as many of these folks began to put together by the early and mid 1980s, roughly 10 years later. Then it looked like calculating effects-- quantum virtual effects-- in this string theory of gravity might actually avoid all these messy infinities altogether.

Why was that? The infinities keep arising in these quantum field theories because people integrate up to an infinite virtual particle momentum. That's like saying-- that's like imagining that we could model the exchange, the scattering of, say, a virtual force carrying photon an electron, that they meet literally at a mathematical point. They meet in a region of infinitesimally, tiny region of space.

That suggests that they could be scattering off each other with a borrowed momentum, up to infinity, up to 1 over 0. The delta r could shrink to zero. That means that the momentum exchange could, in fact, become infinite, which is why you have integrate those integrals up to infinity.

Well, these string objects are actually sweeping out not world lines through space and time, but actually world sheets. They are extended objects, so the analog Feynman diagrams never pinch down to a single mathematical point. In fact, you have these kind of pants legs structures, where you have one kind of extended structure might blend into others, but they never pinch down into a mathematical point.

So if you never have these point like vertices in the kind of scattering among these putative strings, then you actually never integrate up to literally infinite momentum. So maybe virtual processes involving strings would naturally avoid all these divergence problems that seem to hang up the old school or classic approach to quantum gravity. So maybe you could have a finite theory of gravity that has the right particle exchange in the appropriate limit and avoids infinities.

So why weren't we done in 1984? Well, because the same scholars found a pretty interesting catch. Some call it a bug, few would call it a feature. The simplest models that could be written down, that could be applied to a gravitational model, where these strings could be dancing around at the Planck length and giving rise, in the appropriate limit, to Einstein's general theory of relativity.

They only could live mathematically in 26 spacetime dimensions. I put that in bold and italics, so we'll linger on that for a second. The earliest self-consistent models, when applied to gravity to get the right limit at low energies, to make it look like Einstein's theory at low energies, these strings weren't dancing around in three dimensions of space, at one of time-- XYZ plus T. They could only dance around in a mathematically, self-consistent way if they were living in a 26 dimensional spacetime.

It seems pretty clear that we live in a four-dimensional spacetime, so what's going on? A little while later, some of these physicists found that they could actually make these mathematically, self-consistent string models of gravity. If they incorporated a particular symmetry-- strictly hypothetical symmetry-- for which even, to this day, there is no empirical evidence at all, a symmetry called supersymmetry, which suggests that for every known particle, every quark, every electron, every neutrino, and all the rest, there is a partner-- a super partner particle-- with the same mass and same charge, but one half unit more of spin.

So for every spin, one half particle, like quarks, and electrons, and neutrinos, there would be integer spin superpartners and vice versa. So for every integer spin photon, there would be a half integer or three halves integer superpartner, like a photino. For every quark, there would be a squark, and so on.

So at the cost of doubling every single known form of matter, these superstring theories could be formulated in only 10 dimensions of spacetime instead of 26. That's getting more than halfway there, but it still means a lot more structure than what we seem to measure or just observe in our daily lives.

So these early superstring theories for gravity had some unbelievable promise. These people weren't of deluding themselves that these could be of interest. But it wasn't such a straightforward deal.

So we can then go to some of the kinds of plots that I love to make and start counting stuff. So the blue curve shows all physics articles as covered by something called physics abstracts, a kind of worldwide consortium of physics-trained librarians in many parts of the world, trying to literally count up and categorize, by subtopic, all the articles by professional physicists in the peer reviewed journals, in as many languages as they could as they could master.

So that's the blue curve. And you see it's growing fairly steadily over this period. And the red curve-- and I've renormalized it myself-- the red curve is then all the articles on string theory, including superstring theory, over the same time period. To fit them on one curve, I've actually divided the blue one by a factor of 100. So by the end of this period, the red curves are about 1% of all the publications in all of physics. Let's say they're almost equal, but I've cheated by making the blue curve changes amplitude by a factor of 100.

So you see these very distinct structures in the effort expended on superstring theory, compared to global efforts on every branch of physics altogether. You see virtually no attention at all to string theory. It really is a very curious little specialty area until the mid 1980s.

What proponents of this work later called the first string revolution-- which you have to know they only named after a second string evolution, right? No one called World War I World War I until World War II. So what later became called the first string evolution, in hindsight, was identified with this work that was really published by a few independent small groups around 1984, where they began showing that the stringy stuff could include the structure of gravity, if you imagine that string-like filament to be describing gravitational interactions at the Planck length, as opposed to nuclear physics.

And indeed, there were several different, self-consistent, supersymmetric models published all in 1984, '85, and that led to an enormous burst-- an exponential rise of interest-- in string theories, coming very soon after that so-called first revolution. Then there's a kind of plateau for about a decade. And then there was what the proponents came to call the second string revolution, almost right on schedule 10 years later.

What happened with the second string revolution, in brief, was that a few of these specialists found that what looked like five totally separate distinct, but self-consistent, superstring models. They looked different from each other. They all were mathematically self-consistent, even though they looked like they were different candidates.

In the second wave, people like Edward Witten and other colleagues, found kind of one to one mapping. So in fact, these five different models might all be ways of expressing a single shared model. These might be almost like coordinate transformations of a single underlying model, and that became known as the second revolution.

So if one performed what became known as dualities, it really is analogous to coordinate transformations. You can actually map, feature by feature, those seemingly five distinct models into a single structure.

That got people excited again. You see another exponential takeoff of interest. So that by the early 2000s, efforts on string theory, which had once been vanishingly tiny, occupy nearly 1% of all the physics output of all the physicists on the planet.

So let me pause there, see if there's any questions on that kind of alternate approach to an effort to try to quantize gravity. Any questions on that? It's so straightforward. You're all convinced. You're all now card-carrying string theorists. No lingering questions?

Julie is in. Thank you, Julie. I'll tell Andy Strominger. He'll be glad. Any other questions? If not, that's OK, of course. But I'll press on.

Because as if that weren't strange enough, boy, have I got news for you. You think that's strange-- we live in 10 dimensions-- let's see where people take this, since they're now paying attention. OK.

Now I very blithely told you-- and none of you seem to object, so clearly it's the end of the term-- that these superstring theories can only be formulated in a minimum of 10 spacetime dimensions. They still only involve one timelike dimension. That means nine dimensions of space-- height, width, breadth-- and six others all at right angles to them. I mean, I look at where the walls meet in my house and I can't find where the other six right angles would come from.

So it's not just a failure of my own imagination, which fails all the time. This leads to some pretty strange-sounding possible implications. Let's take a very simple one, going back again, right to the very first class sessions from this semester. Think about, say, Faraday's lines of force.

Unless something very strange is happening, we really can't live in a 10-dimensional world. If you imagine a single mass-- some source of gravitation, some ball, with some mass, a spherically symmetric one, to keep things simple-- set it down at the origin of our coordinate system. Then just like a single electric charge that got Michael Faraday so excited in the 1820s and 30s, we can try to imagine how lines of force would emanate, in this case very simply, very spherically symmetrically, lines of gravitational force emanating from that source, that mass M at the center.

Well, we can then try to do like Faraday did and an envelop, kind of surround our source, with an imagined sphere. We can change the radius of the spheres, we can imagine how the strength of that force should fall off with distance. Now again, as you know, the surface area of some sphere will scale as one power less of the radius compared to the volume. That holds in three dimensions. The surface area of a sphere goes like R squared, the volume goes like R cubed.

That same geometrical relation holds for any dimensions of space, any hypersphere, in particular one that's in, say, nine dimensions of space, the surface area we can vary self-consistently project an appropriate sub manifold, to be all super fancy geometrical about it. We can calculate the surface area of a nine sphere, and the area will grow like the radius to the eighth power, to D minus 1.

So if we just redo Michael Faraday's really lovely, ingenious in fact, geometrical scaling, then if we really think gravity lives in nine dimensions of space, then we would expect Newton's law to go like 1 over R to the eighth, instead of 1 over R squared. Why are we finding inverse square laws even in the context of Einstein's general theory of relativity, very high accuracy?

Whereas if we just have gravity spilling out across nine co-equal dimensions of space, we would expect a completely different behavior for the force of gravity. So what's going on when you say that this theory of gravity is formulated in nine dimensions of space?

One of the earliest efforts to address that is called compactification, which really just means make the extra dimensions super tiny. So the idea is that for, as yet unexplained or unknown reasons, the idea is that these extra dimensions of space are not arbitrarily extended. They're not macroscopic. I can't take a walk in dimensions numbers 4 through 9, because, at least hypothetically, they've been curled up onto each other.

And the way the analogy is always made, and you might have heard before, we think about a soda straw or a garden hose. Now if you're very close to that straw or hose, you can see it's an extended object. It has both a length and a width to it. But if you look at that object from very far away, it looks indistinguishable from a dimensionless line, an arbitrarily thin line extended only in one dimension.

So the idea was, what if these extra dimensions of space are not macroscopically large, but in fact, are curled up on each other? They have a kind of internal radius that never becomes large, that's controlled by some, as yet, hypothetical kinds of dynamics, so that from our clumsy human sized scales what is actually a garden hose at an impossibly tiny radius looks to us indistinguishable from being merely a line of zero width.

That's the kind of hand-waving version of how you could compactify, in this case, one extra dimension. So we might live in a universe with X,Y and Z, macroscopic height, width, and breadth, and then W, let's say some fourth dimension of space. But that one is a tiny little circle that we don't probe at low energies, and so we never see this structure, because we're never measuring it on the right on the right length scales. If the curvature of that garden hose, of the radius, were of order the Planck length, we'd have no way to know it directly, because we can't measure anything with such high spatial resolution.

So that's the idea to compactify these extra dimensions. That works pretty straightforwardly if you only have one extra dimension of space to get rid of. But remember, these superstring theories require at least six extra dimensions, nine dimensions of space, plus one of time.

So we have three macroscopic ones that we can self-evidently move around in. So you have to compactify six extra dimensions, not just one. It turns out, again, as people began finding, as early as the mid 1980s, that that becomes an unbelievably complicated problem in topology, that there are estimated to be about 100,000 topologically distinct ways to curl up, or fold up, these six extra dimensions.

So at every point in XYZ that we could walk along, there would be curled up, maybe in some different pattern, six extra dimensions of space, with a characteristic scale of order the Planck scale, 10 to the minus 35th of the meter. And yet the actual curling up-- the shape of it, really, the topology-- could be different from one location to the next. So people start talking about these so-called Calabi-Yau manifolds, a topic of great interest to pure mathematicians, and some of the string theorists wondered would that kind of topology accounting be useful for making sense of these shrunken down, compactified dimensions.

My next book club suggestion is a lovely book by a colleague, Lisa Randall, who's teaches at Harvard. She used to teach at MIT, but Harvard convinced her to move all the way across campus, and a really lovely book called *Warped Passages,* which Lisa walks through some of the reasoning behind these extra dimensions of space, what are different ways to try to make them go away from our everyday experience. So she claims that, I think, really very nicely.

So the first main conceptual approach to these extra dimensions is curl them up, even though it turns out that's pretty complicated to do.

It got even worse. I would call it worse. It got more complicated in the early 2000s, and we're getting kind of close to where we are today. Other string theorists began to realize you don't only have to worry about the actual kind of dimensions of space, which can be topologically complicated in those so-called Calabi-Yau manifolds, but you actually have to worry about self-consistent configurations of, say, these strings within and among those dimensions.

So you don't only have topologically distinct configurations of geometry, you actually have some physical degrees of freedom dancing around on them. And as you can imagine you get quite a different solution if your string is wrapped around a sphere, versus if it's wrapped around a torus, versus if it's wrapped around three torus or whatever else.

So when they began counting up the actual number of low energy string theory states, we worry not only about the geometry of the dimensions of spacetime, but actually how these physical degrees of freedom-- these string-like states-- could be distributed within and among them. They came up not with 10 to the 5, 100,000, they came up with 10 to the 500, which is more than a rounding error.

So they began to be concerned that there seemed to be 10 to the 500 distinct low energy string states-- 10 to the 500. I won't ask for a show of hands but it's pretty hard to come up with numbers like 10 to the 500. I've tried. Let me give you some of my mundane examples.

If internet accounts are to be believed, then Jeff Bezos has 10 to the $5 for every buck that I can lay claim to, and that means leveraging everything-- sell the house, the kids never go to college, forget the car. Every single cent I could rub together, Bezos has more than $100,000 for every dollar I could scrape together. So that's either impressive or depressing. It's mostly depressing. But 10 to the 5 is nowhere near 10 to the 500.

OK, so now let's go cosmo-- let's take the whole universe. Let's put the universe to work for us. If we try to measure the age of our universe in seconds, we get a measly 10 to the 17. If we're a little fancier and say, let's measure the age of the universe with the shortest measurable unit of time that has been measured so far in quantum optics, that's about a femtosecond, so 10 to the minus 15th of a second. That only gets us to something like 10 to the 32. We're still scraping. That's nowhere near 10 to the 500.

Now let's compare, say, the mass of the entire Milky Way galaxy to the mass of a single electron. That's really big. It's a big galaxy. Electrons are tiny. That ratio is 10 to the 71. You get the idea, right? 10 to the 71 is basically equal to zero when we're comparing it to a number like 10 to the 500.

So what became either interesting or horrifying to me, I think-- horrifying-- in the early 2000s was that a number of these string theorists realized that there were unfathomably large numbers of what looked to be self-consistent, low energy string states within these superstring theory models.

Now why should we care, because at least within this superstring theory framework, every single quantity with which we might try to characterize our universe-- the masses and charges of every kind of particle, every electron, quark, gluon, everything-- would depend, in principle, on the particular one out of those 10 to the 500 string states in which our universe happens to land. So even for elementary particles, this is a huge mess. Why is a charged electron that versus something else?

Even for bulk or astrophysical properties with which we characterize our universe, things like the expansion rate-- that's the letter H, the Hubble expansion parameter-- the rate at which galaxies recede from each other, or things like the value of so-called dark energy, or the cosmological constant, even the bulk properties with which we try to characterize our universe at large, each of those numbers, the actual quantitative value would, in principle, depend on which one out of these 10 to the 500 distinct string vacuum states.

So how do we make any predictions or how to make any sense at all, empirically or quantitatively, out of the universe we live in, either from data like the Large Hadron Collider or from all of our astrophysics cosmology, if suddenly we have no way, or at least no practical way, it seems, to relate this kind of seemingly fundamental description of nature at these very tiny string scales to the things we can actually probe, measure, and wonder about? That became this new twist in the string theory saga in the early 2000.

So here, once again, is Alan Guth. That idea about the so-called string landscape-- these 10 to the 500 equally self-consistent, low energy states of string theory-- that set of ideas kind of collided with some cosmological ideas which had first been developed quite independently.

So we talked, in the most recent class session, about cosmic inflation, a set of ideas that I dearly love. It turns out early on in thinking about inflation, people, including Alan and Paul Steinhardt, and actually a number of folks we mentioned briefly last time-- Andrei Linde-- began to recognize something curious about these inflationary models.

Basically, each inflationary patch-- each inflationary phase, let's say-- has some kind of natural lifetime. It's almost like a radioactive decay, that the configuration of those Higgs-like fields that temporarily drive this very rapid, accelerating expansion of space and time, they have a kind of natural decay, or natural lifetime. It's not infinite. In fact, we know it was a very short-lived period of inflation.

And in fact, we can estimate for a given model what a half life would be for how long that phase is expected to last. It's probabilistic, it's a quantum mechanical process, but it's very similar to calculating a radioactive half life.

And what became clear is that for many, many, many of these models, that otherwise seemed to match predictions on our sky beautifully, they're models that seemed to have many nice features, that the half life, the decay rate, so to speak, is actually smaller than the expansion rate. That in regions that have not yet decayed out of that phase, space should be stretching exponentially quickly, more quickly than the rate at which neighboring patches of space will fall out of that inflating phase.

This became called eternal inflation. And the idea is that-- at least it seems self-consistent to suppose, based on what we otherwise know-- that if inflation begins anywhere in the universe, it never stops. It will stop at that location. It's not inflating for us right now. It'll stop at that location, but there'll be some other regions of spacetime that are still growing exponentially quickly.

And that's because there's this competition between the rate for any given location in space to fall out of inflation. That's the kind of capital gamma, the kind of lifetime of inflating phase, and the rate at which the volume of some neighboring region continues to grow.

So even though it's perfectly self-consistent for our universe that we see around us-- our observable universe-- not to be in that inflating phase right now, or to have phonon out of the primordial one, for all we know it looks more likely than not, at least in this scheme, that some regions so far away from us, we haven't received even a single light beam yet, they could still be in an inflating phase. And that could happen forever, because when that neighboring point falls out of inflation, some other volume of space will grow quicker still.

So that led to this idea that Andrei Linde in particular really championed, and Alex Vilenkin at Tufts-- many scholars who were interested in cosmology-- began wondering about a so-called multiverse. Could it be that our observable universe-- everything we've seen, everything we could measure, the CMDB and all the rest-- is that actually just one self-contained region of space and time within an arbitrarily larger region in the midst of which other patches are still inflating exponentially and others have [INAUDIBLE] out as well?

Could you have an infinite multiverse? If inflation never ends, if it really could happen towards T equals infinity, then in principle, you have an infinite volume of space. We occupy, we can observe around us, a very tiny, self-contained bubble within an infinite bathtub. We're like a soap bubble within a bathtub that actually is infinitely large in volume.

So that was the idea of eternal inflation, suggesting-- not proving, but suggesting-- there could exist a multiverse, a huge collection of pocket universes well beyond our own with which we can gather information. And that idea then collided with, or was merged with, this series of ideas from string theory in the early 2000s, in particular the so-called string landscape.

If there are 10 to the 500 distinct, self-consistent, low energy string states, and you have an infinite number of chances for them to be realized, because you have an infinite multiverse within which you can have these self-consistent pocket universes constantly inflating and then falling out of inflation, while neighboring regions inflate, could it be that every single string state is actually realized an infinite number of times? So 10 to the 500 is big. It's infinitely smaller than infinity, right?

You can see where-- I hope it's clear that I'm not endorsing these ideas. I find these ideas pretty unusual, is the nicest way I can put it. But I can at least appreciate why so many of my colleagues have been led to wonder about them. You can see the thought train that leads to this as the next question or tentative conclusion. You can also see how it's getting harder and harder to get the reinforcement, or the input, or the kind of dialogue with empirical data, unlike, say, the early days of quark physics.

I hope I can at least convey to you, as a collegial neighboring skeptic, why people ask these kinds of questions, this kind of merger of ideas from string theory and from things like inflation. OK.

So if every one of the parameters that we measure empirically-- the masses and charges of elementary particles, larger properties like the Hubble expansion rate-- if they were slightly different, people began to wonder, would the universe, as we know it, would our observable universe have shown anything like the pattern of evolution that we have actually empirically measured?

Could there have been galaxies that actually formed stably if the expansion rate were slightly larger? The idea was if the expansion rate were actually a little bit larger in early times, could galaxies never have overcome the expansive pull of gravity, where they've never coalesced to become gravitationally self bound.

If the expansion rate were slightly larger, then it happened to have been in our observable universe, maybe there would never have been galaxies. If there's never galaxies, could there be things like stable solar systems or planets? You can play this game all the way down. Could there be life as we know it, if any of these seemingly fundamental parameters have been slightly different from the values that we happen to measure?

Now it turns out historians can remind us, that's a very old argument. If that sounds familiar, especially to our historians of science in the group, you're right. That idea-- that if any parameters of our environment had been slightly different, then we wouldn't be here-- that goes back at least, in the European tradition, at least to the late 17th century. I'm sure we could find clever anticipations even earlier.

But some of the most famous articulations of that come from this very charming book by Bernard de Fontenelle, a French natural philosopher, translated as *Conversations on the Plurality of World.* It's still in print. You can buy a cheap paperback.

Very soon after that, Isaac Newton was writing letters privately to a theologian friend of his-- they were published soon after Newton's death, his friend Richard Bentley-- where he was musing on this as well. To these folks, this kind of fine-tuning argument was to them absolute, incontrovertible proof that God exists. And to them that was a fairly recognizable Christian God of the Bible, in their reckoning.

The idea was that God must have made the world just for us. If the Earth were slightly further from the sun, it would be too cold for us to have flourished. If the Earth were slightly closer to the sun than it is, it would be too hot. These kinds of fine-tuning arguments have a very, very long history in astronomy and astrophysics. And for a lot of the early advocates, this was taken as literally proof of a biblical story of Genesis, that God made the universe literally for us.

So is that how these later string theorists formulated? Not really-- not at all. So 2/3 of that fine-tuning argument sounds just like what Isaac Newton or de Fontenelle had been saying 300 years earlier.

But some of these later, more recent advocates give it an interesting twist-- the appeal not to a religious or a kind of supernatural force, they appeal back to us in something called the anthropic principle, which again you might have read about. The idea, as I've mentioned before, as even Fontenelle and Newton had had recognized, is that the natural constants seem to have to be within very specific and actually narrow, fine-tuned ranges for anything like life as we know it to have survived, to have evolved and survived.

So if those parameters were not within those finely-tuned ranges, then we wouldn't be here to ask these questions. So it comes back to what's the precondition for cosmologists, or any humans at all, to be asking questions about our universe?

And so again, here are some other very nice, popular books that came out around 15 years ago or so. One by Alex Vilenkin, who teaches at Tufts. He's very nearby, real pioneer in this work. Another book by Leonard Susskind, who teaches at Stanford. And they're both discussing these more recent developments when the string landscape meets the multiverse.

And you can see each of their answers are very clearly not appealing to a kind of religious explanation. In fact, they say the basic structure of the argument is if there are 10 to the 500 distinct string states, each of which has an infinite number of chances-- because this eternal inflation argument-- each of which is realized in an actual physical volume of space, an infinite number of times, then it just takes pure, random chance.

It's almost kind of Darwinian, that we would evolve where we are to measure the constants we do because we had no choice. That is say, any difference in those constants, which might really be happening arbitrarily far away, people like us wouldn't have evolved there to ask those questions.

So the explanation-- why do we measure this value for the expansion rate or that value for the mass electron-- is not, they argue, because we have to know why one special value out of those 10 to the 500 gets picked out, but simply to say they're all out there. Each of the 10 to the 500 is out there beyond our immediate vicinity. We should only ever expect to measure the constants that we do, because of the chain of causes that had to happen for life, at least as we currently understand it, to have been able to come around to ask those questions.

So it's not that you pick out one string state physically. You say, we'd only ever be able to measure a certain very small subset.

So some critics-- I'm going to say say critics within physics-- critics, Nobel laureates, within high energy theory for example, some of them really, really don't like this idea. I personally am still rather ambivalent, to put it mildly. Critics have called it, in print, dangerous, disappointing, a virus-- which was scary even before coronavirus, now it really makes me shudder-- an abdication of what physicists should be doing. This inspires strong reactions.

Lenny Susskind, in this book here, knows that perfectly well. He's no stranger to making bold statements. So Susskind actually borrowed, for the epigraph of his book, the famous, and probably apocryphal, statement from Pierre Simon Laplace in answering Napoleon. Napoleon was so impressed by Laplace's 18th century, Newtonian cosmos. They could account for the motion of planets and comets and everything with such great precision.

Napoleon supposedly had asked Laplace, where is the room for God in your theory? And Laplace, like Susskind, says, your Highness I have no need of that hypothesis. He doesn't need, as Newton seemed to need, an appeal to a kind of religious or biblical supernatural force. He says, you just have to have 10 to the 500 distinct states and an infinite number of chances. And we would measure, around our universe, only those that would be consistent with us being there to measure them.

Again, I'm not endorsing that view. I find that interesting, although I personally am rather ambivalent. But hopefully, you can see how the kind of chain of reasoning has unfolded to where this is now talked about with some great attention, and inspires great passions among physicists and cosmologists.

So let me pause there again. Any questions on the string landscape, on eternal inflation, on anthropic principle, any of these juicy ideas?

Very good, Steven. Thank you. That's a great question.

The short answer is, in principle, yes. If we had a arbitrarily powerful microscope, if we could zoom in to length scales of order-- the Planck length, 10 to minus 35th of a meter, which we can't-- that it could be that each of those was separately realized at each point, that we had separately called X equals 1, X equals 2, X equals 3 of the macroscopic space that we otherwise can move around and measure.

So it's not that they must have been, but actually that they could have been, that these are each self-consistent, viable solutions to this complicated set of superstring theory equations. And, in principle, each of them could independently be realized. Or it could be that there's some kind of coherent structure, where they all look the same in one region, but then in some macroscopically distant region, some other set of values might have taken place.

When the idea gets combined with the string landscape, it's more the latter, that there are actually microscopic regions where there would have been one of those states versus another. But then something like the Higgs field or an inflation-causing field settles into one of those vacuum states versus another, and then the volume of that space-- the three dimensional volume of that space-- grows exponentially.

So when you start combining the 10 to the 500 with ideas about inflation, then it becomes more like we should only be able to measure one of those versions macroscopically, but all the others could still be realized elsewhere. So to the string theorist alone, the idea was each of those could be a self-consistent solution at any arbitrarily small location in the X, Y, Z space we use.

But if you start thinking about combining that with inflation, the idea tends to be that all those are near each other, so to speak, at early times, but then whatever's going to start driving a macroscopic inflation for the dimensions that we live in, will actually stretch the space containing that one string state, rather than the neighbor.

So I guess in an eternal inflation multiverse model, I think the idea would more be like we would expect to have a coherence, that if we could zoom in with our microscope we'd measure the same string state within our own macroscopic bubble, even though, in principle, there could be bubbles out there where it's different.

So Alex asks-- oh, go ahead. Steve, go ahead. Do you have a follow up?

I think, in principle, they could, but if you add on the dynamics, that you're actually expanding-- really stretching one region of space to the exclusion of another-- then you're stretching the space that was filled with one of those states. I think that's the idea.

So Alex asks, of those 10 to 500 states before the infinities, how many are actually livable? Right. So that's a good question, Alex.

And part of the question is, honestly, I don't think anyone really knows. Because we could take what we think we know about the origin of life-- what I know about the origin of life is approximately zero, but people who look at it more carefully-- or even something that may be closer to astrophysics, like what would it take for stable galaxies to form and not get ripped apart by a too fast expanding early universe? That's one that people looked at in more quantitative detail.

So if you change one parameter, in this case, the early expansion rate, you either do or don't get stable galaxies to form. But we have a lot of parameters we could tweak. What if we tweaked the electromagnetic force, and the unit strength of gravity, and the expansion rate, and-- right?

So who's to say, in this multi-dimensional system of constants, even the ones that we know about, that one couldn't find compensating solutions? Could we find galaxies for them if we did have the faster early expansion rate, but if we tweaked three other parameters of our choice? And that's, I think, a well-posed critique of this wiggle one parameter, find things do or don't work.

And to be honest, I think a lot of the work so far has still been, understandably-- to make it computationally tractable-- understandably, it's still mostly been wiggle one constant what changes. If the value of the electric charge was slightly different, could you have stable hydrogen atoms, yes or no. OK, well, if I tweak that in five other things, maybe I can again.

So the short answer, Alex, to your very well-posed question is, I don't know and, frankly, I don't think anyone knows. The way people have tried to pose that is in this somewhat simplified or, let's say, tractable approach. But I agree. If we're honest about it, I think it's a wide open question.

Kay asks, is there any connection between the anthropic principle and the [INAUDIBLE] idea that quantum values don't exist until we choose to measure? There could be connections. It's a great question, Kay.

One of my coauthors-- so now it's getting close to home, I've been tainted by all these crazy people--no-- a very dear friend has actually published on a quantum multiverse. What if the many worlds interpretation from quantum theory-- which I think is kind of like what you're referring to here, Kay-- what if the many worlds interpretation of quantum theory were actually realized in physically distinct but disjointed regions of space, like in the multiverse?

So one can at least try to pursue this question. It's not required. So typically, these are actually separate kinds of many worlds that people talk about. And so typically, they're kept separate. But certainly, conceptually, one can try to build these self-consistent ideas and maybe even see if that leads to new predictions. So far I haven't seen any.

But one can try to see if these sets of ideas could even fit together in principle, let alone do they help explain one another. So Kay, that's a great question. So people do work very squarely on that.

Fisher asks, since these X dimensions are on the scale of the Planck length, I imagine there's no experiment we can perform to determine if they have any physical meaning? Good.

So I should say, the limits, empirically, are that these extra dimensions can't be larger than about millimeter scale. And millimeter is a lot bigger than the Planck length. There's no compelling reason why they should be millimeter scale and not larger, right? So that would still take some explaining.

But the high precision tests of gravity-- that's where these limits mostly come from. Testing gravity in a classical, general, relativistic framework, people have done pretty compelling experimental bounds, saying there's no measurable departure down from like millimeters out to tens of billions of light years. That is just unbelievable.

So could it be that these extra dimensions were as large as the size of a proton, let alone the size of a millimeter? Maybe. So one way that people try to look for them, if these extra dimensions were small on human scale, but not so small, is with something that's called missing momentum. It's different from the missing mass problem.

So at very high energy particle accelerators, like the large Hadron Collider, is there basically momentum that goes missing, in otherwise very careful momentum conservation balance? When you smash, for example, two protons together and collect all the junk that comes flying out, every time they've done it so far, the momentum balance is perfectly. We should think that's really great, that's great.

If there had been some of these particles, in some sense, taking some of their journey, taking a kind of detour, like a shortcut through some of these extra dimensions of space, then the momentum balance that we measure in our three dimensions should, in principle, be off, and maybe by a measurable amount if those shortcuts were over long enough distances, like microns, not Planck length. So there is a program to use particle collision data to put limits on that version of a shrunken down, extra dimension.

I should say, I mentioned Lisa Randall's book, *Warped Passages.* In that book, Lisa talks about one of her own very, very, very ingenious interventions, was what if these extra dimensions actually never got small? What if they are still macroscopically large?

What she found with a colleague back in the late 1990s-- and I did some work on these models, but really Lisa was the driving force-- was could find self-consistent, gravitational configurations in which we would live on a 3 plus 1 dimensional slice. And in fact, quarks would stay stuck here. Even gravity would appear to be stuck on this kind of sliver self-consistently. It's almost like bound states for gravity. It's like an analogy to bound states.

So the graviton might be exchanged, but might be exchanged only within a subspace of all that are out there. And you could find, again, simplified toy models in which gravity would behave that way, and Lisa Randall was really the pioneer for that, starting in, I think, '87, '98.

So there are other ideas around to try to live with, at least conceptually, large extra dimensions, without having to curl them up. Now the challenge there was could one find a model that has the symmetries that we actually observe in our universe and not more? So those models work really very, very lovely, if you have very, very highly symmetric spaces for the ones that we would live on, that look either exactly like Minkowski space or something very similar.

We live in a slightly less symmetric space time than Minkowski space. And it turns out, there was at least no mathematically demonstrated way to embed even that simplified toy model with one fewer symmetry group in that structure. That doesn't mean it couldn't be done.

So that's an open question, but there are many ways to try to live with-- make peace with-- many extra dimensions, but they all require a pretty big leap. And none of them so far has been compelling, forced on anyone, based on new experimental observations or anything else. So it's still in the realm of the hypothetical. Good. Other questions.

I think part of it is that, really, at least to its critics-- and frankly, I'm sympathetic with this critical view-- it seems to give up on the aim of trying to explain the universe that we live in, based on physical forces or laws that we could explain. It basically says that the universe that we live in is the outcome of, basically, random chance. It makes it more like the way we account for change in Darwinian natural selection, but now with infinities instead of just large numbers.

It could just be random twists and turns of fate, and maybe that is the right answer. But that kind of grates against-- maybe it's a philosophical or aesthetic preference, it's not like it's a logical necessity-- but it grates against a kind of form of explanation that has, I think, otherwise been successful as a goal.

It's aspirational for things from Newton's day, through Lisa Randall's day, for that matter. Efforts to try to account for a specific set of, let's say, fundamental forces, acting on a specific set of elementary constituents, whether they're quarks and gluons or superstrings or name your favorite, and being able to account for bulk properties that we could compare with observations.

I think the thing that sticks in the craw, so to speak, for critics of the anthropic principle is that at the end of the day, people have to say, just random chance, and there's no kind of deeper explanation for why we measure the things we do. And they not only could be different, they actually might be different, and maybe even are different, infinitely different ways, out beyond areas where that are kind of outside our own causal horizon.

So it's changing, so to speak, the explanatory goals. It's not that it's logically, internally inconsistent. It's a different kind of aspiration or aesthetic choice or philosophical aim for what one thinks the nature of explanation should be, in an area of science like physics. And they might be right.

Let me press on for this last part much quicker. So I mentioned that string theory has some great features, at least features that got people excited. We could make sense of those bursts of enthusiasm on the publication pattern. But actually, I borrowed the term package deal from Lee Smolin, one of whose books I mentioned earlier.

It's like when you want to go buy a used car off the lot. You don't get to choose exactly which features you want. And so, again, Lee Smolin uses this analogy. It's like, what if the car you really love-- this one on the used car lot-- has just the right kind of transmission you're looking for, but not the stereo system you want, or vice versa. It's like a package deal.

And so string theory has many features like it includes massless spin two graviton, it seems like it could avoid some of these infinities from quantum electrodynamics, and yet it can only be formulated self-consistently with these as yet undetected symmetries, the so-called supersymmetry, doubling every known particle in the universe, and also many extra dimensions.

And so critics like Lee Smolin have said, are the things we don't want starting to outweigh the things that we do want? Is this a is this a good deal in this package deal?

And so again, for your reading list, these are now kind of classic books. Brian Greene's book came out first in 1999. Brian has been, for a long time, one of the most, I'd say, eloquent proponents of the superstring theory approach. And this book was a finalist for the Pulitzer Prize. It's a beautifully written book, very accessible.

And then again, a very clear book by one of these now outspoken critics, Lee Smolin. This one came out in around 2005 or 2006, called *The Trouble with Physics.*

And I think these two together give you a pretty nice range of, again, what the stakes are in these so-called string wars. What are the hopes that got people like Brian and so many people excited, and what's the kind of cold water? What are the cautions that some critics like Lee Smolin put on the table as well? Those are two you might enjoy.

Let's go back to counting things. As I tried to indicate and we've seen many times in our course together, these rivalries or debates are not happening in a vacuum. They're not happening in a string vacuum, not even happening in a macroscopic vacuum.

So let's go back and ask about what else has been going on that might help nudge or push and pull the state of discussion among, let's say, high energy physics, especially but not only in the United States in recent decades?

So here is another thing I like to count. The blue is all-- with this axis here-- are all the PhDs per year in the US on any area of high energy theory-- high energy physics, including [? experiments, ?] all high energy physics, particle physics. And the red, is once again, dissertations now on string theory. It looks a lot like the curve of publications, not surprisingly.

So again, you see a very different set of pattern of when you have a growing number of people in particle physics broadly, and then going into a decline-- a pretty clear trend line there-- starting in the early to mid 1990s. Meanwhile, a very steep climb, a kind of almost runaway growth, on the attention to string theory, and they're kind of out of phase. Overall particle physics starts to decline, while string theory starts another one of these large periods of rise.

Well, that's not happening alone. One of the things that happens right around that pivot point is something that I never stop talking about. It was the cancellation of an enormous project called the Superconducting Super Collider, or SSC. This was a project approved in the US in 1985. It was under construction outside of Dallas, Texas, in a tiny town called Waxahachie, Texas, not too far from Dallas.

It was actually going to be three times larger than the Large Hadron Collider, which at that point, had not yet been built. So it would have been a-- let's see-- a 52-mile circumference or 54-mile circumference ring to accelerate protons and smash them together with correspondingly three times larger interaction energies. The argument was that people could have hopefully found things like the Higgs boson, maybe evidence of supersymmetry, in the 80s and 90s.

The problem was this was, as you might expect, unbelievably expensive. They actually wound up excavating almost the entire tunnel many, many, many hundreds of feet underground. They had not finished installing the superconducting magnets or all the rest.

The cost was growing very quickly. It was approved with a budget of a couple billion dollars. By the time the project was canceled, the budget had ballooned to $15 billion. That's now-- on a US appropriations budget-- that's a lot of money. That kind of budget now conflicts with things like military expenditures, not basic science expenditures. So this project was heading for a collision.

And in fact, after many fits and starts, the Congress canceled the SSC with one fateful vote in October of 1993. I remember the date very well, because I entered graduate school in September of 1993. So I don't take it personally. But the US Congress killed off one of the reasons I went to grad school one month after I entered graduate school. I don't think they had me in mind.

So you can see here, look at US funding for high-energy physics in inflation-adjusted or constant dollars. Funding for the entire field-- not just for that one experiment, for the entire field of high-energy physics-- fell in half in one year. That was an even sharper fall than the otherwise sharp falls we looked at many times in this class in the early 1970s.

In the early 70s, the funding for physics fell in half over the span of four years. This time, Congress was even more efficient and reduced the budget for energy physics in half in literally one year. And over the rest of the 90s, after that, funding for physics across the board continued to lose ground, both because of budget cuts and because of inflation.

Many, many books that have looked at the troubled history of the SSC, in particular. There was even a novel about this by the novelist Herman Wouk. He's the same novelist who wrote *The Caine Mutiny*-- a very accomplished novelist. He wrote a novel about physicists in Texas watching the SSC get canceled. I took it to heart.

Why did Congress cancel funding? In short, the Cold War had ended. There's many moving parts, but the overwhelming cause seems to have been, as many of these scholars have agreed, that the reason to keep spending very large amounts of money on so-called basic research in areas like high-energy physics had always been a competition with the Soviets, and the idea that if an outright warfare broke out, you'd have all these well-trained people with good equipment.

And after the Soviet Union dissolved in summer of 1991, those arguments no longer held the same kind of sway that they had for generations before. And one of the most visible symbols of the end of the Cold War for science funding in the US was this cancellation of the huge accelerator.

Let's go back to that plot of-- remember this is now all PhDs in particle physics, PhDs in the US strictly on string theory. And here, because green means money, green is now that budget curve I just showed in the last plot.

Look at the amazing correlations here, that you stop getting more people entering particle physics when funding falls in half, meaning you don't replenish the people who graduate. So people graduate here, but not as many people enter afterwards. So you start seeing the characteristic, five-year time scale fall off that we can at least correlate with this very dramatic change in funding for the larger field.

Meanwhile, string theory costs approximately pencils and espresso-- well, and health insurance-- all of which are important, none of which cost $15 billion for an atom smasher. So dissertations on string theory start growing exponentially just at the moment when other trends within the subfield are looking less and less fiscally viable.

It's not only the US. This is a complicated plot that I like staring at. Let me just tell you briefly what it's showing. It's comparing the decade by decade averages. I was trying to capture lots of moving parts, across many countries in the world, where the blue curve shows the change between the decade of the mid 80s to mid 90s, the change of that decade compared to the next decade-- 90s to mid 2000s-- the change in proportion of the world physics literature, contributed by physicists in that country. That's a very close proxy for budgets. Other people looked at this more carefully.

So you can really think of this as kind of budget trends. You can see the budget for physics research across the board fell in the United States after the end of the Cold War. That's consistent with what I showed you before.

Likewise, for many of these former Cold War so-called superpowers-- the USSR and then the post-Soviet republics, same with Britain. Meanwhile, some areas of the world during this period were investing like crazy in the basic sciences after the end of the Cold War, countries like Brazil and China for some time.

And you have a very clear anti-correlation between countries that stopped investing so much in physics overall and the proportion of physicists there who began working on string theory, versus the inverse, countries that were investing across the board more and more aggressively, across the full range of physics, and you see either a slower growing proportion or a falling proportion of physicists working on string theory.

That doesn't mean, there's no reason to work on string theory intellectually. It does remind us that these decisions are not happening in a vacuum, that what are the resources available, with which to even ask certain questions. Those change not only based on arguments about string compactifications or eternal inflation, physics, then as now, is embedded in a pretty messy world, a world that, in this case, went through a pretty dramatic change with the end of the Cold War.

So let me wrap up. Early in our own millennium, physics seems to be just as much in the flow, embedded in the world of people, culture, politics, geopolitics, and budgets and all the rest, as it has been throughout the whole period we've looked at this semester.

We saw, for a good chunk of the term, there was a moment in the middle decades of the 20th century, largely coming out of the wartime projects, when physics, especially in the US, seemed to have a unlimited range of resources and a command of respect and so on, as well as heightened scrutiny with McCarthyism and all the rest. It was center stage, for good or bad. It wasn't all good. But it was a central player in a way that was unlike what had either come before or indeed since.

I got my own taste of this about the changing fortunes of the cultural capital, let's say, of high-energy physics myself. About 15 years ago, Alan and I had written a very brief review article on inflation. In fact, it was one of the readings for the previous class. It was published in *Science.*

One week later, there was a point-by-point rebuttal posted on a creationist website that someone emailed us to pay attention to. They went through and showed all the reasons why this inflation account and, even the standard Big Bang, was baloney and, in fact, instead, much as the Christian Bible seems to suggest, at least to some readers, the universe is only 6,000 years old. It's not 14 billion years old. And they wanted to show why every one of our arguments was wrong.

They conclude, quite correctly, with the following. "We had to show you, in their own words, what these MIT eggheads are saying," I said, well, guilty. That one they nailed. That was empirically astute. "Guth and Kaiser need to take up truck driving," they wrote. I'm listening.

"That would get them out of their ivory towers at MIT and into the real world, where they would be forced to look at trees, mountains, weather, ecology." Plus, what do you see on the interstate but Taco Bell and Motel 6-- all these wonderful features of nature that signal the evidence of design, in this case they meant design by a biblical God. "Realities that proclaim design purpose and intention."

So I've had a wonderful time talking with you this term. I'm sorry we had to do it remotely. I hope that was, nonetheless, a reasonable experience for you. Please don't hesitate to email me. But for now, I got to head out because I got to get back to my rig. So it's been great.

Good luck with the end of the term. And please don't hesitate to reach out if you have any questions, any time. But I'll stop there. Any questions on that? No questions about MIT, I guess. I guess that part was just self-evident. OK.