Hello, everyone. Welcome to another episode of Q&A about History of Science and Technology. And I see a number of questions saved up here.

Let's see, Julia asks, do you think there have been scientific discoveries in human history that were genuinely correct, but disappeared because civilization lacked the intellectual ecosystem to recognize their importance?

The answer is surely and definitively yes, on both small scales and larger scales.

People have, whether it's… oh, I don't know, one that immediately comes to mind is things in medicine.

Where people kind of said at some point in the past, oh, all these systems are kind of integrated together, and it's sort of the whole person thing that you have to look at for some purpose in medicine. And then people said, no, no, no, we're going to scienceify this, and we're going to figure out how to do sort of reductionistic science, and we're going to break it down into, you know, diseases of this particular

You know, blood vessel, and diseases of that, and so on.

And not… and kind of ignore the fact that there's sort of a bigger systems kind of thing going on.

And I think that's something where people sort of haven't… didn't quite have the…

paradigm, medicine in antiquity was much more of an integrative kind of discussion than in the post, you know, scientific revolution, 1600s-type period, this more, let's do the reductionistic thing, let's look at the micro-pieces and so on, became much more the thing, sort of realizing how kind of complex systems like us get put together, which we're only just starting

to really have any kind of serious sort of foundational scientific understanding of, something I've been working on quite a bit. That's the… but nevertheless, there are sort of ideas that emerge every so often, you know, whether it's the, you know, the gut-brain axis, or the, you know, these other kinds of things, where actually it turns out

The pieces fit together in much more elaborate detail than was understood on the basis of, kind of, reductionistic science.

exactly what the right methodology to think about that is something which I think still remains for the future, but it's something where people sort of had the right idea back in the day, and it got kind of crushed by kind of the reductionistic science approach. And so that's one example of that.

I would say that, N…

Well, for example, in the idea of computation, the kind of idea of abstract computation.

People were kind of nipping at that in the 1600s quite a bit.

People like Leibniz, people who were working on these sort of philosophical languages for trying to describe things about the world in an abstract way, and so on. There was sort of a lot of nipping at those kinds of things. But yet.

the sort of whole conceptual framework that eventually led to the idea of computation just didn't exist at that time. I mean, the chain that was required to have it exist

was, in retrospect, it looks like a lot of it wasn't necessary, but it sort of had to happen historically, I think, to get there. And, you know, that involved things like the idea of a mathematical function.

the idea that you could say… first of all, people said, oh, well, there's a… first of all, I had to invent algebra.

you had to have things like, there's this thing, it's X squared, and for any X, you're computing X squared. But then there's the idea of, that is just a function.

that goes from X to X squared, and you could pick up that function f and have it be this abstract thing on its own that you can start talking about. And that's really a thing that only became clear in the late 1800s, and that's the thing that you needed to have, this idea of sort of playing with functions in their own right.

not…

with kind of the actual sort of raw material of X squareds and things like this. That, I think, was necessary for the invention of the idea of computation, and this idea that you could do things where you essentially are creating functions, where you're having sort of data that turns into code, which is kind of the key idea that leads one to universal computation. But to have that idea, you had to have the idea that you could have

You know, functions disembodied in some way, and that required that whole chain of things to happen.

So that's… that's another case. I think there are plenty of cases where

kind of the, there's so much sort of belief about how things work that one ignores other possibilities. I'll give you an example that I've been much involved with.

Which is the idea of, is the universe fundamentally computational? Is it fundamentally made of discrete elements that follow discrete rules? Or not?

Well, what happened was people sort of didn't know what the universe was made of.

for a long time, and then the 1600s came along, and folks like Galileo and Newton came along and said, look, we can use mathematics, and eventually things like calculus, to explain, to sort of miraculously explain lots of kinds of seemingly hard-to-understand phenomena about the universe, about the motion of planets and comets and things like this, and many other kinds of things.

And so, that got to the point a few hundred years later where people said, that's how physics works. It works by following these kinds of equations, and maybe they're more elaborate equations, and they're the Einstein equations rather than the, you know, the equations of Newtonian mechanics or whatever. But still, it's equations. It's equations all the way down. That's how the universe works.

So, in the 1980s, when I kind of, started kind of really talking about the

Things in nature as being described computationally, it seemed very alien, particularly in physics.

And in fact, that alien-ness, even at the beginning of the 2000s, it was still feeling very alien in physics. How could it really be the case that, sort of, underneath all of physics is this discrete computational thing? Don't we know it's all equations all the way down?

And, you know, that's a thing where I would say what I noticed is between 2002, when my big book, New Kind of Science, came out, and 6 years ago, when we released the Wolfram Physics Project.

there was a kind of… quite an interesting change in, in kind of attitudes towards things. I mean, I think part of that change was the kind of decrease of self-esteem of the physics community, realizing, no, things like string theory and so on weren't going to deliver the full story. But also, the fact that, sort of, everything had been sort of more obviously… computation was a more obvious component as an important

thing. That particularly came in through interesting quantum information, and so on.

But, you know, that's a place where, sort of, those things had to be primed

For, in the world at large, for at least lots of people to say, oh yes, that might make sense, as opposed to, no, we know it doesn't work that way, so to speak.

So, there's a few thoughts about that. I think that,

In terms of things where…

people had the idea, and it just outright went away. I mean…

sometimes it's… it's… well, okay, a good example would be, again, about computation. People like, well, particularly Ada Lovelace and Charles Babbage, kind of supporting that, this is the 1840s, kind of had the idea of universal computation.

But it just didn't really have the support. I mentioned this thing about, sort of, functions that you can pick up and do things with, and so on.

it didn't really have that kind of support, and it wasn't until, really, the 1930s, 1940s, that, 100 years later, more or less, that that sort of really started getting legs and getting developed. And, I think, that there's… I mean, there are many other examples where,

oh, I don't know, another one would be kind of the…

The fact that there's things out in the universe

that are kind of like what we have here. The thing that Galileo eventually, in 1608, kind of, when he looked through his telescope and saw, you know, the moons of Jupiter, that was something where, gosh, you know, there are little, little things that are orbiting Jupiter, maybe that's like what happens with the Earth orbiting the Sun, and so on.

But the notion that there could be, for example, sort of other

planets around other stars and so on, the notion that there could be kind of an infinity of worlds a bit like ours, or close to infinity of worlds a bit like ours, that was a thing people like Giordano Bruno had that idea, and people had, like, no, that's not a thing, that can't be right. Now, part of that was because of the kind of religious tradition that said.

you know, humans are really special, and we are sort of the only things like us, and it would be… it's kind of a shocking and terrible thing to say that, there could be kind of other sort of things like us out there in the universe. We're not kind of the one and only unique kind of God-chosen thing, so to speak. I will say it's sort of interesting.

that in the ways that we're understanding kind of metaphysics, from things we've done from our physics project and so on, I'm beginning to understand much more clearly the extent of, sort of, human specialness

And the extent to which, in some sense, that sort of religious tradition of human specialness is correct.

not in the sense that there aren't other planets out there like ours, and there are, you know, chemical processes like the ones that happen on Earth, probably, things like this. But the point is that the world as we see it, as we perceive it.

The laws of physics that we perceive, the kind of aspects of the world that we choose to think about.

Are incredibly human-centered.

And, you know, one could imagine thinking about the world with different senses and perceived in different ways, where it's a whole different world that one is perceiving, and that's… but the world that we are perceiving, the world that we care about, is one that really is very much centered on us as humans and the way we happen to be. So, in some sense, that tradition is quite correct.

but sort of not for the reason of, oh no, there aren't any planets anywhere else, but more for the reason of it's… if what we care about is sort of our perception of the world, then that's something that's quite specific to us. And if we say, well, let's be general, let's be more general, what you realize, and that's sort of the lesson of the Rulliad and the metaphysics around that and so on.

is that as soon as you go away from that, you've kind of got nothing. You've both got everything and you've got nothing. You've got… there's no coherence to the picture that you have unless you're anchoring it around something specific, like the details of us humans. So again, it's sort of an idea that was, was kind of right.

for, in some sense, the wrong reasons, but maybe not really the wrong reasons, then was kind of crushed.

By, kind of, the, the weight of, sort of, well, in that case, initially, kind of, sort of religious ideas, then, then this notion that, you know, humans might be special, that's sort of crushed by the idea that science is general, and then one realizes that really isn't true, the general is nothing, and, you know, even the laws of physics depend on the way that we are.

So… A few thoughts about that.

Let's see, sort of a related question from Tarek here. Is there a law governing how fast civilization can absorb fundamentally new ideas?

Oh, boy. I mean, people have sometimes said that the progression of science is measured in a sequence of funerals. That's a rather jaded view of things, but I think there is a dynamic that people… Sort of learn things.

And then they go and apply those things. And there has been, in the sort of history of the development of science, a lot of

what's happened in the last, let's say, 100 years or so, is people go and they say, I'm going to be a scientist, and they go and they get a PhD in some area of science, and they learn a bunch of things for their PhD, and a large fraction of people spend the rest of their scientific career doing things that were basically, you know, the things that they were doing in their PhD. I think there was some statistic that half of all scientists are still working on the

things that their PhD thesis was about.

And so, why does that happen? Well, you know, people put all this effort into learning things, they make some investment there. It's kind of like, okay, I've made this big investment in learning this stuff, let me take advantage of that investment. Maybe they don't have enough initiative to do something different. Maybe they do, but they think, you know, this is what I've invested in, so let me get the benefit of that. And then 30 years goes by, and they realize, gosh, I've just been working on the same thing all that time.

And when something new comes along, people are like, look, I've got this big investment in this thing, I don't want to do this new thing, I'm just going to keep doing what I've been doing, it's working okay for me.

Occasionally, things get dramatically disrupted. As people feel is happening in a bunch of areas with AI right now. If they get sufficiently dramatically disrupted, the people have no choice but to kind of look to something different, or at least many people think that way.

But so, I think one thing to realize is there is a time constant which is career length.

That is, people who get launched in a particular direction and just keep going in that direction, it's like, if you're going to change that direction, you might think you have to wait for a new generation of people to start some new careers.

So one of the things that's happened in the last, probably,

I don't know, 50 years or so, is that career lengths have increased.

I mean, that's happened for some very, very sort of specific reasons and some general reasons. I mean, one is just life expectancies of getting longer. The other is that in the US, for example, since the 1980s, there hasn't been kind of a mandatory retirement age.

So it used to be the case that in positions like being a professor at a university, it's like when you get to age whatever it is, 65 or 60 or whatever it is, or something, it's like, okay, you're no longer sort of a working professor, you become an emeritus professor, there's a new generation that comes in.

That kind of…

you know, as careers got longer, and as sort of the politics of things changed and so on, that… that kind of got erased in the US. It's still the case, I think, in the UK. I have friends who are heading for 67 years old, which I think is an… Oxford and Cambridge is… is kind of still being enforced as some kind of… at that age or out.

Is that a good thing? Is that a bad thing? I don't know. It's, you know, it's more a question… I think there are a lot of detailed questions about, sort of, if…

there are many countries in the world where people say it's hopeless to become an academic. You know, that the only way a new professorship shows up is if somebody dies, and they'll probably be in that professorship for 50 years. There are going to be no job opportunities in my area. So, you know, that's sort of a bad case.

But I think this phenomenon of, you know, careers are getting longer, so insofar as people lock into a particular thing and keep doing that, the time constant for change is going to be higher.

What… what drives change in the end?

I mean, change is often driven by… well, sometimes it's driven by, kind of, sort of external forces, like, I don't know, you know.

A new methodology comes into existence, and people kind of realize, oh my gosh, we don't have to do all this stuff by meticulously counting this thing by hand or something, we just get a machine to do it, and people start doing it that way, and maybe there's some resistance of people saying, oh, you did it better if you do it by hand or something. But then, over a fairly short period of time, there's sort of a disruption, and that changes.

I think…

In, and sometimes there are things like there's suddenly great interest in some particular field as a result of something completely external.

So, for example, there might be interest… well, in computer science, that's a good example of an area where the academic activity of computer science, which was fairly theoretical, suddenly got this sort of huge burst of interest from software engineering that really sort of changed the field and changed directions of it and so on.

That was something sort of from outside the pure academic discipline.

I mean, there are other cases of that, the same things happen with machine learning and so on, but there are other cases which have different origins, like climate science was something which was just sort of going and doing its thing.

And as a piece of, sort of, meteorology meets, sort of geology-type, type thing. And then it suddenly became a politically important area, and then, sort of, it changed its character, got a lot of things injected into it.

So it's a, you know, these things that happen from outside the field. It's not because some big advance was made in the field, it's because something from completely outside the field comes and sort of increases its importance or something. That's another dynamic for sort of change, so to speak.

But I think in, In terms of of, what. It.

takes… For things to sort of change direction.

It works differently in different fields.

when I was sort of doing physics for a living, one of the things people even started plotting was the length of time that different fads lasted in physics, and the number of people in the physics community who were involved in that fad.

So, it had been the case, I don't know, in the… let's say in the 1960s and so on, there were fads. You know, the bootstrap approach, that's back in fashion now, but that had been a thing. The analytic S matrix approach, the this and that and the other. These were sort of fads that lasted, I don't know, 5 years, a bit more than that, and so on.

Okay, by the time… by the late 1970s, there were fads that were lasting 3 months.

And where people observed that the fraction of physicists who got into that fad had increased into the 70-80% range.

And people realized at that time, this is probably a really unhealthy thing. In other words, by the way, you see the same kind of phenomenon in the tech industry. It's like, there'll be a moment when, you know, every startup wants to be a social media platform. There'll be a moment where every startup wants to be an AI hosting platform.

You know, all these kinds of things. And it's a… it's again, it's probably a sign, it's probably almost a theory of it, that these fields kind of, sort of, it's a sign that things are not going well when you end up with these very short very intense fads, where, you know, for a moment, everybody wants to be a prompt engineer.

For a moment, everybody wants to, make, I don't know.

An agentic harness for this or that thing.

I mean, these are… those are probably perhaps slightly unfair examples, but it's a thing where, the, you know, there are situations in which fields churn with this idea of it's new, it's new, everything's new, we've got to do this new thing, and everybody's got to jump into the new thing, and then there are long periods of time when there's very slow change.

So I don't completely know how those dynamics fit together. It feels that this kind of… the urgent fattery doesn't seem like a good sign for fields, typically.

Let's see… Samuel asks, what would it mean for science to end? Not because there are no more facts to discover, but because humanity has found the deepest possible conceptual framework.

Well…

That's… it's an interesting question. I mean, I think with the things that have come out of our physics project.

We have gotten to the machine code.

We kind of know, at a fundamental sort of, almost metaphysical level, we know how the universe is put together. We know that we can think about it in terms of the Rulliad, in terms of the limit of all possible computations. We know something about how we kind of sample that, and so on.

Does that mean science is over? We've got to the edge, we've got down to the bottom, we know what the fundamental components are, we know what the kind of atoms of existence are, and so on. Does that… does that mean science has come to an end? The answer is fundamentally no.

And the real reason for that is this phenomenon of computational irreducibility that I talk about a bunch.

That is the phenomenon where, even though you know the rules by which some system operates, knowing the consequences of running those rules is irreducibly difficult.

Yes, you could just have your computer just run the rules step by step by step by step, but if you want to know what's going to happen in a trillion steps.

It's going to take you a trillion steps of computation to find that out. You're not going to be able to say, oh, I know what's going to happen. I can immediately give you a formula for what's going to happen. I can immediately tell you what's going to happen without going through all that detail.

So, I think even though we know what's at the bottom, we think we know what's at the bottom, in terms of how the universe is put together, there's sort of a very large chunk of irreducible computation between what's at the bottom and what we can now see.

And in fact, when it comes to what is it worth us talking about, it's mostly worth us talking about these pockets of reducibility, these places where you can jump ahead, and you can say what will happen, you can define kind of a natural law which you can describe, you can have something which sort of can fit in a human mind as a piece of narrative that humans can understand.

There are these pockets of reducibility that are what give us our laws of nature, what give us the things that we do science about, typically. And the question is, will we run out of those kinds of things? Even though we know what's at the bottom, the whole point is there's sort of this infinite collection of these pockets of reducibility that can be arbitrarily far away from the bottom. You can have to have assembled

More and more and more pieces before it becomes the case that you're even able to see this kind of regularity.

So I think the answer, then, is that science in… even if we know what's at the bottom.

Science is never-ending.

It's the same with mathematics. With mathematics, if we have a field of mathematics, we at least imagine that we know the axioms for that field. We know that, you know, in arithmetic, we can say for all numbers X and Y, well, all things that we characterize as numbers, X plus Y is Y plus X, is the same as Y plus X, and so on. We have these axioms. We think we know what the right axioms to start from are.

Yet, we don't imagine that we get to say all the results of number theory. We expect, and we can even show that it must be the case, that there's an infinite collection of theorems that you can prove through those axioms. You never… you never reach an end to that.

The real question is, are the things that you are then able to figure out, the theorems that you're able to find, the results in the physical world that you're able to see, are they things that we care about? Or is it just, oh, that's a detail we don't, we don't, you know, that's not important to us? And that's, that's a much more human question.

And the progress of technology has typically been something where we probe more and more parts of the physical world. Things that we didn't think were, we said, oh, that's an amusing phenomenon, but we don't particularly care about it. Suddenly, it's the thing that lets you make, you know, a better touchscreen for your smartphone, or whatever else.

It's a, you know, some weird electro-optical effect that was like, oh yes, that was discovered in, you know, 1873, and nobody cared for a hundred and something years. But then it became something that got less sued into technology, and where people do care about it.

But there's sort of a question of will one get to the point where we've got what we need, and we don't need any more from the physical world, we've mined all the phenomena we need from the physical world, we're just hanging out and doing our thing.

I don't think that's the nature of the human condition, but I can't say for sure that that's impossible. I mean, certainly in human civilization, there are, you know, there are pockets where people say, we're good, you know, we don't need electricity, for example, we don't need whatever else. We're good

You know, sometime in the 1800s. We've got what we need to live a satisfied life.

And, you know, maybe that's something that would happen generally, in which case, yeah, people are going to stop being interested in discovering new things in science, because they kind of don't need it. Or that would be… if it wasn't for aspects of the human condition that lead us to always be curious, hey, how does this work? You know, what else can we do?

there's a question, I suppose, of whether, you know, what are these levels of thresholds? You know, there's science that we care about. Oh, you know, we figured out a bunch of things about materials, science and materials, that's been an important thing for lots of technology we've built. Let's take particle physics as an example.

You know, we know that it might be the case that the structure of atoms, nobody would care. But it turns out, you know, nuclear energy is kind of important, and that requires knowing about the structure of the nucleus and protons and neutrons and so on. But if you're into particle physics, there's a giant zoo of other particles, the strange particles, the kons, the lambda hyperon, the cascade hyperons. These are all particles that,

I was…

very interested in these when I was a kid. There's a whole zoo of hundreds of these kinds of particles. They don't last very long, they might have a lifetime of 100 millionth of a second or something. They're produced in particle accelerators, they disappear. It's like, do we care about the omega minus particle?

You know, or is it just something which is like, huh, kind of nice to know, but we don't really care?

it may be the case that the distance between, sort of, the way we are as humans and the omega-particle and its, you know, rapid decay… how fast does it decay? Maybe 10 to the minus 6… 10… Let's see, it has…

I'm gonna guess 10 to the minus 11 seconds, but I might be wrong about that. But

In any case, you know, gone in an instant, produced in a particle accelerator, and rapidly disappears, is that something that we're going to have a sort of human technological application for? My guess is that's pretty far away.

Because the… the scale of… of stuff that… for which that is relevant is far away from a human scale.

Give you an example, it's a little bit closer at hand, the muon, another part of the particle zoo. Particle very much like the electron, but 206 times heavier than an electron. So, there's a whole bunch of muons that

are going through us all the time. They're produced by cosmic rays. Cosmic rays are things like protons and so on that come from the sun.

That's the most common source of them. They hit the top of the Earth's atmosphere, this high-energy proton is coming in, it collides with some atom in the atmosphere, and it produces a whole shower of particles, and a bunch of those particles eventually turn into muons.

And muons, in their, sort of, natural habitat, last about 2 microseconds, 2 millionths of a second. When they're going very fast, because of relativity, there's time dilation, and they last longer, but basically, there are… the muons are gone in a couple of microseconds, usually. So the question is, do we care? Is there anything of,

Of technological relevance, of human relevance about muons? Or are they just a thing that, like, that's an interesting piece of science, piece of particle physics, but it's irrelevant to us?

Well, they're slowly starting to be things for which muons are relevant.

So, you can use them to kind of, if you want to sort of do tomographic x-raying of the Earth, so to speak. Like, let's say you've got this pyramid, and you're trying to figure out, you know, is there a hidden com… is there a hidden chamber inside the pyramid?

well, you could go and look and see whether the cosmic rays that are coming in and hitting the pyramid, whether they seem to be… whether there seemed to be sort of more cosmic rays than you would expect, because you would think more of them would have been absorbed unless there was a big hole in the middle of the pyramid. That's an actual experiment that was done, I think, in the 1940s, I think?

Soon after cosmic rays were discovered.

But that's… that's, in a sense, an application for muons. There are applications that involve… oh, I don't know, is this container that's just arrived at some port, does it contain, you know, uranium or something? Does it contain some heavy elements? Well, you can potentially tell that by doing muon tomography, or can you find, you know, hidden archaeological or, you know, tunnel structure

underground, well, maybe you can do that with muon tomography and so on. So it's a case where this thing that seemed like it was just a fun fact in science suddenly starts getting lassoed into something that has human relevance.

You know, you might say some of these other particles, like the pions and kaons and things like that, which

last a very short time, and it's like, well, they're interesting, but… but are they human relevant? Actually, there are cases for radiation therapy for cancer and so on, where some of those things are potentially very important because they have different characteristics in terms of going into human tissue, and they're just sort of going in, and they're not really doing much damage, and then they kind of stop in a very short

Region, and dump all their energy, and kill the cells you want to kill, or whatever else it is.

So again, you know, kind of a funny way that one might not have expected that the, you know, the K-ons become important type thing, but that's the question, is whether there are pieces of science that are so disconnected from human experience that we can say, well, that's kind of interesting, but we don't, you know, it's not going to have human relevance. You could say that about a lot of cosmology. It's like the details of how, you know.

when did the first galaxies form, and how far does, you know, what's the gravitational binding of this cluster of galaxies or whatever else? It's like, that's not of human relevance. That's science, but not humanly relevant.

And I think that's sort of a question of what drives science, and does there come a point where people say, we know what we need to know, you know, it's all good, we don't need to know anymore? Now, as I was mentioning earlier, there are times in history where people said, we don't want to know that piece of science.

You know, we don't want to know that, you know, we might not be unique in the universe, so we might… we don't want to know this or that thing about, how

you know, some aspect of, sort of, the deconstruction of the human condition, and so on. And I think,

That's… that's another issue, is are there… are there places where there are kind of boundaries where one says, this just isn't a very good idea? So a good example in… in modern times is, should one create viruses that could wipe out our species?

might be hard to do, one doesn't know how to do it, but, you know, does one want to push in the direction of doing that? One might argue that's simply a bad idea.

And so there might be science that one could learn from finding more virulent viruses and so on, but it's like, let's not do that. That's just not a good idea. And so the question of, sort of, where are those boundaries is an interesting question. I tend to think that,

it's sort of a… if it's easy to do, people are going to do it, whether you say you shouldn't do it or not. And I think the sort of the, you know, the story of, for example, nuclear weapons proliferation is one where it's just not very easy to do. It's like a very complicated supply chain, and so on. It's not like you can just go to the corner store and pick up a lump of uranium-235. So, it's… which is a little different, for example.

In some of these biology areas, there's more concern, because it is potentially a lot more accessible to do things which are sort of bad idea type experiments.

Let's see…

Ratius is asking, is string theory over or not?

String theory has many lives.

Not the 9 for a cat, but it's had at least 3.

I mean, string theory originated in the early 19th… actually.

Kind of the beginning of the 1960s, Where… Well…

It was… it's a complicated history.

And…

part of the history had to do with just, how do we do quantum mechanics for things that aren't point particles? Oh, we can do it for these string-like things. That was sort of a mathematical physics play. Another piece of it had to do with lots of kinds of particles were getting discovered in the late 1950s, early 1960s, and there was a question of sort of, how do we understand the zoo of particles?

what became clear in the end was that quarks were kind of the thing inside all of them, and that became… that was sort of a theory that was proposed in 1964, and but sort of interestingly, and it's kind of a…

kind of a funny sort of connection of what's going on in the world versus what's going on in science.

There was this idea that, sort of, the hierarchical notion of its quarks at the bottom, and from them made all the particles get made, didn't really fit with the kind of social

kind of prejudices of some physicists, so very much centered around Berkeley, California.

kind of a very hotbed of kind of social, social activity, particularly at that time, was kind of this idea, no, no, no, let's not have hierarchy, there are no king particles, let's have everything be democratic.

Every particle is kind of as fundamental as every other particle. That led to this kind of bootstrap idea, which kind of led to, sort of, in the mathematics of it, it kind of led to things which were one of the early versions of string theory. So string theory was originally something relevant to the study of strong interactions and the presence of all these particles and so on. That was kind of existence number one.

The,

It then kind of… well, it came back several times, and then it kind of, got connected with supersymmetry, which is a whole another thing which was originally introduced as kind of a… a sort of a mathematical,

kind of completion kind of idea,

but then sort of got its own, merged with the mathematics that had been done there. And then, in the 1990s, it became clear that the mathematics of string theory sort of connected to lots of other kinds of things, including questions in pure mathematics

In things like the theory of knots and so on, that didn't really have any obvious business to be connected to these kinds of questions that were coming up in physics, but things were sort of elegant enough that it turned out these sort of natural questions in mathematics mapped into these sort of natural questions about sort of things like string theory.

Well, the thing was, That…

as a sort of practical model of, oh, does this represent, you know, particles in the universe? Things kept on going wrong. You know, first, it was the case that sort of vibrations on this string, would end up going faster than the speed of light and cause all kinds of weird problems and so on, unless

the universe happened to be 10-dimensional, or maybe 26-dimensional. And then it's like, well, where are those extra dimensions? Well, there'd actually been a theory from the 1930s that sort of already talked about the possibility of sort of extra dimensions being all curled up in little balls, and so on.

And so that got kind of brought out as a way to save the day for those kinds of things. And then there were all kinds of, sort of, well, somewhat predictions of, oh, there'll be all these other particles found, none of those particles were found, and then there were all these things about how you'll be able to compute things more easily in string theory, and string theory is sort of… it's not like the actual physics of quantum quarks and gluars.

and so on that we know, but maybe it's in the same general class of things. Maybe from string theory, we can learn things about computing things in sort of more practical areas of physics. That's still an ongoing possibility, and still something people are studying, but it didn't… it wasn't quite a slam-dunk, a thing that worked as one might have hoped.

So, I think, you know, realistically, the sort of… as is so often the case, it was kind of a little bit oversold as, you know, it's… it's going to be really hard, but we're going to get there, and then it's actually the there doesn't actually make sense. It's not really… doesn't really answer the questions we want to answer. And I think it's sort of, my own feeling is that string theory is probably an interesting limit

of a bunch of things that we're understanding about, sort of, what the machine code of the universe is like, but it probably isn't the case that's most relevant for looking at the universe as it actually is. It's probably mathematically interesting, and more work should be done on it.

But I don't think it's… it's… it's sort of a mathematical branch

That is informative, and probably rather elegant and aesthetically pleasing, but probably not the branch that is going to lead us to the most practical things in terms of understanding physics as we know it right now.

By the way, I think this whole business about,

How, sort of, there's the dimensions that we know about, the sort of three dimensions of space and so on, and, like, is there anything else?

Well, we kind of know that we can think about, in gauge theories and physics and so on, and kind of think about this notion of gauge as being sort of another coordinate.

And, well, we're coming soon is some way of understanding that in terms of our models of physics, which I think is going to be rather exciting, and we're going to be able to see that just like you can kind of move around in physical space, you can kind of move around engaged space. So, to give one rough physical,

version of that.
When you think about voltage.
You say, you know, voltage is kind of… if there's a voltage difference, it'll push electrons from sort of the, you know, the high voltage, the low voltage, or whatever. It's actually the other way around, but it depends on how you look at it. But in any case, it's, you know, voltage differences are pushing things.
But the question is, what's the actual value of the voltage itself? Well, that's kind of an arbitrary thing. You can run it up and down, and I think there's a sense in which
Just like you can move around in space, sort of choose to move somewhere in space, you can kind of choose to change this voltage, and that's sort of a somewhat analogous thing, and there's a whole kind of direction of things in our physics project that I think come from that kind of intuition.
But, just a few thoughts about that.
Andre is asking, is there experimental evidence of string theory? No.
I mean, it's… it's… there were various things where people thought, let's go look for this, and none of those things were found.
No.
String theory doesn't tell you, you know, this particle will have this particular mass, go look for it in this accelerator. It's more general than that. But the kind of… the funky phenomena that you might think were there, like these supersymmetric partner particles and things like this, didn't get found.
And I think it's, at some point, there's also a,
A complicated story of just what
what does string theory really say? I mean, it's like, you could ask, is there experimental evidence of calculus?
That's kind of a silly question. It's not the right kind of question to ask. Calculus is a methodology for making models of things. You can ask, is there experimental evidence for the particular model of planetary motion that can be formulated in terms of calculus? But if you say, is there experimental evidence for calculus? It's kind of an asking about the wrong category of thing.
And I think, to some extent, string theory is a bit like calculus. It's more a methodology than it is something which
Should be anchored into, oh, there's this particular, specific prediction about some particular particle.
Okay, physics was asking, do you remember the so-called November Revolution, the discovery of the J Psi particle? What was being done in physics? What was
being in physics at that time like? Yes, I absolutely remember that. That was in, late 1974, I think, and I was, 14, 15 years old, and I was very much following these things, and what happened?
Well.
So… there were… let's see what I can remember here… There were… That had been…
So, particle physics… had…
been sort of this… this field that had really been kind of born out of, kind of, the follow-on from the Manhattan Project, of, kind of, the US government was prepared to invest a bunch of money, physics seemed important, particle accelerators were built.

People were smashing protons together at very high speed and so on, and lots of things were happening, lots of new particles were being discovered, but it was a bit messy.

It was kind of like there were these different approaches, and so on. Maybe I should give a little bit of backstory.

So, sort of.

atoms, particles, electrons, things like this. Those were things that were discovered in the early part of the 20th century. By the 1920s, the mathematical formulation of quantum mechanics existed, quantum field theory existed, the kind of, how do we describe how electrons and photons interact?

in electromagnetic processes, that was kind of… the basic idea for the theory was known in the 1930s. There were all kinds of mathematical issues in being able to work things out from that theory. Those got resolved in the 1940s, 1950s, and so on.

So, 1950s, quantum electrodynamics, the theory of electrons and photons, that was in pretty decent shape. That was like, it's a theory, it's working, et cetera, et cetera, et cetera.

Then, there were other kinds of phenomena with particles. There were strong interactions, the interactions that hold nuclei together. They were not understood at all. It was… some description existed of them, but it's like, can we calculate things about strong interactions? No, not at all.

Then there were the weak interactions that lead to nuclear beta decay and have things to do with neutrinos and so on. Neutrinos were discovered in the 1950s, and, the, and it was also discovered that weak interactions had the strange feature of being parity violating, in the sense that, sort of, well, what that means in practice is that there are left-handed neutrinos that have their, sort of, direction of spin

align… anti-aligned with their direction of motion, and there are right-handed neutrinos, but you don't find, at least not very many of them, right-handed neutrinos. Because neutrinos don't have zero mass, there's a slight change of that. But basically, this sort of violation of the left-handers are not… there's a big… there's sort of… it's only left-handers and not right-handers, and so on. That was something people didn't

In fact, that was a feature of Web Interactions discovered in 1956, I think.

So anyway, coming into the beginning of the 1960s, strong interactions not understood, weak interactions kind of weird, electromagnetism, pretty well understood using quantum field theory.

Okay, quantum field theory and the method of Feynman diagrams and so on, which assumes that… that the interactions are fairly weak.

And you can kind of just say, oh, there's one photon exchanged, or two, or three, or four, but you can… by the time it's four photons, the likelihood of that is really low. That approach, so-called perturbation theory, just didn't work for strong interactions.

Because it's like, oh, there's one pion exchanged, but there might be 17 pions exchanged, and that happens at sort of about the same rate as one pion being exchanged. So we can't do this kind of mathematical series expansion, and so on. So people didn't know what to do with it.

And this thing called the S matrix that had been invented in the 1930s was just a thing that said, imagine particles are coming in, and they're colliding with each other, and then they go out. Just say there's an S matrix. There's a mathematical object that describes the transformation from the incoming particles to the outgoing particles. Let's study the mathematical properties of that object.

the S matrix, and it has various properties of being smooth in certain ways, it has various symmetries and so on, and it's like, what can we get from understanding the S matrix as a piece of mathematics, as a piece of functions of many complex variables, and so on. So, the two things coming into the 1960s, the kind of two approaches. One was from quantum field theory, and kind of like, we can look at individual particles and so on. The other was from S-matrix theory, which was like, we've got to just look at the whole thing as a big ball of… sort of a big single ball of stuff.

From S-matrix theory came, eventually, string theory, from the, kind of, quantum field theory direction. Well, the next thing that happened, 1964, the idea of quarks, and… but it wasn't really clear. Quarks were very confusing to people, because, you know, in a proton, there's supposed to be three quarks. Okay, show me a quark.

well, you can't get them out of a proton, and people would bash protons and say, can we get a quark out of this proton? Well, no, it doesn't come loose. It's always confined in the proton, it seems. But that seemed like almost a philosophically wrong idea, that we're saying, there are these particles, but you'll never see one. You'll never be able to pick one up.

And so, it wasn't really clear what that meant. So, in the 1960s, and going into the beginning of the 1970s, it was still like, are quarks real things? Are they philosophically meaningful? Etc, et cetera, et cetera.

1971.

There were a bunch of experiments done of shooting high-energy electrons into protons.

And, what you might have thought was, oh, there's this proton. Protons are pretty…

big by the standards of particle physics, they're 10 to the minus 15 meters across. Electrons, nobody knows how big they are, they seem to have zero size. I think they don't have actually zero size, but they're small compared to protons. You run an electron, you shoot an electron at the proton, it's like, is the proton this big, mushy thing, and the electron just goes straight through, or does it hit a hard bit

in the middle.

Well, back in the 1910s, Rutherford, Ernest Rutherford, had done these experiments where he shot alpha particles at atoms.

And what he found was, most of the time, the alpha particle just goes straight through. It's just sort of empty space in the atom. But occasionally, the alpha particle gets… sort of hits a hard bit and gets sort of scattered at a high angle.

And that's the discovery of the atomic nucleus. Atomic nuclei are very small compared to atoms. There's a small, hard bit in the middle of an atom. It's 100,000 times smaller than the atom, so most of the alpha particles just go through the empty space, so to speak, but just a few hit the hard bit.

Okay, that same thing got repeated in 1971 in what was called deep inelastic scattering, which was shooting electrons at protons, and most of the time, the electron would just sort of go straight through. It's just sort of a blob of low-density stuff, in a sense, in the proton, but every so often, it would hit a hard bit

And get scattered at a large angle.

What were those hard bits?

My friend Dick Feynman called them part-ons, and my also friend Marie Gilman, who was the inventor of quarks, referred to, part-ons as put-ons. Those two had, quite a, a complex relationship, let's say.

But, in any case, so, 1971, discovery of some hard bits.

a lot of confusion about what was going on there. A lot of kind of mathematical structure being created that was, well, very empirical kinds of things. The various kinds of scaling laws, of talking about how… yeah, it's… scaling laws are all the time. You know, you hear about scaling laws in AI systems. There were scaling laws for what happened in deep elastic scattering in 1971, 1972.

And so on. Okay, so, things were… there was something up inside protons, and maybe these partons were, in fact, quarks. That turned out eventually to be the case, but that wasn't clear at that time. It was still, like, we don't really know how this works.

Then… The, the other thing that was sort of one of the other,

Well, okay, the next big thing that was happening was a bunch of new particle accelerators were coming online. There were a few that had been, you know, there were… these particle accelerators were dotted around the world, so the CERN, European Center for Nuclear Research in Geneva, that's sort of a big thing created in the 1950s as a big sort of international collaboration.

That was a big place where there were a bunch of particle accelerators, big…

progressively bigger particle accelerators, Fermilab in the US, outside Chicago to the west of Chicago, another place which was sort of the big, sort of, U.S.

well, one of the big U.S. places that sort of really grew up, I would say, in the 1970s. In the US, also Brookhaven National Lab on Long Island, then the Stanford Linear Accelerator Center in California, in the Bay Area. These were sort of the… as I say, dotted around, there were particle accelerators.

And, then there were some others. There was some in Russia, in Obvasibirsk. There was one in Frascati in Italy.

There was a small one in England, you know, there were… there was one in Sacly in France. These were sort of… these things eventually got consolidated, because as they got bigger and cost more billions of dollars to build, there were fewer and fewer of them built. But back in the beginning of the 1970s, there were several.

And, I remember there was… the big thing that was happening was electron-positron colliding beams. So, normally, you can accelerate a particle, and you, you know, you have this whole big bunch of accelerated particles, and you smash them into a target.

And then things come out behind the target, so to speak. But what was figured out was how to make colliding beams, where you could have in the magnets, you could have, beams circulate inside these

These particle accelerators, and they would, circulate in opposite directions, and, the,

And, okay, one very convenient thing is that if you have a magnetic field that's kind of keeping particles going around in a circle and maybe accelerating them, if

It's accelerating electrons to go one way. If you put in positrons, antielectrons that have opposite charge, those go the other way around the exact same collection of magnets.

So, there were these colliding beam machines made of electrons and positrons. There was one in Frascati in Italy, I think the one in Ovosibursk was also running, and there was one called Spear, I think, which was at the Stanford Linear Accelerator.

And the Stanford Lunar Accelerator was an electron accelerator. These other things, they all… that one was mostly get electrons to go in a straight line, but they had a little storage ring at the end where they would have circulating electrons and positrons. So, okay. So people were crashing electrons and positrons into each other at a high rate of speed. The main, sort of, numbers to know were the so-called center of mass energy.

And the central mass energy was, like, 2 GeV in the units, so a GEV… 1 GeV is roughly the rest energy of a proton.

So…

2 GeV was a certain energy, you could get certain things to happen. When you had 2 GeV of energy, you would be able to produce, kind of, 2 GeV of mass of particles, so you could get a bunch of pions. Pions weigh about a tenth the mass of an electron, so you get a bunch of pions coming out, and so on. So there was stuff happening in the sort of 2 GeV range, and then gradually, things were going up. The energy was going up.

3GB, 4GB. Okay, so this was 1974. The, the thing that, was,

There was something weird seen.

So… There was a calculation of,

I haven't thought about this in decades, it takes me a little while to remember all of this.

The question was, what was the total probability of electrons and positrons interacting? The so-called electron-positron total cross-section.

It's called cross-section, because, like, you're kind of… you're shooting at something, and you ask, if you shoot this point thing at that target, what's the chance that you hit the target? Well, it depends on how big the target is. So, the cross-section is roughly, you know, you're aiming at a proton. If the proton is such and such an effective size, then you'll hit it with some probability and have an interaction.

Okay, so electrons, which, remember, are supposed to be point particles, there's still some effective cross-section. It's as if the electron is a certain size, with respect to how often, kind of, things, you know, you… you kind of get an interaction with the electron. Okay, so…

the electrons… electrons and positrons can interact through purely electromagnetic interactions, just with photons being exchanged and so on. There's a certain rate of that, what's it called? I think it's called barber scattering, is the… is the type of… is the calculation. There's barber scattering and molar scattering, if I remember correctly. One of them is electron-electron scattering, the other is electron-positron scattering.

And quantum electrodynamics allows you to calculate the rate of those interactions. Okay, all good.

Now the question is, when electron-positrons, electron-positron interaction happens, and the electron and positron, they, in a sense, annihilate each other, they produce a virtual photon, which kind of carries a whole bunch of energy.

And that energy's got to turn into something, and it turns into a bunch of particles. And those particles will be, like, a pi plus and a pi minus. There'll be, well, lots of different kinds of possible particles. The higher the energy, the more of those particles can be produced. Okay, what's the total rate at which that happens?

Can one calculate it? So, very low energies, very hard to calculate. As the energy went up.

There was a calculation that was based on, the existence of quarks

That said, the total cross-section should be this value. You basically can generate an up, up, anti-up quark, a down-anti-down, a strange, anti-strange, that's it.

There's a certain number of those things. You can calculate the total cross-section for electron-positron annihilation going to so-called hadrons, strongly interacting particles, of which are the things that are made of quarks. So you could just calculate what should be the total, total cross-section. There was a value, based on the existence of those three quarks.

Okay, so now we're in 1974, early 1974, and the cross-section starts going up.

Was it correct?

you know, people didn't really trust the results from Novosibirsk in Siberia. They didn't really quite trust the results from Frescati in Italy. They kind of trusted a bit more the results from the Stanford Linear Accelerator.

And so on. But there was kind of a bubbling, you know, what's going on, something weird is happening. Now, I have to say, effects in particle physics, you know, it's very hard to do these experiments. There would be effects that happened, and then they would disappear again. There were all kinds of, oh, there's this really weird thing that's happening, oh, whoops, it doesn't really happen.

So people weren't sure whether this rise of the electron-positron cross-section around 4Gev was a real thing or not.

Okay.

So, that was all going along.

Now, okay, another thing I should explain is, when you discover a particle, how do you discover a particle?

Well, one thing you can do is some of the early particle discoveries were made when we weren't using accelerators, they were using cosmic rays, but when what would happen, and this also was done with accelerators, you would have these stacks of emulsion, for example. Emulsion like the stuff that was early, you know, old photographic film.

And when a particle, high-energy particle, goes through emulsion, it, basically is like a photon going through there, and it sort of exposes the emulsion in some sense. So you get a track in the emulsion.

So, another thing you could do, and people did this from cosmic rays, they would send them up in balloons to get… so that they were more high-energy particles that hadn't been stopped by the atmosphere, they'd go on mountaintops and so on. They would expose these emulsions.

And they would look at the emulsion, you know, under a microscope, they would go and slice through different layers, and they would see these little tracks.

Those little tracks were particles, and then they would see this track leads to these other tracks, and so on, trying to figure out the interactions of these particles, very much laid out in emulsion, so to speak.

And, a bunch of particles got discovered that way. Particles with lifetimes of 10 to the minus 10 seconds, 10 to the minus 8 seconds, things like this. The so-called strange particles were discovered that way in the 1950s, and so on. And, the,

the pion had been discovered that way in the 1940s, so are the muon, all these kinds of things.

And by the way, that was a very early use of, sort of machine learning. I remember when I was, this must have been the, mid-1970s, first of all, sort of this was a very high-tech thing. People would be, they would have these big,

photographs, essentially photographic plates, and they would have, in different layers, and they would have all these tracks. They were trying to say, is there a track there? And there were folks who were,

kind of, sort of…

people who were… whose job it was to just look at these emulsion stacks, you know, very carefully, and say, was there a track? Was there a track? Hey, that's a pion. Hey, that's a new kind of particle, whatever. So, that was kind of a thing, and then people started trying to do image processing.

and essentially machine learning to find those tracks, and that was a very early use of those kinds of things. That's now been developed much more dramatically in modern particle physics experiments, but that was already happening in the 1970s.

Anyway, the… okay, so particles that… okay, this is a little bit of particle physics stuff, but particles

that… so I mentioned the muon decays in about a micro… 2.2 microseconds, 10 to the minus 6 seconds.

the, things like the, the K-Ons.

decay in 10 to the minus 8 seconds, or another one, 10 to the minus 10 seconds. These different ranges, the pi zero decays in 10 to the minus 16 seconds. They're these kind of batches of particles that decay in different times.

the… those different decays are associated with different underlying processes. So the ones that have lifetimes of, you know, 10 to minus 6, 10 to minus 8, 10 to minus 10, those decay through weak interactions. Those decay through the same mechanism as associated with the production of neutrinos, etc, etc, etc. So that's one bucket of things. Those things you could discover by looking at

One of these emulsion stacks, and you could just see a little particle track there.

Okay, so then…

There's another range of particles, 10 to the minus 16 seconds or so, the K through electromagnetic processes. They're…

They're a little bit more difficult to detect. And then, there's a whole bunch of particles that decay in 10 to the minus 23, 10 to the minus 24 seconds. You can't see a track for that.

So you have to have some other way of detecting the existence of those particles. You don't get to say, oh, it's, you know, it's… I see it in my emulsion, for example.

Okay, so those are the particles that decay through strong interactions, through the things that are the kind of interactions which we now know associated with quarks and gluons and quantum chromodynamics, QCD, and so on. But anyway, those are all 10 to the minus 24 seconds, give or take.

How do you detect a particle that only lives 10 to the minus 24 seconds?

Well, the answer is, if you shoot two particles in, and it… those particles, most of the time, they just sort of bounce off each other or whatever else. But maybe those particles form another particle that only lives 10 to the minus 24 seconds, but they form that other particle. It's a definite particle with a definite mass.

So what happens is, When you look at the scattering cross-section.

the probability that particles interact, and you look at that as a function of energy, then when there is a… it's usually called a resonance particle, when you form this particle that is this thing that happens to exist, albeit for 10 to the minus 24 seconds, there's a big peak

in the… in the cross-section, in the probability of interaction. So the way that most of these particles discovered in the 1960s were discovered was through looking at the interaction probabilities as a function of energy and saying that that energy

There's a bunch more interaction probability, and that means there's a particle with that mass corresponding to that total energy that's being produced.

So, you know, there's a whole zoo of these things, I don't know, the rho particle, rho meson, with a mass of 770 MeV, so 0.77 GeV, about three-quarters of the mass of a proton. That was discovered in this way. There's a kind of a peak in the probability of particles interacting at a certain energy that corresponds to the mass of the rh particle.

Now, the way the mathematics works out, maybe this is too detailed, but, the,

When there's an interaction like that.

There is both a peak in the chance of interaction.

And also, the fact that an interaction happens slows the… in a sense, you have particles coming in, and they're, like, they act like waves, and they're coming in with a certain, sort of frequency of the waves. And then they undergo one of these interactions that forms another particle.

That leads to a phase shift, a shift in the phase of the wave.

And what happens is there's a… that's another thing you can detect, in, in when you do these particle experiments.

But what happens is that the shorter the lifetime of the particle, the more fuzzed out the peak associated with,

with this interaction probability is. So by the time you're down at 10 to the, you know, low 10 to the minus 24 seconds, it's a very fuzzed-out peak

It's no longer something where, oh, the rate of interaction is very high in this tiny range of, you know, 100 MeV, 0.1 GeV or something. It's fuzzed out over half a GEV or something like this, where the interaction probability is increased.

So, particle discovery, particle hunting, was kind of a hunt for bumps, a hunt for little peaks. And sometimes, quite often, there would be, you know, people would say, was there really a peak there, or was it not really a peak? Is that really a particle? Is it not a particle? I remember the particle called the A2, which has a mass of… can I do this?

Either 1300 or 1400 MeV. So, maybe…

1420, maybe? I'm not sure. I… I'm afraid that's… that's gone from my memory. But, that's from the… I don't think I've thought about it since the mid… mid-1970s, so that's a long-surviving neuron, or synapse, or something. In any case, the A2 meson

had… was the so-called split A2. It seemed to have two peaks, and that was a big production. It's like, why does it have two peaks? Is it some special new kind of physics? It's probably an experimental mistake, or it was two quite different particles, I'm not sure. But in any case, so there's a lot of peak hunting.

But some of these peaks were very fuzzed out, and so on. Okay, now we get to November of 1974. Okay, and we're looking at electron-positone interactions, there's been this increase, people don't know what it's due to, lots of different theories. I had a theory which turned out not to be right. It's actually the… the last time I've had a physics theory which didn't turn out to be right was when I was 14 years old, so I'm… I'm happy about that, that the record since then has been right.

are the good. But, the,

I mean, my theory at that time was that electrons much smaller than protons, but nevertheless have the same kind of interactions as protons do, but just at a much smaller scale. Actually, I think that theory is probably right, but the scale is utterly off relative to what one was observing at that time.

In any case, so, you know, people had theories, nobody really knew what was going on. Then, suddenly, a peak was discovered in,

a chap called Burton Richter, I think, was the head of that team at Stanford that discovered a peak in electron-positron interactions at 3.1 GeV.

A very sharp peak, much more dramatic than the peaks people were kind of fighting over in other places.

Meanwhile.

at, Brookhaven National Lab, Long Island, in proton experiments. Another slightly different way that the experiment was done, proton collisions, actually protons hitting a target, and then producing,
sort of resulting particles, some of which decayed, and you could reconstruct the, the mass of the particle that was decaying, and another peak at 3.1 GeV. That was a group led by a chap called Sam Ting.
So,
They… these things happened basically at the same time, and there was sort of a grand announcement of, you know, the… the group at Brookhaven called the particle the J, the group at Stanford called it the PSI,
I think I'm getting it the right way around. Yes, I am. And so it eventually became called the J psi, because the two groups kind of was duking it out for what the name of the particle should be.
Okay, so what the heck was that thing?
And… the,
people didn't know. It was really dramatic and surprising. And it's like, nobody's seen a really clear signal of a new particle in, you know, at that time, it had been more than a decade since such a thing had been seen, of a dramatic new particle. What on earth was it?
Well, okay.
There had been… a,
My gosh, we're dredging things from my memory here. In… so…
Remember, I talked about quantum field theory?
Quantum field theory had not really been successful in talking about strong interactions.
But there were… but there was a theory that was, okay, if quarks really exist, how would quarks interact?
And there was this theory that emerged from multiple people, in the beginning of the 1970s that was QCD, the theory of quarks and gluons, where the idea was that just as electrons have electric charge, and they have interactions that are associated with photons.
Then, similarly, quarks might have a color charge, a different kind of charge, which would be where the interactions associated with color charge would be associated with gluons.
And one of the, sort of, kicker, weird things would be.
Whereas photons don't interact directly with each other, gluons would interact with each other.
Okay, so that was a theory that, the sort of mathematical theory of quarks and gluons, sort of generalizing quantum electrodynamics, was something that had come out of a thing called Yang-Mill's theory, invented by,
CN Yang and Robert Mills in the mid-1950s as kind of a mathematical theory. And that mathematical theory was originally applied to something quite different in strong interactions, but it was kind of a theory of how you could have sort of things like… things that were sort of generalizing electrodynamics, in… with different kinds of particles, and with different, with the color charge, unlike electric charge, where it's just like, there's a voltage. It's a single linear… it's a single number that represents the voltage. If you did that for color charge, you'd have to have three different kinds of voltages, so to speak, that you described. It's a more complicated setup. It involves the group SU3 rather than group U1, if one's into mathematical physics stuff.
In any case, the, the,
The basic point was quantum
Yang Mill's theory existed, it was applied to quite different things, beginning of the 1970s, and there was a collection of people, and I don't completely know that history. And I've wondered

about that history, and I was sort of… I knew all the people, but nobody was kind of saying, I invented QCD. Well, Murray Gallman sometimes said that. A guy called Harold Fritsch said that, I think, but there were a number of people who said that, but it was never very clear to me how this had emerged. It was kind of a

A… it sort of just, sort of.

blobbed out, that this would be Yang-Mill's theory, you know, generalizes quantum electrodynamics, quarks exist, the idea of color for quarks… oh yes, I should explain something else. That was an idea, I think it was a chap called,

Ollie Greenberg, I think, who came up with that idea. The,

If I remember correctly, that was in the 1960s. There was sort of a mystery about quarks. Because…

There's this principle in quantum mechanics, the exclusion principle, that says you can never have two particles in the same place in the same state.

And that's what kind of determines how chemistry works. The fact that there are electron shells and very different chemistry of different chemical elements is a consequence of the fact that the… when you put more electrons into an atom, they can't be on top of each other, so to speak.

So the same phenomenon can happen with quarks, and it was sort of a mystery that there could be particles that were… that had multiple quarks that seemed like they were the exact

Right, same cork in the same place.

Like, a proton has two up quarks and a down quark.

By the way, in those days, they weren't yet called up and down quarks, they were often called P and N quarks, which was rather confusing. But anyway, the,

In any case, the, and so, I think it was Greenberg kind of had this idea, maybe the two up quarks that shouldn't be in the same state at the same time weren't really the same, really, maybe they had different colors.

in some abstract sense of color, and that was what explained that. And then when the idea of, sort of, color charge as being like electric charge pulled in with Yang-Mill's theory, gave one quantum chromodynamics.

Okay, so by that time, quantum chromodynamics was sort of a known theory, but nobody really cared.

It was just sort of a mathematical theory, yes, you can do this. Then, 1973, several characters who I know quite well were involved. In 1973, the phenomenon of asymptotic freedom was discovered in QCD.

So,

Roughly what happens is that when you have a particle that has a certain charge, it, it'll have a cloud of particle-antiparticle pairs around it, virtual particle-antiparticle pairs, that kind of shield that charge.

And so, if you kind of are probing the thing more and more, you're kind of drilling in and seeing what's… what's the real thing inside, inside that sort of coat of other things, how does the charge effectively change? Are the interactions getting stronger or weaker as you probe harder?

Turns out, in quantum electrodynamics, there's a small effect that makes the kind of interactions get stronger as you probe harder.

Okay. In… and that's characterized by a thing in the normalization group theory, a thing called the beta function.

Okay, it was… it was, you know, and it was… the beta function was something invented in the 1960s. Actually, Marie Gaumin and Francis Lowe were… were some of the inventors of the beta function, except they called it the psi function.

And I… it's an unforgettable conversation that I had with Marie Gohman once, in which I was talking about the beta function, and he's just looking confused, and like he doesn't understand what I'm talking about. And eventually, you know, it goes on long enough, and long enough, and long enough, and eventually he says, you mean G times the psi function.

Which, of course, he knew it all along, he was just kind of being difficult, as he often was. But in any case, this thing, the beta function, big thing, that was the thing that determined, would this interaction strength get weaker or stronger as you drill in, drill in, how much… how much stronger would it get, how much weaker would it get, and so on.

For QC… for quantum electrodynamics, the beta function is positive, the interactions get stronger as you drill harder. Okay. In 1973, two young physicists did calculations of what the beta function was in QCD. It was a person called Frank Wilchek,

and a person called David Gross, both at Princeton, a person called David Pulitzer, who was at Harvard, and was kind of connected to his advisor, who was a chap called Sidney Coleman. So, these people, and I mean, I know

all these people, and I think it's still a source of great, kind of, you know, who knew what when. Actually, another person involved was Gerhard et Hofst, who, had also done this calculation, apparently. And, okay, nobody thought the beta function could be negative. It's just like, oh, that can't happen.

I think maybe Ed Hoft had done the calculation, and had found it was negative, and it's like, that, you know, that can't be right, or who cares, or whatever. Okay, what was realized at some point in 1973 was actually the beta function could be negative, and that was very important.

And what… why was it important? It was important because it said that as you drill harder, the interaction strength gets smaller.

So, in other words, if you do a higher energy scattering experiment, you should see something which is less and less strong interaction.

So what that suggested was that as you… if you… if you just sort of gently bump an electron against a proton, it's just going to say, this proton is really complicated inside, there's a lot of strong interaction going on. But if you, you know, really ram that electron in at very high energy, then it will be kind of drilling harder, and it will then not see such a strong interaction, and it will see something which looks much more like

Kind of a quantum field theory, just interacted once type thing.

Okay, so that was… people in 1973 were beginning to have the interpretation that

deep in elastic scattering, the presence of these hard bits and protons and so on, that that might be something connected to QCD and this phenomenon of asymptotic freedom. It was not well understood. That was a thing which was kind of vaguely in the air, but it was a little bit… kind of a few people that, you know, Harvard and Princeton knew about it, but other people didn't really know about it and believe in it, and so on. It was kind of a…

in the air, but not very much in the air. And, so then…

JPE psi is discovered, particle is discovered, big sort of galvanization, what the heck is going on? Okay, one more piece of the history of particle physics that also plays into this.

There was, okay, so back, we're talking, you know, 1972, three types of quarks known. Up quark, down quark, and strange quark.

Those are the three types of quarks. Those are the types of quarks where people had calculated, based on those types of quarks, the electron-positron total cross-section would be such and such a value. Those are the quarks. We know what they are. Okay.

Okay, there's another phenomenon. This relates to the weak interactions.

So, strange particles, like the K-on, the lambda hyperons, all this kind of thing, these particles, so-called strange particles, that name was given to them by Marigell Mann in the 1950s, strange particles decay through the weak interaction.

Wasn't really knowing how that worked back in those days. Again, there's a whole separate history to that. But in any case, the,

there was… well, let's see. There was starting to be more understanding that the weak interactions might be associated with not photons, not gluons, but things called W particles.

And also, eventually, things called Z particles. Okay, what… The,

Okay, here's the basic, basic point.

In…

standard weak interactions, a strange particle, which contains a strange quark, or two strange quarks, or three strange quarks, but anyway, it contains at least one strange quark. It decays into things that don't contain strange quarks.

And the process by which that happens, underlying process in terms of quarks, is a strange quark decays into an up quark with the emission of a virtual W particle, which turns into an electron and antineutrino, or a muon anti-neutrino, or whatever.

But in any case, it's basically up quark… strange quark decays to up quark. Okay, things to know about this.

An up quark has a charge of plus

two-thirds of unit… elementary unit of charge. The electron has a charge of minus one elementary units of charge. An up quark has charge plus two-thirds elementary units of charge.

A strange quark and a down quark. They both have minus one-third elementary units of charge.

That, by the way, was something very confusing about quarks, was people thought charge was quantized in units of the electron charge, because the electron and proton have very accurately the same charge. One unit, proton is plus one, electron is minus one, and the idea that there could be things with fractional units of charge was confusing to people.

Any case…

Big point, strange quark, mass, charge minus one-third, decays to up quark, charge plus two-thirds, with the emission of a W plus particle, which turns into, let's say, a positron and a neutrino.

Okay. The, that is…

A… my gosh, it's a long time since I thought about these things. When I was…

13 years old, I wrote this whole kind of book-length treatise on weak interactions, just a little bit too early, because it really wasn't talking about the kind of quark model of these things. That must have been

1973. But, in any case, it's, takes me a little while to remember some of these things. Okay, so the basic point is that this weak interaction, strange quark decays into up quark.

is a charge-changing, weak interaction, in the sense that the up quark… the strange quark has charge minus 1 third, the up quark has charge plus… plus two-thirds, okay? Okay. So, something that was never observed was a strange quark turning into a down quark. So a strange quark has a… has a mass… has a charge of minus one-third, so does the… so does the down quark. So this was a, what became called a flavor-changing neutral current.

So, neutral current was… there's no electric charge associated with this. It's, you know, there's electrically… we've got the same charge in both cases. Flavor changing had to do with, do you go from being strange to not being strange?

So no neutral currents, no flavor-changing neutral currents, no, wherever observed.

So, that was kind of a mystery, because it's like, why doesn't this exist?

And there were, there was a kind of a… one of the problems was, even if

There's no direct way

for some particle to get emitted, to go from the strange quark to the down quark, or whatever, there should be something where there's a higher order effect, where there are, kind of a combination of two W particles, with loops of W particles, and all kinds of fun things that happen in particle physics, that there should be a small rate at which strange quarks could turn into down quarks, but nothing had been observed.

Okay, and the problem was that that should have been happening at a rate, sort of higher than what was observed not to happen.

So then an idea came up. There's a thing called the GIM mechanism. I think that's Glacial Iliopoulos Maiani.

The, This was… when was this?

Not sure, actually, probably around 1970.

The, And the idea was, how about if there's another quark?

How about, just like the strange quark has the same charge as the down quark, how about there's a fourth quark that got called the charm quark that has the same charge as the up quark?

Okay, if that happened, it turns out that virtual loops of strange quarks and charm quarks and so on would cancel things out and make it be the case that you could explain why there was no strange quark goes to down quark decay process. Okay, so it's like, maybe charm… maybe there's a new quark, maybe there's a fourth quark that exists.

Okay, so, then…

there was a paper, well-known paper, called The Search for Charm. It was written by John Ellis, Mary Guyard, and maybe some other people. That must have come out

Oh gosh, I remember that coming out, so that must have been 1973 or so. It was from… written by people at CERN Geneva. And it was just a catalogue of, you know, experimental ways you might detect these charm quarks.

Okay, so… and I think in that paper they had mentioned the possibility of a particle that was a charm-anti-charm quark particle.

And, okay, so then the jape psi is discovered, nobody knows what it is, lots of crazy theories, and, what sort of became clear, and I'm trying to remember how long it took.

I think… It was… The idea that it was a charm-anti-charm

Combination is, was not,

Kind of, an immediate… well, it was…

I think some people kind of thought that was the thing

you know, within days, but that didn't diffuse out for a little while. Now, you should understand that the communication mechanism in those days… so, you know, I was just a kid, and my sort of communication mechanism was, you know, reading, you know, New Scientist Magazine or something, a weekly magazine that still exists, or occasional things in newspapers, but then I guess I had also started getting

preprints.

Which were a thing which is, you know.

big nowadays, but it was big way back in the day in particle physics. People would just write a paper, and they would just send out copies to lots of research institutes and individuals and so on. So that was a pretty rapid communication mechanism that kind of went at the speed of mail, so to speak.

And so, that, that was, so…

So people started, putting out papers about quarkonium, which was kind of this… this quark-anti-quark combination thing, and I would say that by…

You know, by… by early 1975, it was pretty…

much generally believed that this was a charm-anti-charm, particle. And then the question was, could you observe a charm quark? Because this proton

This electron-positron total cross-section, that increase

would have to be associated with the production of charm particles. Production of particles that have not a charm-anti-charm combination, but a charm particle and, let's say, an anti-up particle… anti-up quark. And so, those things weren't observed for a little while later.

Actually, I was a bit involved in that, because, in…

Let's see, this is now 1977.

So, so, okay, so the thing… this whole thing about QCD is real, asymptotic freedom, that was kind of 1973, but that sort of gradually came in. By 1976, that was fairly accepted as a reasonable direction, so…

In 1977, for example, I…

was still just a kid, but I was working on… I got interest in QCD and started doing computations in QCD. I spent the summer at Algonne National Lab in the US.

And, got to use a computer there to do a bunch of calculations in QCD, and in particular, the calculation that I did had to do with how you could have gluons that were parts of, of a,

A proton that gluon plus gluon could turn into a charm quark and an anti-charm quark.

And so that was trying to calculate if you have, two protons colliding, that you should get charm quarks produced at a certain rate.

And so, I worked out, based on QCD, there's a certain rate at which charm quarks should be produced.

But they had never been observed. No charm particles have been observed in proton-proton collisions. They've been observed in electron-positron collisions, but not in proton-proton collisions. There was an experiment at Fermilab that said there just aren't charm particles produced at anything like the rate that I thought they should be produced at. Actually, that experiment was done using techniques that were a little bit going out of fashion at that time, emulsions.

the things I mentioned before. And what those guys were doing was to look for little tracks for charm particles in their emulsion.

Well, to know that… to look for a track, the track has to be of a certain length. For that, you have to know what the decay… what the lifetime of the particle is. Actually, the lifetime that was guessed was wrong.

And so, they didn't see any tracks, but that wasn't because there weren't any tracks, it was because the tracks were, I think, a bit shorter than they thought… than they were looking for.

So, in any case, I was a little bit involved in that, because I'd done this calculation of, from QCD, of the rate at which charm particles should be produced. Turns out, in the end, the calculation was actually perfectly correct, and the experiment was the thing that was wrong, and that sort of got resolved in the next few years.

But,

the, so that was, that was kind of what, what happened, and I suppose what happened in, in particle physics at that time, just a lot of, it's exciting, it's in the newspapers, it's, you know, things are happening, we've discovered a fundamentally new quark.

Actually, I remember this must have been the summer of 77,

there were two new particles discovered. The torpton, which was very unheralded. It was kind of a… it's another thing, like the electron, the muon, this thing called the tore lepton, which has a mass of…

See, now you can tell that the things which were discovered when I was kind of leaving particle physics, I think it's…

1.5 times the mass of the proton, give or take. In any case, the,

The Tor Lapton was discovered, I think, at an accelerator in Germany, DESI in Hamburg, and, I think that's… I think that's right. And then another thing was discovered. There was another peak was discovered at Fermilab. It was a peak at around 9.8 GeV, 9.8 times the mass of the proton, roughly. And that was… that happened, I think, in the summer of 77.

When, Fermili was sort of just down the road from Argonne, which was where… where I was, so this was a… this was big news, you know, another big peak.

And that was, that became clear, that was… so by the time people had known about the Jabe Psi, it was like, that… that peak was called the epsilon particle. Leon Letterman was… was much involved in that discovery. And, that was, then…

became clear that was another kind of quark. Nobody expected a fifth quark. Everybody thought four is enough, that kind of matches things. You don't get these flavor-changing neutral currents and so on. We're done. But then there was a fifth one, the B quark.

And, was discovered about mass of about 5 times the mass of the proton. And then, for many years, there was a big search for the top quark, the partner of the B quark, which turned out to have a mass of hundred and…

70, 180 times the mass of the proton. Actually, again, I was a bit… that was discovered long after I was no longer sort of actively involved in the particle physics business. But, actually, one, one thing that I was involved in, this must have been 1979,

Okay, this is my last story for, for, for today, probably. I've been yakking on for too long here. the, That's a kind of a funny story, in a way.

So… I was at Caltech.

And, I actually used to organize… this is 1979, so I was a little bit more of a grown-up, but I used to organize the theoretical physics weekly seminars.

And, so it was a fairly small group of people, kind of collected around a big conference table, and it was myself and a whole bunch of people, including Dick Feynman.

And, who was,

always quite, you know, paid attention to what was being said, and so on. Anyway, I'd invited this person.

And this person gave this talk about, Higgs particles and various things about interactions between… interactions between Higgs particles and so on.

And we're in the middle of this talk, and I'm realizing, wait a minute, there's something wrong here. This can't, you know, this just doesn't quite make sense. If this, you know, if this, then that, then that, and then splat.

More or less.

And I have to say, at that time, Dick Feynman

was… was very kind of… he was a rather competitive fellow in certain ways. And, you know, he would kind of delight in trying to see through, you know, what's the fatal flaw in this… in this presentation that's being given. Okay, so… and he would, kind of…

Sort of roped me in as sort of a co-conspirator in figuring out what's the fatal flaw.

it's kind of a, you know, he was a lot older than me, and a lot more… should have been a lot more grown up. He was three times my age, but be that as it may. But in any case, and it would be, you know, he would, every so often in one of these talks, he would kind of look at me, and he'd kind of, like, like, you know.

wink or something, like, I figured out the fatal flaw, and then he would start asking a bunch of questions. Okay, so in this case, I had figured out something that I thought was kind of wrong, and I started asking these questions, and then Feynman, who was

kind of, you know, very much into this was, like, really pounced, and, like, that couldn't possibly be right to this person who's giving this talk. Well, okay, so it was sort of a weird thing about if you have very heavy quarks, all sorts of terrible things happen, and the universe basically becomes unstable.

Okay, so then I'm like, actually, this is a pretty interesting phenomenon, even though this… this, you know, by the time it was clear there was something wrong with this talk, because there was something where… I don't remember all the details of what was being said there, but it was something where, sort of, the thing falls apart for this kind of reason. Okay, so talk finishes, and the…

speaker is kind of upset, and I'm, you know, taking him out to dinner and so on, and that's… that's all fine. I think that person had a successful career in finance, and probably… probably that event

in whatever it was, 1979 or so, might have pushed them over the edge of, let's not do physics, let's do something else, and finance was just heating up at that time, so that might have been a great, great contribution to a career. But, in any case, then…

I was like, this is an interesting phenomenon, and actually, I was talking to David Pulitzer, who is one of the other aforementioned characters who was one of the discoverers of asymptotic Freedom from 1973, who was then at Caltech.

And, we ended up writing this little paper about, sort of the maximum mass that a quark could have in the standard model of physics without making the universe go unstable. And the bound that we got for that mass was around 200 GeV. So when the top quark was discovered, with a mass very close to that, it was right on the edge. And it was like I was waiting to see whether you know, maybe there was something wrong with our argument, or maybe, you know, the universe really was set up to be quite on the edge. I mean, there…

really can't be. If the things are set up with Higgs particles and things like that the way that one thinks they're set up, then there can't be heavier quarks, or we have to explain why the universe didn't go unstable.

So, anyway, that's… that's a little bit of that story. I think, okay, Jelly asks.

One question related to this. Why were quarks given such weird names, like Up, Down, Charm, Strangeness, Top, Bottom?

Well, it's an interesting question. You know, naming in physics was pretty sober.
up to…
probably Murray Gaumin's contributions, because he was the one that really introduced this… the notion of strangeness. Now, it was very ironic.
Because Murray…
was a great lover of languages. I think his father had been a teacher of languages, and Murray had kind of picked up this kind of interest in many, many, many diverse languages, and was always fond of explaining that, you know, so-and-so's name should not be pronounced this way, but should be pronounced in this completely bizarre way that was almost unsayable by somebody who only knew American phonemes or something. But any case, there was,
You know, he's very into words and languages and so on.
So it is something of an irony that when he was a young fellow, he
came up with this idea of strangeness, and he called it strangeness, which wasn't some nice Latin name that had some, you know, complicated etymological story that Murray would have been very excited about. It was instead just this weird word. And that kind of launched this kind of tradition of calling things in physics by strange names. And I think,
that, that tradition continued for a while. I mean, I think in some ways, physicists sort of realized that at some point, this doesn't make our field look particularly good, it just makes it seem kind of silly. I mean, I would say that the up quark and down quark
That naming was a late 1970s naming. As I say before.
I think they were mostly called P-Qork and N-Qork.
Or they weren't really given many names. Charm, who invented that name?
A European, but I don't remember which of the various groups invented that name. The,
You know, people…
Well, the bottom cork, top cork, people were… at one point, it was the beauty cork and the… and the,
truth quark, but then it became the bottom quark and the top quark.
I think, you know, this thing about names.
in different areas, you know, it's like, oh, we're gonna spend $10 billion on this particle accelerator to discover charm. Well, that doesn't, you know, that sort of sounds a bit unconvincing. I mean, there are fields where this happens, like, for example, in genetics, there are… people gave all kinds of funky names
to,
to various genes. One that comes to mind is the sonic hedgehog gene, which is an important gene, that, controls development. And then people,
People realized that,
you know, there were some kids that were born with defects in that particular gene, and it just wasn't good to say, you know, there's a problem with your sonic hedgehog gene, so those genes got called SH. And I think in particle physics, they would have been well served by doing those kinds of things. Okay, apparently we should wrap up, because in addition to me being late.
at our headquarters in Illinois, there are tornado sirens going off. I'm a thousand miles away from that, so…
And I can't, so I'm not there, but some of us involved with this stream are there, so I hope nothing goes… I hope the tornadoes stay away, and thank you all for joining me, and I look forward to chatting another time.
Bye for now.