Hello, everyone. Welcome to another episode of Q&A about History of Science and Technology. Happy to talk about
things I know about, about general history of science and technology. Also happen… are happy to talk about things that I've personally been involved in, so please feel free to ask about either of those kinds of things.
So, I see a question here from M. Rudeau. When did scientists and engineers split apart?
It's an interesting question. I mean, I think that… somebody like Archimedes.
is, said to have been both a serious scientist and a… and an engineer. That's a sort of data point back in antiquity.
Then, people like, oh, I don't know, I'm thinking of, in the 1800s, people like the,
not Sadi Conneau, the sort of developer of the second law of thermodynamics, but his father, Lazar Conneau, was both an engineer and a kind of mathematical theorizer about engineering. I think by the time we get to people like James Watt.
… in the… and steam engine… of steam engine fame. I think…
he was more of an engineer, not so much of a scientist. What's the difference between a scientist and an engineer? I think, operationally, in the last couple of centuries, scientists have been people who sort of write papers about things. Engineers have been people who file patents about things. Now, obviously, there's an overlap between those two things, but they're slightly different motivation structures and slightly different objectives.
So I, I would say that the, sort of the, the people who
you know, if you look at different kinds of scientists, I don't know, I'm thinking about ones in the, …
in the mid to late 1800s, people like James Clerk Maxwell, developing Maxwell's equations for electromagnetism, was Maxwell an engineer? Well, not in… not when he was doing electromagnetism.
did he do some engineering kinds of things? I think he worked on some kind of physics apparatus. I think he might have also worked on some color matching kinds of things, which went into the kind of engineering domain. Actually, a better example of that is William Thompson, later Lord Kelvin.
Who basically was a scientist, producing hundreds and hundreds of papers about lots of kinds of physics-related topics. But he also was the guy who was on the first successful
expedition to lay a transatlantic cable. I think it was the third effort, third attempt. The previous two, the cable had broken at some point.
Kelvin was kind of on the ship with an electrometer of some kind, measuring resistivity of the cable, and saying, go slower, go faster, and so on, and they successfully made the cable. Now, that was… Kelvin, as scientist, applying his work in an engineering domain.
Kelvin subsequently started a company that essentially made equipment and, I guess, provided services for transoceanic cable laying and so on. And so that was definitely an engineering domain, even though Kelvin, I think, would have identified himself primarily as a scientist, so to speak.
I think it's kind of like…
those of us who, like myself, who do basic science a lot, I also actually build technology, which is kind of an engineering activity. I think it's a thing where the majority of people would identify themselves either as doing science or as doing technology, not as doing some merger of those.
I think that, certainly, in today's world even, those of us who try and merge those two kinds of approaches to things are still, I'm happy to say, alive and well.

But the majority of, kind of, …
Sort of, people are going to be either on the science side, working with the objective of producing papers, or on the engineering side, working on the purpose of producing products, filing patents, things like this.
Let's see…
How to comments?
If alchemy was what turned into chemistry, What turned into physics?
I think the fundamental answer to that is philosophy.
I think physics is sort of a question of how the world works.
And back in antiquity, whether it was Democritus, Heraclitus, Lecapippus, any of these kinds of people, were…
sort of saying, the world works like this, and we can say it as a matter of philosophy. I mean, that's still true with Descartes, and so on, of sort of the assertion, the world is this way.
And that turned into, sort of, that philosophy, natural philosophy, and then the kind of dividing line of those things is probably Galileo and then Isaac Newton, with his, kind of, mathematical principles of natural philosophy.
So I think kind of the thing that sort of made it into hard physics was when it formalized, in that case through mathematics.
In the case of chemistry, the sort of formalization of chemistry is an interesting question. I suppose at the point where, in the 1800s, where there started to be chemical formulas that were routinely written down.
when people understood the idea of chemical elements and so on, that was the point at which alchemy, which is sort of, you just stir these things together and kind of hope that things are going to happen.
that was the point at which it formalized and could be said to have turned into chemistry. So I think that's sort of the dividing point. Now, an interesting question for a field like biology
Is when will biology make that transition from what is essentially, when it comes to things like biomedicine, a lot of it is sort of at the alchemical stage at this point, of not having sort of a theoretical backbone to hang itself on, not having a way of formalizing itself.
There's a certain amount of formalization that's happened in those areas.
Through chemistry, through molecular biology, and so on.
Where one could sort of precisely say this is the configuration of some molecule or whatever else. But sort of broader formalization of the kind that happened in physics through mathematics originally, now I think physics needs to be computationalized, so to speak, and the formalization really is a… should be a computational one, rather than a mathematical one. But kind of how that works in biology, I mean, this is something I've been working on.
Recently, is an interesting question.
I mean, there are other fields where one can ask about that transition as well. For example, let's say economics. You know, when is there… when did there emerge kind of a science of economics? When did economics emerge from being essentially philosophical
to something about which there's science. And obviously, in the 1940s and so on, there was an effort to sort of mathematicize economics. It's not clear, even today, how successful that's been. One might think that one needed kind of a different formal framework.
maybe, something I'm interested in trying to build, associated with computation, a sort of formal framework for economics in the same kind of way that we have successful formal frameworks in areas like physics and so on.

But that's, … It's a good, good question.

Let's see… …

Left asks, where would you put science now? Do people work with science the same way as in the 1800s? What's different in the collaborative style?

Well, science has been much more institutionalized since the 1800s. Scientific careers are more structured, the

the kind of, … in many areas of science, there's a tower of things that are sort of necessary to know to really get started in that area of science. I think that, specialization increased.

I think in terms of, sort of, collaboration, it's an interesting question. That's something that's changed in the last few decades, actually. I mean, there are fields where it used to be the case that most papers were single-author papers.

And now it isn't true. In mathematics, for example, even 30 years ago, the vast majority of papers were single-author papers in mathematics. Now that's no longer the case. In physics, there were many single-author papers. That's now rarely the case.

I mean, I have to say, in my own experience,

I'm… maybe I'm just too egotistical a character or something, but I… I find it… it's… it's great to work with people and so on, but when it comes to writing things and really structuring things, it's a one-person job, I think.

But in any case, people…

the conventions about, sort of, how that works of, sort of, authors of different things. I mean, in…

in areas of experimental science, particularly in the life sciences and so on, it became traditional at some point for the person who set up the lab to add their name to the paper. Sometimes his first author, sometimes his last author, and so on.

In recent times, I mean, since I haven't been doing academic papers, which is now, I last did a sort of formal academic paper in 1986, so it's been a while.

But since that time, there's sort of been a convention that developed of saying at the end of the paper, you know, this person did this, that person did that, you know, this person was responsible for inventing the idea for the paper, this person was responsible for writing the paper, doing the data analysis, whatever else.

it's become a lot more structured, and it's become, you know, the number of names that go on papers has increased. In AI, for example.

it's now common to just put blocks of hundreds of names that are probably everybody who works at a particular company on the paper. And it's now… it used to be the case that when you cited papers in references, you would say, first author, et al.

and others in Latin. But now… now that papers are no longer on paper, it's like, well, it doesn't really matter if there's an extra

you know, 10 pages of citations, and so I see, particularly in AI, it's not uncommon to see people cite all the authors. So, you know, you'll have a paper that's 3 pages long, and then it'll have 20 pages of citations, where most of those citations are just copied out of some citation database.

And by the way, I… the thing that is kind of amusing, if annoying.

is that citation databases are getting more and more corrupted. So, for example, there are things of mine where I occasionally see people citing them, and they've got completely the wrong citation. Because at some point, you know, Google Scholar got an incorrect citation, and it's kind of like, like, like,

Eventually, that fork

that's wrong starts accumulating citations to the citation, and so that means that that fork starts growing at the expense of one that's correct. Now, of course, what that tells you is people are just picking citations out of citation-based databases and plopping them in papers. They've never looked at these documents. And I have to say, this is why I tend to believe that a much more useful
thing to do than just plopping in random citations is to actually write a narrative history of what one's talking about. Because, and it's been true now for more than 25 years, if you have the keywords, you can do a search on the web.
And you can find more information with better context than you could by just saying, here's a random citation that, by the way, I didn't even look at and got from some citation database, and it might have been horribly corrupted.
But in any case, the kind of… the structure of science and its kind of collaborations and so on, it's been a different story in different fields. I think that it's probably not helped
By the fact that in academia, there's been, maybe 20 years ago, 20 to 30 years ago, there was a lot of, kind of, formalization of what it meant to be a successful academic.
I mean, back in the day, it was like, oh, people thought you'd done good work, or people would write
glowing recommendation letters or something like that, or, you know, somebody's, you know, cited you and gave you some big prize, or something like this, or whatever it is. There was a more kind of fuzzy notion of sort of what it meant to be successful in academia.
And then there started to be these citation databases, these databases of papers that contained information on who was citing who. And then things started getting gamed up.
And it was kind of like, let's get a chain of citations. Let's have a citation clique, a citation club where we're all citing each other, and so on.
And then people started trying to say, well, you know, in order to tell whether we should promote this person from associate professor to full professor, let's count how many citations their work has. And then people realized that doesn't really work.
Because some of the journals they're getting cited in are kind of semi-fake journals, so let's now say, well, we're going to weight those citations by the impact factor of the journals that they're publishing in, where the impact factor is. It's self-defined in terms of number of citations that those journals get, and so on.
And it started to be the case that that sort of academia, in an attempt to be more streamlined and more, quotes, fair, was doing things like saying, let's just weigh, you know, how many
at some point, it was kind of like, how big a stack of paper is that… has this person published?
And then it was like, well, some of that may not be, sort of, it just may be sort of fake publication, so let's use impact factors to weight that, and so on.
It's, … it's sort of a very, …
I don't know, formalized setup in some ways that, of course, is deeply gained in a zillion different ways.
The unfortunate feature of such a formalized setup is it doesn't really take account of innovation.
For example, it doesn't take account of innovation in the way that science is communicated or should be communicated.
And so, for example, people who, I don't know, write computational essays, or produce data, and so on, they, in a formalized version of science, where the only thing that matters is published papers with citations and impact factors and so on.

That, then you lose by doing something which might be extremely much more useful to the scientific community, and sort of on the leading edge of innovation.
but doesn't happen to be part of the institutionalized formalization of what's been set up as sort of the value system of science. And so, that's a… that's a thing that I think has, you know, it's distorted in an unfortunate way what gets done into science… in science into something which is more gameable
In the kind of, publish…
You know, the phrase, publish or perish, was one that kind of came into circulation maybe 40 to 50 years ago in the academic world, and it's kind of… that's like, just get more citations, get more papers published, and so on.
And that's certainly that… the gaming of that has certainly affected the way that, sort of, author lists and so on work, and things like this. And again, in different fields, different situations, people have very different conventions, and what's communicated, for example, the order of authors on a paper.
Some people will sort of just… it's always alphabetical.
Some people, it's like, let's, you know, have a… decide who gets what, you know, who's first, who's second, and sometimes now there are footnotes that say, these people were tied for third place in their contribution to this paper, and so on. It's… it's complicated and heavily gained.
In terms of the actual dynamics of collaboration, sort of the big thing that changed a lot of that was the internet, and more recently, kind of, oh, you know, video conferencing and things like this. But it, I think, was the case.
back.
well, I'm trying to think. Even when I was publishing academic papers back in the 1970s and 1980s, I would write papers with people who were not, sort of, physically in the same place that I was.
And it would be a question of sending things back and forth by physical mail and so on. And most of the people… I mean, I used email… I started using email in 1976, but many of the people I worked with didn't use email until
probably sometime into the mid-1980s, or even late 1980s. …
But I think this idea of, sort of, remote collaboration has now become just routine. Back in the 1970s and 1980s, it was definitely slightly exotic.
It was much more common to be sort of collaborating with somebody who was physically at the same university as you were, and so on. I think it's an interesting question. At what point did it become the case that there were many scientific collaborations where the people involved in the collaboration had never met each other?
And that, I'm sure, happened, and … well, in… I'm saying that particularly about theoretical science, where there are a fairly small number of people
who are involved in some particular collaboration, rather than experimental science, bigger experimental science, where there started to be hundreds to now thousands of people involved in some particular experiment. But I'm talking about some paper that might have three authors, maybe they've never met each other.
And that's pretty common now, and it certainly wasn't common back a few decades ago.
Let's see …
It's a question from Lewis.
How did your early work in physics lead you towards computation?
Well… okay.

So… I personally got
interested in physics when I was probably 10, 11 years old or something. Started reading physics textbooks, …
A very early story of mine had to do with a particular book about statistical mechanics that had this kind of illustration of, kind of the dynamics of molecules,
bouncing around in a box on the cover, and I thought it was a very interesting phenomenon, and one of my early computer programs from probably around 1973,
On a very primitive computer, had to do with trying to reproduce that
picture of balls bouncing around in boxes and things. I didn't succeed in doing it at that time. It's something I…
I worked on later, and even wrote this book fairly recently about the Second World War of Thermodynamics, whose cover is kind of a homage to the cover of that original statistical physics book that I had got in 1972.
But that was kind of an early use of kind of using computers for me. The bigger use had to do with doing particle physics. I got involved in doing particle physics around 1974, when I was about 13, 14 years old.
And I guess, actually, no, in 1973, I got involved in particle physics. … And, …
one feature of particle physics is there's sort of a big tower of mathematical methods, particularly around Feynman diagrams, which are sort of the way of computing the
rates of particle interactions according to, sort of, fundamental theories of particle physics, or then fundamental, I would say. We've now gone a lot deeper in what's fundamental in things like our physics project and so on.
But, if you wanted to know
What was the rate of some reaction… interaction between quarks and gluons and photons and so on?
the way to study that was with Feynman diagrams, that… where you would draw these diagrams that represent the interaction processes, and each diagram is associated with some mathematical expression that involves all kinds of integrals.
And all kinds of abstract matrix multiplication, and so on.
And those… doing those abstract matrix multiplication and integrals and things like that is very mathematically complicated. It's very mechanically complicated. And so, I wanted to do those things, and that was something that, particularly around
1976 or so, I was quite involved in doing a bunch of calculations in, well, QCD and some other areas. QCD was then a very young theory. In 1973, it had been shown that QCD had this feature of asymptotic freedom.
Basically, QCD is the theory of quarks and gluons, quantum chromodynamics.
And what had been discovered in 1973 is that if you bash a quark really hard, it will behave as if it's interacting less strongly with gluons than if you don't bash it so hard.
And that meant that in so-called deep and elastic scattering.
Typically, things like firing beams of electrons at nuclei, on nucleons, protons, and so on, that what would happen is, occasionally, the electron would interact with a quark in the proton, and kick the quark very strongly in some direction.
so-called high transverse momentum, event, and that would lead to… that… that sort of kicking process would be one that, if it was sort of violent enough.
The effective interaction of gluons will be small enough that you could say, well, let's just work out what happens if there's one gluon interaction.

That's more likely than if there's two gluon interactions, and so on. You could make a so-called perturbation series where you are making kind of this approximation. Let's look at the zero gluon interaction, the one gluon, and so on, and each one is progressively smaller.

So I was interested and involved with what had newly become possible, which was to do meaningful calculations in QCD on the basis of this perturbation series and Feynman diagrams and so on.

But the mechanical math was very complicated, and so I started using computers to try and do that sort of mechanical math, it was algebraic computation. That was not a thing people commonly did with computers. People usually were dealing with numbers with computers, and, that, …

…

And it was sort of a… people had worked on, kind of, algebraic computation back in even the 1960s. It had been done particularly for celestial mechanics, for doing, kind of, calculations of the motion of the moon and things like this. That can conveniently be done using algebraic expressions and so on, at least at that time. Later on, it kind of the…

The purely numerical methods for doing that actually overtook the methods that were based on doing algebraic computations. But in the case of Feynman diagrams of quantum field theory, there really wasn't any choice but to do algebraic computations, and they were complicated ones. And so.

the, the thing that, I was, excuse me, the, the, … … … the, … …

The thing that, … …

So I got involved in doing that. At the time, there had been a variety of systems that people have built

For doing, kind of, algebraic computation. They were… they were systems that were mostly used by the people who built them. They were mostly not really used or expected to be used by, sort of, end users, so to speak.

I guess I didn't really know that, and I just started using these systems anyway, and it took a certain amount of wrangling, but the result was, by 1976 or so, I was routinely using… using computers to do algebraic computations, do particle physics kinds of things, and I suppose I was… was, conveniently had access to these computers.

Both at a government lab I worked at in England called Rutherford Lab, and then over the ARPANET, what's now the internet, connecting to remote computers in various places, particularly MIT. So that's what kind of got me involved in doing sort of, algebraic computation with computers, I got to be probably the world's largest user of such things by around 1978.

And I did lots of things in particle physics in that way.

1979, I'd kind of outgrown the systems that existed, and I kind of decided, well, how am I going to sort of build for the future? Well, I think I've got to build my own system, and that's what got me started on building SMP, symbolic manipulation Program, which I started in November of 1979.

Now, a thing that… that then… so I built that, first version came out in 1981, that turned into my first company, and it's a whole long, complicated story around that.

But the, the thing that was notable was, …

the… the… having built SMP, I had kind of gotten pretty deeply involved in understanding things about, sort of, what computation really is for the sake of having… building… sort of drilling down to build the right primitives for this,

Computer language, and that got me more involved in, kind of, the foundations of computation and questions about mathematical logic and so on.
And then, after that, I was interested in, kind of, how
Complex things happen in the world, and that got me into the whole question of, well, how do you make models for things like that?
Well, the answer was not use mathematical equations, because that didn't seem to work very well.
Instead, it was more like, well, what else can you do? But I'd already had all this experience with computation and sort of understanding foundations of computation for the sake of making this computer language, and so that made it quite natural for me to think of that
As a possible way to, to sort of study things about the world, to use computation as a sort of foundational idea for studying things about the world.
I think another thing that I realized only very recently is it was very natural for me to do computer experiments, visualize the results of those experiments, and so on.
Partly because I had expected to get intuition from, kind of, the things that computers produce.
It had been more traditional in using computers for people to just say, I'm going to get one number out of my computer, I'm going to get a few numbers out of my computer. It doesn't give one a lot of structure to give one intuition. It's just, well, here are a few numbers. You sort of have to have the intuition coming from somewhere else.
But from working on algebraic computation, it was really routine for me to get some big, complicated algebraic expression and have it be the case that the structure of that expression was a thing
that, would be, the thing that I wanted to study and get intuition from. What kinds of mathematical functions show up in what configuration? Let me get intuition about how things like that work.
And so it was then quite natural for me to say, well, if you run a program, let's get intuition from what the program is doing inside, so to speak, not
from just the one number that it produces as output, but what's the structure of what's happening inside? And that led me to start thinking about, kind of, visualizing the results of… visualizing the process of computation and things like cellular automata, and so on. So that's the… kind of the arc of that story.
What's sort of perhaps meta-interesting about this is this realization that sort of doing algebraic computation ended up being a thing that allowed one to get sort of intuition from the doing of computation, so to speak, rather than just the results of computation.
that… that that was a thing that was significant in realizing that one could do computer experiments and visualize what was happening and things like that. That's a thing I realized only a few months ago. So even though it was a thing in my own life that happened a really, really long time ago.
It still was something where I sort of hadn't pieced together the threads of history to see that that was what was going on until really quite recently.
Let's see… …
Bob is asking, why has the second order calculation for the electron magnetic moment never been published before? I think it's published, I think it's known up to the sixth order now. So I don't know quite what that refers to, but the, …
You know, this is a phenomenon about electrons.

Electrons have this notion of spin, roughly. They don't actually spin around. We don't really know what spin is at a foundational level.

But there's a… there's a definite… the electron acts like a little tiny bar magnet, and you can talk about the orientation of the bar, of the bar magnet that determines the magnetic field associated with an electron.

And one of the features, when an electron is, kind of going around the circle, its… its spin processes as it goes around in that circle, and there is a… it processes in a certain way its G factor, which determines the… the, sort of the… the way that the magnetic

the way that the spin processes relative to the motion of the electron. It's usually equal to 2, but there is a small deviation from 2 that is the result of interactions between the electron and itself. through… through virtual photons and so on. And that's one of the big, exciting calculations of quantum electrodynamics. Originally done in the 1940s, the first-order calculation. It's the… the electron's magnetic moment is the 1, which is actually 2, it's G is 2, times 1,

plus alpha over 2 pi, if I remember correctly, where alpha is the so-called fine structure constant that essentially determines the strength of interaction of electrons with photons, and then 2 pi is, like, pi, like the ratio of the circumference to diameter of a circle.

But, so, this was a calculation originally, I think, done by Julian Schwinger, of

The correction associated with the fact that electrons can sort of interact with themselves through through virtual photons and so on, the correction to the magnetic moment of the electron. So that was… that was an early calculation, it was an early application of Feynman diagrams, but then you can get more elaborate things where the… where the electron interacts with itself twice, or three times, or whatever else.

And it gets more and more difficult to do those computations.

And there were a collection of people who sort of took it upon themselves to just do more and more accurate computations, and then the experiments became more and more accurate.

And the electrons were trapped in these, kind of, traps of electrical magnetic fields, and then you could watch, kind of, what would happen to the magnetic moment and so on. You get very accurate

Calculation at very appropriate experiments on the,

on the value of this anomalous magnetic moment. I think these days, it's maybe one part in a trillion, maybe better than that, that the electron anomalous magnetic moment is known.

Well, then there were these calculations that could be done, and each calculation, it's kind of like in powers of this fine structure constant, which is 1 over 137, roughly.

And so there's, like, a 1, and then there's 137, alpha over 2 pi, 1 over 137, divided by about 6. So that's a small correction, you know, 1 over 1000 or something.

And then the next order term is alpha squared, which will be 1 over 137 squared, times some factor, and it's that factor that you have to compute by doing all kinds of Feynman diagram calculations that are very algebraically and numerically complicated.

So, there was sort of a race on between the experiments and the theory of which could be calculated more accurately.

And it became the case by the 1970s that this was sort of a high-end application for algebraic computation, and it was routinely the case that people would be spending, you know, a year of CPU time doing these calculations to work out these anomalous magnetic moment of the electron.

And in terms of Feynman diagrams, each diagram represents some mathematical expression involving integrals and things like this, and it became the case that there were hundreds of diagrams.

At, maybe 3rd order, and then at 4th order, there were thousands, tens of thousands of diagrams, and so on.

Now, there is a little bit of a squiggly issue here, because what one's doing in working out this electron anomous magnetic moment, one has this series expansion. It's 1 plus something times alpha, plus something times alpha squared, something times alpha cubed, and so on.

That's all well and good, so long as… and if alpha is small, the successive terms are progressively smaller. That's all well and good, so long as the prefactors of those terms don't get too big.

Well, there's a bit of a problem, because what was worked out in the 1950s, I think.

is that you can expect that the prefactor at kth order, the… the… it'll be an alpha to the k, 1 over 137 to the power k, but then the prefactor will be something like the square root of k factorial.

So the problem is, k factorial will always win relative to alpha to the k for sufficiently large K.

So this series that you think you're working out progressively smaller and smaller terms, eventually they'll be big, big, big terms.

Nobody knows how that works. Nobody knows why those very high-order terms involving the exchange of, you know, 10,000 photons or something

Don't, because of the combinatorics of how many diagrams there are and how big this prefactor is, don't sort of overtake this perturbation series and start affecting the results one sees. But they don't, for reasons that are not well understood.

the guesses about why that might be, but it's not well understood. But in any case, there was sort of this… this race on between the calculations and the experiments.

And, that was a sort of big application of computer algebra. I don't know to what extent the details of

What was calculated, have been kind of exposed out there, and to what extent it's just, sort of, trust me, we did the right calculation.

If I were doing it in modern times, I would just put out, you know, the Wolfram language expressions and the notebooks and so on that do all the calculations, and you could, in principle, run them again, even though it might take you years of CPU time to reproduce things.

Now, what happened in this competition between experiment and theory is it got complicated because a large part of the theory is just quantum electrodynamics, just the interactions of electrons with photons. But the way quantum field theory works

There is this idea of virtual particles, particles that sort of exist only for a very brief time and then disappear.

Electron… the particle-antiparticle pairs that exist only for a very short time then disappear.

And that can happen in this interaction between electrons with themselves that lead to the anomalous magnetic moment, and in particular.

The… the results of, well, virtual photons can sort of turn into collections of things like pions, … certain kind of particle that's relevant in holding nuclei together. They can turn into these things for a very brief time, and then turn back into virtual photons again, and then interact with electrons.

those processes, those virtual particle processes, are something where there isn't a theory for those, really. There isn't a good theory for those. It's not like the theory of electrons and photons, it's very precise, you can really calculate everything.

It's something where there isn't a good calculational method, and pretty much you have to do the experiments. So it turned out low-energy electron-positron annihilation

Where electrons and positrons collide and produce, let's say, virtual photons and then turn into pions and things. It turns out that the details of how that works eventually affect the value of the electron anomalous magnetic moment.

And so, in recent years, there have been various anomalies that have showed up, sort of deviations from the experiments, between the experiments and the theory.

That people have sometimes made a certain amount of hay out of. But,

So far, it's always turned out that accurate enough measurements of the… of this particular thing that sort of is an indirect effect on the electromagnostic magnetic moment, that measuring that more accurately has resolved the conflict.

But the reason that one might look for a conflict is that in this whole virtual particle business, you're kind of, in principle, sensitive to the whole spectrum of possible particles and other weird things that can happen at a very small scale. And so, it might be the case that by just measuring this sort of

large-scale electron anomalous magnetic moment, that you're actually probing things that are more fundamental features of physics. So, for example, I've been curious whether dimension fluctuations, fluctuations in the dimensions of space, the number of dimensions of space, might have an effect on the electron anomalous magnetic moment.

They do have a small effect, but it's probably not a good way to measure that kind of phenomenon.

Let's see… …

Bob is commenting, yes, the answer's been published, but trying to find the actual calculations is another story. That's interesting, I didn't know that. I mean, I've, … you know, it has been a very bad thing, usually, that people do not make computable publications.

I mean, I have made a point in the last many years of having it be the case that anything I write about science that has some picture I calculated, you can click on the picture, you get Wolfram language code, it should generate that same picture.

we kind of run software testing tests on all things that I've written to try and make sure that that continues to be the case.

And that there's no weird glitch that prevents that from working. But for me, kind of everything I write is computable and reproducible. And I think that's a great criterion for being able to do, sort of, solid science. Certainly as a practical matter, the fact that I've done that

means that people can build on things I've done and expect that, that, that they can,

that they can sort of follow on from what I've done. But this is a very rare thing in science. Very, very rare. I mean, various times the US government, for example, has tried to push scientists in saying, we'll only support your work if you'll put the data that you have generated out there for other people to use.

But… The records of people doing that are really terrible.

I mean, people put the data in some completely unusable form, they don't understand what it means to have sort of transportable data, and when it comes to code, it's a much more grotesque story even than that. And the chance that something is reproducible is really low.

it's not helped. It helps a lot when people are using Wolfram language, because it's very succinct code and everything is right there. You're not trailing through 8 levels of Python libraries or something, trying to get to the thing that you need.

And saying, oh, it was version such and such, and I have the wrong version, and so nothing works. Or I didn't have the right macros for my spreadsheet, or whatever else.
in Wolfram language, sort of, it's all there, and it's all included, and it's all kind of documented, and you can just run it. So, using that makes it easier to make sort of computable, reproducible science. But still, even with that technology that's, after all, existed now for, what, 38 years or something, the amount of, kind of, reproducible science, sort of code-level
reproducible science that's been done is much smaller than it should be. And people say that the reason for this is, well, what's the motivation for somebody to make
their work sort of reproducibly shareable like that. It… I mentioned before, kind of, the dynamics of academic science and, you know, all these things about citations and papers and gaming those things, that doesn't… that doesn't… you don't get extra points.
In today's world, you might in some future world, but you don't, in today's world, get any extra points for making the work that you've done sort of reproducible and computable.
And that's… that's one thing. You don't get any extra points, so if you're gaming the whole thing, you're not going to do that. But the second thing is that many scientists say, well, I've developed all this stuff, and I want to use it myself. I don't want other people to have access to this. They don't want the thing that I try to do, which is to make it be the case that the things I write
are as readily buildable on as possible. That, for me, is the optimization.
Whereas for many scientists, particularly in academia, the optimization instead is, I want all this stuff to myself, proprietary, so to speak. It's kind of an amusing irony that, you know, scientists will sometimes get on
on some kind of high horse about how they want everything to be sort of free and non-proprietary. You say, well, what about your data?
I said, well, well, I put a lot of effort into getting all that data. I want to have it myself, you know, own it as a sort of thing that only I have access to.
And it's like, well, have you ever thought about what's involved in, kind of, building a big software system? That takes a bunch of effort, too.
Well, it's sort of an irony, and it's, it's one of… it's a fairly typical type of irony, but it's one that, sort of interesting. But it has been a thing that people… at different times, there was a big push about
15, 20 years ago now, to sort of have scientific data be more readily accessible, it really didn't work.
It really… there's a lot of resistance on the part of scientists to doing that kind of sharing.
there are particular fields where sharing was forced. I mean, famously, in genetics, the Journal of Nucleic Acid Research, I think it was, basically said, well, you're not going to publish any more papers about, sort of, sequencing genes and things unless you deposit
the sequence that you got in this thing that was then called GenBank, now it's NCBI, that, is sort of a central repository of those things.
Let's see…
…
Mathematical asks, did Feynman try to learn string theory, or was he not interested, or was it too complicated to learn?
Well, I certainly knew Dick Feynman at a time when one iteration of string theory existed.
… Let me tell you a little bit of the history.
So, back… Around 1960, there was…

Particle physics was chugging along, particle accelerators were being built, higher and higher energy particle collisions were happening, and

One of the notable things that was coming out was more and more particles were being discovered.

Back in the day, you know, beginning of the 20th century, there was the proton, then by the 1930s, there was the neutron, and then in the 1930s, the theory of pions, which were exchanged between protons and neutrons, to kind of bind them together into nuclei.

Then the muon was discovered that nobody expected, then the pione was discovered a little bit later from cosmic rays, and then accelerators got into the business of accelerating particles in… with magnetic fields and so on, and then it became possible to generate lots of particles. And so. huge zoo of particles got discovered, whether it was particles like protons that are things like the hyperons, and the various excited versions of the proton, the delta particle, all this kind of thing. Those got discovered, and then the so-called mesons

Which are particles like pions, that are quark-anti-quark particles.

And then a big zoo of those, the pion, the rho particle, the F meson, the A2, the A1, the G meson, you know, long series of these things. Then the k-ons and the K-star particles. I could yak on about these for a long time. The real question is whether I still remember the properties of these particles, and I bet I do.

Even though I was… I was into these things around 1973, and I… I guess it's a good memory exercise for me. Can I remember that the mass of the K-star particle, I think, is 892 MeV?

And things like this. Very obscure kind of trivia of that are sort of the, the, that have been thought of as the basic particles from which one builds things.

the actual particles that exist in natural form on the Earth are just protons and neutrons and electrons and things. These things, like the hyperons, the sigma particles, the lambda particles, the strange mesons, all these kinds of things, they have lifetimes around 100 millionth of a second or less, and they end up

being things that can only be produced in particle accelerators, or perhaps in some other sort of violent situations in the universe, or in the very early universe. But they're things that, as a matter of physics, are of interest.

Okay, so…

1960, beginning of the 1960s, a whole giant zoo of hundreds of these kinds of particles were being discovered. How to make sense of this? What were all these particles?

Well, there were… there was sort of a prevailing theory that… okay, so one possible theory was these particles, like the chemical elements, were all made of something more fundamental. And there were various theories like that, but the one that really emerged as sort of the serious theory was 1964, the quark model.

But… so that was to say these particles were all made of, sort of, more fundamental constituents.

Around 1960, there was sort of a competing idea, a so-called bootstrap model.

that said that, sort of, all these particles, a very democratic thing. It was a time of… it was a model that came out of the University of California in Berkeley. At that time, I think, a big center of, kind of,

left-leaning kind of thinking, and so on. So this was the… the let's have it be democratic. I think they were more communist, actually, than… than whatever, but…

never mind that. It was kind of a very, you know, an anti-elitist view of particles, that sort of somehow all the particles were as elementary as each other, and somehow there was just this relation between all the particles that would explain how they all worked.

And that was, … that was… that was all wound up with this thing called S-matrix theory, which basically said, don't ask how a particle interaction happens, just ask what goes in and what comes out from the particle interaction.
Okay, so… that… led to a, …
there was a different part of that story, which is a thing called Reggie Theory.
Okay, so what, what, what people…
the people found all these different particles, and one of the sort of organizing principles was that there seemed to be families of particles. The pion, the rho meson, the F meson, the G meson. These all sort of formed some kind of family. They all had different spin values, 0, 1, 2, 3, etc. And somehow, when you plotted a graph that was spin value against mass of particle, you would tend to find that these things followed sort of straight lines. Well, they were either straight lines, or the square of them, or the square root of them was a straight line. But anyway, they were… they were definite sort of progressions.
And so then there was this idea, so-called Reggie theory idea, that somehow all these particles were all connected in some way, and that they were all kind of really just manifestations of the same particle.
And in particular, the idea that the spin, which had been 0, 1, 2, 3, that really there was a theory that could be made where the spin was an arbitrary complex number.
So it could be 2 plus 3… 3.7i or something. And there was a big theory of, kind of, the continuity of scattering amplitudes, the probability of particle scattering, as a function of complex angular momentum, complex spin values, and also potentially complex momentum values.
And this became a very mathematically complicated theory that was being developed in the early 1960s.
And one part of that theory was these scattering amplitures, these probabilities of interaction. There was kind of this idea that you would find
Scattering amplitude functions that were functions of momenta and things like this that have certain properties.
In particular, they have analyticity, that they're smooth as a function of these momenta, and they have this thing called crossing symmetry that means that particle-antiparticle exchanges and things, when you looked at different versions of whether it was scattering particles versus antiparticles and this and that and the other, that there would be a relationship between those different scattering amplitudes.
So then there were some mathematical sort of tricks discovered, lots of things involving gamma functions and so on, that, were the… the sort of the scattering amplitudes that made for kind of a, a, …
a very convenient, sort of, particular model of the S-matrix that one could work with.
So, put that to the side for a second. That had emerged 19… by 1970. I guess that was… that was a thing, of these kind of, models of things about complex angular momenta, continuity with respect to,
to Mement… to, Mementor and so on.
That was kind of a whole area of study. That area of study went into retreat when the quark model seemed to be what was really going on.
And when what was happening in those families of particles that were related were just… it was the same type of quarks and antiquarks, but just like the different levels, excitation levels of an atom, they were different excitation levels of the quark-anti-quark system. And so, that kind of… by, I would say, 1970,

with some other experiments that were done that showed that there were sort of hard bits inside protons that could be identified with quarks. There were a bunch of experiments done in 1970, 1971, and so on along those lines. By that time, this S-matrix theory and Reggie…
theory, and all this kind of thing, and all these mathematical formulas for scattering amplitudes. That was all somewhat in retreat.
Okay. Meanwhile, there had been a quite different tradition, which had to do with making Fundamental theories in quantum mechanics and Relativity.
And… So, the… the equations, the so-called wave equations,
for… that describe the quantum mechanical amplitudes for particles. There have been the Schrodinger equation, which was from 1926, maybe? Something around then. And … and then…
That was kind of an equation for a slow-moving, non-relativistic quantum particle.
Then along came the Dirac equation, Paul Dirac made up, that was an equation for an electron, a particle like an electron, that could be moving at any speed, anything up to the speed of light.
So, that was a relativistic wave equation.
So then there was a lot of, kind of, can we make other relativistic wave equations?
Now, it was tricky to do that, because some of these relativistic wave equations, if you set them up a bit wrong, they would end up with particles that could go faster than light, which weren't observed, and things like this. It's a tricky thing. Even today, higher spin wave equations aren't really well understood.
There's a thing called the Raurita-Schwinger equation, which deals with particles that spin three halves. The electron has spin a half.
For spin 1, things like photons, there's… well, spin zero, there's the Klein-Gordon equation. Spin 1, it's essentially Maxwell's equations that deal with that.
But it was a tricky thing, making up these consistent wave equations. And then people said, well, what about something that isn't just a point particle? All these things, if you have a point particle, and you make it go close to the speed of light, it's still a point particle.
It gets a lot trickier when you have an extended object.
If you have an extended object, the object will get shrunken and do this and that, and it's not clear that you can have a consistent model of a sort of an extended object going close to the speed of light. And so.
Combining that with quantum mechanics, it became a big challenge to make any kind of reasonable theory of that sort of mixed sort of physical extension with
relativity and going very fast with quantum mechanics. So there was a different branch of the extent to study relativistic extended objects in quantum mechanics.
And that led to kind of the idea of a string that was, instead of a particle, which in space-time draws out kind of a world line.
A string in spacetime draws out a world sheet. It's the string that's flapping around, and as it goes through time, it defines the surface in spacetime.
And so, by in the 1960s, early 1970s, I think, people have been studying, kind of, this idea of extended objects in quantum field theory.
And had sort of come across the idea of strings, and that sort of merged with what had been done in regga theory, and that became sort of a first iteration of string theory that was originally a theory for particles, like these mesons and pions and row particles and things like this.
was originally a theory of those things. That was kind of string theory 1.0.
Then, subsequent versions of string theory

which included supersymmetry and things like this, and became theories of fundamental physics, not theories of these complicated particles. So string theory got reborn at least a couple of times, maybe three times even, with different interpretations of what it was about.
The… the version that existed in the… in the 1970s, was… was…
Pretty much. It was sort of transitioning from the version that was purely about …
kind of these strongly interacting particles, and where people lost interest in that, to something that was a theory about supergravity, that at the time, pretty much nobody cared about. It was a very niche
Kind of activity.
So, when I was at Caltech from 1978,
to 1983 or so, I knew Dick Pyman fairly well, and, …
there was also, at Caltech a person called John Schwartz, who had been working with Murray Gelman.
Murray had been the person who developed the Quark model, and Murray had then been interested in sort of mathematically interesting models of things, had been involved in QCD, in a complicated way, there's a whole collection of people who are involved with that, but also was kind of interested in
in the supergravity idea, because Murray's big break, Murray-Gomain's sort of big break, was applying group theory
to particle physics. And the way he thought about the quark model
was not so much, and you just put together these different kinds of quarks, and just sort of… just sort of assemble them into particles, but more he was thinking about, well, you make this transformation that's like a generalized rotation in some abstract space that gets you from one kind of quark to another, and that's sort of the way you're thinking about things, that the k-on is related to the pion by this transformation, this group transformation, so he was very much into those kinds of things.
And then wanted to apply those ideas more broadly, and that led to thinking about supersymmetry, where you have a kind of a group symmetry transformation that doesn't just go between, sort of, rotations in space, or sort of… well, the next thing was rotations in things like isospin space, which are kind of an abstract version of space.
But were rotations that would actually take you from one type of particle to another type of particle.
would take you from an electron-like particle, a fermion, so-called fermion, to a photon-like particle, a boson, and that was kind of a supersymmetry transformation
And that was something Murray was interested in, because it was kind of a group theory thing, and so in the 1970s, there were a bunch of people, actually, at Caltech who were sort of working on that stuff, and Murray was generally involved in that.
John Schwartz kind of came out of that tradition. I think he also
was working on the kind of strong interaction thing, but he was a person who was at Caltech from… I think he may still be at Caltech after all these years, but he'd been kind of continuing to pursue, kind of, this idea of strings and studying properties of scattering amplitudes and all these kinds of things.
And even when it was a very, very unpopular field.
Well, Dick Feynman certainly was aware of… I mean, it was a small group of people, was certainly aware of those things happening. I don't think he ever paid any attention. At the time, he himself

was, … Interested in things Well, he was quite interested in things related to QCD.
At the time when Murray Gelman had been inventing quarks.
that was 1964 or so. By 1970, I mentioned there were experiments being done on the so-called deep inelastic scattering. You fire a high-energy electron at a proton or something and see what happens, and if it's kicked.
far… far… if it's… if it hits a hard thing and sort of bounces back a lot, that's the so-called deep and elastic scattering. And, what had been observed, first at Stanford, about 1970,
Was that every so often, it would get sort of kicked back by something that seemed like a hard bit inside protons.
And so…
the, … Dick Feynman sort of tried to make an interpretation of those experiments, actually. He told me a large part of the interpretation he developed for that, he developed the night before some talk he was supposed to give, kind of staying up all night and figuring it out, and doing a bunch of calculations in quantum field theory to see whether it all made sense, which he then threw away and just told people some intuitive explanation that he had the next day.
But in any case, he had this idea of what he called partons, which were sort of parts of a proton. Murray hated this idea. He always used to call them put-ons. And, of course, part-ons turned out to be quarks.
And that was sort of an emerging fact in the, by kind of the… I would say by 1974, 1975, that was fairly clear that that identification existed.
Well, Dick Feynman then went on to sort of study other consequences of QCD, worked particularly with a chap called Rick Field, who was a younger, physicist who,
… I did some work with as well, who, … …
Was a good, sort of practical doer of write programs, run them, make things happen.
type person, not so much of a deep theory kind of person. In those days, there was sort of an area of particle physics called phenomenology, which was kind of like, how do you take the deep theory stuff and make it practical and compare it to experiments?
So, Dick Feynman was very much involved in that. Murray hated what Dick Feynman was doing. He kept on saying, this is all, you know, it's all mushy, you know, stuff, and you know, you're running these programs, and random things happen, and you don't really know what's going on, and you make some curve, and so on.
The thing that was neat about those curves is you could compare them directly to experiments, and they often worked.
But at that time, that was the kind of thing that Dick Feynman was, I think, mostly interested in, and really was not interested in these abstruse mathematical things of the kind that John Schwartz
just down the hall was, was doing. I mean, they both used to come. I used to organize a bunch of theoretical physics seminars.
at Caltech, and sort of everybody who came fit around some large kind of conference room table. And those two were often both there, so certainly there was some sort of visibility.
Now, I have to say, now that I'm thinking about it, in those theoretical physics seminars, maybe it's my own taste that was, showing through here, I don't remember too many talks about… in fact, I'm not sure I remember any, about, kind of, string theory, John Schwartz-style things. Maybe those talks were happening somewhere else, but they were not happening in the main, kind of, theoretical physics seminar series.

So I think the answer is, Dick Feynman didn't really pay any attention to those things, because it was sort of abstuse at the time, and it didn't really become

at all mainstream until probably, what, the late 1980s or so, with a number of, kind of, supposed breakthroughs. Now, I mentioned the difficulty of making, kind of, relativity and quantum mechanics fit together.

Those difficulties are even greater when you have these extended objects, like strings. And so this whole issue of how do you prevent there being these kind of weird tachyonic modes, these weird things where they're like particles going faster than light.

Well, it's a complicated mess, and it puts a lot of constraints on how string theory has to work. And that's what leads string theory to say, well, it'd be really nice if the universe was 10-dimensional or 26-dimensional. Those are dimensions where certain things cancel out.

And you end up getting something where you don't have these… these unobserved, kind of faster-than-light things, and so on.

And then you have to kind of make up theories that, well, there's 10 dimensions, really, but 6 of them are curled up in little circles and so on.

That had actually been a theory that had come from, from much earlier, from the 1930s. There had been some theories that, that worked that way, the so-called Colusa-Kline theories. But, in any case, that was,

That's a little bit of what I… what I know about that. I think it's… it's… I mean, Dick Feynman was not a believer in, sort of, modern mathematical methods. I think he was very much of a 19th century math kind of person.

It's like, do the integrals, work out the calculus, things like that. I don't remember him being at all seriously involved in anything, even with group theory.

let alone other kinds of things. Now, the Feynman-Parth integral

which is the sort of foundational way of thinking about quantum mechanics, thinking about sort of many paths being followed and so on. That involves a very ornate form of integration, so-called functional integration, which is very mathematically sophisticated and is still sort of somewhat mathematically undefined. And it's kind of one of those ironies that Dick Feynman kind of invented this whole methodology for, in practice, working with path integrals.

Even though he didn't understand or believe in or know about, kind of, the fancy math that people were trying to do to fill that in more precisely.

And I can give you a bunch of other examples where, sort of, I would say he was… he was pretty skeptical about the fancy math stuff. I remember him telling me once about, something he'd worked out about this thing called Bell's inequality in quantum mechanics, which says if you… if you measure

The, the probability that the spin of an electron is in this… is in, let's say, in the up direction, then you measure it also at an angle

say, theta away from that direction, what's the probability that the thing will agree with the measurement you just made? And it goes in quantum mechanics, like cosine squared theta,

And there's a question of whether, if you just say, well, there is a sort of a coloring of the sphere that represents the possible directions for measuring the spin, there's a coloring of that sphere where you already know in advance that you're going to measure this or that.

And so, Feynman had been interested in, is there… what kind of coloring of the sphere would you have to have to get this cosine squared theta effect?

And what's the… what's the coloring of the sphere where if you measure it as up in this direction, that you have the correct sort of falloff of measuring it as down, having some probability to measure it as down in a related direction?
And he'd worked out something about how if you color the thing half black and half white, that's the best case. But then there have been people who've been talking to him about these very ornate kind of spheres that were covered with sort of fractal, sort of arrangements of black and white and so on.
And that becomes a very elaborate piece of mathematics related to things like the Barnock-Toskey paradox to do with, sort of having… building up
well, doing things like taking a curve, a space-filling curve, and being able to fill a whole plane with just a line, and so on. These kinds of ideas. Feynman was very skeptical about those things, and was like, yeah, there's some fancy mathematician who's talked to me about these things, but, you know, what I know is this or that thing. You know, he didn't really believe in those kinds of, methodologies. I don't think he ever…
I'm trying to remember whether he…
I mean, he did sort of…
basic proofs of things, but I don't remember him being terribly interested in, sort of, a proof-type thing. He was more interested in, let's calculate this or that thing.
All right, I have to wrap up soon here, but if there are any more questions about, …
this, …
Well, let's see… … Alright, I'll answer one question here.
About, from living, about at what stages and decision points in your life did you engage with non-disclosure agreements, clearances, and so on.
To whatever extent, can you explain your own path, or just general advice on how it might be like for researchers today?
Well… You know, non-disclosure agreements… Are a thing that were… I would say common by the…
Late 1980s, mid to late 1980s, not so common before that.
… It's… I think…
I wonder what led to the growth of that. I think trade secrets, the idea that a company would have things that it knows internally, and it's…
sort of legally impermissible for somebody to just take those and start doing things with them. To preserve those, you have to have non-disclosure agreements. Otherwise, people will say, well, you didn't really keep it as a secret, so how can we help you in protecting it from somebody sort of taking it? I think there were…
you know, a bunch of things that were increasingly kind of thought of as trade secrets. Not only the formula for, you know, Coca-Cola.
but which is a long-running trade secret, but many other kinds of things became things that people wanted to protect as trade secrets. I think that in the past, maybe the point is that before software was so common, the things that
You were making, that were inventions that you were trying to protect, were more in the nature of widgets.
And you could protect them with a patent.
But if you had the widget, you could take it apart, and you could see how it worked, and you could build a widget like it. But by the time you had compiled code for software, where there

was a source code thing, but it was all compiled into machine code, and you couldn't tell what it was doing.
It became more relevant to have it be a trade secret, what that original source code was, because the thing you were giving people wasn't something where they could just take it apart and see how it worked.
So I think that's probably what led to the rise of non-disclosure agreements.
Was things like software, where you really had to say, just by virtue of the law, so to speak, you can't, you know, we're not going to tell you how this works, so to speak, rather than you can figure out how it works, but the law says you can't make a copy of this while the patent is still in force.
So, my guess is that's what led to that. Now, in terms of clearances in classified material.
That was a thing that, I don't know, in the US, that must have been a thing from maybe the 1920s, 1930s? Certainly by the time of World War II, that was a very well-understood concept.
It's been a little different in different countries. Like, I think in the UK, the… well, in the US,
If you get classified information, then if you… if you just happen to find it on the street.
and it's something which has a big thing that says classified stamped on it, you can go publish in the newspaper. There's no constraint on you if you're just a person who happens to pick the thing up off the street.
But if you have gone and got a security clearance, you're entering into a contract with the US government that is like a non-disclosure agreement that says, you know, I'm not going to tell people about this thing.
it's a little different in different countries. Like, in the UK, I think it's the case, I don't know if this is still true, but that it can be the case that the government says, this is secret, and even if you didn't enter into any kind of contract with the government, it's still… you can't tell people about it.
And that's… I don't know how… I haven't really followed how this has evolved there, but so…
In the US, it's sort of a decision you make. Do you get access to classified information where you've agreed with the government you're not going to reveal these things, or do you say, I don't want those clearances, I'm not going to do that?
Actually, sometime in the 1980s, I sort of had the reason to get, sort of, the whole suite of clearances, U.S. government clearances, and I kind of decided I was young at that time, and I decided, look, I don't want to sign up for something where I'm going to know a bunch of secrets about how nuclear weapons works, and how cryptography works, and all kinds of things like this, and for the rest of my life, I'm not going to be able to tell people about them. I thought, that's just not… I don't, you know, while I'm curious how some of those things work, and while I wouldn't mind being helpful in figuring out about some of those kinds of things. It's sort of just not a good personal calculation to do that, so I didn't.
when it comes to non-disclosure agreements, it's… it's kind of at this point in dealing with companies that are developing things and so on, it's kind of just a courtesy. It's like, look, you're going to tell me about something you're doing internally, and I'm not going to tell people about it.
And I think in, that's something where, the point where people are kind of, you know, having legal fights about non-disclosure agreements, many things have gone wrong.
The thing is, it's, as I see it, it's more like a courtesy of, look, you know, if I want to know about something you're developing internally and you're not ready to tell the world about, I'm going to

agree that I'm not going to be the one telling the world about these things. And that seems like a common courtesy to do that.

I think that's, that's been a thing that's emerged in modern times. Now, you know, does that entrain kind of things? If you're a scientist, for example.

Do you get kind of… does something you're doing get kind of grabbed immediately as something that is secret? That's a complicated issue. I mean, there are various cases of people I know, in fact, a chap I knew called Ed Fredken, who died a few years ago, I wrote a long obituary about him.

Was very… always liked to hack the system.

And had gotten to the point where he was working for the US government, and at some point, the things he wrote

He did not have the security clearance to read.

So he would write something, and in principle, it was immediately stamped, you know, secret, classified, and then he wasn't allowed to look at it again.

Which was, of course, a rather pathological situation, because he wrote it in the first place. But, you know, this is a thing where, sort of, having things that, immediately kind of, you can't look at them again is a slightly different story. I think in the case of…

I think it's sort of a common courtesy. If you're dealing with a company that is trying to control how it releases the material it's releasing, then

you know, if you want to know what it's doing before it's ready to tell the world, you've got to say you're not going to tell the world ahead of it. That seems to me like a kind of a simple common courtesy. Now, you know, there are other uses of non-disclosure agreements which have become

sort of used in all kinds of purposes to sort of prevent people from talking about things that have been going on that maybe they should be talking about, and whatever else. I mean, it's the same thing has been done with classification

from governments, of a government is embarrassed about something, so let's classify that information so nobody can know about it. You know, these are probably not the best uses of these kinds of,

kinds of mechanisms, but I think that's, that's different from the one that is sort of the more common kind of product and development-related kind of thing.

So I think, you know, my… my attitude towards these kinds of things is, if, you know, you're hearing about what's happening in some organization before they're ready to talk about it, it's like, why wouldn't you agree that you're not going to spill the beans, ahead of them if you want to know about these things ahead of time, so to speak?

… I think, … … It's… Yeah.

Okay, I think it is time for something…

that I need to do for my day job.

So, I probably have to sign off here, but I see a number of interesting questions that I will be happy to answer another time.

But, thanks for joining me here today, and, see you another time.

Bye for now.