[SQUEAKING] [RUSTLING] [CLICKING]

DAVID KAISER: DA, thank you. It's an excellent point. You're absolutely right. So I think both statements stand. So Compton's experiments convinced even more of the community. To win the Nobel Prize then or now you have to convince three people in Stockholm, which is hard. To convince the larger international community, which even back then included several thousand people, that's a different-- that sometimes occurs on different timescales by different processes.

So you're absolutely right, and I did not emphasize this. As many of you might know, and DA reminds us, Einstein did not win the Nobel Prize for any of the work on relativity even though he had achieved kind of renown, both within the profession and beyond, as we talked about briefly a few classes ago, because of his work on relativity, in particular, for the Eddington eclipse expedition stuff, which was released to the world in November of 1919. And yet, even after that, what he actually won the Prize for was indeed his heuristic suggestion about light quantum.

And so I think the idea there, DA, to make sense of it-- and I agree with you. It is a little funny timing-- is that there was growing awareness his work was of value and of interest. And then Compton's work really convinced, so to speak, the remaining skeptics, of whom there were many.

In fact, even Compton's work didn't convince every single remaining skeptic, but that did a lot of the work for the larger community beyond. And you can see that they were coming out kind of around the same time, but not literally one after the other. And I think a measure of that is how quickly Compton won his Nobel Prize for that work. So Einstein's prize was something like 16 years later, and Compton's was like 3 and 1/2 years later approximately, something like that.

So in terms of the way that the community recognized the value of Compton's experiments, I think that does point to a broader kind of coalescing. But you're quite right that Einstein did receive the Nobel Prize. When he won it, he won it for the light quantum hypothesis. Excellent point.

To be honest, I'd have to go back and check more carefully. My sense of that had always been Einstein's paper in 1905 that introduced the light quantum itself described three or four or five separate instances in which the light quantum effect-- light quantum hypothesis would help to make sense of things. So his paper on the light quantum covered several phenomena, including things like black-body radiation and other things and not only photoelectric phenomena like from [INAUDIBLE].

So I guess I had always thought they meant the light quantum which could be applied to things like photoelectric effect and other phenomena. But it's a great question you raise, Orson. To be honest, I'm not sure.

OK. So today we're going to launch into-- we're going to move into what starts being known even in real time as quantum mechanics, not this or that portion of a kind of old quantum theory. And we're going to look today-- for kind of 2/3 of today's class, we'll be looking at work most closely associated with Werner Heisenberg. Again, a name you probably already knew.

One of the readings for today was his very, very brief paper published in 1925. We're not going to start with that, we're going to warm up to it. So in fact, what we're going to do today is have one more look, a little more kind of detailed juicy look at the last gasp of what became known as old quantum theory.

Not just because it's old, but I want to talk about it because it helps us, I think, understand really what was Heisenberg practicing. This is the work he himself was pursuing as a graduate student, working with some people we'll talk about for part one-- he and many of his peers. And also, in a sense, what finally made Heisenberg want to make a break with that.

So I think it's worth sitting for this first part of today's class session on work that, again, was rapidly seen as part of the old not the new. Because I do think it helps us understand what was Heisenberg self-consciously trying to do when he did start composing this new work in the spring of 1925. So let's jump in with that.

A quick reminder. I think this was likely familiar for many of you even before our previous class session. But nonetheless, we looked last time at things like the Bohr model of the atom first introduced in 1913. And Bohr had kind of finally in frustration-- he had first wanted to treat very complicated arbitrary electrons with many, many-- excuse me-- arbitrary atoms with many, many electrons. And in the end, finally settled to try to tackle just the simplest kind of atom-- hydrogen atoms with a single electron.

And again, he had this kind of backwards/forwards, one step forward, one step back kind of approach to start with thoroughly familiar expressions from Newtonian's mechanics, from Coulomb's electrostatics-- really what we would now call classical physics. And then he kind of attached these new so-called quantum conditions and then try to see where that would lead him. And that led him, for example, to suggest that the motion of electrons in a hydrogen atom-- the motion of a single electron in a hydrogen atom-- might be restricted to very discrete states of motion.

These radii or the average distances would not take any old value across a continuum, but would snap into place-- 1 squared, 2 squared, 3 squared and so on time some unit length we now call the Bohr radius. And when he did that, he was able then to have an explanation where really none had been in hand before for empirical phenomena like the so-called Balmer series, again, shown here.

This is actual real data from a spectrograph-- not taken by me, taken from Google image. But someone performed an actual more modern investigation of the emission lines, the spectral lines that get emitted from a gas of hydrogen atoms when you add some extra energy. You excite the atoms. They relax by emitting not just light of any old color, but light of very particular colors, especially those to which our eyes are sensitive here in the visible part of the spectrum.

Well, that was really very exciting and got a lot of people's attention. But, of course, the work wasn't over then. Even before Bohr's atom-- model of the atom-- it had been known by the same kinds of experts who were making very precise measurements of the exact color-- the real frequencies of this line and that line and that one-- that all kinds of things can happen to the pattern of light that comes out to the very specific values of the colors of light if the gas of atoms is placed in an external magnetic field.

So what would appear as a single sharp line like this kind of bluish turquoise line, or this very sharp, single red line, when there's no external magnetic field, those would split. They would typically split into triplets of lines. So this is my simulated data using the Paint program in PowerPoint.

Imagine we took this real measurement-- this line here. This is what it would look like in the absence of an external magnetic field. In the presence of an external magnetic field, that single line would split into three very slightly different colors, but very closely spaced. So again, if you look at a kind of spectrogram. They'd be very close to each other and just marginally different in the numerical value of their frequencies when the excited atoms were placed in an external magnetic field.

Many researchers had noticed that. One of the people who was most careful and precise in quantifying this effect was a Dutch physicist named Pieter Zeeman, who became known as the Zeeman effect-- the splitting of single spectral lines into-- usually into triplets in the presence of an external magnetic field.

So that set in motion some work by colleagues of people like Niels Bohr, who wanted to see could they make sense of this empirical information about spectral lines. One of the most significant in this was Arnold Sommerfeld, who was the senior professor, the single ordinarius professor of theoretical physics in Munich in Germany.

And Sommerfeld is interesting for many, many reasons , among them he really took up the kind of mantle of Bohr's model, as we'll see for much of this first part of today's class, and he did some really, I think, very creative kind of conceptual moves we'll look at a little bit.

He's also fascinating, I think, historically because he was a remarkably prolific and productive mentor of younger physicists. So coming out of the Sommerfeld school, his immediate advisees in Munich included many, many people who would go on to win the Nobel Prize, and otherwise, make a lasting impact on our understanding of matter-- people like Vera Heisenberg and Wolfgang Pauli and later Hans Bethe and others.

So in Munich, Sommerfeld set up this really remarkable school to focus on what they began to call atomic structure-- atombau and spektralliniens. So what about these emissions of lines from excited atoms-- can they use that information in it's very fine-grained quantitative detail to try to learn more about the structure of, say, electrons motions in atoms?

And they began, as we'll see, by trying to generalize Bohr's model to what they consider the more general state of motion-- elliptical orbits. And this was the first edition of what became a remarkably influential kind of research monograph-- not exactly a textbook-- but often used to train the younger generation.

OK. So remember we saw last time, for simplicity when Bohr was putting together his first atomic model in 1913, he actually considered circular orbits for a single electron at a time.

So he knew, as all of us would know, that in general, under a central force, whether we're talking about the motion of planets in the solar system or a lonesome electron around the positively charged nucleus of a hydrogen atom, the general state of motion will be ellipses. It will be not a circle necessarily. For simplicity, Bohr started with a circle.

Well, Sommerfeld knew his celestial mechanics as well as anyone. He said the more general state of motion would be ellipses. So again, much like Bohr had done, Sommerfeld and his students go back to the perfectly familiar totally Newtonian kind of classical mechanical expression for the energy of a body of mass, m, in an elliptical orbit-- sorry-- an elliptical orbit, in this case, with a positively charged nucleus as one focus of the orbit.

So whereas Bohr had neglected this middle term, we need to include that term when we describe elliptical motion. This is proportional to the angular momentum. I should say, by the way, one of my favorite books to make sense of this-- and I'm just going to give you a little taste-- is by a friend of mine, Suman Seth, a book came out a couple of years ago called *Crafting the Quantum,* and does a really deep dive into Sommerfeld and his whole school.

I think it's really fascinating. Some of you might enjoy learning more about that. So I'm going to give you the short version of some of the things that we learned from Suman's work.

OK. So whereas, a circular orbit has only one degree of freedom-- things can vary really only by changing the radius-- an elliptical orbit, in general, has two independent degrees of freedom. We could think of them as like radius and one of these angles-- a kind of azimuthal angle.

So Sommerfeld proposed basically take Bohr's prescription and just do that for every independent degree of freedom. He's thinking very formally in terms of the classical mechanics. There are ways of characterizing all the ways that this body and motion can kind of wiggle-- all the independent states that would characterize its motion for each of those, subjected to this new quantum condition. So this is what had been Bohr's original one. This is where Bohr had written down mvr equals nh, which we saw last time.

In Sommerfeld's hands, that just becomes one of potential whole series of quantum conditions. There is now an independent degree of freedom related to this angular motion, and it goes by the angular momentum capital L times this accumulated angle. What if that also could only take integer values set with a scale set by Planck's constant?

So if you go through the same series of exercises as Bohr had done, but now for the more general expression, you arrive at a new expression for the energy, which depends on two of these integers, not only one. It's the same series of steps. It's just Sommerfeld's starting from the more general states of motion. And these integers have to obey certain kind of relationships to each other.

Now, if one follows that through, Sommerfeld continues-- he's doing his work starting in 1915-16, very soon after Bohr's work, and finally brings it together in this monograph in 1919. If one introduces that second more kind of ellipse-specific quantity-- this is really telling us how much the orbital motion deviates from being merely a circle-- subject that to only take on integer units proportional to Planck's constant-- then the projection of that vector quantity along any other direction z will also be discrete. It will be quantized.

So now you have another value that typically became labeled by M-- M sub l-- we still use that notation to this day-- that we have the original Bohr principal quantum number, the one associated with the momentum that's conjugate to the radius.

We have now the second quantum number Sommerfeld introduces related to this kind of ellipse to the deviation from circles. And we have a third integer quantum number given by the projection of that kind of elliptical motion along any particular direction in space we might consider. We'll call that the z-axis.

And so, for example, if you had one unit of deviation from circular motion, if this new quantum number associated with l Were equal to 1 instead of 0, then this projection could only take on one of three possible values-- either minus 1, 0, or plus 1. If you were kind of, so to speak, two units away from a pure circle, then this projection could take one of five values.

The point was for any integer value n sub phi associated with that kind of ellipticity, there would be an odd number of these possible projections-- always an integer, and there will be an odd number of distinct values it could take. Some of this might be familiar to you from chemistry classes. We still use this in spectroscopy to this day. This is really where it starts to come from.

So in place of only one quantum number, as in Bohr's very simple circular model, Sommerfeld and his students began considering three quantum numbers. The Bohr principle one, you might think about a kind of ellipticity one, and the projection, each of which could only take on integer values.

So again, don't worry if that's brand new and seems confusing. I just want to focus briefly on what's the kind of form reasoning? What are the arguments that Sommerfeld's putting forward?

Just like Bohr had been doing, he's going to start with these thoroughly familiar classical descriptions of the objects motions, familiar from things like celestial mechanics. He's not inventing that part new. He's taking it off the shelf.

And then kind of midway through his calculation, he'll just snap on some new so-called quantum condition. He won't derive it. He won't really, so to speak, justify it or prove it.

He's going to make-- he's going to do like Niels Bohr had done-- take classical descriptions of motion and then start adding on these kind of ad hoc constraints to force quantities that might have taken on any one of a continuum of values in the classical mechanics in the Newtonian case. Those will now snap into kind of integer values for a scale set by Planck's constant.

Now, why would anyone do that? Whoever bought this crazy book, *Atombau and Spektrallinien.* Because Sommerfeld began giving-- showing some real payoffs for that. If one follows this seemingly, again, kind of complicated almost Baroque series of steps, then Sommerfeld could come back to the quantity he started with, the spectral lines, including things like the splitting of the Zeeman effect when these excited atoms-- gases were put into external magnetic fields.

So again, he begins reasoning, begins with thoroughly familiar expressions from electromagnetism, even pre-Maxwell, going back to the days of Coulomb and Ampere and others. He goes to the early 19th century and then starts kind of bringing them into this new framework.

So one of the first things that Sommerfeld and his students do with this new kind of generalized framework of the Bohr-Sommerfeld approach is to reason that any object with electric charge q-- any little body with some electric charge that's moving with some angular momentum in some orbital motion will have something called a magnetic moment. Again, that was something that the French scholars of electricity had worked out in the very early 1800s. This part was not new by the early 1900s. That's a classical effect.

And likewise, had been worked out often in what we now call classical electrostatics is that in an external magnetic field, such a moving electric charge, the energy of that system will actually depend on the relative orientation between this vector quantity, the magnetic moment, which is proportional to this vector related to just angular momentum, the orientation between that vector and the vector-- excuse me-- the orientation of this vector field-- the external magnetic field.

So in fact, the energy of a system will be lowered when the magnetic moment lines up perfectly in parallel with the external magnetic field. Then this quantity together would be maximally positive.

There's an overall minus sign in front. You would reduce the energy of the system by the largest amount. The system will be most stable when the quantity called the magnetic moment lines up perfectly in line in parallel with the applied external magnetic field.

Well, now Sommerfeld says, if that's true for any kind of objects even in the macroscopic or classical world, what if we bring those ideas now and mix them together with this Bohr-Sommerfeld notion of quantized states of motion for an electron in a hydrogen atom.

So one now had quantized this state of motion, the angular momentum akin to the kind of ellipticity-- So if this thing can only take certain integer values proportional to Planck's constant, then this thing, the magnetic moment, will likewise now be quantized when we think about atomic systems or parts of atoms.

So now, let's do the same trick. The energy levels of an electron as it whips around its nucleus, if that entire system is now placed in some external magnetic field, the energy associated with that electron's motion will now be split.

In fact, it will be split into exactly M sub l distinct levels. Remember, the projection of this thing along some orientation space will also only take some integer values given by that third quantum number that Sommerfeld had introduced.

So now, these Zeeman triplets, the splitting of what had otherwise been a single frequency of emission into three closely spaced lines, Sommerfeld and his student said, here's what must be happening. It must be that there was an electron in an elliptical state of motion-- we'll say one unit of this kind of elliptical motion, one quantum unit associated with the quantity l. Therefore, the projection along any direction in space could either be exactly anti-parallel to it, perpendicular, or exactly parallel with it.

There's only three options when we project this discrete quantity onto a given direction in space. And so what must be happening is there's an electron with one unit, and therefore, three projections, that's dropping down to a state where this ellipticity vanishes and drops down to a circular orbit again.

And so you had three different energies for that starting point before the electron jumped down to the lower energy state. That would correspond to three slightly different amounts of energy released in the form of the radiation. The spectral lines and therefore their colors, their frequencies, would be slightly different from each other because you had an electron in a more general state of motion than Bohr had considered. That's the idea. That's why people start paying attention to any of this kind of complexity.

Sommerfeld starts, again, scoring new wins empirically by trying to do the same kind of trick that Bohr had done-- thoroughly classical expressions, kind of snap on some new quantum conditions, and then reason about some empirical regularities.

So now, this approach addressed what had already by this point become known as the ordinary Zeeman effect, the ordinary splitting, which was into triplets. There was also by this time, by the later 19-teens evidence of what became known as the anomalous Zeeman effect. So there was the ordinary and anomalous.

For the anomalous Zeeman effect, it was not a splitting into triplets, it was a splitting into doublets. So again, with my simulated data, take that, say, that single purple line of the Balmer spectrum, in an external field sometimes these single lines will actually split not into three closely spaced lines, but is splitting into only two.

And this now could not be explained by Sommerfeld's previous approach because remember, as you may-- I went by it quickly. You can check on the slides. Sommerfeld's early argument would only work for an odd number of splittings because the projections always went by 2n plus 1. There would always be an odd number of splittings. And therefore, an odd number of slightly different energy levels.

So you could account for triplets or even five part splittings. You could never use that argument, as they recognized, to account for doublets. How do you get an even number of splittings-- split lines from that argument? You can't.

So that became like the next big challenge for Sommerfeld and his most ambitious young students, one of whom was Wolfgang Pauli, a contemporary and a very close friend of Werner Heisenberg. They were students together in Sommerfeld's school in Munich.

So Pauli finished his PhD. He took up a postdoc with Niels Bohr in Copenhagen. And this challenge was very much on his mind and many people's minds. And I love this story that Pauli recalled many years later. I'll just quote it for you.

He says, a colleague who met me strolling rather aimlessly in the beautiful streets of Copenhagen, as shown here, said to me in a friendly manner, you look very unhappy. Whereupon I answered fiercely, how can one look happy when he's thinking about the anomalous Zeeman effect?

With all the hard things on our minds, I'm sure we can recognize his dire frustration. Maybe we have worse things on our minds. The point is this really was just terribly frustrating, and many people were really concerned about the anomalous Zeeman effect. How could you explain doublets?

As I mentioned, it was not only Sommerfeld and his very bright students like Pauli who worried about that. Many folks certainly across Central Europe who were in touch with each other, this was on many of their minds.

So another set of very young students, still PhD students at the time, were also thinking hard about these anomalous effects, these doublet splittings for excited gases of atoms in an external magnetic field. So these now included students working very closely with Hendrik Lorentz. We know some of Lorentz's work. He was by now a very, very senior professor in Leiden in the Netherlands, and two young PhD students, George Uhlenbeck and Sam Goudsmit. They were working also to try to figure out this strange kind of anomaly.

So they began reasoning kind of similarly to how Sommerfeld had been doing. They knew his book. His monograph had been out for years by now. This looked like the path forward. They realized that in analogy with the motion of planets around the sun, the Earth has two different kinds of angular momentum.

It moves around the sun in its yearly orbit. That would be like the elliptical motion that Sommerfeld was building into his new kind of complicated models. But the Earth also spins on its own axis, as of course, we all know. That's what turns the day into night.

So you have the orbital motion that accounts for the changing seasons over the course of the year in the analogy for the Earth. There's also the different spin, a different kind of angular momentum, of the Earth spinning on its own axis to turn day to night.

So they began-- the younger students, Uhlenbeck and Goudsmit, realized-- reasoned that what if the electron also had some second kind of ingredient of angular momentum? What if it had some intrinsic spin along its own kind of axis separate from this kind of elliptical motion that Sommerfeld had focused on?

And then they do the same trick that everyone was doing. If that second kind of intrinsic spin or angular momentum is associated with an electron, and if we snap the value that that spin could take into discrete values with a scale set by Planck's constant, then the electron would have an additional magnetic moment. In an external magnetic field, it would have a new vector quantity proportional to this new spin vector with a magnitude fixed by Planck's constant.

So now they go back and, again, follow Sommerfeld's reasoning very, very closely. Place that entire assembly in an external magnetic field, there will again even just classically, one would expect, a shift to the energy of the entire system when the spinning charged object is placed in an external field.

The energy associated with the magnetic field on the system will be minimized when the magnetic moment of the spin vector lines up perfectly in parallel with the external field. And now, Uhlenbeck and Goudsmit reasoned, that if this spin, if the intrinsic angular momentum could only ever line up exactly parallel or exactly anti-parallel, opposite direction, to that external field, then you would expect doublets.

The same kind of argument that Sommerfeld and his students had made could now be applied to a two-valued offset in the energy instead of a three or an odd numbered-offset. And, in fact, they showed very compellingly that if you really do set the value of this magnitude of that intrinsic spin to be Planck's constant h divided by 4 pi, or as we saw last time, h bar, the common abbreviation, h divided by 2 pi itself divided by 2, then you would actually quantitatively match the actual values of the splittings of these frequencies.

They didn't call this spin, interestingly. They call this space quantization, which tells you a bit of where their thinking was coming from. The idea was that this spin vector, this new vector S, this axis around which the electron was imagined to revolve, could only point in discrete directions in space, that it would have to snap into position either exactly parallel or exactly antiparallel to this external field. So the orientations in space along which that vector could point were quantized. They called the space quantization.

And as I just mentioned, by fixing the magnitude they actually, again, could really get a remarkably close match to these very finely carefully measured optical results from the latest spectroscopy. But it wasn't so clear cut. And again, they recognized this themselves, to their credit.

With that magnitude, by putting in this much, this amount of the kind of intrinsic spin, intrinsic angular momentum for the electron its own imagined little axis, then it started to sound pretty outlandish. A point on the electrons equator-- and they had ideas coming actually from Lorentz himself, their main advisor, about the kind typical size they would expect the electron to be-- they had a sense of a radius based on its charge distribution and all that, that if you actually-- if that little ball of a given size was spinning that quickly around its own axis, then a point on the equator would actually be spinning faster than the speed of light.

By 1922 and '23 and '24, that seemed like a no-no. By that point, relativity was very widely accepted. Moreover, if you have something spinning that quickly, it looked like the mass of the whole electron would diverge. So they get some remarkable kind of clarity in terms of this very frustrating doublet of the anomalous Zeeman effect. But it starts to get harder and harder to make it all fit together in a single kind of visualizable model.

And so by 1924 when these young students were pursuing that, it actually became kind of impossible at the same time to assume this is really a genuine angular momentum along some axis of rotation of a physically spinning tiny little object, and match both the empirical values they were aiming for, and be consistent with things like kinematics, like the motion of an object with mass. It became impossible to visualize.

So they themselves worked that out. They talked about it with their main PhD advisor, a very senior Hendrik Lorentz. Lorentz said, actually, you know what? Let's not publish that yet. Don't publish that because it did seem to have such absurd conclusions or such unvisualizable conclusions.

They had another advisor, much younger and much more brash, named Paul Ehrenfest also in Leiden. Behind their back, Ehrenfest actually submitted their paper to the journal without telling them, which is remarkable. And he supposedly told them, you're still young enough to afford a stupidity. And I have my little joke here that that's completely unethical. We would never do that today.

Today, the advisor would steal their student's own work, put the advisor's name on it, and then submit it to a journal behind their back. That's a joke. We wouldn't do that. I just find this whole scenario quite amazing, that Ehrenfest was like, oh, yeah. I already sent that in. And they were like no, no. Lorentz told us not to.

OK. Last little bit, and then we'll pause for some discussion for this first part. This is the longest part for today.

So independent of these students in Leiden, independent of Goudsmit and Uhlenbeck, Wolfgang Pauli, that young graduate from Sommerfeld school, he was still wracking his brain against this anomalous Zeeman effect as well. He came up with what to him seemed like a really totally separate explanation.

In time, people, including Pauli, began to put it together with this work which did see publication behind their backs by Goudsmit and Uhlenbeck. So this came in really just weeks after Goudsmit and Uhlenbeck's own work had been submitted quite independently.

So Pauli was well-trained in the Sommerfeld school. And by this point, Pauli was trying to abstract from these very mechanical kind of visualizable models of the solar system and was trying to think about a kind of algebraic approach. A kind of what are the numbers with which we can associate abstract states of motion?

And one of the lessons that Pauli took from Sommerfeld's work was that they had to add more and more of these integer values, some new quantum numbers n, almost always proportional to Planck's constant with which to characterize states of motion of, say, an electron in an atom.

Pauli, unlike his advisor Sommerfeld, was less and less concerned about coming up with an actual physical model of is it just like the Earth around the sun? He was just more like we have all these relationships involving quantum conditions, new integers, scale set by Planck's constant.

And so he argued, very abstractly, not based on a kind of visual model, that if one included yet another quantum number, this fourth quantum number, that had what he called a classically indescribable or unvisualizable double valuedness-- it was an [GERMAN]. It was literally two values, like two-faced. If this new quantity could only take on one of two values, then one could account for the Zeeman effect for the rest of the reasons very much like what Goudsmit and Uhlenbeck had argued.

Pauli was not talking-- thinking at first about a spin or angular momentum. He just said maybe there's another abstract property of this state of motion. We lock it in place. We quantize it with a new integer. This one can take one of two values. Whereas, the others could take different sets of values.

This became known as the Pauli exclusion principle, which is a notion that, of course, we still take very seriously today. But what Pauli was arguing was that electrons in an atom should be described by a total of four distinct quantum numbers, as we still do to this day-- the original principal quantum number-- that was when Bohr introduced; the kind of elliptical part that Sommerfeld introduced; the projection part that Sommerfeld introduced of the orbital motions; and then this new one that we would now associate with spin.

Pauli at first thought Uhlenbeck and Goudsmit's idea was ridiculous because of these visualizable things. He later realized, oh, maybe these really are relatable. And so the fourth quantum number becomes now associated with the spin.

The next part that Pauli put forward was not just that we characterize an electron with these four separate integers, but that no two electrons can have the exact same set of those quantum numbers at the same time. That's the exclusion principle.

He later recalled that he was inspired by this kind of no two can have the same set of values by watching can-can dancers, which was all the rage in the 1920s in many cities in Europe-- famously associated with, say, the Moulin Rouge in Paris, but in Copenhagen as well.

And the idea was that these dancers were so skilled that one would have just left a little physical location on the dance floor immediately when the next-- when her partner had to step into that same spot. They never collided.

They were close to each other. Their motions were similar, but no two of those dancers, much like no two electrons, occupied exactly the same state at the same time. So that becomes known as the exclusion principle. So let pause there, and we can take some questions and discussion.

So DA asks, doesn't a circular orbit have angular momentum? It certainly does. And so the idea, DA, is that in the elliptical orbit there are two separate degrees of freedom, is the fancy way of saying it, the two different ways with which to characterize the orbital motion. Whereas, in a perfectly-- a uniform, circular orbit, there's only one quantity that is required to characterize-- to completely characterize the motion.

And so that's why I think of this second one Sommerfeld introduces as more like the-- it's sort of like the ellipticity. It's not exactly that, but it's related to that. If the orbital motion is deformed away from a perfect circle, then you'll need at least two numbers to characterize that motion completely, not just one.

So to DA's excellent point, the electron moving in a perfect circle certainly has angular momentum, but you can characterize that completely with only one number, one parameter. Once there is a generalization, you need at least two. So that's where Sommerfeld is going with that.

And Jade also is correct. And in Jade's response, you put that whole assembly in an external magnetic field, and there would be no mechanism without accounting for these other states of motion to account for these what became known as fine structure splitting. That's right.

Gary says, Heisenberg, de Broglie, and Einstein most noted work happened when still young scientists. That's right. So Gary, you'll be-- as an economist, as we've got to know-- economists have actually studied this quite a bit. And there are a number of studies-- I can send you some references afterwards-- suggesting-- although other counterstudies that muddy the waters.

Some early studies suggest that in some fields of study there a kind of advantage to not knowing too much. That's how the economists had characterized it. So not having to feel like you have to answer every last thing you've tried to explain your whole career.

Their explanation was a kind of conceptual freedom. That's how they tried to make sense of a modest statistical signal that at least in some fields, including quantum physics, is one of the areas that I know of from one of these studies, that there was a kind of early entrance kind of bonus, we might say-- early starts.

Other data or studies have muddied that relationship a bit. But certainly, there are a bunch of economists in particular who are interested in life cycle and innovation is how they would describe it today. It's an excellent question.

Lucas asked about space quantization-- very different from how we talk about spin now. It certainly is. We also have modern program of trying to quantize space time. Good.

No. That's a-- so Lucas, it's a great question. And that's, again, we know too much, or at least we're exposed to too much, that was-- it is-- it's really funny vocabulary. I wanted to harp or emphasize Goudsmit and Uhlenbeck's first idea. Because they really agreed this was kind of nutty sounding.

It was how could a three vector only point in one of two orientations. It was as if it was-- as if its orientations in space were quantized as opposed to space [INAUDIBLE]. They agreed that the x, y, and z in which we live would be continuous and have the kind of classical or relativistic behavior. So they weren't trying to challenge that. And the notion of trying to quantize space time, that begins to kick in by the late '20s, early '30s, but not quite-- that's not what these folks were thinking about just yet.

Alex asks a question about conventions. Why is it m sub l and not n sub l? Ah, very good. I don't know. I have a suspicion.

This third quantum number is, again, some of you might know from chemistry classes as well as from physics ones, it's nowadays often called a magnetic quantum number because it really is about the orientation of the orbital motion with respect to an external magnetic field.

So it's now called the magnetic quantum number. And even in German, it would be [GERMAN]. So it wouldn't surprise me if it gets renamed as an m later. That's a good question.

I don't actually own a copy of Sommerfeld's first edition. It's a very expensive book now. It would be interesting to go back. It's probably on Google Books. It's probably been scanned to see back in 1919 were people using N sub l or M sub l. I don't know.

I suspect it gets-- if it wasn't put in that way from the start, I imagine why we use an M these days is because it is, as I say, called the magnetic quantum number. It's an interesting question. Other questions or thoughts about any of that stuff?

OK. Keep the questions coming if you want. Let me press on now and see how are some very smart, young, and ambitious people reacting to that body of work, in particular, one of Pauli's closest friends, Werner Heisenberg.

So Heisenberg and Pauli were almost exact contemporaries who are almost exactly the same age. They both finished their PhDs around the age of 22-- the PhD-- not their undergraduate degree. They're both very precocious young students of Sommerfeld.

And by this time, Heisenberg, we know from his letters with people like Wolfgang Pauli-- many, many of the letters have survived-- he was also growing frustrated with what had become the pattern of this so-called old quantum theory, even from their own otherwise very beloved mentor, Sommerfeld-- this idea of starting with classical descriptions of motion and then kind of tacking on what seemed like ad hoc quantum conditions.

So by 1924 at the tender age of 22 or 23, Heisenberg then began his first postdoc position. He began working even more closely with Niels Bohr in Copenhagen. And that's where he introduces this the paper that we had for part of our reading today. It was submitted-- a few months later, he moved to Gottingen. But he really did a lot of this work really fresh out of his PhD.

So he now starts to set out a kind of program. The opening paragraph of this paper reads like a manifesto. We have to do something new, is how I read this very young author in that opening paragraph. What he's arguing for is a first principles treatment of the quantum realm, rather than what felt even then as a kind of kluge, or a grab bag, or a series of ad hoc rules of thumb.

So he wanted to break this impasse of the kind of Bohr-Sommerfeld approach. And in fact, he drew very clear inspiration from Einstein in 1905. And again, we know that partly from his letters at the time, as well as his later recollections.

He thought that much as Einstein had made a big deal, as we saw a few classes ago, about the mock-like positivism-- let's only focus on objects of positive experience, otherwise we'll get ourselves tied up in knots-- Heisenberg says we have to do the same thing all over again now for understanding the behavior of atoms and parts of atoms.

And so in particular, Heisenberg says, again, in his opening paragraph, he says, we can never observe an electron in its orbit within an atom. So why are we trying to calculate properties of that motion that we could never observe in the first place? And here, again, as you've seen in the reader, he says it seems sensible to discard all hope of observing hitherto unobservable quantities, by which he means things like properties of an electrons orbit-- Keplerian or circular or otherwise.

Instead, it seems more reasonable, he goes on, to try to establish a theoretical quantum mechanics-- his terms-- a theoretical quantum mechanics-- analogous to classical mechanics, but in which only relations between observable quantities appear. And that's really-- he has a kind of echo in his head of the once brash, young Albert Einstein and Mach.

Previous approaches could be seriously criticized on the grounds that they contain as basic elements relationships between quantities that are apparently unobservable in principle-- it's just like textbook Ernest Mach here-- such as position and period of revolution of the electron.

Supposedly, Heisenberg tells us in later recollections, he shared his excitement about this move with Einstein around this time, and around 1924 or '25. I was so inspired by your work, Einstein, that kind of thing. Einstein, by this point, was quite senior and very renowned.

And supposedly Einstein's retort was a good joke shouldn't be repeated too often. As we'll see, Einstein was less convinced this is the way to go by the 1920s, even though it had been so critical to his own thinking in the early parts of around 1905.

OK. So what does Heisenberg do with all this? It's one thing to have an opening paragraph about a kind of guiding philosophy. What does he do? So as we saw, he argues that physicists should focus on quantities that are in principle observable or empirical.

And what he has in mind as he goes through in the rest of this brief paper are things like the frequencies, the numerical values of the colors, the frequencies of these spectral lines. How do we learn about the structure of atoms by measuring the stuff that comes out-- things that we can literally observe and very carefully measure, like the frequencies of spectral lines.

And in particular, he goes back to say that these spectral lines obey a law of addition. Remember, we saw last time even in the 1880s, it had been clear that there were these kind of empirical relationships between the frequencies of the color lines-- really, like how many hertz, how many cycles per second, is this blue line? And then these combinations of inverse squares of integers. That was the Balmer relationship, and Bohr gave a kind of explanation for it with his atomic model.

Well, if you start from that, you realize that the sum of the frequencies of two different spectral lines associated with transitions between different states of an electron inside, say, the hydrogen atom, they will sum. So the sum of any two of these frequencies that actually appear will give you-- add them together, you'll get a frequency that will also appear somewhere in the spectrum.

And so that would be like-- and by the way, and you can keep going. This is not true only for adding any two of them. You could keep going for an arbitrary number of the actual numerical values of these frequencies of the spectral lines. And for all the colors that actually come out and can be measured, even beyond-- pardon me-- just the visible portion of the spectrum into the infrared, into the ultraviolet, they obey this law of addition.

And so what that would be is like an electron could jump from say a principal quantum number of 6 to 1 all at once, that will give a certain color of the corresponding emission. Or it could make a series of hops. It could jump from the principal quantum number 6 down to 3, 3 to 1, or 6 to 3, 3 to 2, 2 to 1, or 6 to 5 and 5, et cetera.

Each of those jumps or transitions corresponds to an emission of light of a very specific frequency given by this relationship. And if you add up the frequencies of any of these jumps, you'd see they would all-- they could be arranged in this format.

So Heisenberg began saying, we should be focusing on that stuff. These are our empirical objects of positive experience that we can see. We can measure. We can gain quantitative information about the frequencies of these spectral lines. So he begins trying to construct the kind of arrays of these lines that kind go together through this law of addition.

In the midst of that work-- by now, this takes him from the fall into the spring of that academic year, spring of 1925-- Heisenberg actually has a very famously-- an unusually bad attack of hay fever. He had allergies all his life. He has a particularly bad attack of hay fever. So he actually had to leave Copenhagen.

He travels to the nearby island of Heligoland, which is actually in the North Sea between Denmark and Germany, as I understand it. Over the years, both countries have claimed it. So the point is it's pretty close to where he was in Copenhagen, but gets out of town.

And he's actually then basically on his own in this tiny little kind of touristy island for several weeks still trying to puzzle through this stuff. So he's even more isolated than he had been just as a postdoc. He literally leaves town.

It's during that short stay that he continues trying to sit with this notion of the behavior of these frequencies of emitted spectral lines. Then he goes on to think now on his own these frequencies new refer to their characterizing the light that comes out. And he knows, again, even classically the way we would characterize electromagnetic waves, waves of light, would be some amplitude and some frequency, some phase, the phase being proportional to the color we see.

The point is that the quantities he's now trying to build up in these arrays, these kind of new 1, 2, new 3, 4 and so on, they appear in the exponent when you actually try to mathematically describe the light that comes out.

So then he goes on to just reason on the island. If these frequencies up here are adding, if the exponents are adding, that suggests that these amplitudes should multiply. Just think about even in elementary multiplication, if you have two numbers that we write in kind of scientific notation that have a coefficient and an exponent, if we multiply those two numbers together the exponents add. That's what he saw the frequencies doing. And that goes along with the coefficients of the amplitudes multiplied.

So he says, well, maybe that's what goes on with these spectral lines. All the information we can glean about these spectral lines, the amplitude would be related to the intensity of the brightness. And so let's see if we can make similar arrays with more information we can glean of the structure of these atoms from the empirical information.

So taking this analogy or this mathematical relationship, if these things add, these things should multiply. And that's what he finds all alone on Heligoland while suffering from hay fever, that somehow these arrays of the amplitudes start looking pretty funny to him.

It turns out the order of multiplication changes the result, that if he just tries to multiply them, the order in which he multiplies them changes the answer. Whereas, of course, that doesn't happen if you add these things up. You can add them up, and the sum would not change.

He writes up this brief paper. He submits it once he then actually moves to his new position in Gottingen back in Germany. And now, he's working not with Bohr, but most closely with the mathematical physicist, Max Born.

So Born's more senior. He's more like Albert Einstein's age. Born, like Sommerfeld, was very, very well trained in classical mechanics, celestial mechanics, other mathematical techniques. And Born's reaction when this excited young Heisenberg arrives is basically saying, you're really an idiot. You're dummkopf, is what I imagine Born saying. You're studying matrices.

The point is for the very-- even the very elite training of someone like Heisenberg and Pauli at this leading school of theoretical physics in Munich, throughout his entire training he'd never taken a course in what we would now call linear algebra, much like Einstein hadn't learned about non-Euclidean geometry, so on.

So Born, now Max Born, had to tell and/or remind young Heisenberg that the notion that two arrays of numbers, when you multiply them, you get different answers depending on the order. That's what you should generically expect when you're handling matrices.

So I've just taken two kind of random matrices just to remind ourselves 2 by 2 matrices, two arrays of numbers-- any random numbers would do, and any random components. Let's first multiply this one times that one in that order. And as I'm sure you all have learned how to do the matrix multiplication, this element here will result from the sum of this times this plus this times that. You know how to multiply matrices, I'm Sure

So we can go through the exercise and multiply this matrix times that in that order. We'll get a new resultant matrix. It will also be a square matrix, in this case, 2 by 2.

If we had multiplied these in the opposite order, if we multiplied B times A-- the exact same matrices multiply in the opposite order, again as all or most of you have likely seen by now, we get a different answer-- a perfectly legitimate answer. It's still a square matrix. But the actual matrix is quite different. The entries are quite different.

There's no 6 over here. There's no 1 over here and so on. A times B does not equal B times A in general when you're multiplying matrices. Heisenberg didn't know that in 1925. And so Max Born, his newest kind of mentor and colleague in Gottingen had to explain to him what most of us now learn very early in our training. Most of us learn that because of Heisenberg's work.

We now have many reasons to want to be able to manipulate matrices in all kinds of investigations. So matrices-- the fancy way to say this is that matrices do not commute. That just means that the outcome of an operation like multiplication depends on the ordering.

So to get-- that's, again, a bit abstract. And if you have not spent a lot of time with that, that's OK, of course. I find the following example pretty helpful. I always mess this up when I try to do this in person. So this is the one benefit of doing this over Zoom is I have this picture to guide me.

One kind of matrix we might consider would be the rotation matrix in three dimensions. We saw rotation matrices when we talked about Minkowski and special relativity. Now, imagine we want to have-- describe rotations in three dimensions of space-- x, y, and z.

So imagine I have a book-- this little rectangle here. In class, I would do holding a book. And let's say I want to first rotate this book in the yz plane. I'm going to rotate it down first. Then separately, my next operation is to rotate that book in the xy plane.

So I'm going to rotate it down by 90 degrees and then over by 90 degrees, and my result of applying those two matrices-- the matrix corresponding to rotation only in the yz plane by 90 degrees, then applying a separate matrix only rotation in the xy plane by 90 degrees. Here's the result. So now, the book is pointing in a certain orientation different from where it started.

Let me apply the exact same two rotation matrices but in the opposite order. So let me start with the book in the same starting position. Now, I'll rotate in the xy plane first by 90 degrees and then in the yz plane by 90 degrees. You see the orientation is different.

This is, for me at least, I find that helpful, especially when I don't mess it up in person, as a reminder that the outcome or the product of the multiplication of matrices depends on their order. It really can be as simple as that thinking about moving things around in an ordinary three-dimensional space, let alone any abstract matrices we might be manipulating.

So this becomes known, therefore, as matrix mechanics. The work that Heisenberg introduces in 1925, and soon he works very closely with Max Born and another colleague in Gottingen, Pascual Jordan, they develop an entire first principles mechanics of the quantum realm-- a quantum mechanics built around these matrices. So this first version is called matrix mechanics dating from 1925 originally. Any questions on that?

I still find it just stunning that this super hotshot young kid, Werner Heisenberg, didn't know what matrices were and hadn't realized that you could actually multiply arrays of numbers and worry about the order-- have to worry about the order.

Any questions about the framework or the motivation? We can see, again, he's clearly trying to do the kind of young Einstein, Ernst Mach kind of thing. That's what sets him down that path. And he's literally removed from the conventions. He has to move to the island of Heligoland for a short break. I find that pretty interesting.

No questions? It's all perfectly obvious. All of you would have done that? I wouldn't have done that. Jesus asks, did Einstein have any particular reasons for thinking Heisenberg should be careful of positive-- oh, thank you, Jesus. Great question. Really good.

And I think the answer is-- well, the answer is yes. I think Einstein did have reasons. And it really tells us, so to speak, more about Einstein than about the value of that kind of argument. By 1925, Einstein's thinking was really quite different than it had been in 1905, and a lot of it had to do with his experience in between with what became known as general relativity.

So we saw that in 1905, Einstein was very concerned with this kind of Machian positivism with what we might call operationalism. Tell me exactly how I measure the time-- I have my little hand on my watch-- all that kind of spelling things out empirically and kind of operationally.

And he gets more and more kind of separated from that, or drifts away from that, let's say, as he begins working more and more on general relativity, where he gets more and more involved with pretty fancy, but also pretty abstract mathematics. He's thinking eventually not only about what would an observer see sealed up in an elevator or a spaceship, but actually about global properties of an entire universe that might be warping and so on.

So by 1915, especially by 1917 and later, meaning before Heisenberg's work, Einstein has moved to thinking about things like cosmology, entire universes, which certainly not all of which could be subject to the kind of Mach-like empiricism that had driven his 1905 work.

So he becomes more driven by both a kind of beauty and power of mathematical reasoning, which had really not been clearly a driver for him in 1905, and also, just moving away from the kind of litmus test of, is it measurable? Can I smell it or taste it?

So I think Einstein's comment-- if it ever happened. We learned that only through Heisenberg's much later recollections. It might be an apocryphal story. But it sort of adds up. Even if it didn't really happen, it's plausible because of Einstein's own kind of shift in his intellectual approaches.

So I take that to be more a comment about Einstein than about is Machian positivism good or bad for science? I think we can still debate that on separate terms. I think it tells us about Einstein's thinking.

I agree with Alex. It's hard to believe a time when leading scholars, let alone anyone at all, would never have heard about matrices. I agree. I think that's part of what's so kind of-- I don't know-- kind of touching about this story.

Heisenberg was both like this brash kid, saying everyone-- all my teachers are wrong. That's not unique in the history of science. Young people often say that. But also, in some sense, kind of just so naive.

So like, I'm going to set off on my own, literally leave town. Everything that's been done [INAUDIBLE] for these 22 years that I've been on the planet-- he's really young-- that's all just a dead end.

And yet, also just be like, how are you going to do your next steps there, buddy? Have you heard about matrices? It's a remarkable kind of moment to me. And so I just find that story really compelling.

Fisher asks, is there is anything in this class has taught me that you can never [INAUDIBLE]. Fair enough. So the course 18 rolls will now swell from this class. Taking more and more math is probably good for one's soul anyway, and certainly can be helpful in unexpected ways.

By the way, we'll see this kind of thing happen up happen even later this term. Well after the middle of the term, we'll see examples from later in the 20th century, where I think the same kind of thing happens, where some physicists kind of accidentally or for other reasons find that they get really interested in ways of classifying large groups of objects-- what's like with like-- without having studied things like group theory, which was by then a very, very well-established branch of pure mathematics, lead groups and all the rest. So there's a not uncommon kind of pattern that's not only limited to Einstein or Heisenberg or these other folks.

It feels like the flip scenario of the TRIPOS, where those students could do so much math, but didn't typically learn. OK. Yeah, the tables [INAUDIBLE]. That's right. That's a good example. So Heisenberg clearly was not a Cambridge student. I think it's an excellent thing to keep in mind.

And that's not to make fun of Heisenberg. He was very well-trained. He was trained at one of Central Europe's finest academies for training young scientists. He and so many of his cohort would went on to-- would go on to earn extraordinary recognition in the field. It was just a different kind of training-- different choices about what to prioritize even for very young students.

I find that fascinating, the kind of different options that are being prioritized at different times. I think that's one lesson we can learn. We can step back and take in a kind broader view, a sample across a few different institutions.

So it seems like physicists are getting younger too. He was in the Swiss Polytech as a teenager, where the Wranglers were into their 20s. Ah. So I'm not sure that's quite right, Fisher. So I think the idea that there is a lot of people will just never leave Cambridge. It's still true, actually.

So the actual people competing to be Wranglers were basically undergraduates and usually on what was a kind of canonical age for that. But then a bunch of them would stay on in town. The Wranglers would stay, sometimes get fellowships to then do almost like a research fellowship, which sometimes would be like grad school and sometimes wouldn't.

There's a whole tradition of never bothering to get a PhD from Cambridge. They would be called Mr. They were at this point exclusively-- almost exclusively-- men. They'd be called Mr. so-and-so, even though they were on they were paid to do research full time. Or they would kind of tutor TRIPOS students while pursuing their own later degrees.

So the Wranglers-- many of them would hang around longer. And they'd have the Wrangler title for life. But the actual-- they had to sit for their exam in the Senate house when they were approximately 21 or 22, typically. So Heisenberg had skipped grades, in that sense. He was finishing his brief PhD around the age of 22. Yeah, anyway.

So some of you might know-- we'll come soon actually in coming-- well, not so soon. A couple of weeks we'll come to the work of the preeminent physicist Freeman Dyson, who actually just passed away this past spring in his 90s. Dyson was one of these British guys who just never got a PhD.

He was hired as a full professor at Cornell at like the age of 26, and he was Mr. Dyson. So I mean, that was a very kind typical British-- like, almost like in your face. Like, I'm so good I don't even need that stupid grad school thing. That's a separate kind of tradition we can talk about when we come back to Freeman Dyson in a few weeks.

OK. Let me go. And for the last part for today, unless there are any more questions on Heisenberg and matrix mechanics. And again, it probably is already obvious to you, but just to say if parts of the kind of maneuvers that Heisenberg does in that paper that is on the reading for today, if it's hard to follow all the ins and outs of that paper, first of all, you're in good company. It's hard even for today's physicists because it's just not how we do things today. So it looks weird even to the pros, so that's OK.

And for our purposes, I think what's most fascinating is really just the first opening steps, this kind of manifesto, the kind of Mach-like manifesto. And then just this concentration on frequencies-- obey this empirical relation, frequencies appear in an exponent. Let me follow that down.

If that much is clear, that's what I hope we'll get out of the paper even though he goes into a little bit more detail and gets pretty-- a little more complicated after page two. Don't worry about that if it's hard to follow.

Jesus says, it's cool how in the late 19th century in Britain, a lot of leading physics was based on complicated mathematical models. Whereas, Einstein and Heisenberg, who seemed to have cared less about math than the Wranglers-- certainly at that stage-- focus on unintuitive non-mathematical ideas as the basis for their work.

I think that's true. Or at least there's a kind of spectrum. I think then, as now, there's a range of ways that have proven to be productive in trying to learn about nature. And isn't that a good thing?

And now, I think we hopefully have learned that lesson even more so, if I may get on a soapbox for a second. Shouldn't we be working even harder to get even more perspectives and life experiences and people in the mix?

Because there's not one way to do this right. And so shouldn't we work even harder than frankly we've been able to today-- than we have to date-- to increase the type-- the numbers of people and the types of people from whence they come with the whole grab bag of experience.

Just for this lesson, even we only look at very narrowly demographically a range of ways to succeed, doesn't that lesson suggest even broader [INAUDIBLE]? Hopefully, we'll keep that in mind not just for this term but beyond.

But it's a great point. I agree, that there was more than one way to do pretty interesting and original work circa 1910, 1920. And that's a lesson, I think, again, we'll see throughout this term. OK. Good.

Let me go through this last part because now we want to see what does Heisenberg do once he knows that there are things called matrices in the world? He keeps thinking about this, of course. He's not done.

And so with Max Born's help, he's clarified some of the basic mathematics, what he had kind of stumbled into thinking about were these arrays of numbers, like matrices. He then had learned kind of bumblingly on his own and with more formal help from Born in Gottingen that the outcome of transformations depends on the order of operations. These matrices don't commute. Now, Heisenberg, again, is still trying to make sense of that kind of physically or conceptually. It still doesn't kind of sit with him all at once.

So a few years later now in the spring of 1927, just two years after his trip to Heligoland, he returns to Born's Institute in Copenhagen for another visit-- a basically, kind of longer-- another postdoctoral visit. And now he's trying-- he's still kind of trying to make sense of what this noncommuting matrix stuff might mean physically or conceptually.

And he returns to, again, a kind of simple thought experiment, one that he can kind of picture or visualize in his mind. It comes to be known as the gamma ray microscope. And he thinks about it really in these kind of cartoon-like forms. So my cartoon here is, I think, at the level that I think Heisenberg was beginning to grapple with is when he comes back to Copenhagen spring, summer 1927.

So let's imagine we're trying to identify or measure the location of an electron. Obviously, electrons are small. We can't just look at them. We have to use something like a microscope. How do we see any small things or characterize their positions? We use a microscope.

So electrons are really small. And so we have to use light of a particularly small wavelength, which is to say, a very high frequency. And so we'd have to use something like gamma rays-- rays of light, little beams of light, that have particularly small wavelengths on the-- comparable to the size of the thing we're actually trying to measure.

So how does a microscope work in general? We have some object we're trying to resolve-- we're trying to make an image of to measure its location. We shine light on it and collect the scattered light in our microscope. That's what we do, sort of generally.

So if we want to measure the position of this really tiny thing like an electron, we'll do that. We'll bounce light off of it. We'll collect some of that scattered light in our device.

The electron's very small. So we have to be a little sensible about the kind of light we'll use. We'll have to use light of a wavelength that is at least as small as the object we're trying to measure and as we have to have reasonable resolution.

We also have to keep in mind there's something called the resolving power of any actual optical instrument. It's related to diffraction, or sometimes it's called the diffraction limit. Some of you might have learned this even in kind of classical E&M or optics coursework.

So any of the light that is scattered into our collecting device, into our microscope, has to fall within some kind of angular cone or aperture with some angle we call theta. And it turns out if we try to squeeze down this aperture to be smaller and smaller, if we squeeze theta to be a smaller and smaller angle, we actually-- we lose the ability to make a sharp picture. You have to get the balance right between the wavelength of light you use-- you want that to be small.

So in principle, it could differentiate small features of your target. But you can't make your aperture too small or you'll get a fuzzed-out, diffraction-limited image. There's a trade-off, an inverse trade-off that was known, again, even kind of classically in optics.

If that's not familiar to you, by the way, you're in good company. Heisenberg almost failed his PhD qualifying exam because he couldn't remember the diffraction limit for how actual microscopes work. So he had forgotten this quite familiar classical result. It stuck with him after that, and so he was back on his mind in 1927. There's this trade-off in what we call the resolving power proportional to the wavelength, inverse or proportional with a measure of the kind of aperture or angular size.

So the aperture will collect light, the scattered light, again, with a range of scattered momentum. So not any old scattered light beam will fall into our device. There'll be momenta, whose vector quantities fall within this kind of angular cone-- a cone of angular size theta.

So that means these scattered photons in order to fall into our device could have any component in this direction. I'll call this vertical direction now the x direction. We're trying to measure the position of the electron in this direction.

What's the component of the scattered photon's momentum that would enable that photon to be captured by us in our device? Any component delta px that falls within the aperture, and that's just the sine theta component of our hypotenuse here.

Well, now we've just-- as Compton had already learned, we've now just basically produced a collision, a kind of two-body collision, between an electron and a very high-energy photon. So there'll be some recoil of the electron. There'll be some scatter of the electron because it's just been smacked by some very high-energy photon.

So whatever the uncertainty in the scattered momentum of the photon is, we'll have a comparable uncertainty in the resulting momentum of the electron. It will recoil just like in Compton's scattering.

What's its recoil momentum? Well, we can't identify that with perfect accuracy because we've clumped together, so to speak, we've lost the ability to distinguish or differentiate among scattered momenta-- components of the scattered momenta within some cone of angular size or with theta.

So we have some uncertainty in the post-scattering recoil momentum of the electron from a kind of Compton scattering argument. But now we know what that component is. We can actually fill this in.

Because using Einstein's arguments from photoelectric effect and so on-- again, we saw this with the Compton scattering in the previous class, or maybe two classes ago-- the actual momentum of that photon is given by Planck's constant divided by the wavelength of the associated wave. That's how Einstein began reasoning, as we saw, combining the kind of poynting vector classical electromagnetism with his notion that photons are discrete bundles of energy with [INAUDIBLE].

So now, Heisenberg says let's combine those things. We can't make the resolving power, which is say how fine a picture or how fine a determination of the location in space that electron is-- we can't make that arbitrarily small while also making the uncertainty in the scattered momentum arbitrarily small.

In fact, if we multiply them together, their simultaneous values cannot be made zero. In fact, the product is going to be roughly of order h, Planck's constant. If we make the aperture too small, then we fuzz out our location information. However, by making the aperture not small enough, we allow in scattered momenta of a sizable range of components.

So we have this kind of seesaw trade-off. The wavelength appears opposite to each other between the resolving power and the scattered momentum. The effect of the aperture width is one is downstairs. One is upstairs. So we can't simultaneously make both delta x and delta p arbitrarily small at the same time.

And it gets there from thinking about where operating on these things, he thinks about this inspired by the fact that his matrices don't commute. So the order in which we do things should matter. What does it mean to be acting on this electron? And he goes to this physical kind of cartoon model.

So at first, Heisenberg says this is really us being clumsy. Heisenberg's own interpretation of his own uncertainty principle, which he publishes in the spring of 1927 while visiting in Copenhagen, is basically that we humans are big, clumsy animals. We can't help but disturb tiny little things like electrons.

After all, we're the ones who released this kind of enormously energetic light beam on our poor little electron because we wanted to make a sharp picture. But the energy scale of the light was comparable to the kind of relevant energies or momentum of the electron. So we've intervened. We've disturbed this object. So of course, we can't make a clear picture.

He's talking about this in Copenhagen with Bohr. And Bohr strongly, strongly disagrees. And here again, I want to think back to that really quite, I think, very compelling piece by Megan Shields Fermato that I put on the reader, I think, just for the previous class. I think this actually makes even more sense here.

Again, this tells us about how Bohr was operating all the time-- his entire career, often with these young, sometimes very brash kind of alpha male style young assistants. But also, even more often with his very patient wife Margrethe. He's always working in dialogue.

He's always talking these things through orally and sharpening his arguments like-- it's like a championship debater. And Margrethe was in the mix from the start. Bohr's also always doing that, especially with his younger assistants, people like Heisenberg. That's what they do throughout the spring and summer of 1927.

Heisenberg comes excitedly to his advisor and mentor Bohr, almost a kind of father figure for Heisenberg by this point, Niels Bohr. Look, I've learned this new thing. There's this trade-off because we humans can't treat the atomic realm with kind of enough kind of daintiness or with enough sufficient care.

And Bohr says no, you've misunderstood your own work. There are shouting matches. There are crying fits, mostly Heisenberg, not Bohr.

There's name-calling. This becomes intensely emotional as they continue this kind of behind the scenes discussion in Copenhagen really for weeks and weeks, not just for a day or two. It continues throughout the spring and summer.

So Heisenberg and Bohr tussle over how to make sense of the exact same equation. Again, we've seen this over and over again. Bohr is not saying your math is wrong. Bohr is not saying I don't believe your expression or you're off by a factor of 2 pi. He says, you're not interpreting your own mathematics correctly. I make meaning of your expression differently than you do. And Heisenberg doesn't-- it is not a comfortable discussion.

What Bohr argues instead is that this expression, what we now call the uncertainty principle, is not a result of our interventions, of our clumsy interventions with small little-- a fragile, quantum world. But Bohr says it's actually about the quantum objects independent of ourselves. It's something deep about how nature works on the scale of atoms, he's now convinced himself, that they simply do not and cannot have simultaneously sharp values for certain pairs of properties, for complementary pairs of properties.

One set of those pairs is position and momentum. That's the component of momentum in that same direction-- so delta p sub x here. That that's one of these examples of a complementary pair of properties-- delta x delta p sub x.

They go on together to work out-- a similar one would be the energy involved in a certain interaction, delta e, and the time over which that interaction takes place. That becomes another pair of complementary properties or quantities that can't be simultaneously identified with arbitrary precision.

So there's another version of the uncertainty principle. Delta e delta t is of order or not less than roughly h. So to Bohr, he says that's something we have to learn about nature not about our own kind of clumsiness, that the electron even before we smacked it with this high-energy photon simply did not have on its own simultaneously sharp values of position and momentum along that direction even before it was smacked by the photon.

So now, that begins to lead to even broader kind of sprawling discussions, much more kind of philosophically freighted discussions which they're still working out often during strolls in the gardens right near Bohr's Institute in Copenhagen. Really, again, this occupies them throughout the spring and summer of 1927.

So if the uncertainty principle holds for quantum objects even on their own, not just when we interact with them, then they begin to puzzle through together there could be no such thing as a sharp trajectory-- a specific path through space and time for quantum objects.

After all, what is a trajectory? It's a collection at every moment in time of exactly where an object is and exactly where it's going. And that's precisely what Heisenberg's uncertainty principle seems not to allow us to attribute to things like an electron.

We make a trajectory by saying at this moment, the electron is right here and it's moving in this specific direction at this rate and so on. That's what a trajectory is. And the ingredients for a trajectory are what we're now told by this unfolding work is simply not available to us. Because in nature, at least according to Bohr, these things were never sharp enough at any given moment.

So to Bohr at least, and eventually to Heisenberg through this kind of-- to call it a dialogue is to paper over the emotion involved and the real struggle. But eventually, Heisenberg is kind of, so to speak, converted as well to this Bohr line.

It becomes known as the kind of Copenhagen interpretation because so much of it is being worked out in Bohr's Institute through these long walks in the gardens, often with Margrethe and with Heisenberg and Pauli and the younger postdocs basically cycled through Bohr's Institute.

So the real lesson of Heisenberg's own equation is that given some position of some object at some initial time, say, x at T0, like we would do even with Newtonian mechanics, we can't know its position at some later time with certainty. Because to do that, we'd have to know both its position and its simultaneous momentum to arbitrary accuracy.

So that's not just interesting to Bohr. This heralds an amazing conceptual revolution. Bohr is always talking in this period in these kind of grand philosophical kind of sweeping statements. And eventually, people like Heisenberg and Pauli come along to that as well.

It suggests the fall of determinism-- this grand plan that stretches back to the 18th century, that given the present state of a system and knowledge of all the things that are impacted that used to seem, at least in principle, to be sufficient to protect with certainty-- to predict with certainty what would happen in the future.

And instead, what Bohr eventually convinces Heisenberg of and many others, is that that's simply no longer possible, that the whole notion of causality, or of A causing B, or of determinism-- I know this now, therefore, I can with certainty what's going to happen later-- that starts to fall into an enormous amount of challenge in the quantum realm given these arguments about things like the uncertainty principle.

So let me stop there. We have a moment or two at least for questions. Sorry I went on a little bit long So let's see. DA says, isn 't this work really around the time of de Broglie's work? Oh, very good. So very good.

So Heisenberg knew a little bit about de Broglie's work but was not terribly influenced by it. They were actually-- the first inklings were coming out independently and in parallel. So when Heisenberg was working on what becomes known as matrix mechanics, de Broglie's thesis had been submitted to the Sorbonne or whatever the Parisian University was but was not yet widely known.

And in the coming months, they get in touch and they begin comparing notes. And we'll actually talk more about the impact of de Broglie's work in our next class. And meanwhile, DA, you say, Bohr's argument contrary to his earlier work? Yes, it certainly was.

So Bohr was not stuck at one moment in time either. Bohr's work in 1913 depended on having very, very specific trajectories. In fact, very simple ones-- an exact uniform circular motion. He was convinced by Heisenberg a decade later that these things are not subject to observation. That's kind of a leap that gets us into a model. So Bohr's own thinking was not kind of fixed only in 1913 either maybe, you might say, to his credit.

And then he dug in even more over the course of that more recent work with people like Heisenberg to think that there was a bigger conceptual lesson that they all had missed, including himself. So he's making his own break from his own work from 1913 as well.

Jesus asks, what's the competing interpretations of quantum mechanics other than Copenhagen? Yeah. Very good. OK. So let me pause on that both because there's no time. That's-- and also because we will talk a bit about some of that in the coming classes. It's a huge question, Jesus, a great question, one that's very dear to me. And we'll get a little sampling at least in a few class sessions-- a great question.