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DAVID KAISER: So for today, we're going to continue this story, or this unit, of what we began thinking about together last time, a body of work, a set of ideas that came to be grouped together under this title old quantum theory. And as I mentioned last time, that only makes sense once there's a new quantum theory to contrast it with.

So this is a term not only made up many, many years after the fact, this was introduced even by many leading physicists right around the years that we're describing. So by the later years of the 1920s, it already had become common to make this kind of marker to distinguish between the scattershot developments that had been unfolding between roughly 1900, and 1924 or '25, or so, and to bracket those together under this umbrella term, old quantum theory.

Because as we'll see, starting actually in the next class, by the mid 1920s, there had been some new bodies of work put forward that look like this could really become the new quantum mechanics and not only a kind of hodgepodge of suggestive ideas within what then became known as old quantum theory.

So as last time, I just want to remind you these dates that I include here are convenient and limited. So as we've seen throughout the entire term so far-- we'll keep seeing examples of this well beyond even our quantum theory unit-- the dates that we assign to these things have to be taken with some grains of salt. We have to squint at them a bit.

They're helpful by giving us a rough sense of the flow and where within this evolving body of work do these particular developments fit in. But remember that it's not like someone conducted a single experiment or published a single paper with a date. And then afterwards, everyone agreed and moved on. And again, we'll find that to be true for these examples today. We've certainly see it true in previous discussions.

So with those caveats, today, we're going to look at this more purplish rectangle to contrast it with this reddish pinkish one. So last time, we looked at some of these moments of physicists sometimes very begrudgingly rethinking the nature of light, not always going into it with that as their plan, but coming out having rethought how physicists should describe light.

And we saw instances of more and more attention to quantization, discreteness. Maybe light even had a kind of specific particulate or particle-like behavior. And today, we're going to look at a kind of inverse or the other half of those discussions, as physicists began rethinking the nature of matter. Again, not always going into that with that in mind, but that's how these developments really began to add up.

So today, we'll look at this second half, the mirror half of the old quantum theory, rethinking matter. So by the closing years of the 19th century, really in the last say two or most three decades of the 19th century, there really seemed to be a coalescence, a pretty amazing achievement among physicists and chemists in many, many parts of the world.

It looked like physicists and chemists, natural scientists more broadly, had figured out a really deep regularity to the stuff of which the world is made. Here's an illustration of one of the very first published versions of Dmitri Mendeleev's periodic table. It doesn't look quite the way we would expect it today. It's actually oriented in the opposite orientation than what we're used to.

But this was a remarkable effort at ordering, ordering the stuff from which all other things seemed to be made. And on the strength of work like Mendeleev's and many others, by the late 19th century, it seemed clear that matter consisted of chemical elements that would behave certain ways in chemical reactions.

And a given chemical element-- hydrogen, beryllium, iron, uranium-- consisted of physical atoms. So there was a two-part way of trying to make sense of the world. And that seemed really great until suddenly more complications began to really crowd in.

So not too long after this really amazing, synthetic achievement to bring together lots and lots of disparate phenomena in things like the periodic table and this association between chemical elements and physical atoms, pretty soon, there were some new challenges to that whole scheme, starting really in the mid 1890s and moving sometimes very quickly. This caught a lot of people by surprise.

Some of this was accelerated by new types of instruments. There were whole new kinds of ways of trying to probe the behavior of matter and radiation-- cloud chambers, fluorescent screens that would glow and they interacted with certain matter or energy, photographic techniques. Remember, photography of any kind was still pretty new in the middle decades of the 19th century. So trying to make reliable, repeatable use of photographic plates, that was still pretty new, even by the 1880s and '90s.

So through this constellation of new techniques, some accidental incidents in various laboratories, unexpected, researchers in many parts of the world, in the 1890s in particular, began to identify several different kinds of radiations or emanations, some kind of energy that could be flowing out of what had once seemed like stable, boring matter.

And so there are a couple of these points along the way that became worldwide sensations in real time. Not only do we look back and say, oh, that was exciting. But even at the time, this became literally headline news in many, many parts of the world-- I mean, throughout multiple continents. And so we can again attach names and dates to them, but understanding these were extended activities.

One of those had to do with what became known as radioactivity and though there were many people involved in this, very quickly among the most prominent, the most famous researchers on this area included Pierre and Marie Curie, shown here. Marie Curie, originally Marie Sklodowska, a young student from Poland, she'd made her way to study in Paris. She met Pierre, who was quite a bit older.

They got married. They teamed up. They became a remarkable research team, investigating some of these radiations or radioactive behaviors of a range of elements-- in fact, identifying whole new forms of matter and radiation interaction. Right around that time, roughly a year later, there was a distinct set of developments coming, in this case, out of Cambridge University, associated with JJ Thompson, who is shown here.

He was experimenting with different kinds of stuff, and different instruments, and different techniques. He began identifying what became known as cathode rays. We would now call those electrons, a different sort of energetic beam that could come out of one form of matter and cause effects on other forms of matter.

So all these activities, which were often quite surprising when first identified, they suggested that atoms could fall apart and that they have internal structure. And this was a big deal because, as you may know, the word atom itself was derived from an ancient Greek word that means indivisible or unbreakable.

And so all this effort from Lavoisier through Mendeleev to understand the regularity of chemical elements, associate them with physical atoms, that was really under some new strain if atoms themselves were actually not atomic if they really could be broken apart, if they have some internal structure.

And so by 1900, with this remarkable series of developments throughout the 1890s, researchers had begun to identify different kinds of stuff that could fly out of matter, different kinds of radiations. And they had different properties. So for example, some of these emanations, or radiations, were easily deflected by an external magnetic field. You could actually watch, see the bending path. Others seemed not to be deflected at all by magnets.

Some of these could fog, could begin to expose a photographic plate, leave a kind of smudge, even if the plates were protected, wrapped up in dark paper in some desk drawer, not in the direct line of fire. So they had different properties. So what researchers began to do was just try to classify them by different kind of neutral sounding names.

They just began at the start of the Greek alphabet and labeled one kind alpha rays, another kind beta rays, a third type gamma. And they figured they'd just keep going, as many letters as needed. They didn't know what caused these emanations or radiations. But they could at least begin to classify or distinguish among them because the radiations themselves really did seem to have some different properties.

And again, this was remarkably interesting news at the time. The Curies, in particular, became really media sensations, especially Marie Curie. As you may know or can imagine, this was really, really unusual to have a world class leading researcher in the sciences who happened to be a woman.

And there was tremendous speculation about was she just like other women, was she a caring mother or not, really from today's point of view horribly, horribly gender stereotyped, often very unfair attributes attributed to her in the wide press. When her when Pierre passed away, she had a relationship with another researcher. It was a French scandal. And people wanted to know these scurrilous details of her private life.

On the other hand, she was constantly in the news. And people were really curious. And she was, of course, the first person to win two Nobel prizes-- one in chemistry and one in physics. And I just find this remarkable. This wasn't only true in turn of the century Paris. But as recently as last year, there was a major film some of you might have seen starring a major film star Rosamund Pike playing, yet again, yet another film adaptation of the life of Marie Curie.

So her personal story continues to fascinate as well. And that was true really from the beginning. In real time, she was already a kind of media sensation, as were these findings, things like these mysterious ghost-like rays that often people's eyes couldn't register, but could have effects across the room. It was a really remarkable conjunction.

So one of the younger folks who got really excited about this was Ernest Rutherford. He was a bit younger than the Curies. He was the next generation coming up. He grew up in New Zealand. But he was a very promising young student. And he received a fellowship to come to England, to Cambridge, to finish his studies there. He actually studied under JJ Thomson, the person who, in 1897, was experimenting with these cathode rays that I mentioned briefly before.

He joined Thompson's group at the Cavendish really right at that time. Rutherford was there just in the midst of this tremendous excitement over some of these new rays, like Thompson's cathode rays. Rutherford became fascinated by this topic of radioactivity. It was all the news at the Cavendish, in Paris, and beyond.

And he began identifying new sources that could emit by now familiar rays, meaning the alpha rays had been identified people now knew they had different properties than the beta rays and so on. But Rutherford found new sources that could give off that kind of radiation. And he also began trying to systematize the study of radioactivity more generally.

It's to Rutherford whom we owe the concept of a half-life. The Curies were very much involved with this. Rutherford helped to formalize it-- the half-life being a term that's probably quite familiar to us today, the time during which the radioactivity of a sample was measured by Geiger counters or other things would fall by half.

So that's the characteristic time during which you would measure these kinds of emanations coming out. And Rutherford in particular became fascinated by what we now call decay chains. So he could start, much as the Curies would do, with a certain material, and then watch that material decay, for example, by emitting alpha rays. And then that original substance would transmute transform to a new kind of substance that would later be called radon.

If he began with radium, one of the elements first scrutinized by the Curies, what would happen to a sample of radium after a bunch of alpha particles would come out? And that was the kind of thing that Rutherford wanted to study and systematize. So the decay products from alpha decay of radium would be identified eventually as radon. The chain we keep going.

Radon would emit alpha particles, become radium a. But now, it's called polonium, actually in honor of Marie Curie's homeland of Poland, and so on. So Rutherford got very quickly into this game, into this study of radioactivity. One of the things he did then, when he was only a little bit further along, was to team up with another colleague, Hans Geiger, originally from Germany, but at this point working in the UK.

We now refer to Geiger counters. They were actually developed in partnership between Rutherford and Geiger, in this first decade of the 20th century. Here's one of their earliest own illustrations of how this would work. This is, of course, a more modern version, a kind of Wikipedia version.

And the idea was that this was a device with which to measure these emanations, especially electrically charged radiations coming out of these radioactive materials. Very clever and actually very inexpensive and robust. That's partly why these things are still in such widespread use to this day.

Again, in the modern version, you have two conducting coils or beams-- the anode and the cathode. And you have a voltage difference between them. Inside the chamber, you have inert gas. It could be a kind of noble gas. If ionizing radiations, one of these emanations from some radioactive source, enters the chamber, it can actually ionize those otherwise neutral and fairly inert atoms.

It can, as we now know, rip off an electron or something like that. So now, you have a charged ion running around in this chamber. It will be attracted to either the anode or cathode. You'll complete a circuit. You'll have a readout. So you can convert certain kinds of physical interactions within the chamber into measurable electric outputs from this otherwise very simple closed device.

So by 1908, this had now become a new kind of tool in many, many laboratories because it was both cheap and reliable. And the idea behind it was pretty easy to convey. Using these kinds of tools, Rutherford, and Geiger, and their immediate circle in and around the Cavendish were able to determine that alpha particles actually carried twice the electric charge of Thompson's still pretty new beta particles, but of the opposite side.

Basically, they would go to the anode, not the cathode, and vice versa. So you could begin measuring more and more properties of these different kinds of radiations or emanations. So very soon after there was a reliable way to characterize these new rays, these alpha particles in particular, Rutherford got a new job. He was now a young professor at Manchester University in northern England. He built up a big, big flourishing research group.

And the idea now was not only to study alpha particles as the subject of research, as he'd been doing since his student days, but to flip it around and use alpha particles as a tool to learn new stuff about other things. So the alpha particles were domesticated, I think, really remarkably quickly from being the topic you'd scrutinize to being just another tool on your lab bench to learn about other things. And Rutherford was very proactive, as others were. But Rutherford was really very creative in that transition.

So beginning as early as 1909, his group began directing alpha particles. They would have a radon source of the kind he'd been studying for years. It would be what's called now an alpha emitter. So the natural form of radioactive decay of a radon source is to shoot out beams of alpha particles.

So that was in this block here. And they could kind of collimate the beam. They could encase the radon source in lead shielding with a very narrow opening to have the alpha particles shoot out in one direction. They would shoo them towards a target of a very, very thin metallic foil, often gold foil very, very thin, and then surround almost an entire foil by this new fluorescent screen.

So when the alpha particles would ricochet off that foil, they would light up this fluorescent screen. This was incredibly-- that's very easy to describe. Even a nice Wikipedia-level cartoon makes it look so straightforward. This was incredibly painstaking work. The researchers had to sit in a darkened room for like a minimum half an hour just to let their eyes adjust.

Imagine sitting basically in an entirely blackened room-- your mind starts playing tricks on you. You start seeing flashes where that might not be any-- just so that their eyes would be sensitive to these very, very modest little flickers from the fluorescent screen. And you basically had to sit there alone in the dark and count not just how many flickers but, as I'll say in a moment, where along this circle, what angle did you see most of these little tiny short flashes of light appear.

So what they began to find is that most of these alpha particles would pass right through the foil, meaning most of the little flashes of light would line up directly behind the target. There would be a straight line. And they began to reason, Rutherford and his team, that these alpha particles were probably considerably smaller than the target atoms within that foil.

If that was a gold foil, they would assume these gold atoms are really big on the scale of alpha particles and spread out on the scale of alpha particles. So most of the time, these little emanations, these alpha particles, could zoom right through that thin metal foil and never get deflected or scattered at all. So in that case, you'd expect most of the very faint short flashes on the fluorescent screen to line up directly behind the source. There's no deflection.

Sometimes, there'd be a glancing scatter, a small angle scatter, a few of them over here. And that is, indeed, what they found most of the time. But every now and then, in a way that by doing this over and over and over again over months in the darkened room, painstakingly-- they could quantify it was roughly one out of every 100,000 scattering events. An alpha particle would actually scatter by very large angles.

It became known as backscatter. It would scatter almost 180 degrees, coming back almost towards its own source. So most of the time, there was either no deflection or very minimal deflection. But some small number of times, there would be large angle scatter instead. They began making essentially these histograms of the number of scattering events as a function of the scattering angle.

And as you can see, small angles is where almost all the events would cluster. And very rarely, about one out of every 100,000 approximately, you'd find a very large angle scatter event. That was not at all what they expected to find. Rutherford very famously recalled-- I love this quotation.

Referring to his experiments, he said, "It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you had fired a 15-inch artillery shell--" think of it like from a cannon-- "fired that huge shell at a piece of tissue paper and it came back and hit you." That's what it seemed like to have these little tiny alpha particles ricochet all the way back for very large angle scatter.

So they were collecting that data starting around 1909 or so over months and months, ultimately over about two years to really build up lots and lots of data and statistics. During which, Rutherford then worked hard to make sense of the pattern quantitatively, to work out why it might be that you have not just rare scattering events at large angle, most scattering events at small angle, but really to retrace this particular shape of that curve, that particular kind of exponential decay of the scattering rates with angle.

And he finally convinced himself with a pretty short derivation that this kind of scattering pattern would only make sense if most of the mass of those target atoms, the little gold atoms in the thin gold foil, were concentrated in a very dense nucleus in the center of those atoms, that most of the mass of the atom was actually in the very, very center. And otherwise, most of the rest of the volume that the atom might take up was empty. Atoms were mostly empty space.

In fact, about one part in 100,000 was the ratio between the size we might attribute to an atom and the size we would attribute to this massive inner core of the nucleus. The nucleus, much like the alpha particles, he goes on, must have a positive charge. They were repelling each other, he concluded, whereas these lightweight electrons, which at this point were what Thompson's cathode rays or beta particles have been identified with, those must have the opposite charge.

And again, you could get a sense of the size of the charge from things like Geiger counters. So the model that Rutherford begins to piece together, building on these multiple years of data collection, is a kind of solar system picture of the atom. There's a very, very tiny massive core, the nucleus, that's kind of like the sun. And lighter weight objects are going to somehow move around it. And that's what makes up an atom.

So he introduces this solar system model by around 1911. But of course, that led to several new questions on its own. So one question was these electrons, these JJ Thomson beta particles, would be constantly accelerated as they moved around the nucleus.

If you thought about them like the Earth in the solar system, the Earth is constantly being accelerated as it moves around the sun. Its direction of travel is changing. Its velocity, in fact, is changing. Its velocity vector is changing. It's suffering constant acceleration. Wouldn't that be true of these negatively charged electric particles, the electrons, as they move around in any old pattern around the positively charged nucleus?

And then the question comes up, according to Maxwell's remarkably successful work on electromagnetism, anytime electrically charged objects accelerate, they should radiate. They should give off light or electromagnetic radiation. So question 1 from this new Rutherford model was, where's all that light?

If every atom filling every bit of matter in the universe has its inner parts constantly accelerated, electric charges accelerated, why don't we see everything glowing all the time? And perhaps an even trickier question was, if those electrons are actually emitting radiation, if they're giving up some of their own energy in the form of light, it's going to carry that energy away, then why are any atoms ever stable?

Wouldn't those electrons be giving up their energy as they irradiate? Wouldn't basically their orbits decay, decay, decay? Wouldn't they just crash into the nucleus? Why don't atoms fall apart all the time and, in fact, very quickly? So these two questions became at least as challenging as the successes of Rutherford's nuclear model.

Let me pause there and see if there are any questions about that. Alex asks in the chat, were all the detection events detected by eye? Yes, in the very first ones, they were. This is, again, astonishing. There were no the fancier things we would use today with, say, CCD cameras and so on, CCD detectors. Obviously, those didn't exist yet.

There were early photographic methods. They didn't have the resolution that people would need. And they didn't have the timeliness. If you just set up the photographic plate and didn't have a way to rapidly take those plates out, and develop them, and put new ones in, then you would lose the kind of information they were looking to find.

And so they really just had to have researchers sitting in the dark counting for hours, locked in a dark closet, both because the human eye, once you sensitized it, was, in its day much, more sensitive than these photographic techniques at the time, both in terms of spatial resolution, but also for temporal response. Your retina will go back to neutral much more quickly than these than these large photographic plates would. So they would really do basically naked eye observations. And I just don't know how they could do that for so long.

Silu asks, did they choose to use gold foil for a specific reason? Why not any other metal? That's a good question. I think partly it was very pliable. So they could make very thin gold leaf. Then as now, bakers can make gold leaf on cakes. I mean, you can make very, very malleable, very, very thin layers of gold leaf.

And there were already chemical notions, chemical measurements, to suggest that the gold atoms should be relatively massive. They wanted big scattering targets, basically. Even by the rough estimate of, say, 1 mole of gold atoms, how much would that weigh, they knew that had a place on Mendeleev's periodic table that looked pretty massive compared to other lighter targets.

So those are the kinds of reasons. I think they did experiment with multiple types of thin foils. But they were all choosing them from around that part of the periodic table and that were both relatively inexpensive. They didn't need tons and tons of gold bullion.

They just needed a little trace amounts of gold that was workable, had the right kind of macroscopic properties, and that also had a placement within Mendeleev's table, which already was ordered, roughly speaking, by atomic weight. So I think those were the kind of criteria they used, though indeed they did experiment with multiple types of targets.

Those are great questions. Any other questions? I mean, we all have the benefit of many things in 2020, like high school, including high school chemistry classes. So the fact that atoms are made up of nuclei that have a lot of mass and positive charge, the scales might not yet have fallen from your eyes because you come in knowing that. But that was really, really pretty surprising, very surprising in 1911.

The prevailing models until then had suggested that atoms really were made up of different kinds of charges. It was clear, by this point, that Thompson's cathode rays carried a negative electric charge. It was clear that alpha particles carried a positive electric charge. It was clear that atoms were electrically neutral. So people figured there must be equal numbers of these constituent parts, so the total electric charge balances out.

But the prevailing model actually at the time, before Rutherford's experiments, was known as the plum pudding model, which is very British. The idea was that atoms might be just like this kind of mush, like a plum pudding, which I think is actually pretty gross, of this undifferentiated goo with positive and negative charges just distributed at random so they average out within some volume, that they didn't have a kind of structure, let alone that the positive charge was associated with such an imbalance of the mass.

I mean, all those things were really quite unexpected, which is, I think, why someone like Rutherford says, it's like firing a cannon shell at tissue paper and having it smash back. If you fire a cannon shell at porridge, you don't expect it to come back in your face. So the fact that these atoms had very, very hard, massive scattering sites, these very, very dense nuclei in the middle, that was genuinely unexpected. Even though now we're like, oh yeah, I grew up knowing that. So sometimes, the novelty is hard to put our heads back into that moment.

Any questions about what Rutherford was up to or the broader excitement about radioactivity and all that stuff? If not, I think I'll press ahead. As always, of course, please feel free to chime in, in the chat. We'll have another question break pretty soon. Oh yeah, Gary asked, why did Rutherford think the alpha particle is like a cannonball. Very good. Thank you, Gary.

So by this point people knew, thanks to some of his own work, that the alpha particles were unbelievably heavy compared to the beta particles. These things were on the order of 10,000 times more massive. In their artillery minds, it would be like a BB gun versus a cannon blast. It really was, in that sense, a huge difference in just the heft.

People would conduct what soon became called charge to mass ratio experiments. Some of you might know this. You might have even done some of these in junior lab or earlier. This was building on things that JJ Thompson had been doing in the 1890s and that Rutherford learned as a student.

Once it became clear that both beta particles and alpha particles carried some electric charge, then people were like, great, let me go mess with them. One of the best ways to mess with electric charges is to use magnets, use a strong external magnetic field. And then you can measure the ratio of basically how strong an external magnet you need to get a certain kind of arc in the trajectory of those particles. And that will help you learn the ratio of their charge to their mass.

Then separately, with things like Geiger counters, you can start measuring the ratio of just their charges. You combine all that. You say, oh, the mass of those alpha particles was, again, on the order of 10,000 times greater or more than the mass of the beta particles.

So they were using each of these to fire on stuff. These quickly both became projectiles to shoot at new targets. But one of them seemed like it really was just this monster of the radioactive realm. And to have that kind of cannonball shell, artillery shell bounce back if you thought you were firing it at otherwise undifferentiated porridge, I mean, I think that's what Rutherford's surprise was coming from. That's a great question.

Any other questions on that? OK. Let me march on now. We're going to hear about some work by one of Rutherford's postdocs who was named Niels Bohr. So Bohr was a young Danish physicist. He had just finished his PhD in theoretical physics in his native Copenhagen. He grew up in an actually relatively well-to-do family. His father was a very prestigious professor of physiology at the University of Copenhagen.

He grew up in an academic household. He remembered from his childhood having lots and lots of distinguished scholars come visit for dinner all the time, that kind of thing. And Niels himself was a very ambitious young physicist. His younger brother went on to-- Harald went on to a distinguished career in pure mathematics, a very elite, academic family in Copenhagen.

The place he most wanted to go after finishing his PhD was to join Rutherford's group. This was right at the time that Rutherford was doing all these amazing experiments on things like the scattering that led to the nuclear model. So Bohr was accepted as a young postdoctoral scholar with Rutherford's group. He moved to Manchester and spent about two years there, typical postdoc time, right at the time that these scattering results were first being shared.

So Bohr became really fascinated by this new notion, partly for the reasons I was describing. It really did seem so unexpected that atoms would have such a hard, dense, compact scattering center deep in their middle and other [INAUDIBLE] mostly empty space. So that was really cool for Bohr. But he was puzzled, as many of Rutherford's contemporaries were, about how to make sense of the stability of matter.

If these electric charges are somehow whipping around that central mass like planets around the sun, how come they're not radiating all the time? How come they're not losing all their energy? Why do atoms not just collapse on themselves in very short times?

So Bohr, maybe because he came from this very elite academic family, he set himself a very modest goal for his postdoc. He said, I'll explain the stability of every single kind of atom forever. It was remarkable. He wanted to basically solve for the stability of every single entry on the periodic table.

And you got a taste of this in the reading for today by historian John Heilbron. One of the things that I think we often forget-- I didn't know when I first learned about the Bohr model. And I think that's my favorite part about the piece by Heilbron-- is that Bohr's first attempt was actually to focus on multi-electron atoms and even molecules.

He didn't first start with hydrogen, the way we now usually associate with his work. He actually figured, maybe there is some kind of what we now call classical equilibrium, a kind of compensation mechanism that would allow these electrons to be outside of a nucleus, but not constantly accelerated, constantly losing their energy due to radiation.

So he wanted to find some combination of electrostatic repulsion between the electrons repelling each other and the magnetic effect that he thought could maybe counterbalance and make a kind of equilibrium. And maybe that would explain both the structure of atoms and why atoms don't immediately fall apart. That's quite an ambitious postdoc project. And it didn't work.

And pretty soon, it became clear he just could not get this kind of equilibrium compensation thing to work. The multi-electron systems were very complicated mathematically. Any time you have many things acting on many other things, as you know to this day, the mathematics quickly becomes pretty, pretty intense. So he finally agreed to try a simpler case, which is always a good tip in physics.

Don't try to tackle the hardest, most complicated case first. Try to tackle a simpler case first. So in a desperation, Bohr switches gears and starts to think hard about the simplest of all the atoms, the very first placeholder on Mendeleev's periodic table, the hydrogen atom, which even by then was known to have one electron and therefore was assumed to have one compensating proton in that nucleus.

So that way, you don't have to worry about multi-electron repulsion within a single atom. And again, for simplicity, he considered a circular orbit. It could have been a more complicated motion. But he said, let me just get started by assuming really like a simplified version of the solar system, that the proton, the positive nucleus, sits in the middle. And the electron orbits in a perfect circle in a simpler version even than the planet Earth around the sun.

In that case, now he knew how to start. And again, this is how I think any of us would start. He knew there was an attractive force between the negatively charged electron and the positively charged proton. There'd be an electrostatic attraction, the strength of which would go like the square of their charges divided by the square of their distance between them.

There'd also be a kind of mechanical motion of that electron constantly moving around, undergoing centrifugal acceleration. And you could just take the a Newtonian expression for that. You have the planet Earth moving around the sun or a ball moving on a string or in a circle. There'll be an outward-directed effective force, the centrifugal force.

And he just said, there must be a stable orbit when that inward directed force of electromagnetic attraction is perfectly balanced by that outward tendency of the centrifugal force. Balance the forces, and now, he can solve for this speed, the speed squared, associated with one of those stable orbits. This is all Newtonian and Maxwellian or even pre-Maxwell, just Coulomb.

So as far as Bohr was concerned at this stage, both the speed, the magnitude of the velocity, v, and the average radius, the distance between the electron and the nucleus, these were assumed to be variables that could take any one of a continuous range of values. It could be at 1 unit, or 1.003 units, or 1.035 units, et cetera.

Then Bohr says, well, if we just stick with that, we have all the challenges of Rutherford's model. Why are these atoms stable? Why aren't they radiating? And so on. So now, Bohr takes inspiration partly from Max Planck. Remember, this is now 1911, '12, '13. This was a decade into to Planck's work on blackbody radiation.

But we know from Bohr's writings and letters at the time, he was even more directly inspired by Einstein, who was just a couple of years older than Bohr himself. And by this point, Bohr was thinking about Einstein's re-explanations of things like blackbody radiation and the photoelectric effect.

And Einstein, even more than Bohr, had been emphasizing a kind of discreteness, that we have to break the classical rules, at some point, and say certain quantities can only take quantized units, like Einstein's explanation for Planck's work saying that light quanta could only hold fixed amounts of energy-- h times nu or 2h times nu, but not any non-integer value.

So Bohr says, let me try that trick again. Basically, by now, it's a strategy to try. So Bohr just gives, again, a heuristic hypothesis. He doesn't prove it. He doesn't derive it much, like Einstein was doing with his heuristic suggestion of light quantum.

Bohr says, what if a quantity that ordinarily we would treat as having any old value at all, the angular momentum of that electron's motion in the atom, which classically for a perfectly circular orbit has a very simple expression-- the electrons mass times the magnitude of its velocity times its radius-- and what if we just impose now a discreteness, much as Einstein had done in other contexts?

What if we say the angular momentum of that moving electron can only take integer values of a scale set by Planck's constant? And again, as many of you likely have seen already, there's an abbreviation that very quickly becomes common. This is called h-bar. So h, lowercase h, had been introduced by Planck in his 1900 paper, became soon known as Planck's constant, had a very particular numerical value in, say, erg/seconds.

And then a very convenient shorthand is to divide that numerical value by 2 pi, which comes up a lot if you're thinking about things like circular orbits. So Bohr says, what if the total angular momentum of that moving electron could only take unit values at a scale set by Planck's constant? He doesn't say why. He says what if. It's a heuristic kind of hypothetical suggestion.

So now, Bohr has two expressions for v. He has the classical balance of forces expression. He now has a new quantum condition expression. That's his term. He can solve each for v and set them equal to each other. And then he can solve the resulting single equation for r. So now, he has basically two equations with two unknowns. He wants to know both v and r. He has two equations-- one here, one here.

Equate them, and then he can solve for the allowable radii, distances from the nucleus at which an electron should be found if its angular momentum is subject to this new quantum condition. And he finds that the radii-- the radius can't take any old value. It no longer falls within a continuum of possible values. It now snaps into place with n being any positive integer.

So it could be 1 squared, 1 times some basic shortest length. It could be 2 squared or 4 times that length, 9, and so on. But it can't be 2.05 or 8.96, right? So the radius at which one should expect to find electrons, if we impose this new discreteness, this new quantum condition, becomes set by some new integer, n, which becomes known as the principal quantum number. It's just any old positive integer.

And there's now, again, a scale in which Planck's constant plays a central role. The combination of these constants of nature, h-bar squared divided by the mass of the electron times the square of its charge, that has dimensions of length. That's the unit radius. An electron should be found at 1 times that, 4 times that, 9 times that, et cetera.

The radius itself is about half an angstrom, if you've heard of the units angstroms. 1 angstrom is 10 to the minus 10 meters. And so this is about half of that. So roughly 0.5 angstroms is what this new unit of length works out to be.

So now, Bohr has this picture emerging not just of a nuclear atom, like from Rutherford, where most of the mass and all the positive charge are concentrated in this dense central nucleus. But now, there's a more finely articulated structure for where one should find the electrons outside of the nucleus, as the electrons circle around, at least in a simple atom like hydrogen, which only has one electron.

As I was saying a moment ago, the electron can be found at 1 squared times its basic length, a0, at 2 squared times that length, 3 squared, and so on. And so you have this series of discrete orbits. So now, Bohr wants to know, well, if we have this solar system model where the electron can only be at certain distances, certain radii from the nucleus, how would the energy of such a system-- how could we characterize the energy of such a system?

So just as he'd done before, he goes back and starts with the perfectly normal textbook expressions from both Newton and Maxwell. You have a charge, a charged object in motion around some other charged object. The moving thing has some kinetic energy. That's always 1/2 mv squared.

There'll be some, in this case, potential energy. It's an attractive energy because the charge is-- the electron and the proton have opposite charge. That's why they attract each other. So the potential energy has an overall minus sign. Now, already from his previous classical balance of forces argument, he already had an expression for v squared. So now, we can just sub into this expression for the energy.

He also, from the previous exercise, had an expression for r. Now, the energy depends only on r. Well, he knows he has an expression for r. This part's newer, once he uses that discrete quantum condition by quantizing the angular momentum. And so finally, he can plug in his new basic unit of length. It soon gets called, in his honor, the Bohr radius.

And now, Bohr has an expression for the energy of an electron in any of these quantized orbits or quantized forms of motion. It depends only on various constants-- the mass of the electron, its charge, and Planck's constant, and that integer n, which marks which of these orbits or states of motion the electron is in. So the electron now has very particular kinds of motion it can undertake. There are quantized amounts of energy associated with any of those orbits.

So now, that means one could take the difference in energies between any of those discrete allowable orbits. What's the difference in energy between, say, an electron over here with n equals 3-- so 3 squared is 9-- versus the energy of an electron here for n equals 2 or for any two discrete states of motion labeled by some integer n? The energy difference between them then just has this pretty simple expression. It'll be the difference of the inverse squares because the energy for either state alone goes like 1 over n squared.

And now, Bohr starts to get excited. In fact, this is why anyone began taking him seriously at all. Much like with Einstein, he hadn't proven that the angular momentum should be quantized and discrete. He hadn't even really given very good arguments for it. But he basically says, give me a moment. Let me show you what we get if we adopt that assumption.

And why was Bohr excited? Because by taking on that simple looking but otherwise not well-motivated assumption, this special quantum condition, he now found that the energy differences between these discrete states of motion of an electron in hydrogen should reproduce the already well measured, well characterized behavior of the light that comes out of hydrogen atoms when we excite them.

If you add some energy external energy to a gas of hydrogen atoms, they will radiate at very specific colors, the emission spectrum. The spectral lines that will come out of excited hydrogen have a very specific pattern to the frequencies. And this is actually real data. You can actually see these with the naked eye.

Three of the lines that come out are in the visible range for ordinary human eyesight. And they're very discrete emission lines, the so-called spectral lines. These have been measured in the 1880s-- in fact, it turns out, the very year that Bohr was born. These were measured by a German schoolteacher, a German language-speaking schoolteacher named Johann Balmer. These were known as the Balmer series.

After that, there were other series that had been measured that were in either the infrared or the ultraviolet. But the three characteristic lines to which our eyes are usually sensitive is a simple series of three lines. And these had a numerical regularity to them that had otherwise been pretty much unexamined and unexplained.

But it became clear you could identify each of these parts of the Balmer spectrum as being a transition between one integer squared and another. And there was some overall constant called R named for a different researcher, Rydberg, that set, again, the quantitative scale for the actual frequencies of that shade of purple, that shade of blue, and this shade of red.

And so the Balmer series would come about if you simply said there's some overall empirical constant, R, the Rydberg constant. And the light that we see would correspond to a transition between, say, n equals 3 to n equals 2. That would be the red one, relatively long wavelength, lower energy. There could be a transition from n equals 4 to n equals 2. That would light up to our eyes like this nice turquoisey blue. Or from n equals 5 to n equals 2, that would give us the deeper purple.

So now, Bohr could say, well, there's an energy difference between these discrete states of motion. When an electron moved, made a transition from an excited state, and we had to add external energy to these hydrogen atoms, maybe, Bohr says, that bumps them up to an excited state of motion. n gets larger.

At some point, the atom will relax back down to a lower state of energy. And maybe the light that gets submitted, this emission spectral line, is simply the difference in energy between the excited state of the electron's motion and the lower energy state. So the energy that comes out in the form of light, the energy difference is exactly the difference between these otherwise stable, quantum stable states of the electron.

So not only did Bohr find the same form for the difference between the energies of those two states, he can now calculate this empirical constant, the Rydberg constant, from first principles. And when he plugged in the values for the mass of the electron, the charge, and Planck's constant, he got a remarkable match to this empirical value for the Rydberg constant.

If you have trouble keeping that in mind, I recommend this historian's way to remember the Balmer series. Many, many years later, well after Bohr's death, his collections of his most important works were republished in a three-volume set. And the editors very, very amusingly colored the books to match the Balmer series. So volume 1 is the red. Volume 2 is the turquoisey blue. And volume 3 is the purple.

OK. So at first, Bohr was actually reluctant to publish those results. Partly, remember, he'd only treated hydrogen. And he set out at the start of his postdoc trying to explain every single atom on the periodic table, every single element, which is pretty ridiculous. So he had fallen approximately 100 steps short of his goal, 90 some-odd steps short.

Happily, his postdoc advisor, Ernest Rutherford, said, explaining hydrogen is pretty good. And you should publish because you have this remarkable, unexpected agreement, an accounting now for things like the Balmer series of these spectral lines. So Rutherford convinced the young Bohr to publish. And Bohr actually published his trilogy of related articles in 1913, *On the Constitution of Atoms and Molecules.*

So what really became central was this new kind of hypothesis of this quantum condition. For reasons that still weren't at all clear, they were not proven, they weren't even really derived or motivated, but if one takes a heuristic suggestion that maybe the angular momentum associated with the motion of electrons could only take on quantized or discrete values and h-bar, then that would immediately restrict the locations, the distances between the electrons and the positive nucleus.

Now, you have a discrete series of states of motion. Only for those, for some reason, would the atom remains stable. There's some new ingredient that had been missing in Rutherford's own picture that now might account for the stability of matter. When an electron is in one of these special quantized units, it won't radiate. It will only radiate when it jumps between these levels. And that will account for the emission spectrum.

Now, of course, some questions still remained. Why don't we see this kind of discreteness in ordinary experience? So one of the next things that Bohr occupied himself with-- and it actually went over the next four or five years. It was a non-trivial exercise-- became known as the correspondence principle. Again, this was something that Bohr really championed, and worked out, and then became enormously influential in these kind of later years of what we call the old quantum theory.

These quantum ideas seemed to impose some new break from experience. Something that we usually think of as continuous can only be discrete. In our ordinary experience, we don't see that kind of lock in step discreteness. And so Bohr wanted to make sense of the two different regimes and see if there was a way one could slide over into the other.

And so that's what became known as the correspondence principle, which was to say, in the limit of very large quantum numbers, for very highly excited atoms in this case, where n is much, much larger than 1-- still an integer, but much larger than 1-- these quantum systems should begin to reproduce the more expected continuous or classical behavior.

And again, if these next few steps don't make immediate sense, you're in good company. Don't worry. The slides are on the Canvas site. You can go through these steps, the algebra on your own. I just want to make sure that we understand his reasoning here. So I to go through his main argument for the correspondence principle.

According to Maxwell's, again, highly successful work on electromagnetism, the frequency of light, or of any electromagnetic wave that's emitted-- so we'll call that new radiation. That's the frequency of the electromagnetic waves that come out-- that should be equal to the mechanical motion, the frequency of the mechanical motion.

If you think about a dipole antenna, if you shake an electric charge up and down on a conducting wire, the electromagnetic waves that come from accelerating an electric charge should be directly related to the motion of the charge itself. So the frequency of radiation should be equal to or maybe a higher multiple.

But it should be basically proportional to, if not directly equal to, the mechanical motion of the charge whose acceleration is emitting that radiation. That comes directly out of Maxwell's work. And we use that all the time in things like antenna theory. It still works to this day.

So Bohr said, OK, well, let's figure out these two different quantities and see how they relate to each other. He begins by thinking about the mechanical motion, the frequency of that electron constantly whirling around the nucleus. So the frequency would be 1 over the period-- how long does it take the electron to complete an orbit, just like the Earth taking a year to go around the sun.

So the mechanical frequency is 1 over the orbital period. And that, for a perfectly circular orbit to keep things simple, would be the orbital velocity, the magnitude of the velocity, so the speed divided by the distance it has to travel. It has to travel one circumference, 2 pi r. It does so at a speed, v. That's how we determine the inverse period, or the frequency.

Well, once again, from the balance of forces, he has an expression for v, from balancing the classical forces with the quantized angular momentum. So he can substitute in for v. He has a new expression for the mechanical frequency. This is now of the electron in motion around the atom.

And now, he wants to turn to that other thing he wants to compare it to, the frequency of the radiation that comes out. Now, in his model, the radiation frequency is not given in that earlier derivation by anything having to do with the mechanical motion around the nucleus of the electron.

It actually comes from the energy differences when an electron in an excited state will emit some of that energy and drop down to a lower energy state, rather than having anything to do with this mechanical motion. That's why it looks like it's in conflict with Maxwell's electrodynamics.

So we saw Bohr had derived this expression for the energy, for the frequency of the light that comes out as radiated. But again, he can start plugging in things he knows. He can use the definition of this radius, the Bohr radius. And now, he can consider not the regime of n equals 1, or 2, or 3 of small principle quantum numbers for states of motion that are near the lowest energy state.

But he wants to think about large quantum numbers, away from the most quantumness of the behavior of this hydrogen atom. Is there a regime? Is there a different regime in which the behavior predicted by this quantum theoretic description will carry over to the behavior of the expected classical system?

So let's consider a very large principle number n, a very highly excited state. And now, let's consider the radiation that would come out between two neighboring, but very large high n states. So consider both n1 and n2 to be very larger than 1 and near each other. Let's say the difference between n2 and n1 is small compared to either them alone.

Then we can expand this 1 over n1 squared minus 1 over n2 squared-- just do a Taylor expansion-- to first order in this tiny difference. We get an expression that looks like this. Now, we can begin comparing the frequency of the light that comes out, the electromagnetic radiation, compare that to, indeed, the mechanical motion, motion of the electron itself in one of these very high n, large n orbits.

And now, we just take the ratio of these expressions, once again using other things that we've already worked hard to learn. Now, you find that in that regime, not near the lowest energy state, where we expect the quantumness, the discreteness to be most on display, but at some very highly excited state, the ratio should, in fact, go back to 1. Maxwell said that ratio should be identically 1.

In the limit of large principle quantum number, that ratio, once again, goes back to 1, even though the way we account for it is totally different. We don't say, according to Bohr's model, that the radiation comes out with frequency nu because that's the frequency at which the electron is physically spinning. But it turns out the difference between neighboring highly excited states obeys basically the Maxwellian relation after all.

This becomes known as the correspondence principle, that the behavior should smoothly morph onto the classical Newtonian Maxwellian behavior in an appropriate limit. It might break from that expectation dramatically in a different regime-- in this case, small principle quantum numbers, when n is 1, 2, 3, or 4. But there should be some smooth, almost like an asymptotic matching because, then as now, Maxwell's equations work really well for understanding things like the radiation coming off of a dipole antenna.

So Bohr didn't want to lose all the extraordinary successes of Maxwell or Newton, but wanted to have an account for why things might not always only look like that when you get really down to the scale of atoms. So for large quantum numbers, quantum systems really do start to behave like classical ones. In more modern language, we'd say the emission spectrum looks basically continuous, even though it looks highly discrete far away from that regime.

OK. I'm going to wrap up the Bohr part. We'll pause for discussion in just in a moment. So Bohr further developed Rutherford's solar system or nuclear model of atomic structure. He remarkably was able to account for empirical results, like the emission spectrum from hydrogen. But much like Rutherford's work, this starts raising several new questions, even as it starts to answer some.

For one thing, again, despite Bohr's remarkable ambition, this whole scheme only seemed to work for single electron atoms or ions-- so a hydrogen atom, which is electrically neutral, or an ionized atom of helium, which one of its two electrons has stripped away, you only have one electron in orbit, or doubly ionized lithium, that idea.

Moreover, again as I've tried to emphasize, Bohr really didn't have any answer for why we might impose this brand new seemingly ad hoc quantum condition that the angular momentum should only appear in integer units of h-bar. Another question, when people began to try to visualize these warring electrons in these discrete states of motion, what happens to electrons between stable orbits?

Are they literally falling from some large, excited state towards another one? What determined when an electron would make one of these sudden discrete jumps? How did an electron know how much energy to radiate? It has to give up a delta E exactly equal to the energy to be radiated away.

And yet, if the electron can only land at a particular value of its lower energy state, how does it know, so to speak, in advance where it's going to land and, therefore, how much energy to radiate? How come it never messes up and radiates a different amount of energy for which it could no longer land at a stable orbit?

And then another question was-- it became clear well before Bohr's time that, in addition to a very particular pattern of the colors, the frequencies of these emitted spectral lines, they also came with different brightnesses, different intensities. Not every line that you could measure of these emission lines was equally bright.

And Bohr's model didn't seem to have any way of accounting for that other kind of equally salient set of measurements about the spectral lines. So some remarkable and surprising positive steps from this Bohr model. But again, opening up a whole new set of questions as well. So I'll pause there and ask if there's any questions on that.

Oh, very good. So there's a question, as you can see in the chat, we have this reading, which I like a lot-- and we'll talk a little bit more about this reading in the next class as well, the reading by Megan Shields Formato who, by the way, used to TA this class. So it's a nice homegrown body of work. Megan is a great colleague. And when she was still a grad student, she worked with this question.

And Megan has this really, I think, very lovely, very eye-opening essay about what kinds of work got credited ther or now. So we always talk about Bohr did this, Bohr did that. And Bohr did a lot of things. But he was almost never doing them on his own. And we'll see a lot of examples of that, again, actually in the next lecture. I placed it in the wrong slot. And I think that reading works even better for Wednesday's class.

But Bohr was not only always, always talking with younger postdocs and younger students of his own, people who would come to be like Werner Heisenberg, and Wolfgang Pauli, and the whole circle. He was always talking with his very, very patient and talented wife, Margrethe, who did not have a formal training in physics, but could certainly follow a logical argument.

And she seems to have been a very steadying force to help discipline Bohr's own unfolding syllogisms-- if A then B kind of thing. She was his constant sounding board and a kind of like a-- not debate coach, but just his-- he would try things out. And they would talk about things very actively.

And then he would write them up because she had helped him crystallize or clarify the order of his arguments and say this is a red Herring, this doesn't make sense. So Margrethe was doing, as we now know, a ton of unseen, behind the scenes work, actual, conceptual work for which we would then usually just credit Niels Bohr himself. It would be written up usually only with Bohr's name, not Bohr and Margrethe, not Niels and Margrethe Bohr.

And I think Megan's piece really draws it out very, very evocatively with some pretty cool use of primary sources. I think she's exactly right. So we'll see that again and again, especially with Bohr himself next week. So the question here is, did Einstein have a similar relationship with his own partner? His first wife, Mileva, I've mentioned a few times, had also been trained-- in fact, they met as university students.

Mileva was studying physics and mathematics at the ETH. And they were classmates. And here, it's really complicated. I won't spend too long on this because if I don't watch myself, I'll talk about it for the next eight hours. And you all have things to do with your lives. But I would be glad to direct you to lots of readings.

The short answer is they began talking together all the time, very much like we now know that Niels and Margretha Bohr were doing. Einstein and Mileva Maric were doing that absolutely all through college. They have their charming, almost sappy sweet love letters between them when they would be apart. I can't wait to talk about my problem set with you, my dear cherub. They were very sweet and geeky in a way that might appeal to this group in particular.

They were study buddies, as well as developing a real deep relationship. And as I mentioned before, Einstein would cut his classes. Mileva was a more diligent student. She would often take notes and let him study them. And so there's plenty of evidence of them also actively talking through questions about, for example, electrodynamics from like 1896 through 1901, 1902.

Then their path begins to be different, at least from all we can reconstruct from the historical documentation, different than that between, say, Niels and Margretha Bohr. What happens in the Einstein marriage case is that they have a young child. In fact, they have a first child out of wedlock, a little girl named Lieserl, who then was given up for adoption. And no one knows what came of her.

So they first have a first child they don't raise on their own. Then they very quickly get married. Then they have another child and eventually two. And then Mileva starts doing what was very typical, gender-typed family responsibilities expected of the time and with very little variation at the time.

Even though they both considered themselves radical bohemians, who cares what people think, I don't have to live by society's rules, pretty soon, they began living by pretty typical Central European rules or norms. And that meant Mileva did not pursue her scientific career. In fact, I think she didn't ever complete her degree. She dropped out, I'm pretty sure. I can look that up.

But she either barely got through with her degree after some interruptions or perhaps had to interrupt her studies. I can't remember. I'll look it up. She becomes basically a full-time homemaker, is what we would now say, raising their children while Einstein then had the full-time job at the patent office.

And then they seemed not to have talked with anything like the same intensity about the scientific work, starting a couple of years before 1905. So there have been all these kinds of efforts to tie relativity, in particular, to a joint conversation. Maybe Mileva was a key thought partner. And I would love for that story to be true. I mean, it would cap off this college romance. It would just be thrilling.

And I've been convinced by the work by historians who've looked very squarely at this, including a lot of women's history, gender and science scholars, who were very attuned to partnerships that don't often get a lot of credit, people whose work we'll read this term. And I'm convinced by their argument that that seemed not to have been the nature of the Einstein marriage relationship, starting in like 1902, 1903, let alone '05.

They quickly became really embittered. And so by the later 19-teens, they were basically in open warfare. And they couldn't wait to get divorced. And so Einstein couldn't afford to divorce her until he won the Nobel Prize. He was so sure he'd win the Nobel Prize that he promised his Prize winnings to be the divorce settlement before he won the Prize, like four years before he won the Prize.

Then the Prize came through. They got divorced. Einstein, by that point, was already seeing his second cousin, Elsa, who soon became his wife. So the story seems actually pretty different than this very lifelong intellectual partnership between Niels and Margretha, which, nonetheless, never really got credited the way that today, I think, we would find much more appropriate.

That was a long answer to a really good question. I'd be glad to say more. There's really a lot, especially in the Einstein marriage relationship. But I'm glad that you raised a question about Meghan's piece about Neil's marriage as well. That's a great point. Any other questions on that?

Let me jump in to the last part for today. The last part is shorter. We have plenty of time to do this last part. We saw, with Bohr's work, there were all these dangling questions about the Bohr model. And so we're going to see this last moment that really helps to at least better motivate some of these open questions from the Bohr model comes fully 10 or 11 years later.

It's introduced by literally a French Prince-- we don't often get to talk about royalty in this class-- a French aristocrat named Louis de Broglie. He was actually the younger brother of Maurice de Broglie, who also, by this point, was a very accomplished experimental physicist. They had a laboratory in the basement of their enormous castle.

So Louis had first studied history, like a good aristocrat, and all the rich humanities, and then stubbornly said that, like his older brother, he'd like to study this grubby physics stuff instead. So he finished his PhD in 1924 in theoretical physics, I think at the Sorbonne or some very elite Paris School.

And here, he began to wonder, is there a way to account for Bohr's powerful yet as yet unexplained hypothesis about the quantum condition, that the angular momentum of an electron should take integer units set by h-bar? So de Broglie tries to make an explicit symmetry. He really has a this happened here, can I make the same thing happen here kind of framework. He's very explicit about that.

He also returns to Einstein's work on light quanta, which, by this point, people are now taking seriously. This is now fully two years after Compton's results with X-ray scattering. The notion of returning to Einstein's work in 1924 was no longer controversial the way it would have been in 1905, or 1910, and so on.

So for de Broglie, that looks like a pretty sensible place to start. And as we saw in the previous class, there's a Maxwellian expression for the momentum of a light wave related to its energy and its speed. We can then take on Einstein's light quantum hypothesis. The energy of that light quantum can only take certain units set, again, by Planck's constant, h.

And so then as we saw, Compton himself used this relation. The momentum for an individual particle of light, a light quantum, should be inversely proportional to its wavelength with a proportionality set by Planck's constant. That, by now, had become pretty familiar, even to people who were grudging in that.

What de Broglie says is simply assert-- he doesn't derive. He doesn't prove. He simply asserts whether the same thing happens for particles of matter. This is all about the behavior of light. So de Broglie says, what if we relate the momentum of a little electron, for example, moving around the nucleus in an atom?

So there would be some expression for the momentum of that particle coming just, say, from Newtonian mechanics. What if we also relate that to an inverse wavelength, just as Einstein had by this point convinced many physicists to try for light? In that case, you can rearrange this expression and say, what if there's some waviness associated even with chunks of matter, particulate solid matter?

de Broglie says, what if there's an inherent waviness of a scale set inversely by the momentum, just like you'd expect here, and proportional to Planck's constant? Why would anyone ever take this crazy French Prince's word for it? We never see this in real life, in ordinary life.

This is now after things like Bohr's correspondence principle. de Broglie knows to wonder about the scales at which one might ever see such a strange sounding phenomenon. So if you imagine a fastball-- he didn't use this example, we can, for those more familiar with American baseball, or cricket, or fill in your favorite human-sized ball throwing game.

If you imagine a fastball pitched in a baseball game and calculate 1 over its momentum scaled by h, the waviness associated with that, if we took de Broglie at his word, would be unbelievably small, 10 to the minus 32nd of a meter. That's smaller than the smallest known elementary particles. So of course, we don't see this somehow inherent waviness on everyday scales.

But de Broglie goes on and says, for an electron, if we plug in a typical momentum that we estimate for its motion, say, in a hydrogen atom, the waviness, the actual wavelength somehow associated with that electron, is on the same length scale as its own radius, as its own orbital path, that Bohr radius, a0.

For an electron, this new, hypothetical quantum waviness is on the same scale as its own mechanical motion. So maybe, de Broglie says, we shouldn't ignore it. We can absolutely ignore this somehow hypothetical quantum waviness when playing cricket, or baseball, or any kind of sport. But maybe it's actually really critical when we're talking about the behavior of atoms and parts of atoms.

So if there really were some inherent wavelike structure, wavelike feature associated with the motion of an electron in an atom, could that begin to address this biggest as yet unanswered question about the Bohr model, why certain configurations or certain discrete states of motion for an electron would lead to stability of matter rather than unstable behavior?

And he says, what if there is a kind of constructive interference? And what if the only stable states for an electron are those for which there's an integer number of wavelengths that can wrap around the orbit? So the wave, in some sense, joins back on itself and constructively interferes to make a stable standing wave, basically a standing wave.

Whereas if the radius of that orbit were ever so slightly different, you would no longer be able to wrap an integer number of wavelengths around the radius. And you'd start getting the wave out of phase with itself. So the crests would actually start meeting up with troughs. And the whole wave would basically fall apart.

So de Broglie is saying, if we grant this hypothetical as yet unseen waviness to things like electrons, could we start accounting for that stability of matter? What would be the criterion then? You have to have an integer number of these wavelengths fit around the circumference, fit around 2 pi r of that circular orbit.

Well, now, he has an idea for what that wavelength should be. He's now working directly in analogy to Einstein's work with light quanta. So plug in what he thinks lambda would be. And then presto chango, he's now derived, or at least found, an expression that exactly matches the form for what Bohr had put in by hand in a totally unexplained and ad hoc manner.

So now, this might account for why this quantity Bohr had begun with, the angular momentum of an electron moving in a circular orbit, could only take on integer values of h-bar. That arises from this physical criterion of stability, of constructive interference to get the waves to line up into basically a standing wave pattern. That's pretty astonishing. Einstein reads a thesis, and is immediately impressed, and helps to draw news to it, draw attention to it.

It's still, again, merely hypothetical. It's clever, but it's not like it's been proven. And so quite remarkably-- and the pace of this I find just astonishing-- just within three years of that clever suggestion in a French aristocrat's PhD thesis, there was some pretty compelling new empirical evidence to start taking that seriously, in this case coming from what would soon become Bell Labs.

It wasn't quite Bell Labs yet, but from an industrial research laboratory here in the United States, work led by Clinton Davison and Lester Germer as early as 1927. So they were actually firing electron beams in a souped-up version of JJ Thompson's experiment. They had basically a cathode ray from which they could beam electrons at a certain crystal by other techniques they new, characteristics of the crystal structure of that nickel, what was the spacing, for example, between layers of atoms.

And then they could measure the amount of scattered electrons as a function of angle. So as they moved their detector to different angles, they could measure how large an intensity they would measure of the rescattered electron beam. And they found exactly this wavelike interference pattern, that there would be a criterion for these two scattered waves from scattering off distinct layers within that crystal to line up with an integer number of wavelengths between them that changes with the scattering angle.

They just did a kind of classical wave interference analysis, not having known things like the lattice spacing for that crystal. And they found exactly this kind of waviness interference pattern on exactly the scale to expect given de Broglie's surprising hypothesis.

So let me wrap up. Between roughly 1900 and 1924, '25-- we'll see even a little bit more of this in the coming lecture-- in this period that comes to be called old quantum theory, physicists really went through a pretty dramatic series of new ideas and assumptions about both light and matter, almost interwoven among each other.

So in contrast with these great triumphs of 19th century physics-- the wave theory of light that we've looked at in the beginning of the class-- several physicists for several lines of reasoning begin to explore a kind of discreteness or particle-like attribute of light. We saw several steps along that way during last class.

Meanwhile, in parallel, there's an inverse motion led ultimately by people like de Broglie to salvage this other new form of discreteness, Bohr's quantum condition, by suggesting that just as wavelike light could actually be thought of as particle-like, what if particle-like matter had some wavelike attributes?

And so by the early 1920s, as each of these partial successes begins to draw attention, many, many physicists were convinced that something had to change. There was some quantumness that simply was not consistent with either Newton's physics or Maxwell's physics. In fact, they began to use the term classical now to speak about what was not this new quantum stuff. They used that new label.

And the pattern seems familiar. Start with the textbook classical expressions for force, or energy, or distance, or speed, and then somehow just plug in, staple in some as yet unexplained new discreteness condition-- either the energy, or the angular momentum, or some wavelength. So you have this ad hoc stapling onto otherwise perfectly familiar classical expressions.

That began to really upset a bunch of people. There's a marvelous book I used to assign in this class. I dropped it this year. But I encourage you, you might enjoy it. It's a novel by historian Russell McCormmach shown here called *Night Thoughts of a Classical Physicist.* What McCormmach does is try to really, I think, very movingly, very compellingly recreate what it felt like to be a physicist living through this period of conceptual disruption.

What it really felt like is the solidity of the world began to shake. And it didn't just shake. It looked ugly. It looked piecemeal and ad hoc. So McCormmach, who had poured over just literally tens of thousands of letters, and laboratory notes, and publications from a whole generation of these mostly German-based physicists during this period of change, he distills that into a 160-page novel of a composite character named Victor Jakob, to watch Jakob react to this kind of like-- that kind of makes sense.

But it's disjointed from over there. I can follow you here. But this looks really weird. It just starts to feel broken. It starts to feel disorganized and ad hoc. And so when people began calling this old quantum theory, it was both to say this happened before something new, but also to try to bracket it saying, it's old and kind of broken. It just felt conceptually dissatisfying to a number of people. And people were pretty well convinced that couldn't be the last way these things will stand.

So I know we're just about at time. I see there's a question from Iabo. How did de Broglie waves impact the wave particle debate? Thank you, Iabo. So basically, it heightened this even further. I mean, the short answer is it made it even harder to ignore this strange, unsteady, and unstable combination of concepts which otherwise had seemed clear, but separate.

And that's the kind of thing that this novel I just mentioned,*Night Thoughts of a Classical Physicist,* is actually really beautifully helping us appreciate, that it looked dizzying. People would have like a physiological reaction. What's solid? I know what a wave is. And by this point, think about the Wranglers, or Lorenz, or anyone else, there was a huge, highly successful quantitative series of tools with which to characterize wave phenomena-- interference, refraction, diffraction, wave equations, and all the rest.

And then people would say, I know what particles are. And they could use Newton's laws to study ballistics and so on to extraordinary accuracy. And now, it looked like they had to somehow mush these together in a way that didn't-- without any rules, in a way that didn't seem to have any roadmap.

And so the de Broglie hypothesis, especially after 1927 with his Davisson-Germer work-- and they received the Nobel Prize instantly. This, again, was seen as immediately important work when they did those experiments. That was just heightening this clash and made the stakes, the conceptual stakes seem even higher. It's a great question.

And we'll see how that plays out, actually, in the next several classes. It doesn't get resolved. What happens is physicists resolve themselves to keep dealing with this strange connectedness as opposed to saying, now, it's one or the other, as many of you likely know from other classes.

Any other questions on that? I don't want to keep you too long. I'm sure you have a full afternoon. Anyways, good to see you all. We'll pick up the story again on Wednesday.