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DAVID KAISER: We're going to launch in today on the first part of our new kind of material, new set of material on quantum theory. So we talked a bit about the road toward relativity in the first few class sessions. We're going to pivot now and talk about this other amazing edifice of what becomes known as modern physics, quantum theory, as that was getting pieced together over the first quarter century or so of the 20th century.

So for this material, we're going to spend this class session and the next one talking about what came to be known as old quantum theory. Of course, it only became known as old quantum theory once there was a new quantum theory that seemed to replace it. And so physicists themselves introduced the term old quantum theory within the time span that we'll be talking about.

So in their own reckoning, by the mid to late 1920s, physicists began to refer by the term of old quantum theory to this collection of work that had unfolded really between around 1900 and 1924. That looked like the old period, once the new stuff had begun to coalesce in 1925, and '26, and so on.

Now, here, I have an asterisk. I hope you can see that. These dates are approximate. One of the things we'll be really sitting with throughout these first few discussions about quantum theory is just how much in play these developments remained well after the nominal date at which they were put forward.

Much like we saw with relativity, it wasn't that Einstein published his paper on the electrodynamics of moving bodies in 1905, and then the next day everyone woke up and was a devoted Einsteinian or a convinced relativist. Likewise, here with these developments on the early steps toward quantum mechanics, these ideas themselves were unfolding over time. They were subject to sometimes quite wide ranging debate and varying interpretation.

So these dates are really meant to be approximate to help us understand the general flow of this body of work between roughly 1900 and 1924. So I find it helpful to divide up that first period of roughly 25 years or so into a set of developments in which physicists began rethinking the nature of light. And that's what we'll talk about today. So today is rethinking light.

And really interwoven with those developments, as we'll see in our next class session, were a series of equally surprising developments in which physicists were rethinking the nature of matter. They didn't always make that division so clear at the time. But again, with a bit of hindsight, once the newer work began to coalesce, what we now call quantum mechanics, this division of the strands within old quantum theory made a bit more sense.

And again, just a reminder for today, it's thoroughly optional, as always. But for those who might be interested and have a bit more time, I did post some additional lecture notes on the Canvas site that are going to go into a little more detail about parts 1 and 3 for today's material in particular, both blackbody radiation and Compton scattering. So if some ideas go by really quickly or you have no idea where that particular equation came from, there is a bit more that you can delve into on the course site.

OK. So before we even talk about who was rethinking light where and when, it's really helpful and very important to step back and remind ourselves again of where this is happening and why it was happening at that time. So a lot of the work that we're focusing on today's class in particular-- not all of it, but a lot of it was happening within this newly unified country called Germany.

We saw this a few times. There was no country of Germany until 1871. There were German-speaking territories. But a single, unified, national country called Germany emerged really as one outcome of the Franco-Prussian War, the Prussian state war against the country of France not too long before Einstein himself was born.

One of the things that the new country of Germany began to do was to invest very aggressively in a program of rapid industrialization. Once it became a single unified country, the new leaders of the country looked around and were concerned that they were falling far behind other European neighbors in basic industrial capacity, especially Britain. To some degree they worried about France, though they had prevailed in the recent war.

So the country begins investing quite a lot in industrialization. And that often meant investing in science and technology, what we would now call science and engineering. So a few years into this new country's life, its existence, the leaders put together a new kind of institute-- not just new to Germany, kind of new even across Europe-- a specially designed, government-funded institute called the Physikalisch-Technische Reichsanstalt, which is fun to say-- we can just call it by its initials of PTR-- that really stood for the imperial or the German National Physical Technical Institute, the PTR.

The idea was that this new kind of space, this new institution, should foster several kinds of research and try to get them talking together to really help jumpstart what the country's leaders hoped would be this very rapid pace of industrialization. So the PTR was designed on purpose to foster research into basic science, into curiosity-driven research in physics, and chemistry, and related areas, but also support work in applied research and industrial development.

So it was pretty similar to what would soon be developed in the United States. It was originally called the US Bureau of Standards. Now, you might know it by the name the National Institute of Standards and Technology, or NIST, which has several National Laboratory sites throughout the US. The PTR was in that mold. It was really forming that mold-- government-sponsored to try to encourage certain kinds of research, both in basic sciences and industrial applications.

One of the earliest pressing priorities for this new institute was to evaluate competing proposals for public works projects, like large scale electric street lighting. Some of you may know, the incandescent light bulb, the electric light bulbs were actually quite new. People think about people like Thomas Edison and others in the United States, similar work going on elsewhere.

And one of the earliest efforts was to put this new kind of technology, an incandescent light bulb, an electric light bulb, into use in public spaces. Imagine the difference for city life, for commerce, for communities in general, if cities were pitch dark just as soon as the sun went down.

Now, of course, there were many, many competing ideas about how to do that-- how to do it efficiently, how to make the right kinds of bulbs that could give out a lot of light with hopefully not too much power usage and so on And so one of the first questions that this new PTR had to wrestle with was how to compare these competing proposals for things like electric street lighting. What they were asking about is, how can you measure the amount of light that comes out from these very hot filaments in these electric bulbs?

Now people had known for a long, long, long time, a long before the PCR-- humans had known that when materials are heated up to a sufficiently high temperature, they will begin to glow. They'll give off some kind of radiation, some light. Think about very casual observations like embers glowing in a fireplace or charcoals on a grill or now, tragically, the photographs we see from these horrific forest fires out in California and up and down the West Coast.

When you heat stuff up to a high enough temperature, it will give off light. It will glow. And in fact, the color of the emitted light shifts with the temperature to which the object has been heated. So the colors, the main frequencies of light that dominate that glow, will shift with temperature. And that was, again, something that was known casually long before there was a PTR.

Well, that feeds into this very specific work at this new institute, the PTR, because one of the tasks of the new group was to figure out calibrations for these different kinds of electric light technologies, these proposals. And what the PTR researchers began to notice was it looked like there was a kind of universal or shared pattern in the kind of light, in the pattern of light that came out from various objects when they were heated to high temperatures.

And they thought it was universal because it looked like the pattern of light, how much light came out at which particular colors, seemed to depend only on the temperature to which those materials had been heated, but not on the material, the chemical makeup of the materials themselves.

Different kinds of filaments, for example, even if they were made up from entirely different atoms and molecules, would glow with the same kind of pattern once they were heated to a sufficiently high temperature. At least, it looked like that might be the case in these early tests to calibrate these different kinds of electric light fixtures and so on.

So these researchers began to postulate that there was some ideal so-called blackbody. The idea was imagine an object that absorbed all the light that fell upon it and reflected none back. So it would appear to our eyes to be black. It would reflect no light at all. We'd see it as emitting or reflecting no light at all.

So you want to remove any accidents of the kind of light that might have shown on it and concentrate on the light being emitted by this so-called blackbody when you heat that object up to a high temperature. And it shouldn't have mattered whether the blackbody was actually made of wood, or charcoal, or anything else. If it really is a universal glow that you can imagine as this idealized, otherwise unspecified kind of object, a blackbody.

And therefore, when you heat this object, this mysterious or hypothetical object up to a sufficiently high temperature, the pattern of light that would be emitted should tell you something about universal properties, not the accidental features of this or that chemical material.

So this universal blackbody spectrum, or at least the hypothesis that there might be this universal behavior, that seemed interesting for at least two reasons of exactly the kinds of things that this new PTR had been set up to foster. This could be really useful for calibrations and standardization.

Take any new electric light fixture or similar device. You should now have a universal standard against which one could measure its own light output-- what pattern of light would come out from, say, this or that filament because now you have a universal standard with which to compare it.

And it also suggested this might tell us something deeply fundamental and very basic about the interactions between light and matter. It shouldn't matter whether you're talking about this kind of material for a filament or that kind. This tells us perhaps something very universal about light and matter at their core.

So these researchers at this newly generously funded Physikalisch-Technische Reichsanstalt, the PTR-- they began conducting more and more sensitive experiments on the pattern of light, what became known as the spectrum of light, that was emitted from these blackbodies over the course of the 1880s and 1890s, soon after the PTR itself had been founded.

And they began finding curves that looked like this. Now, in the early data-- this is an example of real published data from the researchers, Lummer and Pringsheim-- they often would plot the spectrum, the amount of energy that came out as one varied the wavelength of light.

Typically, these days, we tend to characterize it in terms of the frequency. But remember, that's an easy choice, a trade-off. We can always relate the frequency of the light that comes out, nu-- we'll use the Greek letter nu-- that's inversely proportional to the wavelength, like in these data here. And the constant of proportionality is just given by the speed at which those light waves are traveling, the speed of light.

So let's look at this curve, U. U is a certain kind of quantity called a spectral energy density. That's kind of a mouthful. And again, I go through this in some more details in the optional lecture notes. What these curves are showing is the amount of energy per unit volume in some region of space per frequency-- so how much light gets shown out in the form of-- how much energy gets shown out in the form of radiation in some box of fixed size within unit volume as you vary the color of the light.

And so this suggests you get a certain amount of energy radiated from one color. As you vary the frequency of the light, a different amount of energy comes out, in this case more energy, as you go to a higher frequency, different color, and so on. What the researchers kept finding was that the nature of this curve depended only on the temperature to which the objects had been heated, but not the material composition of those objects. This universal feature became more and more evident as they conducted more and more tests.

And so this is a plot now showing the form of this spectral energy density for three different temperatures. It varies only with temperature. Let's consider a relatively low temperature here, some medium temperature, and some high temperature. Already, we see the trend becomes pretty clear.

As you raise the temperature to which you've heated that blackbody, that object, the amount of energy that's radiated grows. So you get more and more energy coming out. The area under this curve, of the blue one, is much larger than the area under the other curves.

Total energy output, or the intensity, increases with temperature. And the peak frequency, the frequency at which most energy gets emitted in the form of light, that also shifts to higher and higher frequency. So at a relatively low temperature, the peak frequency is a kind of reddish color. As you increase the temperature, it shifts through the spectrum to orange and ultimately to the blue end. So you have this pattern emerging.

So as the empirical pattern, as these empirical measurements in the laboratories became more and more clear, how do you make sense of that pattern? That actually was very, very non-trivial. That was not at all clear to these early researchers. So one of the first to really delve into this to try to give a theoretical explanation for that pattern was Max Planck.

So Planck had only recently moved to Berlin. He was one of the first people in all of Germany to have this full professor, this special Ordinarius professor chair in theoretical physics. Remember, we talked several weeks ago, several classes ago, that until the later years of the 19th century, especially in the German language universities, there would be one full professor, one Ordinarius, for all of physics. And that person was, by default, an experimental physicist.

Only by the later decades of the 19th century were there sometimes two full professors of physics-- one to handle experiments and one for theory. Planck was one of these early Ordinarius professors of theoretical physics in Berlin, starting in the early 1890s. He was not at the PTR, but he was nearby. And he was in touch with his colleagues there.

Planck was an expert, in particular, on the new work on statistical mechanics by people like James Clark Maxwell and Ludwig Boltzmann and others. How do you make sense of large collections of objects like molecules in a gas? How do you assess their collective properties? That's what he focused on.

Because he was now close to the PTR, he began paying more and more attention to this blackbody research as well. And he realized, as many others did, that it was really, really hard to make sense of this characteristic pattern, this seemingly universal shape of this blackbody spectrum, the amount of energy per unit volume per frequency.

And in fact, if you used quite standard-- by that point, very familiar arguments-- from people like Boltzmann and Maxwell, the kind of people whose work Planck was an expert in studying, you should predict a very different behavior from first principles. In fact, it should look like this dashed purple curve, which doesn't look anything like the ultimate curve that was measured. They agree very well at very low frequencies. They both overlap here. But then you see a very significant difference between their patterns.

And in fact, this purple one became known as the ultraviolet catastrophe. I love that term. It's a catastrophe. If you follow the then standard arguments-- and I go through these, again, in some more detail in those optional lecture notes-- then the theoretical model seemed to predict that you should get more and more energy per volume as you raise the frequency. And it should grow without bound. It should never stop. So it seemed like everything should be glowing infinitely all the time. We clearly don't see that.

That argument came from two different pieces. Again, I go through that in a bit more detail in the notes. But the high level summary is that, according to work by people like Maxwell and Boltzmann, very well established by the time Planck began to worry about this stuff, in thermal equilibrium, very generally it had been argued, each degree of freedom, each way that these either the light waves or little bits of matter in that blackbody-- each of these things should share a kind of average energy proportional to the temperature to which the object had been heated. And with a constant known as k, we now call that Boltzmann's constant.

This was known as the equipartition theorem, that each way in which a system could wiggle or move, each so-called degree of freedom, should have an equal average energy. That's step 1. Step 2, using Maxwell's treatment of light, of electromagnetic radiation, the argument was that the number of radiation modes, degrees of freedom per unit volume per frequency should go like nu squared.

Combine those together, you get this expectation that the spectral energy density, u, should grow quadratically, grow as a square of the frequency without bound. It should rise without limit. So everything should be glowing all the time, giving off an infinite energy. That clearly is not what we see.

So Planck, as I say, had special access to his colleagues doing these experiments in real time. He knew better than anyone that the real curves looked nothing like this constantly growing curve of the ultraviolet catastrophe. Everyone knew that these curves must begin to fall with increasing frequency. Planck had an extra insight, or extra information, not just that the curve should fall, but how it should fall the, actual shape of that curve, like this part here-- actually, this part here in wavelength.

How that curve begin to fall as you go to shorter and shorter wavelengths or higher and higher frequencies? So he began to tinker. And in fact, very famously, on a particular date December 14, 1900, he presented a paper to his colleagues at a Physics Society meeting, in which he presented this form for this spectral energy density, that it should rise not as nu squared, like the ultraviolet catastrophe.

But in fact, it should have this a bit more complicated structure. And this was really interesting because it would match very well both the arguments from people like Boltzmann and Maxwell in one region of the graph-- sorry, for very low frequencies, very long wavelengths, so over here on this real data, or over here on this plot.

It converges to the original expectation. But then it has this natural turnaround so that at higher frequencies, at shorter wavelengths, in fact, you should have this gently decaying tail, this exponential tail. Both of those features are contained in this one expression, as you take the appropriate limits.

He also had to introduce this new constant, h, which had not been introduced before. We now call it Planck's constant. That was at least in part to make his units match. The quantity kT has units of energy. T is the temperature, k Boltzmann constant, together form units of energy. Nu here is the frequency, 1 over time, 1 over seconds, Hertz. So that doesn't have the same units as energy. So he added this extra kind of fudge called h to make sure the units would match and also to try to begin to match the actual quantitative shape of the data from his colleagues.

So in this treatment, the very closing days of the year 1900, Planck introduced not only a new form for the spectral energy density-- and there's more on that in those lecture notes-- he also introduces this new universal constant we now call Planck's constant. It's this number, h, just a constant that really sets the scale for these departures based on the calculations we would make from Newton's physics or even Maxwell's equations.

How much do these newer ideas of what becomes known as quantum theory-- how much should they depart? Or when should we expect a deviation? And Planck's value that he inferred from his colleagues' measurements at the PTR was actually remarkably close to the modern value we use to this day. Here's a modern value. Planck's inferred value was pretty close.

In units that are familiar for macroscopic everyday human activities, the so-called CGS system, where we measure distances in centimeters, masses in grams, and times and seconds-- that's where the CGS comes from-- energy, as you may recall, is given this unit of erg, which really is just 1 gram centimeter squared per second squared.

In those CGS units, h is incredibly small. It's exponentially tiny in those natural units for human-scaled affairs. So h is small. If we consider, for example, drop a single grape from a height of 5 centimeters, roughly 2 inches, and then ask what's the kinetic energy that grape will acquire over that very short journey, let the grape fall from rest a total of 5 centimeters. Multiply its acquired kinetic energy times the duration during which it was falling, roughly a second or so.

And you get an enormous amount of energy times time, if we measure in this Planck-- in these Planck units. So just that tiny little grape falling for about 1 second from roughly 2 inches high has 10 to the 28 of Planck units of energy time. Some of you might know that this particular combination of units, or energy times a times unit, erg seconds, is also the unit in which we measure things like angular momentum. What's the momentum of circular motion?

So again, let's take a household example. Instead of dropping a grape, imagine some really annoying housefly buzzing around your head at some radius of a couple centimeters from your head, super annoying. What's the angular momentum of that tiny little fly as it just buzzes lazily around your head? Again, it will have angular momentum exponentially large if we were to measure it in units of Planck's constant-- again, around 10 to the 28.

So this is not a scale in which we expect to find strange quantum features in our everyday life-- grapes, houseflies, automobiles, and so on. And yet, Planck could only make sense of that PTR data if he introduced this new scale, this new constant, very different from human experience, and yet not 0, some finite value, even if it's small, in our human terms.

Now, one of the readings you had for today was by the historian Thomas Kuhn. Kuhn wrote a really fascinating book. Here's the book cover. And the article version that you had draws on the same body of work. And I find this super fascinating. So we know that Planck published that formula, the one that I showed before, where the frequencies go like nu cubed over e to the h nu over kT minus 1. We now call that the Planck spectrum. We use it all the time.

We know exactly when he wrote it down and published it. What we still don't really know, or still is controversial, is what did Planck think he was doing when he wrote down that expression. How did Planck interpret his own equation? Here we are, yet again, thrown into this really, I think, delicious question of how a single equation could be subject to many often competing interpretations.

And Kuhn was among the first to try to argue, in particular, that Planck's own roots to that now very famous equation looked nothing like how we interpret the result. So when we derive Planck's expression-- like, for example, in my own notes or any textbook-- you might look it up in a modern textbook-- we say we get Planck's result for that form for this spectral energy density by making a new conceptual leap by requiring that the energy exchanged between matter and radiation can't take any old value.

It can't be 6, or 6.01, or 6.02 in appropriate units, but has to come chunked. It has to come quantized in these units of a particular size, Planck's constant h times the frequency of the light involved, rather than treating the energy exchange between matter and radiation as continuous the way we would if we were describing light, for example, as Maxwellian waves.

It shouldn't matter what the frequency is of that wave. We know how to calculate the energy associated with any process. And there should be no limit, no chunking or quantization. So Kuhn was asking very directly, did Max Planck think that's what he was doing in around 1900 when he introduced what we now call Planck's formula?

And so just to give the highlight of Kuhn's article-- it's a complicated article. If it was confusing, that's OK. I want to talk through what I consider the headline news, the main takeaways from Kuhn's argument. It's a very complicated argument. Here's what I consider the biggest revelations.

So Kuhn argues that, in Planck's original derivation, this now very famous canonical formula, Planck fixed the total energy of the system, all the imagined little resonators or make believe molecules in that blackbody-- he fixed the total energy of that system to be an integer number of these units h nu.

But according to Kuhn, Planck did not fix each actual resonator, each degree of freedom of that system, to separately have this value h nu so that Planck was fixing the total energy for convenience as a kind of accounting trick, not fixing the unit of each moving part. Whereas today-- again, as I go through in some details in those notes-- we derive Planck's result by fixing the allowable energy of each individual part, each E sub i, so to speak.

OK. In Planck's description, as Kuhn reconstructs it, the energies for each of these subsystems, each kind of moving part according to Planck's original derivation, was actually assumed to fall within some continuous range between E and E plus delta E, not fixed, snapping in place in these quantized units.

So Kuhn goes on. It's a very complicated argument. The book is even more complicated. But according to Kuhn's analysis, Planck uses bins of size-- we might use the Greek letter epsilon-- bins of size h nu for his accounting-- but again, only to say how many of these resonators or oscillators-- again, roughly speaking, moving parts how many of them had energies within 0 and epsilon, within 1 unit, between 1 and 2 units, not that they had to have exactly 1 or 2 units, the way we'd say today.

Moreover, Kuhn goes on again-- he does this in more detail in this longer book. Years later, six years later, Plank was giving lectures at the university about this topic. And he still spoke in his lecture notes of a kind of continuous rather than quantized energy exchange. It wasn't just a momentary lapse in December of 1900, argues Thomas Kuhn. It was years and years during which Plank thought about his own equation quite differently than the way we would today, even though the equation itself hasn't changed.

Now, that's Kuhn's argument. He has lots of evidence that I found very interesting. It's also really complicated. There's a fascinating much more recent article by the physicist and historian Michael Nauenberg you can look up that actually draws the opposite conclusion, that Plank had a much more similar notion to what we have today, even as early as 1900. It's really complicated. So I don't know who of these analysts is right. What I do know is it was really, really not clear, even to Planck's own contemporaries, what exactly to make of this new formula.

And Planck himself was not shy about that. He wrote to a colleague decades later, 31 years after deriving his now famous equation-- he wrote, what I did back in 1900 can be described as simply an act of desperation. He was trying to match the updated data from the PTR. He knew that curve couldn't just keep rising forever. He was desperate.

Introducing these bins of fixed quantized size, Planck continued, was purely a formal assumption. And I really do not give it much thought, which is interesting. So let me pause there. That's what I wanted to share about Planck and the blackbody spectrum. Any questions about that?

Ah, Jade asked a good question in the chat. How did they measure the spectral energy density? Very good. So the short answer is this is the kind of thing that the experimentalists at the PTR were very, very good at. So they could use basically things like diffraction gratings-- and they might have even had access to fine prisms-- to measure very specific energy outputs in very specific wavelength bins.

So that's the kind of thing they were getting very good at. I think they did mostly use diffraction gratings. I'm not positive. So they had a cavity, a vacuated chamber, that they could heat up in an oven with a little hole in the side. So mostly, they had this empty space that would heat up. And they would let a-- it was a cavity-- let a little bit of this light sneak out a little window.

And that's what was taking the place of this blackbody. So no light was coming in onto that box. It was sealed up like a metal trap, cavity. A little light escapes through a little porthole. And so what you should be measuring is only the light due to this thermal radiation because nothing's reflecting on that stuff. That's how they made the blackbody in real life, in the actual experiments.

Then they could subject that light that came out to very fine measurements by splitting it up into its colors with things like diffraction gratings or prisms, and then measure-- let's see. They must have measured the intensity, the brightness. They must have had some kind of photometers. And I'm not sure how they did that. We can look it up.

But they were good at measuring very finely detailed wavelengths. And in fact, they were getting better over the 1890s in various parts of the spectrum. They knew some of the early data points early on, the ones that matched this rising curve that looks like it would lead to this runaway energy, this so-called ultraviolet catastrophe.

The earliest data were at low wavelengths, small frequencies, where it really did go like nu squared. And what was most important, as everyone knew, was to measure the other end of the spectrum. That couldn't keep rising forever. And the researchers, like Lummer and Pringsheim, were getting more and more data at the other part of the spectrum, at shorter wavelengths, higher frequencies, and watching just how that curve began to fall, what we now would recognize as this exponential tail. So they were doing that, again, with more and more precision. Excellent question.

Johan asks a very good question. How did they do that without a graphing calculator? He was no Cambridge Wrangler. But Planck was a pretty well trained mathematical physicist. It was-- and the the other fact, it was painstaking. And also, how did he come up with these particular forms of his equations? You can read Thomas Kuhn's 400-page monograph, which I have forced-- rather, encouraged-- some of the TAs to do directly. I will say encouraged.

I reread it every few years. It's really complicated. I mean, what was Planck doing what was he doing when? What did he think he was doing? That is really complicated. And other really smart, dedicated researchers like Michael Nauenberg come back to some of the same materials, the same obscure lecture notes, the same publications. He says, no, look very carefully at equation 25b. He does something else there. It gets really, really hard.

What is clear, as I'll say actually in the next section, is that some other of Planck's own contemporaries, like a still very young Albert Einstein-- Einstein was convinced that Planck actually hadn't gone far enough. So it's not just historians who debate this the better part of a century later after Planck. Even some contemporaries who were trying to make sense of Planck's own argument thought that Planck's reasoning was, at best, muddled and maybe different from what they themselves thought.

And that's why I like that letter that Planck wrote in the early '30s saying, yeah, I didn't know what I was doing, is essentially how I read that quote. I was desperate. So I think that's really interesting. Any other questions on the blackbody spectrum or Planck's formula? If not, let's go on and see what Einstein begins to do with that in that same famous year of 1905. So let's go to that next part. So now, we're talking about Einstein and what becomes known as the photoelectric effect.

It turns out Planck was concerned about the interaction between light and matter. He wasn't even, in his own writings, talking about the propagation of light on its own for which he just took right off the shelf Maxwell's by then quite standard treatment that light is clearly a continuous wave spread out through space.

And so in 1905, just a few years later, young Albert Einstein, as we still know patent clerk third class, began to think about the nature of light on its own, even when it's not necessarily interacting with matter. This was actually the first of these four kind of amazing or surprising papers that Einstein wound up submitting to the *Annalen der Physik* in that year, 1905.

The first of them that he sent in to the journal, back in March of that year, was the one on what he calls light. Now, Einstein was thinking not only about Planck's work, though he was thinking about that. He had other recent experiments or descriptions of light in mind as well, one of which-- the one that he was even more focused on-- was a series of, again, puzzling experimental results that had just been coming out through the years up through 1902 by the German researcher Philipp Lenard.

If that name sounds familiar, it's because we just talked briefly about Lenard in the previous lecture. Lenard went on, years later, to become one of the front men, so to speak, of that Deutsche Physik movement, one of the people who began denouncing Einstein and relativity starting as early as 1920.

Lenard was conducting these experiments on what became known as the photoelectric effect in the early years of the 20th century. The sharpest, clearest experimental results were published in 1902. He was recognized very early on for that work. He won the Nobel Prize in 1905, the very year that Einstein begins trying to come up with a theoretical explanation for these experimental results.

So what were Lenard's results? Here again, in of cartoon form, are the fundamentals of the experiments that Lenard was pursuing. In fact, others around the world were doing similar things. Lenard had, in some sense, the cleanest data that forced the most sharp showdown with how to make sense of these results. Others were doing similar things.

He had a very simple kind of apparatus. So these two blue pieces here represent metallic conducting plates. And across, Lenard could apply a voltage of an amount he could vary. He had a tunable voltage between his conducting plates. He would then direct an ultraviolet light source-- sorry, an ultraviolet light source onto one of those plates. So he has some source of ultraviolet light shining it on one of those plates.

And under certain conditions, the light source, when it irradiates that plate, would eject, would kick out, some electrons. The electrons would then travel toward the other conducting plate, completing a circuit. So when the light kicked out electrons, ejected electrons from this so-called cathode, from that metal conducting plate, you could complete a circuit because the electrons would now travel through the intervening space and hit this plate.

And you'd know you had electric current flowing because he hooked up an ammeter, a measure of electric current. So he knew he had current flowing. He knew electrons had been kicked out of the metal when the ammeter measured an electric current. And then he could use this tunable voltage-- he could change the amount of voltage applied-- to basically ask, how much energy were those electrons kicked out with, by asking how strong a voltage he had to tune up to block their passage.

So when he applied basically no voltage, the electrons would come across the plate and complete the circuit. So how much countervailing voltage would Lenard have to apply to block their passage, a kind of repulsive electric force to basically repel the electrons away from reaching this far plate? So when will the current stop?

Then he knows what's called the stopping voltage. And that tells him exactly how much energy the electrons had because he had to counteract that much energy, that much electrostatic force, to turn off that current. So now, he could start comparing the stopping voltage, which for him is a measure of the energy with which these electrons are ejected. So he can compare the energy of the electrons with the frequency of this light source.

He shines light of certain frequencies within the ultraviolet range onto this metallic plate. And he measures the amount of energy of the ejected electrons. And again, in cleaned up form, the data started looking like this. It had a very specific, very striking shape.

So on a plot of the energy of the ejected electrons versus the frequency of the incident light, the energy electrons seemed to rise linearly, but only above some threshold frequency. So as the light was tuned to lower and lower frequencies-- the light that's being shown from this source-- there would be no electrons ejected at all. There would be zero electrons ejected, no energy crossing that gap.

After you reach some very specific threshold frequency of the light, you start kicking electrons out. As you increase the frequency of that light, the energy that the electrons carry rises. The energy of the electrons seems to rise only with-- seem to depend only with the frequency of that light, not with the intensity.

You can make this a brighter or less bright light source, holding the frequency fixed. And that had no impact on the stopping voltage, on the amount of energy required to stop those electrons from traversing that gap. That seemed pretty strange. Why was that so strange?

Why was it strange to have a threshold frequency? And why was it strange to have the energy of the electrons independent of the intensity of that light, depending only on the frequency of the light? Well, again, if one took on Maxwell's by then quite standard description of electromagnetic waves, including in the ultraviolet part of the spectrum, the energy carried by those light waves from Lenard's device should have been proportional to the wave's intensity.

The energy, script E here, was proportional to the intensity of the waves. The intensity, in turn, went by the square of those field strengths, the electric and magnetic field strengths. So why shouldn't Lenard have been able to get very large amplitude fields, high intensity fields that happen to have a long wavelength, that were a low frequency? That should carry as much energy as a short frequency wave, but of a smaller amplitude.

Meaning, why couldn't he tune the intensity up and still get electrons ejected all the way down here? If light really acted like Maxwellian waves, why should there be any threshold frequency at all? You could always tune up the intensity of lower frequency light, was at least the expectation.

Likewise, according to Maxwell's work, turning the argument around, once you do kick electrons out above that threshold, why should the energy that they carry be independent of what people thought would be the energy imparted by the light? The energy imparted by the light, again, should vary like the intensity. And yet, the energy that these electrons get kicked out with seemed not to depend on the brightness, the intensity of the source, only on the frequency.

OK. So that was becoming increasingly clear experimental data, so clear that Lenard received the Prize just a few years later, the Nobel Prize. But it was by no means clear how to make sense of that combination of results. So Einstein's first paper of this remarkable year of 1905 was trying to tackle this directly.

In fact, in a letter to one of his friends from the Olympia Academy-- his friend, Conrad Habicht-- here, Einstein actually called this his most radical of all the papers he was working on that year. He thought this one was the most strange or most unexpected-- more so than relativity, more so than all the rest.

So what he offers he calls a heuristic explanation. You see in the title, [GERMAN], a heuristic kind of hypothetical or suggestive explanation. He's not saying, eureka, I found it. He's being maybe uncharacteristically a bit more modest here. He says, here's one way to think about these results.

A heuristic idea would be to say, what if light itself were quantized? What if light traveled through space not like a continuous spread out wave, like Maxwell's theory would suggest, but instead like a collection of localized quanta? Here's a quotation from this paper in 1905 translated to English.

He writes that, it seems to me these observations about the production of cathode rays-- those ejected electrons off that cathode by ultraviolet light-- he's now talking about Lenard's experiments. It seems to me these observations are more readily understood if one assumes the energy of light is discontinuously distributed in space.

The energy of a light ray spreading out from a point source is not continuously distributed over an increasing space like a wave would be, like a Maximilian wave, or an ocean wave at that, but rather consists of a finite number of energy quanta, which are localized at points in space, which move without dividing-- they literally can't be further divided. They are quantized-- and which can only be produced and absorbed as complete units.

These become known as light quanta, as little particles or corpuscles of light, rather than what was by then, going back to 1800 let alone the 1860s, the taken for granted assumption that light was a wave, going all the way back to, say, Thomas Young's work or the work of several French scholars.

So Einstein is suggesting heuristically-- he's not saying this must be the case. He's saying it's suggestive and worth considering that experiments like Lenard's might make sense if we change an everything we know about light. So why would that help? Let's go back to this cartoon version of Lenard's data. Einstein was, in fact, following Lenard's experiments, the publications quite closely-- much, much more closely than anything like the Michelson-Morley experiment. He really was looking up Lenard's papers and looking at the graphs and tables very carefully.

Einstein recognized that it wasn't only a linear relationship between the energy of those ejected electrons and the frequency of the incident light. But the slope, in particular, looked awfully close to this new value that had just been introduced in a totally different context by Max Planck. The slope of this plot looked very much like Planck's constant, h. That's something that Einstein begins to recognize by going carefully over these experimental results.

So that leads Einstein to think in the following way. What if each of these discrete little bundles of light energy-- what if each individual light quantum carried a fixed amount of energy proportional to the frequency of light with the constant of proportionality being this new constant introduced by Max Planck? What if light quanta, these indivisible pellets of light, had to carry a fixed, quantized amount of energy, not some continuous range that could have been any old value?

If that's the case, then you have on this photocathode-- on this conducting plate in Lenard's experiment, you have raining down on it pellets and pellets each carrying a fixed amount of energy, h nu. So then you can imagine an individual light quantum, not some spread out wave-- an individual particle basically smacking into an individual electron. Now, it looks like colliding billiard balls.

So how will the energy of the electron change after it gets smacked by a discrete pellet of energy, a light quantum with energy h nu? The electron should acquire a predictable amount of energy. It should absorb that energy carried by the light quantum. And yet, there will be some extra energy holding that electron bound to its atoms and molecules in the metal. That became known as the work function. That's kind of like the binding energy.

These aren't free electrons in space. These are electrons bound to a piece of metal. There's some intermolecular or atomic forces. Whatever they are-- and they might vary by material-- there's some function that is often called by the Greek letter capital Phi-- again, a kind of binding energy. And so the electron won't be kicked out of that piece of metal unless the energy it absorbs from that discrete light quantum exceeds that binding energy. The energy holding it in place has to be overcome or exceeded by the light-- by the energy transferred by that light quantum.

If that's the case, then, of course, there should be a threshold frequency. Only when each individual quantum of light, each little pellet, discrete carrier of energy in that incoming light carries enough energy individually to overcome that binding energy or the work function-- only then would any electrons be ejected from that piece of metal. So the threshold frequency should depend on that binding energy, that work function, divided by Planck's constant.

Basically, what's the threshold that would set this energy exactly to 0, as opposed to it having a negative energy, which would say it's bound in place? So you just solve for when does the electron just cross that threshold as energy. Ah, it's when nu equals phi over h. That would give you the threshold. Any additional energy, any higher energy carried by light quanta, as you tune the frequency of the incident light higher and higher, each electron will absorb more energy than that threshold. And it will continue to grow linearly with slope h.

So the idea that Einstein puts together-- this heuristic idea, this suggestive idea in this 1905 paper-- is that the light shining on this metal plate is not some extended continuous wave lapping up on shore like a Maxwellian wave, but actually a shower of discrete marbles, each of which could collide like two-body collisions, like billiard balls on a billiard table, or on a pool table. They would have two-body collisions, each of which could then impart certain energy to their colliding electrons.

So I like to think of it this way. Imagine electrons are like bucket full of ping pong balls. So ordinarily, they're bound in place. That's like that work function. The ping pong balls aren't free to move any old place. They're stuck. They have a certain kind of binding energy. Think of that as like that work function phi.

Then you start chucking marbles at it. You imagine flinging marbles into that bucket. If the marbles individually carry enough energy, they're going to collide with individual ping pong balls. And if the marble's incoming energy is more than this binding energy holding the ping pong balls in the bucket, you'll kick a few out. That's the picture that Einstein has in mind.

In that same actually quite long article, this very same article in 1905, Einstein also revisits Planck's derivation of the blackbody formula, that function that we called the spectral energy density, little u. And he rederives Planck's formula assuming that the radiation being emitted from that cavity radiation could be treated like a gas of these individual particles, each of which has a quantized energy E equals h nu.

This is the modern derivation that we inherit and use today that's much more like the derivation I give in those brief optional notes. So in the same article, Einstein both offers a heuristic explanation for Lenard's otherwise quite puzzling experiment results, he revisits and reinterprets Planck's own by now rather well-known expression for blackbody radiation, all from the starting point of view that light is quantized and each individual light quantum carries a unit amount of energy set by h nu.

So we might figure, OK, he solved everything. Everyone must have been convinced, right? No, no, no. We've learned from this course already, things hardly ever work that way. And in Einstein's case in particular-- remember, he was still a little known patent clerk out of the main elite centers of research-- this was facing enormous skepticism, even years after Einstein himself became much more prominent.

It was really more than 15 years until the majority of members of the physics community really took this idea at all seriously. And here's, again, one of my favorite examples of that. Here's, again, our old friend, Max Planck, writing this, a letter of recommendation. He's trying to say how great Einstein's work is by 1913. Now, Einstein is no longer the unknown patent clerk.

Planck was trying to convince his colleagues to offer a very prestigious new job to Einstein at the Prussian Academy of Sciences, which, in fact, Einstein would soon be offered. He would move to Berlin. And in his letter saying how great Einstein's work is, Planck says the following, "In sum, one can say there is hardly one among the great problems in which modern physics is so rich to which Einstein has not made a remarkable contribution." That sounds nice.

"But he may sometimes have missed the target in his speculations, as, for example, in his hypothesis of light quanta cannot really be held too much against him," which is to say, a lot of the work this person has done is very interesting. He's clearly mistaken about light quanta. But let's let him into our new club anyway. I find that terrific. OK. Let me pause there again to ask any questions on the light quantum learning.

Fisher asks, in Einstein's paper, did it make any nods to Newton's original thought that light was corpuscles? Very good. Honestly, I don't remember. That's an excellent question. Let me back up. Others might know as well. But it's worth emphasizing.

The big picture question-- does light consist of waves or particles-- that actually has a really long history. I guess I gave a little preview of that, although I didn't linger on it, even in one of our first class sessions. For this class, you might know it from other readings or other classes.

The reason it was actually a big deal around 1800, going back to early in Michael Faraday's own career-- it was a big deal to think of light as waves because, since at least Newton's day-- another century and a quarter before that, going back to the later years of the 17th century, the 1660s and so on-- Newton and other leading scholars had convinced themselves that light actually consisted of a stream of particles. Newton called them corpuscles, exactly as Fisher says.

So Newton was convinced that light was corpuscular. We might now say the light was made up of a stream of particles. That's in 1660s, '70s, '80s. It would become super influential. He writes a whole book called *Opticks,* Newton does, first published in around 1703 or something like that. It was like, OK, Newton has spoken. Light's particles. Got it.

And so it was a pretty big deal when, about 100 years after that, the consensus shifts, both in Britain, and in France, and ultimately many, many other places that, hang on, light seems to have all these wavelike properties like interference, and diffraction, and refraction, and all these things that helped set in motion the ideas of a light-bearing ether. If light is a wave, what is it a wave in?

So you have these huge century-long shifts between assumptions about just what's light made of in general, let alone these specific questions about how it interacts with matter. So it was certainly well-known well before Einstein's day that Newton had been, let's say, a corpuscularist. But it's a great question, Fisher. And I'd have to go back to my own copy in English translation by [INAUDIBLE] to see whether Einstein paused to reflect on that. I don't remember. Interesting question.

Gary asks, having written as a heuristic, did Einstein believe it'd be quanta in 1905 or only later? Very good, Gary. So my best understanding-- and again, I haven't looked as directly in this. But from my colleagues who have really pored over Einstein's, at the time, unpublished correspondence, his notes, and so on-- again, we have a huge documentary base of how his thinking was evolving in those years.

It seems that Einstein thought this really, really was true. But for once, he kind of hedged rhetorically. So it seems that he was offering a heuristic explanation and going through actually a whole number of episodes or scenarios where this suggestive hypothesis might be helpful. But it seems that he thought that this was not only a fiction. He was, however, not sure how to square this.

In 1905, he wasn't sure how to square that with a centuries worth of evidence that light had wavelike behavior like reflection, refraction, diffraction, interference in general. So it's not that Einstein said there's nothing ever wavy about light, it's always only particles. But he was certainly-- and in his own more private musings, he seemed to think that one had to adopt this it's really like particles a position in some scenarios more than others.

He was groping toward what we see actually in the next part of today's class and what will chase us through upcoming class sessions. He was beginning to grapple with what would come to be called wave particle duality. He certainly wasn't calling it that yet in 1905. But as his additional documents from the time seemed to suggest, he was already grappling with we really, really need to treat light like waves for certain scenarios-- interference fringes being an obvious one.

And yet, in his own heart of hearts, he seemed pretty convinced that this corpuscular or quantum nature seemed more than just a coincidence in other kinds of scenarios. And that would become more and more of a paradox to some or a challenge, even beyond Einstein himself. And so the real question actually becomes, why was Einstein hedging his bets rhetorically rather than what he really believed?

Because we saw it in his paper just a few weeks later that he submits on the electrodynamics of moving bodies, he's certainly not shy about dismissing the ether. He's making other rather bold and almost irresponsible statements, given the standard assumptions of his day. He wasn't shy, in 1905.

With this particular one, I imagine it was more like he didn't yet know how to square the circle here of what comes to be known as wave particle duality. So that's a great question. And again, we'll see an example of that even in the next part today. And we'll see echoes of that throughout the next few classes. Good. Any other questions on the photoelectric effect, on what was Lenard measuring, on Einstein's explanation? It's all pretty clear? OK.

Right. Good questions. Let's look at this last part for today. Why did anyone begin to take light quanta a bit more seriously? One of the most compelling sets of new information, new inputs for this came yet again from a laboratory in the United States, which, again, is itself still pretty unusual.

This was among the next set of really significant experimental work in physics that got even the experts, sometimes very snobby experts in Western Europe, to pay attention. This was now decades after Albert Michelson. And the Michelson-Morley experiment is now the early 1920s, not the 1880s. But this was, again, an example held up almost immediately for real acclaim.

A series of experiments performed by the physicist Arthur Compton at the University of Chicago in 1922-- here's Compton. He looks like a kind of movie star. You might recognize the name Compton. Arthur's brother was Karl Compton, also a trained physicist. Karl Compton then went on to a different kind of career. He became the president of MIT starting in 1930. And he served for nearly 20 years.

So a number of enormously consequential reforms at MIT were put in place by Karl Compton just at the moment or around the same time that his brother Arthur Compton was experimenting on the nature of light. And again, as I say, this is one of the sets of new kind of inputs coming out of US-based laboratories that really began to make even experts in Europe pay close attention.

So Compton, Arthur Compton, had not set out to test Einstein's hypothesis. He, like everyone, practically everyone in the field, remained pretty skeptical, much like in that letter from Max Planck from 1913. They figured Einstein, by this point, was pretty smart, had some amazing successes to his career. But this light quantum thing was probably not one of them.

Compton instead was really interested in the behavior of high energy x-rays and how these x-rays would interact with little bits of matter. So he was conducting experiments by basically beaming x-rays, very high energy electromagnetic light-- as far as he was concerned, Maxwellian waves of very short wavelength, very high frequency-- and bouncing them off of graphite, basically carbon molecules. So he wanted to bounce them off the electrons in carbon.

What Compton was measuring in particular was change in the wavelength, in the wavelength of those electromagnetic waves, the x-rays, after scattering. So you might be familiar, of course-- for any of these Maxwellian waves, we can measure the wavelength, the distance, the physical distance between neighboring crests, neighboring peaks, of, say, the associated electric field.

So what Compton was doing in his laboratory was measuring the wavelength prior to scattering and the shifted wavelength following scattering. So the prime here means after the scattering. He knew the wavelength ahead of time. He could control that with his apparatus. Very much like the PTR, he could use very finely spaced diffraction gratings and similar techniques to measure the shift in wavelength after the x-rays have basically scattered off these electrons.

He found that there was a shift. The wavelength following collision was not equal to the wavelength prior to collision. And there was a very specific angular dependence that the angle at which these x-rays bounced off, this angle theta, was somehow related to the degree in which their wavelength changed. That is to say, the shift in wavelength went like 1 minus cosine of the light wave's scattering angle.

And that's what he could measure. You can see here it's mounted on a wheel. He could actually pivot his diffraction gratings essentially and measure the scattered light at different angles as it bounced off that graphite target. So Compton was convinced that this could be made sense of using totally standard, by that point, Maxwellian electromagnetic waves. These were high frequency Maxwell waves scattering off of a certain target.

And yet try, and try, and try as he might, he could not come up with any sensible explanation or accounting for this empirical relationship. When he kept finding conducting the experiment measuring more and more carefully the shift in frequency with the scattered angle, he couldn't come up with any kind of theoretical explanation that would account for that shift until, in yet another act of desperation much like Max Planck, he then grudgingly tries on-- takes on this Einstein hypothesis or heuristic suggestion.

Only when Arthur Compton, again kind of grudgingly, adopts this suggestion from Albert Einstein to treat light-- in this case, the very high energy x-rays-- as little collections of discrete particles each with their own discrete packets of energy rather than as Maximilian waves-- only then can he make sense of this empirical pattern in his laboratory.

So here's the diagram from his actual published article in the*Physical Review.* It was submitted in the very closing days of calendar year 1922. It was published spring of '23. So here's the actual illustration from his article. And again, in the accompanying optional lecture notes, I go through the algebra a bit more slowly. So I'm going to show you where it gets. Don't worry if some of these steps are hard to do in your head. They're hard for me to do in my head. Take your time, if you'd like, and go through the algebra in the notes.

So what Compton winds up doing is treating the interaction between the incoming x-ray-- that's what's here. He now, by the published version, has decided to call this incident light quantum. He's already now borrowing Einstein's ideas. It's not restarted. Pardon me.

So here's the incoming x-ray. And how would you characterize the energy or the momentum of that incoming source of light? That's what Compton starts with, before collision. So even by using Maxwell's theory, people knew they could calculate an associated momentum carried by a light wave as related to things like the Poynting vector, if you want to get super fancy.

But basically, Maxwell's equations suggested the momentum carried by radiation, by light, is equal to the energy carried by that light wave divided by its speed. That was, again, a classical result, even from wave theory of light. And now, Compton starts adding in this new heuristic stuff from Einstein. What if each individual light quantum-- and now, I'll be a little anachronistic and just use the term photon. That's not the term that Compton used. It gets introduced soon after Compton's results.

Within the 1920s, it had already become common to use the term photon, as we still do today, to describe these light quanta. So now, let's take on Einstein's suggestion that each individual photon of light carries a quantized amount of energy proportional to its frequency. Then let's use the usual relation for any wave between frequency and wavelength with the constant of proportionality, the speed.

So now, you can come back to this Maxwellian expression for the momentum carried by a classical light wave, add in this new Einstein-like quantum thing, and come up with an expression for the momentum carried by each individual photon, or light quantum. And that would be the energy divided by its speed, h over lambda. Remember, that's not where Compton starts. That's where it gets out of desperation.

So now, Compton has an expression for both the momentum and the energy of this light quantum prior to scattering. He's going to treat this, as I say, like a two-body problem, as if these were two billiard balls rather than like a wave scattering off of a little chunk of matter. So prior to the collision, he's going to say this electron-- over here, this target, this little dot here-- is just sitting there at rest. Its momentum is zero. It has no velocity.

And it has some rest energy, mc squared. Remember, by 1922, Einstein's work on relativity was indeed much more better known and seemed to have passed a number of tests. So Compton had no qualms about borrowing Einstein's work on relativity, including things like this rest energy, mc squared, even though he was not going into this convinced of the light quantum stuff.

So now, we have the ingredients we need to describe the two scattering billiard balls, the two objects, prior to collision. We can characterize the energy and momentum of the incoming x-ray as a collection of discrete pellet-like light quanta. We have the energy and momentum before collision of the target, this electron just sitting still about to get smacked.

After the collision, the light quantum gets scattered off at some angle, theta, compared to its incoming direction. That's the angle theta here. It now has, in general, some different wavelength. So the momentum of that photon-- Compton now taking on Einstein's work-- is going to be given by some universal constant. That hasn't changed. That's just Planck's constant. But the wavelength indeed might have changed. In fact, he was measuring exactly that change in wavelength.

Likewise, the energy of the photon therefore will change because its wavelength will have changed. So he has now an expression for energy momentum of a light quantum following collision. And likewise, the electron has now been smacked by some very high energy pellet, this high energy light quantum of the x-rays.

So now, it will recoil in some other direction, some angle-- let's call it phi-- after getting smacked by the incoming light quantum. So now, it has some non-zero momentum. In fact, if it's a high enough energy incoming x-ray-- x-rays are very energetic light waves in general-- then the electron might actually acquire a relativistic momentum. Its recoil speed could, in general, be comparable to the speed of light. It could get quite a jolt from that collision.

So again, Compton uses by then the standard relativistic expressions for momentum and likewise for energy. And again, I have a set of lecture notes that were optional on the Canvas site from when we talked about Einstein and relativity to go over things like E equals mc squared, relativistic momentum, and all that.

So you have a chance to go back to those notes, if that's not familiar. For what we need to know today, this stuff was, by that point, standard issue. Compton was doing nothing controversial here in adopting the relativistic energy and momentum for the scattered electron.

OK. Now, he does what anyone would do when you have two particles colliding-- not a wave smacking into the shore, but two discrete little bundles of energy that collide like billiard balls on a pool table. One thing we have to do in any two-body collision is conserve energy, just as we would do in ordinary Newtonian mechanics.

So what's the total energy coming in before collision, the incoming energy of the photon, this one here. The incoming energy of the electron is just this rest mass. And that total energy has to balance the total energy of the system following collision. So he has his corresponding expressions for the energy of the photon and the electron following scattering. And we can now fill in from his table what those values should be.

Likewise, we have to conserve momentum. As we all know, momentum is a vector quantity. So we have to conserve momentum both in the x-direction along the original direction of travel for the incoming light quantum, as well as in the perpendicular direction, in this case the y-direction. And so again, this is done in a bit more detail in the notes, if this is hard to parse in real time. But the upshot to this it Compton's doing completely standard stuff for two-body scattering, once he's done the non-standard thing of treating this light as an incoming pellet or particle.

So now, what's so great is Compton has three equations and three unknowns. I've just now rewritten those expressions we just had. By treating the problem like ordinary two-body scattering between discrete localized particles, Compton has three expressions-- conservation of energy, conservation of momentum in each of the two perpendicular directions-- and he has three unknowns. He doesn't know what the scattered wavelength is. And he doesn't know what the angles of scatter are-- either the angle theta for the light quantum or the angle phi for the recoiling electron.

So now, it's just algebra. He has three equations for three unknowns. And as you'll see in the notes, now it's really just a short number of steps to relate the change in wavelength, the lambda-prime following scattering, compare that to the incoming wavelength, which you can control. He controls the x-ray source. And it gets related in an automatic way to that scattering angle in exactly the form that Compton had been finding empirically.

Not only does he get the angular dependence correct, or at least matches his empirical results, he actually even finds a quantitative shift, this coefficient. The amount by which the wavelength should shift is now fixed by these universal constants-- Planck's constant, the mass of the electron, that target, and the speed of light.

And so Compton is treating light like particles. And he's measuring changes in wavelength. Just going back to Gary's point from a few moments ago, Compton is now finding further examples where half the time he has to pretend that light is just a wave, because he's measuring wavelike properties-- like wavelength instead of necessarily a wavelike property. It's the distance between crests of a wave, after all.

That's what he's controlling and measuring. That's his empirical input, is a wavelike quantity, a shift in wavelength. And yet, he's accounting for that by talking about a discrete particle scattering off another discrete particle. And so this becomes known as the Compton wavelength, this combination of fundamental constants, that tells us how much the wavelength of the scattered light should shift as one changes the angle, what's the overall scale by which this wavedness should shift.

And so this becomes a kind of yet another hybrid or blend of particle-like and wavelike ways of reasoning about light. As I mentioned I think in the lecture notes for today, this work, much like Philip Lenard's, was immediately greeted as very important. Compton received a Nobel Prize, I think, by 1927. The paper was published in spring of 1923.

So much like with Lenard's work, just a handful of years passed between the very first publication of these experimental results and the work being honored with the Nobel Prize. So people were paying attention to Compton's work very closely, very carefully in real time.

And it was this kind of work that began to convince even the remaining skeptics that Einstein's heuristic suggestion that light should be treated at least in some aspects like a collection of particles-- that finally begins to gel and achieve something like community agreement whereas in Einstein's his earlier expressions that had been lacking.

OK. Let me sum up. Then we'll have time for some more questions. So these three moments in what became known as old quantum theory-- each moment is physicists grappling with the nature of light. And remember, this is not happening in a vacuum.

To begin to understand why anyone cared at all, let alone invested with such priority in these particular experiments, that really does go back to larger framing of a newly independent country of Germany, decisions by its leadership to invest in industrialization to make new kinds of places like the PTR and so on.

It's within those spaces for specific industrial applications, like electric street lighting, that they were really committed to studying things like blackbody radiation and what it might reveal about universal properties of light and matter.

Max Planck had just moved to Berlin as this full professor of theoretical physics, still a pretty rare job to hold within any German University. He was close by to PTR. And he was getting updates from his colleagues, sometimes day by day, with their increasingly precise measurements of that spectrum, the pattern of radiation emitted when you heat up any material to a sufficiently high temperature.

No matter how he got there, by the end of December, Planck had introduced this now-famous expression for the spectral energy density. To get there, he had introduced this new constant of nature. We now call it Planck's constant-- very, very tiny on human scales, and yet not zero. And then we saw that whatever Planck thought that meant, within a few years, Einstein takes that up and, in a sense, treats it more seriously than Planck himself had even done.

So Einstein not only rederives Planck's result from a very specific conceptual starting point-- what if light consists of collections of discrete quanta, soon to be called photons? And that same new concept, new set of ideas helps Einstein make sense of experiments like Lenard's on the photoelectric effect. However, as I've emphasized many times, that seemed to be compelling to Einstein in 1905, but to very few others for years and years later.

And it really took the better part of two decades until consensus began to converge around this idea of light quanta, or photons. A very important piece of that was these new set of experiments by Arthur Compton at Chicago, where he could only make sense of his own new results by doing this kind of dance, by talking about x-rays as waves with wavelengths, and then accounting for the shift of those wavelength properties in terms of the particulate, or quantum-like scattering of discrete bodies.

So I'll stop there. We've got time for some more questions and discussion. So let's see. Stanley writes, is there a reason Compton was able to measure-- was able to assume energy was conserved? Oh, very good. How did he know energy was not lost? In a sense, that was actually an assumption, Stanley. It's an excellent question.

And in fact, it's a very powerful question because, at around the same time, other leading figures, like Niels Bohr himself-- almost exactly the same time, by 1923 and '24, in fact. Niels Bohr, in responding to other curious features of recent results, suggested that maybe energy is not conserved at the scale of atoms and parts of atoms. Compton didn't go that far. And most of his colleagues never went that far. Even Bohr switched back and said, nope, energy should be conserved.

So Compton was basically taking on board the assumption-- just an assumption-- that energy is conserved always even at the atomic scale. That was not proven. That was an assumption. What Compton was trying to do was basically make as few changes as needed to a straightforward way of analyzing the scattering data. How would any of us analyze, say, the collision of two balls on human scales, two billiard balls, a baseball and a baseball bat, or any of these things?

We would assume energy is conserved. We would assume momentum is conserved in each of its relevant components. And that's what Compton is doing. And that was actually what most physicists continued to do. Although, you're quite right to point out, Stanley, that really was an assumption. It wasn't written in stone. It wasn't guaranteed.

Nowadays, when we look back, we have tons and tons of reasons to think that energy really is conserved, even at the atomic scale, even when we think about light quanta. That was by no means hard and fast evidence at the time. Compton assumed it. And that helped him get the results that he otherwise was aiming to explain. Excellent question.

Obi asks, it's interesting that Einstein accepted this duality because he rejected the ether before because it required conflicting explanations. Obi, I agree. That's an excellent point actually. You're totally right. So one might have expected him to say, the assumption of Maxwellian waves is merely superfluous, right? That was the same language he used about the ether.

You've got me back to Gary's point from earlier. I wonder if Einstein didn't make that move here because he himself knew that there were things that he thought wavelike properties really could explain. I think Einstein had convinced himself, by 1905, that invoking the luminiferous ether didn't help people understand anything that it was meant to explain, that the ether was meant to explain.

Thinking about light as waves still had explanatory power for things like interference, diffraction, refraction. So I think he was-- I'm assuming. We can go back and look more. And other scholars look more carefully at this. My assumption is that Einstein didn't want to give up on waves as never being helpful to think about light because of this body of evidence that he thought really could be for which a wave explanation was actually helpful.

Whereas he had convinced himself, partly from his reading of Ernst Mach and so on, that invoking an ether seemed never to be helpful. Wavelike behaviors, there were all kinds of phenomena, even in casual daily experience, let alone in precision experiments, where things like interference, fringes, and so on were essential.

That's my guess for why Einstein called this heuristic, didn't announce the notion of thinking about light waves again. And again, Einstein becomes one of the most, frankly fearless-- one of the most conceptually daring, I think is the word to say, in really trying to pursue this wave particle duality over the next two decades. He really sits with it. He doesn't say, oh, this hurts my head. It must be one or the other. He actually digs in actually, in a series of developments throughout what becomes known as quantum mechanics.

So he really found this delicious and enticing. Maybe those are the wrong words to use. But he certainly found it worth sitting with, as opposed to it must be A or B. And we'll see some examples of that again in the coming class sessions. Excellent observation, though. Any other questions?

Since I have three minutes, I'll share the following. One of the things that was done by not Arthur Compton but by Karl Compton soon after he became president in 1930, president of MIT-- and this is for all you physics majors. And my apologies to every other engineering major. You know who you are.

Compton decided that MIT had become too closely aligned to both engineering and industry. He thought, in Compton's own words, that MIT had sold its soul to industrialists. And so it was time for MIT to redouble on the basic sciences. It was at this time, starting in 1930, when Compton, Karl Compton, put into a reform that every single undergraduate at MIT had to take two whole years of physics.

You little wimps-- kidding, just kidding. You wonderful students only have to take one year of physics because MIT relented in 1965. For 35 years, Karl Compton's vision held. And literally every MIT student, whether they were management majors at the Sloan School-- I'm looking at Gary-- or economics majors, or history, or mathematics had to take two years of physics. It wasn't quite the Cambridge Wranglers.

But we had two years because Karl Compton, the physicist, was convinced that we had to have a whole new emphasis on the so-called basic sciences. It's at this time when the laboratory requirements start coming in for biology, and chemistry, and mathematics. And it was only 35 years later, in the mid-1960s, when some colleagues long after Karl Compton stepped down said, maybe that's too much physics for every single student.

And honestly, when I started teaching at MIT in 2000, some of my by then rather senior colleagues still remembered 1965 and thought everything had gone downhill since then. Those people have since retired. But MIT has never been the same, in their point of view, since we gave up forcing/inviting every single undergraduate to take two whole years of physics. So that's one of the legacies of Karl Compton, not Arthur Compton.

It's true. Alex puts in the chat-- weren't the problem sets originally questions from industry? Many of them were, in fact, if not problem sets, then certainly research projects. There was a kind of research for hire program, which never really ended, but was a very high focus at MIT starting soon after the First World War, in the years right after 1918.

It was called the MIT Plan or the Tech Plan. And roughly 12 to 15 years into that, there was a kind of course correction. And people like Karl Compton said, we have to rejigger that. Fisher says, I assume this requirement naturally only applied to physics and-- oh, no.

That's right. Fisher's exactly right. So the requirement of two years of physics coursework applied only to physics for every single undergraduate. And grudgingly, if you had to take chemistry or biology, fine, that was required. You have to take laboratories. You had to take at least one year of mathematics.

So we can recognize parts of our so-called GIRs to this day survive from Karl Compton's era, even though critically the one horrible moral lapse, according to some of my colleagues, was dropping the two years of physics in place of only one. I leave you with that thought.

I invite you all to take more than one year of physics. I think many of you have been anyway because you're physics majors. That's Karl Compton. We'll actually talk more about Karl Compton at MIT in the coming weeks when we talk about MIT, and the Second World War, and radar. We'll hear Karl Compton's name again soon. In the meantime, we'll pause and sit with Arthur Compton's results for which, again, there's more of the algebra in those notes, if that went by too quickly.

Any other questions about this material? Anyone wish that everyone had to take two years of physics? Or any other comments on today's class? If not, I'll leave it there. I'll wish you good luck wrapping up your paper 1 draft. Please don't forget, it's due this Friday. And we'll pick up the story of old quantum theory again with our next class. Thanks so much, gang. Stay well.