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Robert Hofstadter and Nuclear Bombardment

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Abstract: I will re-analyze the data from three of Hofstadter's important papers from the 1950's, showing that the mainstream has misinterpreted all of it. Not only do they misapply the form factor math, getting the radius for the wrong particle, they also misapply the diffraction math, obtaining the wrong cause for the maxima and minima in the graphs. Using my nuclear diagrams and straight mechanics, I will show the real cause of the data. In doing so, I will also be able to throw out the elastic/inelastic explanation, replacing it with a mechanical explanation that is much to be preferred.

In my first paper on nuclear structure, I mentioned the research of Robert Hofstadter from the 1950's and 60's. Hofstadter was a researcher at Stanford, and he won the Nobel Prize in 1961 for his work on bombarding the nucleus with electrons. More recently I have begun studying Hofstadter's old publications closely, which has led to this paper. In Hofstadter's 1957 paper NUCLEAR AND NUCLEON SCATTERING OF HIGH-ENERGY ELECTRONS[1], we find this graph [fig. 17]:

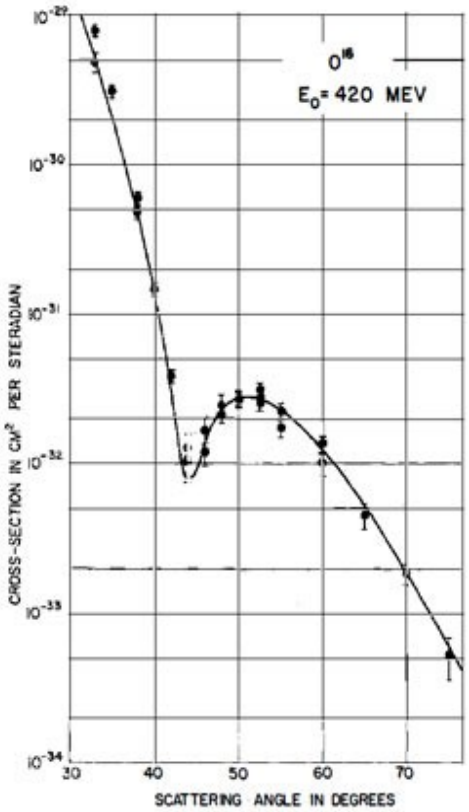


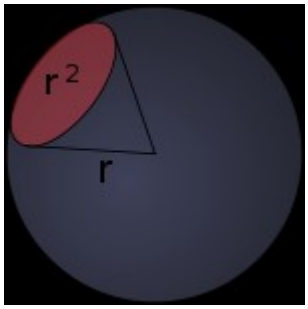
FIG. 17. New experimental data of Ehrenberg *et al.* (64) obtained at 420 Mev for the O^{16} nucleus. The figure also shows the exact calculations of D. G. Ravenhall for the harmonic-well model for which α=2.0 and a_H=1.75×10^{-13} cm.

That is the graph for the Oxygen nucleus. Hofstadter and others have used that data (and similar data) to calculate a radius for the nucleus, but I will show in this paper that they have misread the data. The turn-around point there is at a size of about 10^{-16}cm, as you see (just take away the square). Since that is where the line reverses, they take that to mean the interaction is indicating the size of the nucleus. They then plug that into some complex (and faulty) equations to calculate a radius of Oxygen of about 2.6fm. Unfortunately, they have once again made a simple and basic error in mechanics and data analysis. The turn-around point on the graph doesn't indicate the size of the nucleus, it indicates the size of the electron. *They are measuring their bombarding particle, not their bombarded particle.*

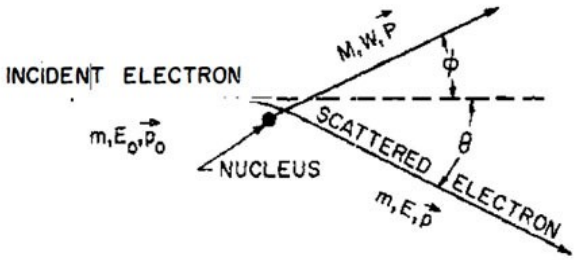
Remember, my radius for the electron is about 10^{-17}m, and I have shown their quantum equations are off by about 50 times (too small). That is why we are at a size of 10^{-18}m here in their graph. The cross section is a rough and indirect measurement of the electron radius.

You see, what they have done is simply matched their focus (or aim) to the radius of their bullet. Given a spherical *emitting* target—and the right energy—we will then expect to see something extraordinary happen at near 45° [see below for much more].

But these physicists had assumed the opposite. They had assumed that the turn-around point on the graph was an indication of the nuclear size. They assumed this because they had lazily assumed that the cross-section could be applied to the surface of the nucleus, as in this diagram:



But their cross-section doesn't apply to the surface of their bombarded object, it applies to the fields emitted from their own machines. They can't measure the surface of the object before they have measured it, can they? No. The only thing they can measure is what they are in control of, which is the focus of their emitting device (and the focus of their magnets). The cross-section applies to that focus, not to any steradian on the surface or interior of the nucleus.



To prove this, I will show you why they cannot be correct. To start with, notice how limited the angles are in the Oxygen graph. We go from about 30° to about 80°, a spread only 50°. That is curious, because if you bounce a beam of particles off a real sphere or spherical charge field, you should get a wide range of angles. In fact, you would get 180° of possible deflection from only 90° of sphere. If the nuclear equator were in line with the electron beam, and electrons only hit from 45°N to 45°S, you would get 180° of possible deflection. If electrons could hit pole to pole on only that one side, you would get a full 360° of possible deflection. I will be told the limited range is due to their detector, which can't be positioned at all angles at once. That is true, but it can be moved. If we study the data, we find the detector wasn't moved over a wide array of angles because the counts peaked in one small gap. All these graphs show a steep drop with angle, which indicates very many more counts at lower angles. That already disproves their first assumption. Here is their first assumption:

The nucleus can therefore be represented by a static, spherically symmetric charge distribution.

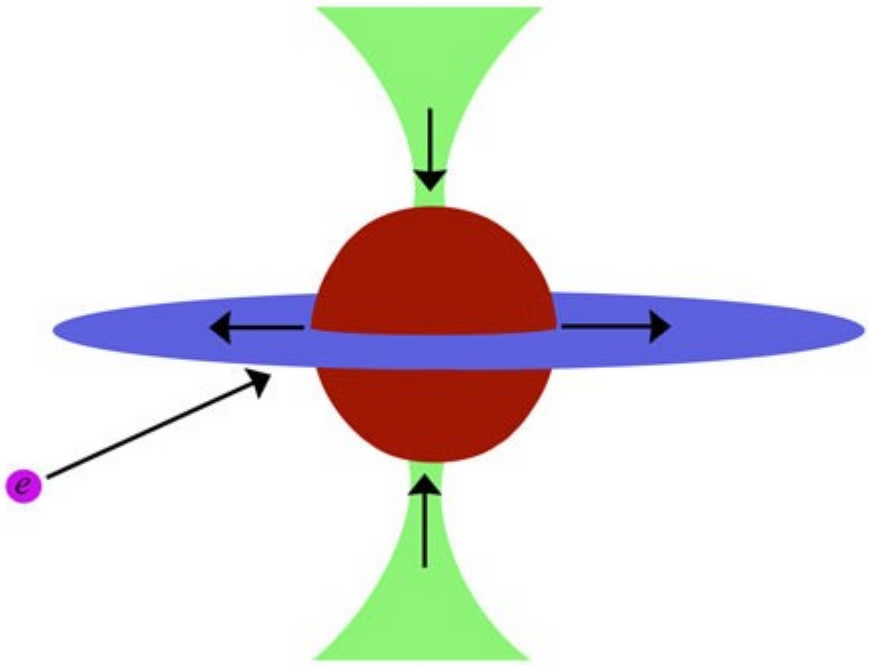
That is the first assumption of all Born-approximations, and all mainstream nuclear diagramming. That is just a raw assumption, though, and we have already seen how it is counter-indicated by the first data that came in. Despite that, they then rigged *all* the math to that *very large* and *falsified* assumption. Since the assumption is demonstrably false—and since their own experiments falsify it—it must falsify all the form-fitting math and all later theory. This is in fact what has happened. Since that assumption *is* false, all subsequent math and theory has to be thrown out. We have to start over from scratch.

Although the mainstream would deny it, I will show that all this can be explained simply with visualizations, as usual. We don't need form factors or pages of finessed math, we just need the right nuclear diagram to start with. If we use my nuclear diagrams to analyze this problem, we realize that the nucleus is emitting charge only from the carousel level, or nuclear equator. Anywhere near both poles, the nucleus is pulling *in* charge. Since electrons follow charge, electrons will be "scattered" only

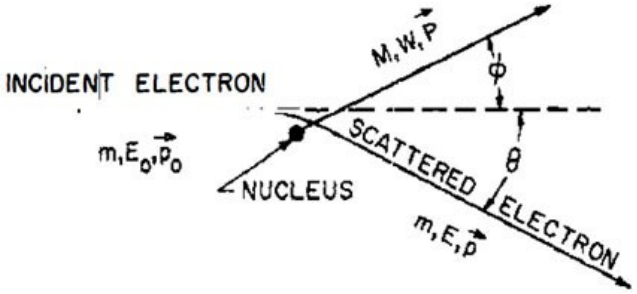
if they hit near the equator. If they hit anywhere near the poles, they may be sucked into the nucleus (where they may be absorbed, trading places with an existing electron, or be kicked out in a direction undetectable to our machines—on the backside of the nucleus, for instance).

Hofstadter sort of admits that in passing, when he says that the charge profile of the nucleus is heaviest in a single narrow band. His data shows the charge is *not* spread evenly across the nucleus, with a symmetric spherical distribution. Even if we remove the manufactured diffraction profile, the nuclear charge is still not symmetrical in any way. So why continue to assume it is after it has been proven that it isn't? Hofstadter's first data should have disproved the Born assumptions, but instead his data was fit furiously to the Born approximations. They still are. With some updates, the theory of 1957 is still the theory now.

Due to charge channeling, we have to include an entirely new field variance in our scattering equations. This new field variance will completely change our deflection expectations. All the electrons their machines pick up as [elastically] scattered are being scattered near the nuclear equator. None are being scattered from near the poles. None are even being scattered at ±60°. This is the first thing that helps us explain the narrow positive band they are finding for primary scattering angles. The other thing that helps us explain this is the way the electrons must interact with equatorial charge. Since the equatorial charge is emitted in a disk, low-energy electrons coming from anywhere below the equator can't get above it, and the reverse:



As you see, that electron will be scattered by the blue charge disk, not by the red nucleus proper. This is why the electron can't (usually) get to the opposite side of the nucleus. Since this electron is coming in from the south, it can't impact anywhere on the north half of the nucleus. This is why we don't see any negative angles in the experiments from Hofstadter and others. You will say, “What if the electron comes in right on the equator or pole?” Well, we don't see that in experiment. Notice that Hofstadter has drawn the nucleus traveling at some angle to the incoming electron.



That angle alone will prevent either a pole or equator alignment, since the nucleus will be being driven by some charge field. Only if the nucleus were moving on the same line as the electron would the pole or equator alignment be possible. But the experimenters don't want that, because they know that gives them no data, or data they can't use. So they always do these experiments at some angle to the axis. In which case we see why the electron is interacting not with the nucleus, but with the charge field emitted at the equator. This is why we see the limited positive angles we do.

This also immediately explains something that mystified Hofstadter. He mentioned several times that the nucleon and nucleus both seemed too big. They didn't really fit previous equations or expectations. In his 1956 *Physical Review* paper[2], Hofstadter says,

The size obtained from this work, and the work of others, has come to be called the “electromagnetic size” in contrast to the “nuclear size” determined from pure nucleon/nucleus interactions. [p. 1131]

Well, since the electron is bouncing off the charge field, not the nucleon or nuclear “core”, this explains the extra size. The charge field extends beyond the particle proper, no matter what particle you are looking at. In fact, the electron itself also has a charge disk, just like the nucleus. Although that won't factor into interactions like this except at the third decimal point, it is worth pointing out. In measuring the radius of the proton, it becomes significant—which is why the proton radius is different when measured by an electron and a muon.

As we saw in my paper on the proton radius puzzle, the size of the bombarding particle must be taken into consideration when using it to measure a radius. Only if you were measuring with a near-zero mass particle like the photon could you measure a real radius. All other particles require some sort of correction, due to the fact that they themselves interact with the emitted charge field.

Another mystery explained by my diagram is the fact that the nucleus was discovered to have an indistinct boundary. This was already known by the early 1950's, and Hofstadter confirmed with his earliest experiments, including those published in *PR* in 1953. In the first line of that paper, he says the nucleus does not have a “sharp boundary.” If the nucleus is taken to be a collection of nucleons in shells, this indistinct boundary is not explained. But if the electrons are actually interacting with an emitted charge field of real photons, it is. This charge field will dissipate with distance from the nucleus, both in density and strength.

Before we get into a close analysis of the mechanics, I will be told that my analysis above doesn't hold because neither Hofstadter nor anyone else ever simply treated the nucleus as a sphere. I will be told we have to look at all the math he does, including the Born approximations and all that. But as usual, all that math is just the attempt to dodge the fairly simple questions of mechanical interaction here. Hofstadter, like everyone before and after him, diverts you immediately into the math, ignoring the

kinematic questions begged by the interaction. He mentions kinematics in the first paragraphs, but then utterly ignores it afterwards. After reading the 86 pages of his 1957 paper, we still have no idea what is actually happening between the electron and nucleus.

If you take the time to read the paper, notice, for instance, that none of the math he does addresses the questions I ask above, about why the angle of interaction is small and positive. If—as they thought then and still think—the nucleus is a roughly spherical assemblage of nucleons, with the protons emitting charge semi-spherically, why wouldn't we see a wider range of deflection angles? Putting the nucleons in shells doesn't answer this question. The only thing that does is allow for blocking. But if you don't know what charge is or how the nucleus or proton is creating it, no amount of blocking math can help you here. The huge amount of overly complex math is inserted specifically to prevent you from asking these questions. If you are trying to sort through the pages and pages of dense equations, you haven't time to ask any mechanical questions. About the last thing you are likely to do after learning all that math is question it at the end. You don't want to undercut all the work you just did, do you? This is why no one ever asked the questions I am asking from the time of Bohr to Hofstadter, and why no one has asked them since then. They are very inconvenient questions, since they basically show that all the math is fake. As with all the other famous math I have pulled apart over the past decade, this math is also diversionary. It is inserted in all these papers at the beginning to prevent you from studying the mechanics (or lack of it) closely. I may go line-by-line through this math at some point in the future, but the more of this kind of math I destroy, the less interested I am in looking at more of it. I have already proved my point a thousand times, and I begin to wonder how many more times I have to do it? Am I really required to deconstruct every line of math and physics ever put down on paper?

Also notice that as we go from Hofstadter's 1953 paper to his 1956 paper to his 1957 paper, we find more and more desperate attempts to push data in line with old theory and math. Whereas the 1953 paper is short, containing mostly a report of data, the 1957 paper is an extended fudge, burying you under more than 80 pages of mathematical pushes. The 1957 paper is nearly unreadable, not only for all the manufactured math, but also for the cloudy language. Even when he is not doing math, Hofstadter is clearly trying to bend your mind. He has gone from straight researcher to dishonest ally of the dishonest theorists.

Before we move on, I will point out a couple of things that disprove the math without even looking at it line by line. To start with, on page 241, Hofstadter tells us,

Many nuclear form-factor results have been given for elastic scattering (1, 9). Figure 2 shows a typical form-factor curve for a charge distribution due to an independent particle shell model of a nucleus for an infinite harmonic well potential.

Here is figure 2:

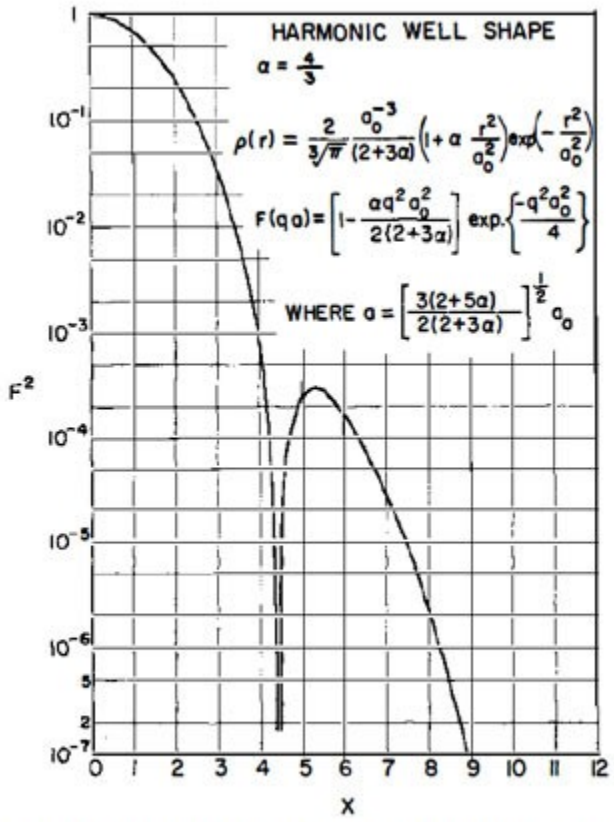


FIG. 2. Born approximation for the absolute square of the form factor associated with the harmonic-well shape in the case, $\alpha = 4/3$, which is appropriate to carbon. $x = q\alpha$.

That's not from data, that's from math only. It is based on several assumptions Born and other made about nuclear charge decades earlier. But since none of the nuclear models before mine used charge channeling, they are all useless. If you write math based on any assumption that the nucleus is made of "independent particle shells for an infinite harmonic well potential," that math must be wrong. All the Born approximation form factors from the beginning are based on false assumptions at the foundational level—which is why they still believe in the ridiculous strong force, just as one example. They believe protons are repelling one another in the nucleus, so how could any of this math be right? The mainstream has never even been close to understanding how the nucleus acts as charge entity, so how it could it write math to fit it? Up to the present day, the mainstream still believes the nucleus is orbited by electrons, and that these electrons are capable of shielding one another. They then borrowed this shielding idea for nucleons. Hofstadter includes this shielding in his early math. But since charge around the nucleus isn't created or shielded by this method, we can save time and just skip their math entirely. It is a waste of time to disprove math that is based on faulty field assumptions. You have to get the fields right first. Only then do you start trying to write math for it.

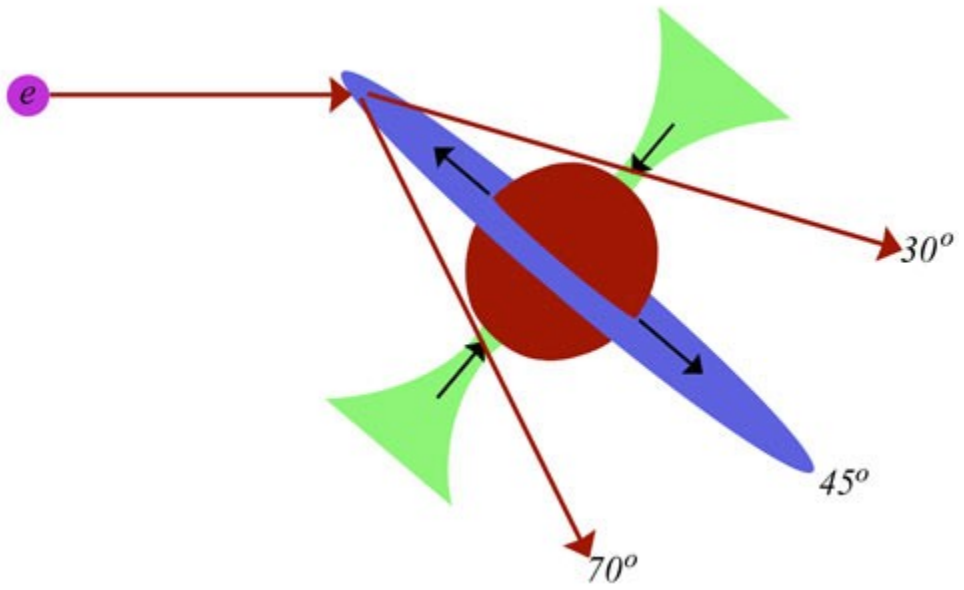
As another example, half the math Hofstadter does is for point nucleons. Why would anyone write equations for point nucleons? You will say it is easier, for a start, since we have relatively simple equations for point charges. Yes, but since there is no such thing as a point charge, those equations fail even in the simplest real experiments. Therefore, writing pages of these equations can only be seen as wall building.

Hofstadter sort of gives up the farm in the 1953 paper[3], when he admits near the end [p. 986] that

The Born approximation is clearly quite poor for a uniform distribution of charge because of its true zeros.

But he wasn't able to stand firm on that true assessment, since there was simply too much pressure from above to conform to the Born approximations. All the other approximations and fudges were based on the Born assumptions, so there was no possibility at the time of going in another direction. There was simply no other direction in which to go—no viable alternative theory to explain any of this. And even if there had been, it is doubtful it could have gotten a fair reading. Even in the 1950's, the Bohr/Heisenberg/Born dogma was too powerful to resist.

So we have seen that they have misread the graph. But how do we read it correctly? What is causing the turn-around point at near 45°? Well, the first thing we find with more research is that in these experiments, they were holding the target at 45° from the electron beam. Hofstadter neglected to tell us that in this in the 1957 paper, and we have to go to earlier paper [*Physical Review*, 1956[2]] to discover it. So of course we would expect to find something crucial happening at 45°. We can see why by going to my diagram again:



As you see, I have now tilted the target by 45°, as Hofstadter tells it us it is. You will say, "Tilting the piece of Oxygen doesn't necessarily tilt the constituent nuclei." In this case, it does. Once you have applied an external field (the electron beam *is* an external field) to a very thin slice of material, the nuclei will respond by aligning to the face of the material. Why? Because the electron is carried along in a stream of photons. That is what any charge field is. The energy the electron has, it has due to the charge around it. The researchers had to apply charge to the electron in order to move it at the target. It is charge that is accelerating the electron beam up to 420 MeV. Well, the nuclei in the target must respond to that incoming charge. The nucleus doesn't care much about the incoming electron (unless it is ionized and needs it), but it cares a lot about the incoming charge photons. It feeds on these photons, so it will turn its south pole to the incoming charge as much as possible, to give the photons the best route through the nucleus (see below for much more on this).

You will say, "Then why not just turn the south pole of the nucleus right at the beam?" Because the

nucleus is also responding to charge from its neighboring nuclei, and it is held in a solid structure by that charge. Unless the external charge field can completely overpower the internal charge field of the solid, the nucleus can't turn right at the incoming charge. But that means that in some circumstances it *would* do that. If you applied a strong enough field to this solid structure, it would align its pole to the external field. But these electron energy beams aren't that powerful. They are also too focused.

Anyway, you will see proof of that right now, since my last diagram will allow us to explain the first graph from Hofstadter [Oxygen] very easily. The 45° angle is giving us what they are calling a turn-around point or minimum simply because that is the angle at which the nuclear core is in the way. My diagram is a bit of a simplification, since I have made the blue disk much smaller than it really is, and the red core much larger. It is actually easier for the electron to get around the core than it looks like here. But that is the whole reason we find the weird blip at that angle.

The reason the blip is at 43.7° instead of 45° is that this blue disk isn't really a perfect disk. It spreads out a bit as it leaves the nucleus, and since the electron normally hits some distance out, the angle is something less than 45°.

This also explains what Hofstadter is calling a cross-section here. A larger cross-section per steradian allows the electron to penetrate the charge field, even while being deflected (or in this case, more like refracted). But a smaller cross-section cannot penetrate the charge field, and is deflected back, as from a wall. This is why I said way above that the cross-section wasn't telling them anything about the nucleus (except its charge density relative to the electron). What it is telling us is the size of the electron.

When the graph is between 43.7° and 50°, the electron is neither being deflected back nor being refracted through the charge field in the blue disk. It is sort of being absorbed into the disk, you see, matching its angle. Well, the electron can only do that when its energy matches the energy of the charge field. And that will only happen when the cross-section per steradian is the square of the electron radius. In that case, the energy of the electron matches the energy of the field, and so it joins it. This is why the cross section number with the angle tells us the radius of the electron.

You will say, “If the electron matches the energy of the charge field in the blue disk, and joins that field, why doesn't the electron deflect back at 45°, or at -135°? That is how you have drawn the charge arrow there.” The reason is because the charge field has both an electric and magnetic component, as we know. My arrow indicates only the linear, electric component. But since all the photons emitted by the nucleus will be spinning, they will impart that spin to the electron as well. This means the electron captured by the blue charge disk will actually join a widening charge circle, like a pinwheel. It will be spun off and may *or may not* be detected by the Cerenkov counter.

As you see, this also explains the turn-around in the first graph in this paper. Because the blue charge disk has an expanding width as we go out from the nucleus, there is actually a width of angles [from 43.7 to about 50] where the cross sections are *increasing*. This indicates the electron interacting with the front face of the charge disk for the first number, and the back face of the charge disk for the second number.

My diagram is also indicated by Hofstadter's early low-energy work with Beryllium, which led to this graph:

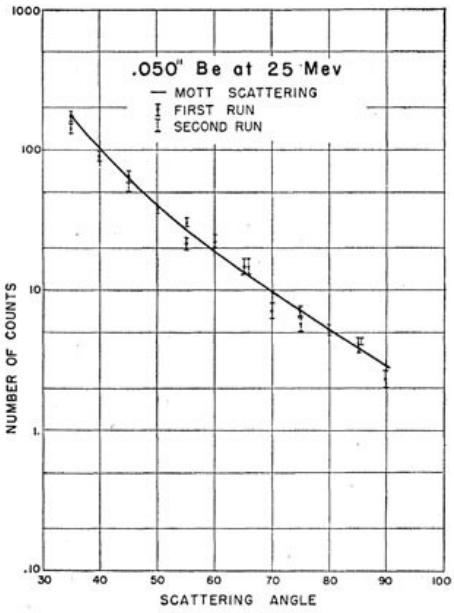


FIG. 7. The angular distribution of electrons scattered from a Be target, 50 mils thick, at 25 Mev. The target plane was at 45° with respect to the beam. The Mott curve for a point charge is shown. Arbitrary normalization is made at 35°.

That is from the 1953 *PR* paper. As you see, there is no turn-around or blip there, and no humps. Hofstadter takes that to mean there is no indication of diffraction, but it is actually indication that the equatorial charge field of Beryllium is too tightly focused to show the turn-around at this energy. The charge disk has no appreciable width relative to the electron beam. Another way of saying that is that at this energy, the only way the electron can be deflected is if it hits close to the nucleus proper, where the charge is densest. Further out from the nucleus, the charge field isn't strong enough to deflect the beam. But closer to the nucleus, the disk is more focused, having less width. So there is no spread of angles where the electron has the right energy to join the disk, and therefore no turn-around or line reversal on the graph.

If you read his paper, you see that Hofstadter doesn't even try to explain the specific motion of the line on his graph. When it doubles back in later papers, he doesn't tell you why it reverses, or why it reverses over that period. He only gives you this Born-approximation form factor

$$F(qa) = [1 - (\frac{\alpha q^2 a_o^2}{2/(2 + 3\alpha)}) \exp - (q^2 a_o^2/4)$$

by which you are supposed to be able to insert the scattering angle to get a nuclear radius. That is from the 1957 paper. But he could have made this easier, since there is no angle in that equation. He says that α is the current density Dirac operator and q is the "four-dimensional momentum-energy transfer", so it is very difficult for a reader to see how he is inserting an angle in there. You have to go back about 30 pages to eq. 8 to figure out how to insert the angle. Since this is the most important math in the entire paper, why is he skipping it? Why doesn't he run at least one equation with the angle in it? He has also previously defined a_o as the curvature of the well [p. 272], but on page 276 he suddenly applies it to the nuclear radius.

Despite all the math in the 1957 paper, you can't make heads nor tails of any of this without returning to his 1953 paper, where a was originally defined as the rms radius of the nucleus and q was defined as

$q = (4\pi/\lambda)\sin(\theta/2)$

Why couldn't he admit that in the 1957 paper? I will tell you why. He doesn't want you to notice that the nuclear radii he is calculating are dependent on an rms radius, previously calculated independently of the current data. He wants you to think the radius is being derived from his own experiments, based only on scattering rates. He also doesn't want you to notice that this makes F dimensionless, since it makes the two graphs difficult to fit if F is dimensionless and cross section is not.

He is also diverting you with the terminology, which makes no sense. He calls *q* the four-dimensional momentum-energy transfer, but as we have just seen, the only dimension it contains is the wavelength. That's not four-dimensional, that's one-dimensional. And since it doesn't have the dimensions of either energy or momentum, it is misdirection to label it as a momentum-energy transfer.

But that hardly matters, as I have said, since all the math is bombast. What Hofstadter and the other guys are actually trying to do is match their data graphs to those asinine harmonic well shapes, based on Born assumptions:

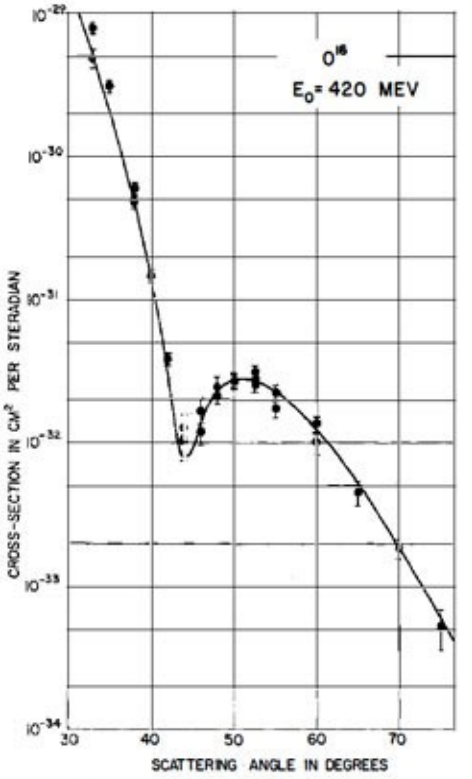


FIG. 17. New experimental data of Ehrenberg *et al.* (64) obtained at 420 Mev for the O^{16} nucleus. The figure also shows the exact calculations of D. G. Ravenhall for the harmonic-well model for which $\alpha = 2.0$ and $a_0 = 1.75 \times 10^{-13}$ cm.

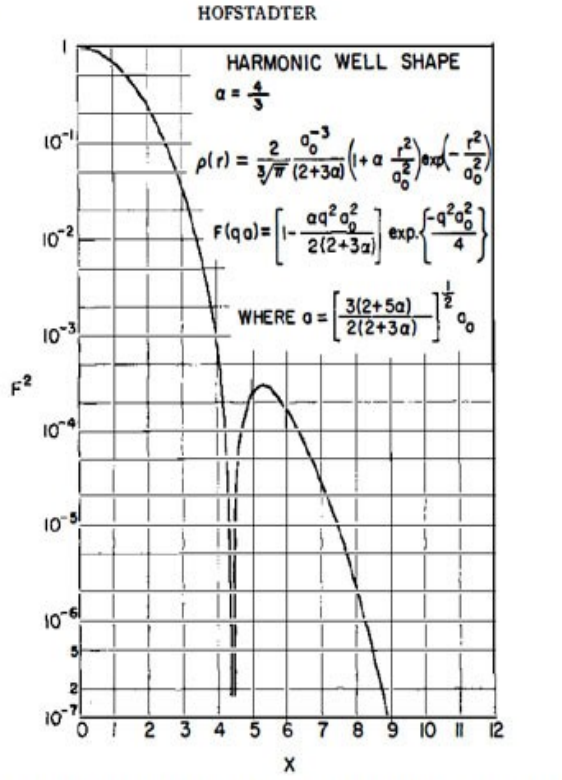


FIG. 2. Born approximation for the absolute square of the form factor associated with the harmonic-well shape in the case, $\alpha = 4/3$, which is appropriate to carbon. $x = qa$.

They are trying to match or fit those two shapes, as here with Carbon:

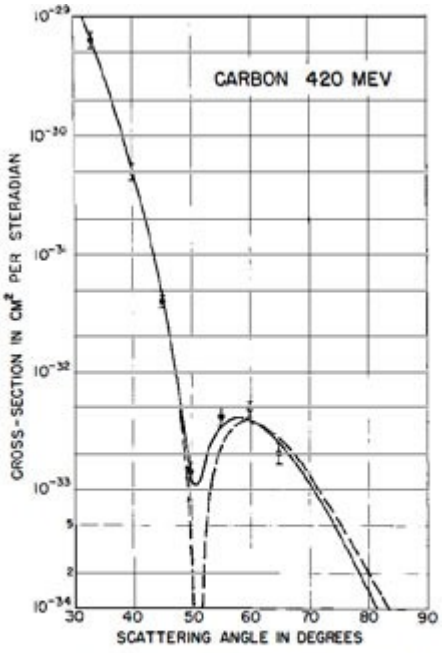


FIG. 4. Recent data in C^{12} observed by Sobottka & Hofstadter (10) at an incident electron energy of 420 Mev. Two theoretical curves are presented for comparison. The dashed curve is the Born approximation for a harmonic-well charge distribution corresponding to Fig. 3 ($a=4/3$). The solid line is the accurate phase-shift calculation of D. G. Ravenhall, which appears to fit the experimental points rather well.

Can you even begin to imagine the amount of equation finessing and theory pushing it took to come up with that fit? It brings a tear to my eye. All that work for nothing.

Again, what they have done is create a so-called diffraction structure based on a model of the atom where outer levels shield inner levels. You see this in fig. 2 just above, where the charge distribution has these big humps. We are told that figure has two humps because the nucleus in question has two shells, the *s* and *p* shells. Unfortunately, that theory doesn't work from the start, because Vanadium at number 23 already has three shells, *s, p*, and *d*. But its graph only shows two humps, not three.

Hofstadter also admits the data doesn't match the Born approximation humps in that there are no diffraction zeros in the data. See how the dashed line in fig. 2 goes way down, but the solid line doesn't follow it? So mainstream theory has to come up with another push to explain that.

Another problem is that if this diffraction were caused by blocking, then in the larger elements we should see quite a number of humps. The outer level of nucleons would contain more than two or three nucleons, so we should see several humps from any relative position. But the data of Hofstadter never contains more than two minima and three maxima, even for the largest elements. I can explain that (see below); the mainstream can't.

But the greatest problem in trying to fit data curves to these Born humps is that the axis assignments don't have any possible match. In the real data, the curve is following an increasing scattering angle on the x-axis. But in the Born approximation humps, the line is following a spherical charge distribution. In the data, they are finding a greater angle at less energy. But the Born humps don't correspond to anything like that. Even if we assume scattering angle is a function of charge strength, the scattering angle would have to rise, then fall, rise, then fall. In the graph from experiment, we don't see that. We don't see rise, fall, rise, fall. We see a rise in angle all the way across, with only the rate of rise changing. We have a similar problem on the y-axis. In the graph from data, we are plotting cross

section. In the graph from the Born approximation, we are plotting F^2. Since F is the form factor, we should ask how it changes with the cross section. They purposely make this hard to discover, because they don't want you uncovering their fudge; but we can pry it out of them. As a first indication that we are being snowed, I send you to the encyclopedia entries, which admit

a form factor is a function that gives the properties of a certain particle interaction without including all of the underlying physics. It is measured experimentally when a theoretical calculation is unavailable or too difficult.

An admission of fudge. But also false, since a form factor isn't "measured experimentally." A form factor works backward from data, yes, but mainly it works forward from a form given by theory. A form factor is a piece of form-fitting math, and the form is given by a theoretical assumption. Specifically, the form factor in these equations is completely dependent on the assumption of evenly distributed spherical charge and charge shielding by outer nucleons. Again, if the form is wrong, the form factor cannot be right.

But we already have the equation for F above, and the variable assignments, so we can uncover the fudge. The Dirac operator α cancels, so we only have q and a. Since a is already a length, qa can't also be a length. A momentum-energy transfer can't be dimensionless. An energy transfer *is* energy, so qa should have the form energy X length, or force X length squared. But as we have seen, q is neither dimensionless nor does it have the dimensions of energy, force, or momentum. It has the dimensions of 1/length. So why call it a momentum energy transfer? Again, to stir your brain. You are being fudged, and this is how they do it.

But all that hardly matters, since the real question is: as the form factor F gets smaller, would the angle get bigger? To figure it out, all we have to do is study the variable q again. Since q is the numerator, as the "energy transfer gets bigger", F must also get bigger. To match the two graphs, as F gets bigger, the angle has to get smaller. That means a larger energy transfer should cause a smaller angle of deflection. Unfortunately, that is opposite to data and all expectation. It works with my theory—which has different expectations—but utterly fails with their theory of a harmonic well. Obviously, if you have a greater energy transfer, you should have a greater angle of scattering. So these two graphs have been forced together only for convenience.

Again, there is absolutely no possibility any of this mainstream math is right, since it was rigged to data from the beginning, almost a century ago now. Yes, physics should follow data, but the math has to fit a real and correct field mechanics. You can't just rig a false field to your data set by hammering on it with math for decades. They have the wrong model for the nucleus, and no amount of finessed math can make it right. As I have shown, once you have the correct model, you can throw 99% of this math out the window. The math is just piles upon piles of pushes, and with the right mechanics, you don't need any pushes.

Critics sometimes complain that my kinematics is so simple it can't be correct. They say the quantum world is not that transparent, much less that concise. But most of the complexity of the standard model comes from these multiple fudges. When you start with the wrong model, of course you are going to have to push it every time new data comes in. All those pushes are what causes the complexity. If you start with the right model, you don't need any later fudges or pushes. You start clear and simple and remain clear and simple as new data comes in.

I am now going to switch to Hofstadter's 1956 *PR* paper, entitled "High Energy Electron Scattering and the Charge Distributions of Selected Nuclei." [2] On p. 1132, we find this:

The loss of energy due to recoil of the target nucleus, at 183 Mev and at a scattering angle of 90°, amounts in Ca to 0.9 Mev, and in Au to 0.18 Mev.

That is some very interesting data, since it allows us to compare the two. The difference is exactly five. But if we go to the atomic numbers of those two elements, we find the difference to be 3.95. So the charge strength of the two elements isn't a straight function of the protons. Since the mainstream believes the neutron is neutral, that data is somewhat surprising. The neutrons shouldn't be involved in creating charge. I will be told the recoil is caused by atomic weight, not charge, but those numbers don't work, either. In that case, the differential is 4.9. We have 2% miss. And we have a logical problem in the claim that atomic weight causes the recoil, rather than charge. Remember, if you try to explain these quantum interactions as mechanical interactions between real spheres, the mainstream will shout you down, telling you that this interaction must be an E/M field interaction. So if they try to go to atomic weights here to explain recoil, they have just contradicted themselves. To explain the scattering angles, they want E/M field interaction, but to explain the recoil, they want straight masses. Why is scattering an E/M interaction but recoil is not?

They will say they use both, since they try to explain the 2% gap with charge and charge blocking. But I will show you a simpler method, one that proves my theory and disproves theirs. I will solve using charge strengths only. Calcium has an Argon core and Gold has a Xenon core. According to my diagrams, the charge differential between those is 3. But Gold has 25 protons in the 4^{th} level; Calcium has 2. That is a differential of 12.5. Combining those two numbers gives us 4.1666. Gold has 5.9 times more neutrons. If the neutrons are also channeling charge, they will affect the total as well. If we combine those two numbers, we get 5.03. Already, my estimate is better than the mainstream's.

You will say we didn't weight each affect, which is true. So let's firm up those numbers. If the neutrons are channeling at only .667 for each proton*, that would seem to bring the neutron's number down. But Gold has 1.494 times as many neutrons as protons, so if we include both those facts, we are back to about even: 1.494 x .667 = .996. That brings our total down to 5.02.

That simple math indicates my theory can explain data more easily than mainstream theory. It also indicates something else extraordinary. From that last calculation, we can finally see exactly why elements are using the number of neutrons they are. That is, you should find it astonishing that the last two numbers resolved to very near one. It indicates the larger elements are closing in on a 1.5 ratio of neutrons to protons, and the *reason* they are doing that is due to the charge profiles of proton and neutron. It is no accident that this ratio is 1.5 and the magnetic moment ratio of the two particles is also 1.5. As the elements get larger, they channel charge at more nearly full strength. The proton begins to channel at more nearly 1 and the neutron begins to channel at more nearly .667. In smaller elements, this was not the case, because the 4^{th} level wasn't pulling in charge at anywhere near what the core could take. For instance, Calcium's two 4^{th} level protons couldn't even begin to pull in what its 18 core nucleons could handle. But with Gold, we are much nearer that maximum. Therefore, the neutron to proton ratio will more nearly match the magnetic moment ratio, you see.

So we have seen that the interaction in these experiments is indeed a charge interaction. But it isn't the charge interaction they think it is. They appear to assume that incoming electrons are "scattering" off orbiting electrons (or, later, nucleons), but they aren't. They are scattering off the charge disk coming out of the nuclear equator. Absolutely no orbiting electrons reside in that disk. All electrons reside in the nuclear interior or on the nuclear poles. I may be asked, "If that is so, why do we have data that can be separated into elastic and inelastic collisions?" Well, the mainstream has always just *assumed* the two sets of data could be given to elastic and inelastic collisions, but we have never had any evidence for that and know it can't be true regardless. We only have to look at the definitions of elastic and inelastic. In an inelastic collision, kinetic energy is not conserved. But since we are explicitly including the charge field here, all collisions should be elastic. Inelastic collisions are allowed only when you are not including charge photons. In that case, photons may carry off energy not conserved by the larger particles. But we can't allow that fudge here. If photons are carrying off energy, we have to include them in the conservation. We have the charge field to work with, so there is no excuse for dodging off into inelastic collisions. In my theory, I don't use that dodge. Since I have real photons to work with, I can always conserve energy.

A more logical assumption in these experiments is that the incoming electrons that they are dismissing as inelastic are actually electrons that are hitting something else besides the main charge disk. A few electrons obviously will impact nearer the pole of the nucleus, and in that case they may bounce off a nucleon directly, careening off a core alpha. We have direct indication of that from Hofstadter's own graph, fig. 1:

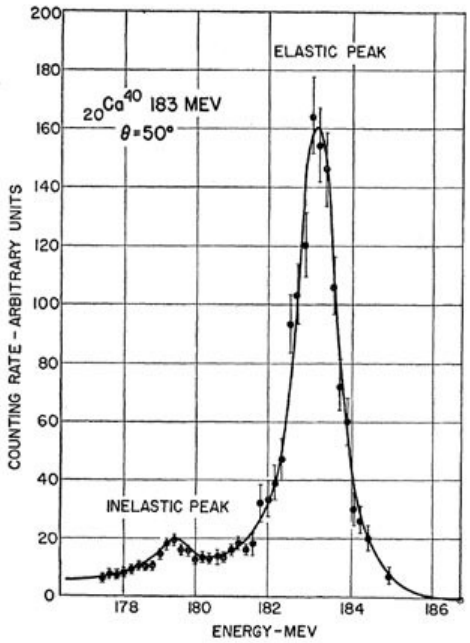
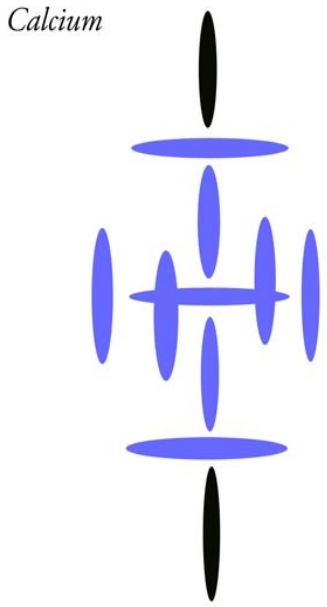


FIG. 1. A typical curve of counting rate *vs* electron energy for calcium at 183 Mev and $\theta = 50°$.

See how the inelastic peak is 8.25 times smaller than the elastic peak?



As you can see, that indicates a bounce off the charge field emitted E/W by the lower blue double-alpha, instead of the main charge disk. Since there are 9 double-alphas in Calcium, that one blue disk is channeling at 1/9th of the main field. Since that secondary field is at 1/9th the total strength, the incoming electrons will be about 1/9th as likely to interact with it. So you see, the "inelastic" counts are not really inelastic; they are from a different set of interactions altogether.

Another problem is that Hofstadter and his buddies end up finding multiple inelastic peaks. They don't explain how that is possible. Collisions should be either elastic or inelastic. There is no third choice there. A third peak obviously indicates that the division of peaks isn't elastic/inelastic, it is main charge stream, secondary charge stream, tertiary charge stream.

Now let us return to the problem of the blip, or the turn-around on the graph. We have seen that the blip of Oxygen at 420 MeV is at about 45^{o}. But with larger elements at lower energies, the blip seems to run closer to 70^{o}.

So what is really happening here? To find out, we have to go all the way back to Hofstadter's 1953 paper at *PR* [3]. In that paper, we find this graph, fig. 7:

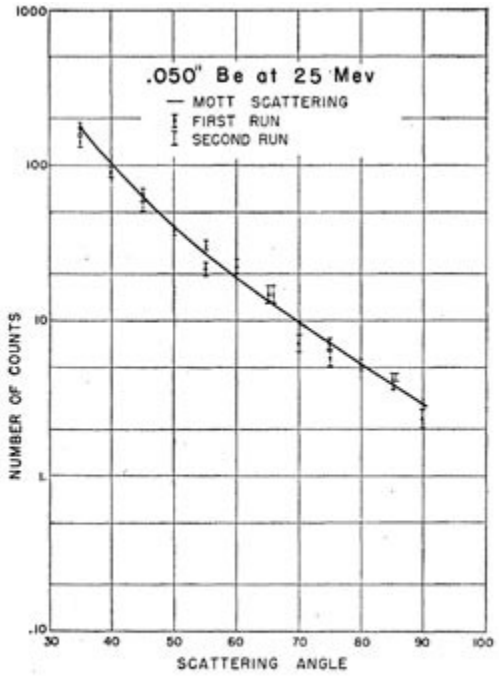


FIG. 7. The angular distribution of electrons scattered from a Be target, 50 mils thick, at 25 Mev. The target plane was at 45° with respect to the beam. The Mott curve for a point charge is shown. Arbitrary normalization is made at 35°.

As you see, Hofstadter originally graphed angle against counts, not against cross section. He does this throughout that 1953 paper. But there were two problems there. One, the low energy of 25 MeV didn't give him any humps to match to diffraction models. Two, "number of counts" was too transparent as a y-axis title. Anyone could make sense of it immediately. What was needed was to give it an opaque title like cross section per steradian. That way, when he later tried to fudge from the diffraction graph to this one, few would notice that cross section per steradian and form factor F had nothing in common —and were actually upside down to one another as a matter of mechanics.

By 1956, Hofstadter had stopped using counts on the y-axis, switching to a differential cross section over $cos^2\ \theta/2 // sin^2\ \theta/2$. This was explicitly "to display diffraction structure"—diffraction structure we know did not exist. That's strange, since if the diffraction structure *did* exist as they claimed—as a simple shielding by real particles—the graph with counts on the y-axis should have shown it. There should be higher counts where there is less blocking. So we already see indication the data is being pushed to fit these graphs to the harmonic well graphs.

At first, the y-axis was rigged this way in "arbitrary units". Only later did Hofstadter switch to units based on the electron's reduced de Broglie wavelength. But since Hofstadter admits the experimental cross section is not known, this is again just a guess from previous equations.

Here is yet another problem:

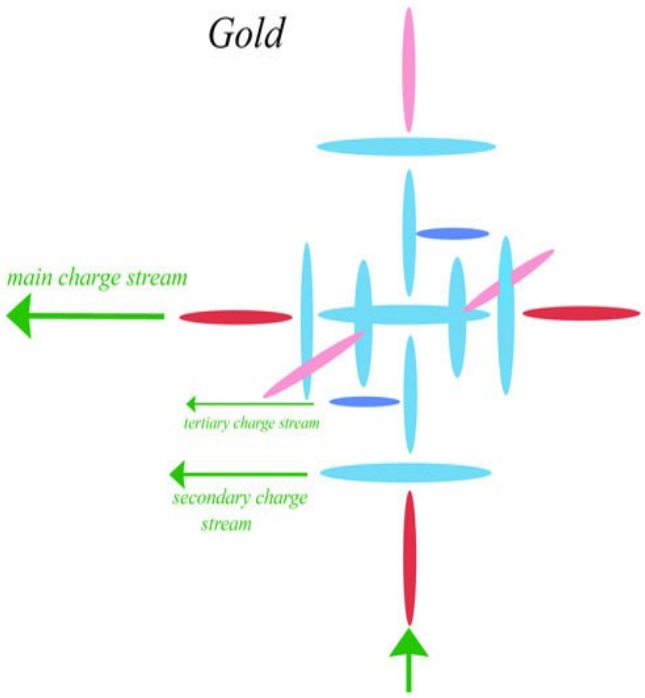
The absence of pronounced diffraction peaks suggests that, from the viewpoint of the Born approximation, heavy nuclei do not have sharp boundaries. [1953, p. 985]

With the angular resolution attained in these experiments, the absence of diffraction maxima and minima in the observed scattering of 125- and 150-Mev electrons from Ta, Au, and Pb suggests tentatively the concept that these nuclei have charge distributions tapering off gradually from near the

center to the outside. [p. 986]

So they are taking the lack of humps in the line on the graphs as indication that nucleon shielding doesn't work on larger elements the way it does on smaller. They take it as an indication of "less sharp boundaries." Is that logical in any way? No. Why would nucleons shield in large elements differently than they do in smaller elements? A nucleon cannot gain fuzziness just because it is in a larger nucleus. A nucleon either shields or it doesn't. Given their assumed mechanics of shielding, there is no possible way that "boundaries" could become fuzzy. The boundary of what? Fuzzy in what way, specifically? No answer.

My diagrams of the larger nuclei explain this logically and clearly, since larger nuclei have more charge streams, and all the streams are stronger. When the mainstream is talking about boundaries, they are talking about boundaries inside the nucleus, between nucleons. But to understand this, we have to study the *external* charge streams, beyond the boundary of the nucleus proper, and beyond the outermost nucleons. Let's use my diagram of Gold for this:



Rather than shielding, we have charge channeling. Since that is a Xenon core, each cyan disk contains three alphas or six protons channeling charge. The main charge channel is in north and south and out on the equator. The red disks are four protons and the lavender disks are three. So Gold has 14 protons emitting equatorial charge. Those charge streams inside each disk will be adjacent, but not equivalent. Therefore, the red disks indicate more spread in the charge width than the lavender disks.

Since smaller elements have fewer protons in the 4th level (carousel is 4th level), or none, their equatorial charge streams will be narrower and weaker. That is the main explanation for the "lack of diffraction peaks", but there are two others. The secondary and tertiary charge streams of the larger nuclei are also wider. The secondary charge streams of Gold are created by the top and bottom cyan disks. Those disks channel charge from pole to equator, in the main line, but they also emit a lesser amount of charge along their own equators. Because the charge is wider, it overlaps with the wider

charge from the carousel level protons, which is parallel to it. Because the secondary streams overlap and join the main stream, the minima are much reduced. This flattens out the humps and makes what they are calling the diffraction peaks all but disappear.

And we even have a tertiary effect, created by the inner level nucleons. In Gold, that would be the blue inner disks, plugged into the axis level. Each blue disk contains two protons, so we have four total down there. Those protons also emit charge equatorially, parallel to the main emission. Smaller nuclei commonly have only one proton in each hole, or none. So larger nuclei further fill in gaps using these tertiary streams, further minimizing any humps.

It is in this way that I am able to tell them the first element that will show the second minimum. Remember, Oxygen showed only one. This is because Oxygen has no cap alphas (north and south), so it can have no secondary channels. So the first with cap alphas would be Sodium. Neon is actually the first, but its channels are so weak the variation probably wouldn't show. It would also be very difficult to use in an experiment like this.

With all that in mind, let us return to Oxygen. Why is Oxygen tilting at 45° at 420 MeV while the larger elements are tilting at 70° at 183 MeV? And I also have to include Carbon in this mix, since it seems to skew my explanation above. You see, although Carbon is smaller than Oxygen, it tilts *more* than Oxygen, like the larger elements. That doesn't seem to give us a logical progression, does it? Well, you have to remember that Oxygen is normally a gas, while these other elements are normally solid. This means that the Oxygen nucleus will turn more easily in structure than these other elements, including Carbon. Even if we supercool Oxygen to make it solid, it still has weak bonds with itself. It acts like a solid only because there is no charge to break it up. But as soon as you hit it with an electron beam, it can be turned at the point of hit. This should tell us that the easier the nucleus is to turn in the structure, the closer it will go to 45° in these experiments. Carbon turns to 50° because it is small and therefore easier to turn than the larger elements. As we go up the Periodic table, the larger nuclei become harder to turn. Nickel turns at about 60° at 183 MeV, and Gold turns at about 70° at the same energy. At lower energy [153 MeV], Gold turns at about 85°. And at even lower energies, we see no turn at all with the larger elements. Their lines flatten on the graph.

Even with my diagrams, this is difficult to understand until you study closely their experimental set-up.

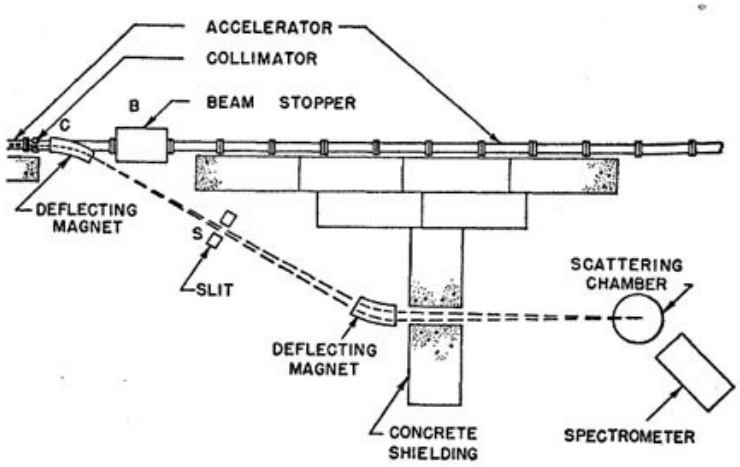
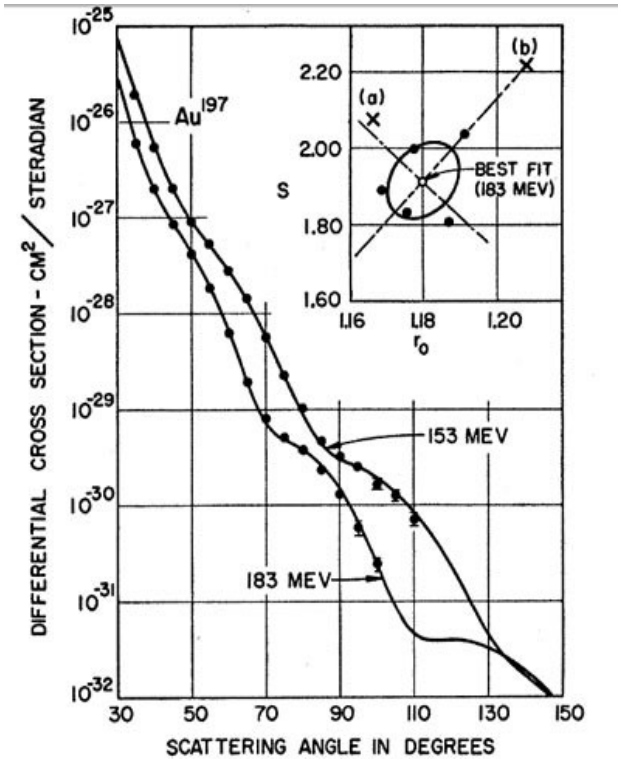


FIG. 1. The experimental arrangement of the electron-scattering system.

Notice how close their spectrometer is set up to the scattering chamber. In the 1953 paper, we learn this analyzing magnet weighs two and a half tons and its entrance port is only one inch away from the scattering chamber. I would therefore assume that this magnet is affecting the target. They have both the target and analyzer in separate vacuums, so maybe they are assuming all fields have been removed, but of course that isn't true. They are ignoring the photons, as usual, which cannot be vacuumed or kept out.

Now notice that if the target is held at 45°, it is parallel to the magnet. This means the magnet will align the nuclei in the target to 90°, before any beam arrives. After the beam arrives, we then have competing fields at the point of hit. The magnet will try to align the field to 90°, but the beam will try to align to 45°. Obviously, the magnet has the least affect on Oxygen and the most on the large elements like Gold. Which is why we see Oxygen nearest 45°, and Gold at much higher angles. The lower the energy of the beam, the closer Gold stays to 90°.



What you should notice is that the centermost of three minima shifts toward 90° at lower energy. On the top line, that would be where the arrow is pointing. We have three and only three humps, which indicate my primary and secondary charge emissions. This confirms my model and contradicts theirs, which should have more (and varying numbers of) minima with the larger elements. Also, with their model, the "diffraction lines" should not shift like this with energy. If the humps are caused by charge shielding, higher energy would show larger humps, not shifted humps. To be even more specific, any shift we might see with their theory of spherical diffraction should be a shift out from some central point on the graph, not a shift of the entire spectrum sideways. For the spectrum to shift like that would require shifting of the outer nucleons.

All the graphs and data confirm my model and contradict theirs. They can match their data to their models only with many pages of manufactured math. But, as you have seen, I can match their data to my model with very little effort.

Conclusion: Coming into these papers, I had hoped to match Hofstadter's data to my diagrams, showing the plusses and minuses in the charge field. Although I haven't been able to do that, I have been able to match his data to my diagrams in many other ways. In doing so, I believe I have already proved that my theory of charge channeling is far superior to the mainstream theory of charge shielding.

The reason I wasn't able to match my charge channels to his data is that he and his co-researchers never attempted to map the entire nuclear surface. To facilitate their own procedures, they always bombarded at 45°, which precluded any direct data from the equator or poles. Therefore we never got to see direct evidence of my charge going into the nucleus. To see some very anomalous effects, Hofstadter's team should have run the experiment for some period at the normal angle, to prime the field. After that, they should turn the test strip to 90°. This would have the effect of putting the nuclear pole right in line with the electron beam. This would have caused some serious heating, as well as other effects, all of which would have clearly indicated channeled charge.

From all this we can see why Hofstadter's data was so easy to bury and finesse. He buried it himself through his desire to fit in. He didn't wish for his data to conflict with previous theory or math, so rather than attack the mainstream, he spent years conforming his data to the wishes of those around him. In fact, we must assume this is why he won the Nobel Prize. As we know, you don't win the Nobel Prize—or any other prize—by stepping on big toes. You win these prizes by further cementing in the standing theories. Although his experiments obviously showed charge variations across the nucleus—and although he admitted that from the start—Hofstadter did everything possible to match those charge variations to variations predicted by very old spherical shell models. He never even considered any other possibility. Yes, he considered several variations of the same model, but didn't consider any variation based on a radically different charge mechanism. There was no reason to do that, since everyone wanted confirmation of the old models. Newer Nobel Prizes depend on older Nobel Prizes, and old living Nobellists sitting on the committee are not going to look kindly on new research that disproves their prize-winning theories. In fact, they will do everything in their power to bury it. That is what we have seen throughout history, but perhaps most especially in the 20th century, when politics ruled physics.

*See the so-called magnetic moments of proton and neutron.

[1]Hofstadter, Robert. **NUCLEAR AND NUCLEON SCATTERING OF HIGH-ENERGY ELECTRONS**. ***Annual Review of Nuclear Science***, 1957.

Hofstadter, Robert. **High-Energy Electron Scattering and the Charge Distributions of Selected Nuclei**. [2]*Physical Review*, volume 101, number 3. February, 1956.

[3]Hofstadter, Robert. **High-Energy Electron Scattering and Nuclear Structure Determinations**. *Physical Review*, volume 92, number 4. November 1953.