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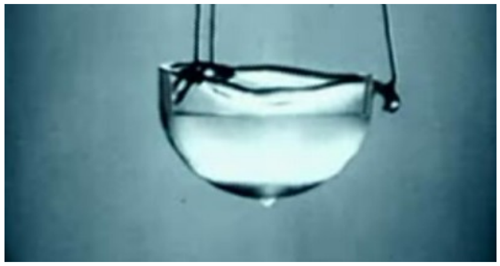
Helium4 a Boson? No.



by Miles Mathis

There are several competing definitions of "boson." One is a particle that obeys Bose-Einstein statistics. Another is a particle with integer spin. Another is a particle that can occupy the same quantum state as another like particle (another boson), confuting the Pauli Exclusion Principle of fermions. We will look at all three, finding that Helium4 utterly fails to satisfy the last two definitions, and only seems to satisfy the first.

We will look at spin first. In quantum mechanics, spin is called an "intrinsic" property. What does that mean? It means spin isn't real. It isn't angular momentum, as motion about a center or axis. They couldn't make that work in their half-baked math, so they dropped any mechanical assignment of spin. In this case, "intrinsic" means the same as "virtual". Spin is only a placeholder in the equations, and you could call it whatever you like. We could call it "direction." Since spin is a vector in the math, this would be appropriate.

The current theory is that bosons have an integer spin while fermions have a half-integer spin, usually ½. Does this mean bosons are spinning twice as fast as fermions? No. They use these numbers only to make the M values work out. M values are the spin quantum numbers in the wavefunction.

Why do bosons have integer spins? Only because fermions have half-integer spins. The fermions came first, and the bosons were tagged "integer" only to differentiate them from fermions. OK, so why were fermions given half-integer spins? Due to the Stern-Gerlach experiment from 90 years ago. Reading the data made the physicists think that the M-value could be quantized at 1, 0 , or -1. This was mysterious, because according to the probability math it meant that the quantum had to be less than 1. Why? Because we have three possibilities here, 1, 0, and -1. They thought, "Gee, how do you get integer quanta to add up to 1, 0, -1? We can explain the 0 value, of course, because that would be a 1 meeting a -1. But then the other M values would be 2 and -2 [1+1, or (-1)+(-1)]. The only way we can get 1, 0, -1 is if the quantum is ½."

However, [in my paper](#) analyzing the mechanics of Stern-Gerlach, I have shown they were wrong. Due to a vector mistake (ironic that) in the math, the M values were only 1 and -1. No 0 should have been expected, since the data was moving parallel in the experiment. Therefore, physicists were always free to keep the spin quantum of fermions at 1. We never needed half-integer spins.

OK, but what does that tell us about bosons? Not much, because even if the spin of fermions had been tagged as 1 from the beginning, they would have still needed to differentiate bosons from fermions. They found that the most important boson, the photon, didn't act like a fermion in some ways. Since pairs of photons seemed to be created from point-particle fermions like the electron in certain collisions, physicists assumed photons could inhabit the same spot at the same time. After all, the tracks themselves were telling them that. You could actually track the photons back to the same spot and time in accelerator collisions. If that were so, the photon was not obeying the Pauli Exclusion Principle. *Therefore*, the photon was a different sort of particle altogether. So it was called a boson. To give this difference some hook in the wave and quantum equations, they decided to give the difference to spin, since spin was sitting there basically unassigned. They didn't have any mechanical assignment for spin (and still don't); but they had a word for it, so why not assign this photon difference to intrinsic spin? Great, but how to do that? Well, since all their fermions had half-integer spins, the integer spin was free. Give it to the boson! After that, all you need is an interpretation. There is no mechanics here to get in the way, so the *interpretation* takes the place of the physics. So these guys (they were mostly guys, led by Pauli) came up with the idea that half-integer spin particles excluded one another from the same place (or quantum state), and integer particles didn't. They had data, they had theory, so they were finished!

Or that is what they told us. Remember how Feynman was always trying to force down our throats the idea that new physics was nothing but data and math that fit it?—Well, not only Feynman, but he was the prince of this attitude, and led the way for decades. He got the idea from Bohr through Pauli, and he brought it into the last decades of the 20th century. Feynman was so charming he not only succeeded in carrying this threadbare idea into the 1990's, he actually glorified the idea beyond what even Pauli had been able to do with it. Feynman sold it to the glossy magazines, and through them to the public. He shined the idea up to such a gloss, it has basically sold itself since his death. His students only needed to drag it along.

But as with so many other ideas, it has turned out to be wickedly false. This holds no matter what problem in physics we apply it to, but we will apply it to the problem here to prove this. As I have shown, they had data and a theory (interpretation). Did they thereby have physics? Let's see. If we look at the quantum value for spin in the wavefunction, we find it is a vector (or tensor), as I said. This just means that at any time it has a direction. Now, what quantum physicists do is use probabilities to fudge an interpretation at this point. [You can follow along with this fudge by consulting the page on Identical Particles at Wikipedia.] What they do is say that fermions are anti-symmetrical particles, while bosons are symmetrical. What does that mean? Basically it means that if you bring two particles together, you have to subtract the fermions from one another, while you add the bosons. Therefore, if you have just two fermions, and you bring them together, their probabilities will sum to zero, and they can't exist in that state (together).

Well, if they could prove that those two fermions that we brought together had to be of opposite spin, this might begin to make some sense. For instance, if one particle were ½ and the other were -½ , then the two spins would exclude one another mathematically and perhaps physically, and with a bit more theory we might have something. Is that what they try to do? No. They seem to realize that can't be done, not with just two particles and not with a field of particles. Since the fermions can be either ½ or -½, at any point in the field they can sum to 1, 0, or -1. They admit that in other places, so why not here? They even mistakenly interpret the data in Stern-Gerlach that way. So why not here? Why is the sum always zero here, creating exclusion? Simply because they want it to be.

They will say that if we sum over the whole field, the probability must indeed sum to zero, due to conservation of energy as well as probability. But that only seems to be true if you don't look closely at the way we must be summing probability. The Pauli Exclusion Principle applies at a point or to a very limited quantum state, not to the field as a whole. Therefore we are always summing a probability at a quantum state, not of the whole field. Even if the field had to sum to zero spin for some reason, no point in the field has to sum to zero. There is no conservation of energy theorem for every point in every field. If there were, there could be no energy in any field.

To say it another way, there is absolutely no reason that given a certain place and a field of fermions, the fermions we try to sum at that place have to be of opposite sign. What if two fermions with ½ spin try to occupy the same state? How are they anti-symmetrical? The truth is, they aren't, and the theory fails. Even if we call this interpretation physics, it is just bad physics. It doesn't add up. What they say is true isn't true. They have to fudge the math to make it fit their own interpretation.

They seem to understand this, because they don't even try to justify the exclusion based on a sum to zero. Study the text at Wikipedia—which is orthodox regarding current theory—and you will see they say, "the anti-symmetric expression gives zero, which cannot be a state vector as it cannot be normalized." Notice that they *don't* say that the zero sum implies a probability of zero that the two fermions will be in the same state in the same place at the same time. They don't say that because it isn't true. In quantum mechanics, the probability is never zero, it only approaches zero. They use that fact in many other places, and don't wish to lose it. But they don't want the Pauli Exclusion Principle to be a soft rule. They want it absolute. So they say that zero can't be a state vector *because it can't be normalized.*

Huge cheat there, because there are a lot of things in quantum mechanics that can't be normalized, but that doesn't automatically mean they are proved to be impossible. In general, the fact that something in quantum mechanics can't be normalized just means the zeros and infinities can't be removed using the current bag of tricks.

In fact, claiming that because something can't be normalized indicates it is impossible is very strange in itself. Remember, even Feynman admitted that normalization was a lot of hocus-pocus, and he invented large parts of it. It is like saying that because an equation can't be fudged, that means that whatever the equation applies to is impossible. "I can't finesse this equation, therefore the physics is impossible." Does that make any sense? No. Not only is there no rule of that sort, it isn't even logical.

To look at all this another way, consider the wording of the Pauli Exclusion Principle: no two fermions with the same quantum numbers can occupy the same quantum state. But as I just showed, to make this true above, the fermions would have to have opposite spins. That is what anti-symmetrical means. Only in this way could they sum to zero. But if they have opposite spins, they don't have the same quantum numbers. *Their spin quantum numbers are opposite, not equal.* This doesn't even match the interpretation when this is applied to electrons. Electrons **can** occupy the same state with the same quantum numbers, as long as their spins are opposite. The whole theory is garbled, and doesn't even read the same for different fermions.

I will be told that I am confusing spins with probabilities, but I am taking this straight from the texts.

In the math at Wiki it says,

Let n denote a complete set of (discrete) quantum numbers for specifying single-particle states (for example, for the particle in a box problem we can take n to be the quantized wave vector of the wavefunction.) For simplicity, consider a system composed of two identical particles. Suppose that one particle is in the state n_1, and another is in the state n_2. What is the quantum state of the system? Intuitively, it should be $|n_1\backslash\, |n_2\backslash$.

First of all, notice that they misdirect you here, making you think the two particles must be in different states. What is to prevent us from having two particles both in state n_1?

But here, *n* doesn't stand for either the spins themselves or the probabilities. As you see, *n* stands for the set of quantum numbers. We then get the equation for fermions:

$|n_1\backslash\, |n_2\backslash \;-\; |n_2\backslash\, |n_1\backslash$

Where did that come from? It comes from applying probability math to the terms. Since the two particles are indistinguishable, they can combine in either way. We don't know which particle is which, so we have to write the probability both ways.

The problem with that is that the equation still contains the original assumption, which is that the particles are in two different states. As I have shown, the particles can be in the same state, which shows up how this math is being pushed. What the equation is actually telling us now that it has been written as a probability is that it doesn't matter how we combine them: they will sum to zero either way. Which is true, provided that they sum to zero at all. If they are in two states, and those two states are opposite, as with spin, then yes, they will sum to zero. Reversing them, they will still sum to zero.

But if they *don't* sum to zero in one configuration, they won't sum to zero if we switch them. In which case this equation is doubly misdirecting. For instance, if both particles are +½ , the spin won't sum to zero. If we now switch the particles, the same is true. Still no sum to zero. Which means the equation

$|n_1\backslash\, |n_1\backslash \;-\; |n_1\backslash\, |n_1\backslash \neq 0$

The form of the equation fools you into thinking it would sum to zero, but it won't, because *n* isn't spin; *n* is the set of quantum numbers, and the equation is now a probability equation. If the two particles don't sum to zero in either combination, they can't sum to zero in the differential, by the rules of this kind of math. They are just assuming you don't know the difference between different kinds of math. You are being hoodwinked. You are being finessed by magicians.

I will be told that this is actually a tensor product space, with Hamiltonians *H* x *H*. But that only proves my point once again, since it is admitted

In other words, symmetric and antisymmetric states are essentially unchanged under the exchange of particle labels: they are only multiplied by a factor of +1 or −1, rather than being "rotated" somewhere else in the Hilbert space.

There it is, in plain English: no rotation. If there is no rotation they can't muck this example up further with angle cosines, and it doesn't matter if this is a tensor product space or not. It doesn't matter if it is a Hilbert space or a Dilbert space. All their fancy math after this is just more misdirection. All that matters is the 1 or -1, which I have shown cannot be made to sum to zero in all cases. The exclusion of fermions has **not** been shown by the math. Yes, data shows it, but the current math fits the data only with a terrible finesse.

My bringing this finesse in the open basically destroys many things, including the rotten spin-statistics theorem. The spin-statistics theorem takes this finesse as its first principle and goes from there, so if this is false all of spin-statistics is falsified from the ground up. Physicists don't have to argue about the place of Relativity in spin-statistics anymore. Although spin statistics has always been fudged in its fake use of Relativity, that doesn't matter anymore. Although I have written dozens of papers on Relativity, I have preferred to destroy spin-statistics without getting into Relativity. The historical arguments and justifications for Relativity in spin-statistics are now moot, since I have proved spin-statistics was DOA, long before Relativity was imported to muck it up further. All of spin-statistics is based on a mathematical finesse at the ground level.

Obviously, if the proof of fermions fails, the proof of bosons also fails. So I have dispensed with the first two definitions of "boson." Helium4 cannot fit those definitions, since the simple fermions don't even fit those definitions. The definitions are manufactured from nothing and the math doesn't support them.

What about the Bose-Einstein statistics? Well, here again we have fuzziness from the very beginning. Although photons are classified as bosons, created photon pairs are now given opposite spins. This is simply to conserve energy, of course (as well as symmetry), but it conflicts with the definition of boson. If bosons can occupy the same quantum state, what is to prevent photon pairs from occupying the same quantum state, including spin? Another problem is the conservation of energy by a field of bosons. If a B-E condensate is composed of bosons in the same quantum state, as we are told, how does this conserve energy? Shouldn't the total spin be nonconserved? Isn't a B-E condensate wildly flouting symmetry?

But it is much worse than that, because it turns out the Bose-Einstein statistics rely on the same statistical fudge I uncovered above. Remember, both Bose and Einstein worked up their solutions in 1924-25, *after* Stern-Gerlach in 1922 and all these other mistakes I have uncovered. In other words, they were creating their new statistical solutions from a horribly compromised math. They were taking for granted all the interpretations of the wavefunction I have shown were false, including the huge Stern-Gerlach error, the pushes in the Schrodinger equation, the earlier pushes in the Bohr equations, and the misunderstanding of the Lagrangian itself.

But I don't even have to show that the B-E math is pushed (I will look at that in another paper), because even if it is true, it can't differentiate bosons from fermions. All the B-E statistics do is show that, given indistinguishable particles, their peculiar distribution is a possible outcome. But since I have shown there is no theoretical reason fermions trying to occupy the same state must have opposite spins, fermions are not excluded from B-E statistics. They *may be* excluded empirically, in some cases, with some particles we call fermions. But the current math fails to tell us why. Neither the half-integer spin nor the B-E statistics are able to exclude fermions, as long as the fermions are indistinguishable and spinning the same.

In fact, we now have data proving what I just said. Helium3 is called a fermion, due to some of its characteristics; but it has been found to act like a B-E condensate in almost the same way as Helium4. The temperature is a bit lower, but the superfluid qualities are the same. This fact alone destroys the first definition of “boson”. It would appear that Helium3 becomes a B-E condensate while being a fermion. The fermion-boson delineations are now *known* to break down in experiment.

And I can tell you why. The whole integer, half-integer spin idea was wrong from the beginning. As I have shown already, the particles in Stern-Gerlach were spinning 1 or -1, with no zero expectation. And that spin was real, about a normal axis. I re-ran the math, including my corrections, finding that the spin speed was never over c. So that is solved.

As for other particles and particle combinations, they also have real spin, with spin radii and spin speeds determined by the particles themselves, as well as their environments. We have to calculate each experiment anew, since there is no standard spin for categories of particles. Yes, electrons at rest all have the same spin, provided they are in the same charge field. But if we put them in a different charge field, each and every electron will gain spin. So that is solved.

What this means is that if we are going to assign a particle a number for spin, the number should apply to the real spin, not a fake spin. Giving particles fake spins and then trying to order them based on those fake spins is not physics. It is a form of mental illness.

As for photons, they are no different than electrons or protons, except that they are much smaller. They have real spin, and can be either 1 or -1 relative to one another. In other words, they can be up or down photons.

Fields of photons, electrons, protons, and other particles can all be polarized or spin-matched, in which case they will be “bosonic.” This can be done with any matched particles, but it cannot be done equally easily. In the same way, if we superfreeze this field of particles, it will act like a B-E condensate. This can be done with any field of matched particles, but again it cannot be done equally easily. It is simply easier to polarize and then superfreeze Helium4 than most other particles, which is why we discovered it first.

So Helium is neither a fermion or a boson. It only accepts polarizing and superfreezing in a consistent and fairly pliable manner. The question is now why? If Helium4 is an easy condensate, and if this is not explained by its being a boson, what explains it?

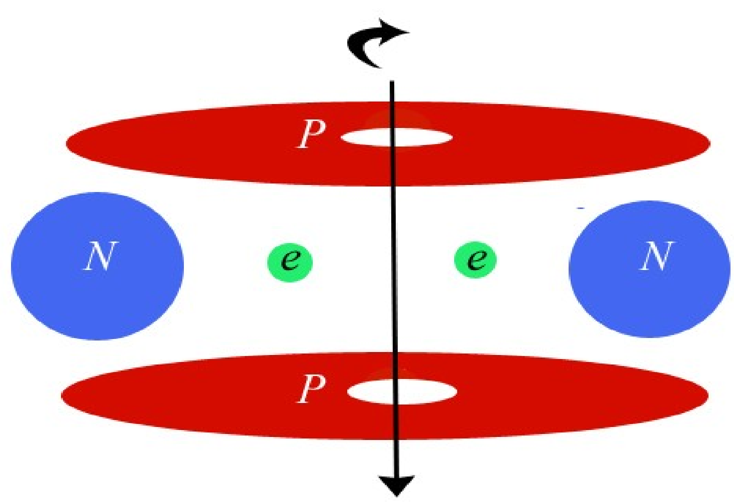
Before I show you, notice that this is why the fermion/boson difference is still trumpeted so loudly: it covers up the necessity of finding a real mechanical explanation. If these superfluids can be explained with one word, boson, there is no need to go in and try to actually understand what is going on. As usual, real physics is covered up—and ultimately prevented—by fake physics.

The only thing current theory has right is the central place of spin in the explanation. But since they don't make the spin real, this is either luck or coincidence. Remember, the spins of bosons are intrinsic, not real. They call Helium4 a boson, so they are not using real spin to explain any of this. You should have asked yourself long ago how Helium4 can have an integer spin, just like the tiny photon. In fact, Helium4 is given even less spin that the photon, with Helium4 having spin0 and the photon spin1. Of course, in the current models that doesn't mean Helium4 has less spin than the photon. It just means the manufactured vectors work out that way. In these models, zero is not less than 1. But still, it is curious to find Helium4 having any spin similarity to the photon. Helium4 is not a fundamental particle like the photon, so it is strange to see compound particles being called bosons. The wavefunction isn't found in the same way, just as a start. The wavefunction was originally applied to electrons, remember, and Helium4 is two electron, two protons, and two neutrons. And yet the total spin quantum number of the complex is treated just like the constituent spin quantum numbers. Either level can cause the particle to be called a boson.

Shouldn't the spin of Helium4—even if it is intrinsic—be at a different level in the field and in the math than the spin of the electron or photon? Since the electron *composes* Helium4, the two spins can't be at the same level in the math. So how can they be represented by the same level of numbers? In other words, the numbers .5 and 1, as applied to electron and Helium4, are both at the same level of size and counting. This despite the fact that Helium has a mass over 7,000 times the electron. And we can apply this logic to the photon as well, which I have shown is 3 million times smaller than the electron and 23 billion times smaller than Helium. Does it make any mathematical sense that "bosonity" or "bosonitude" can be determined by either level of spin? No.

It turns out that what makes Helium prone to superliquify is the way it breaks local spin symmetry. Remember above, where I said that bosons appear to break spin symmetry, in that we have a lot of same spins in the same general area? This is true, and it doesn't break any laws since spin symmetry doesn't apply locally and never did. If symmetry applied to small areas, we couldn't have any spin differences to start with. This is one of the many things the matrix math often fails to account for, and it explains why and how the gauge math has failed to model beta decay. Yes, when particles like photons are "created" in decay, the symmetry law kicks in; but when we just have particles collecting in a general area, there is no requirement they maintain spin symmetry. The excesses in spin can be shed into surrounding areas, maintaining global symmetry.

So, again, the spins we are now looking at are real. The Helium nucleus has a real spin about its central axis, as in this diagram:



[I have drawn the protons as disks, as seen from the side. This simplifies the diagramming. To see more on nuclear construction, you may go to my long paper on that. We can ignore the electrons in this analysis, since they are just along for the ride.]

With Helium4, the two protons are spinning in the same direction, so in the first instance we have double the local spin of Hydrogen. If we use an external field to spin-match all the Helium atoms, our internal field is now extremely high in total spin. Since spin is what physically channels charge, we will have very strong charge channels. And the denser our field is, in either charge or Helium, the stronger the channeling will become. This creates the possibility of superfluidity at any temperature.

However, when all charge is moving in the same direction, as here, the charge field can become so dense it resists itself. In short, we have so many charge photons in such a small space, they can't all get through the channels in the nucleus. This sort of photon density only occurs in the nucleus itself, so this phenomenon isn't found anywhere else.

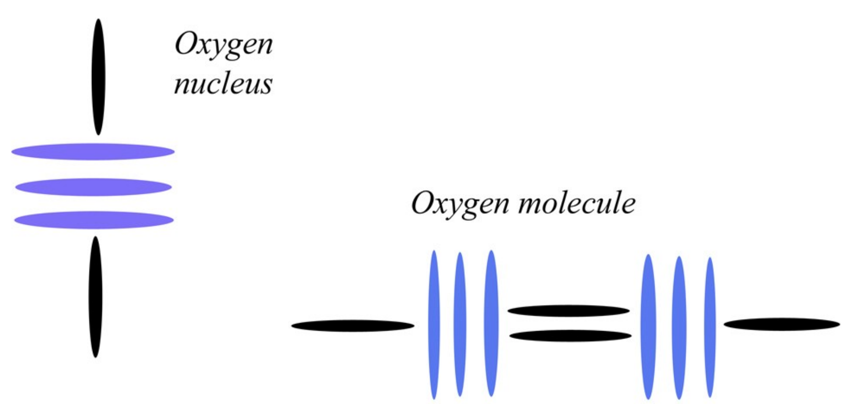
This is where freezing comes in. Freezing basically removes a lot of the charge photons, giving us a lower density. That is what heat is: photon density.   So we now have an optimized channeling as well as an optimized photon density, creating an optimized conduction. Conduction is charge channeling, you see. This is the mechanical explanation of superconduction.

But why Helium and not (usually) any larger elements? I have explained how Helium has more spin than Hydrogen: that is almost self-explanatory, given my diagram above. But why not diatomic Hydrogen, which has a similar diagram?   Why not Oxygen, which I have shown has three same-spinning protons in the central level? And why should Rubidium be a candidate, as with Cornell-Wieman-Ketterle in 1995?

Let's start with diatomic Hydrogen. Yes, orthohydrogen can match the proton spin configuration of Helium, but it doesn't have the neutrons. This matters because the axial charge channel of orthohydrogen actually competes with the equatorial charge channels. Remember, it is the axial charge channel that creates the diatomic bond, as I show here. All nuclei have both channels. But since Helium is monatomic, the axial channel is very weak. It doesn't become diatomic, and that is why: the axial charge channels aren't strong enough to create a bond. The double equatorial channels overwhelm the axial channel, and there is no axial or diatomic bond.

All this is caused by the neutrons present in Helium, which act to block the axial channel, forcing more charge to channel equatorially. This tends to optimize charge in the equatorial plane, across the entire gas or fluid. Orthohydrogen doesn't have this optimization. Some of the charge is dissipated in the along the z-axis, and so the x-y plane is weakened relative to Helium. For more on charge channeling or blocking by neutrons, you may go here.

Now Oxygen:

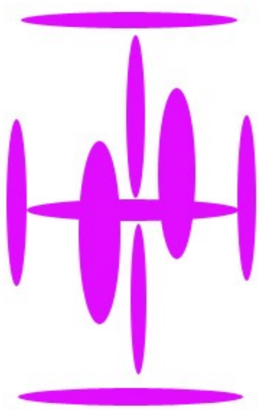


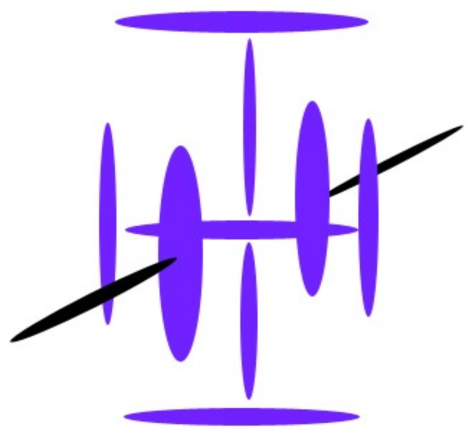
Although Oxygen has those three same-spinning alphas (blue disks) at the center of each nucleus, it again has a strong axial channel, which is what creates the diatomic bonds. The black single protons in these diagrams show the direction of axial charge channeling, since they act to augment it. So although Oxygen channels very strongly equatorially, it also channels quite strongly axially. This axial charge channeling tamps down or interferes with equatorial channeling, which means that Oxygen isn't as spin maximized as a planar field as Helium. As much as possible, Helium keeps all channeling in one plane, which is what we want if we are going to create a condensate.

Now Rubidium. If you study the nuclear diagrams in my many papers, you find that, generally, larger atoms create axial channels that interfere with equatorial channeling. They therefore aren't good candidates for condensation. But since Rubidium is just one proton up from a noble gas, it can be forced into a condensate in the right conditions. Here is why: the noble gases aren't good candidates because they aren't channeling strongly in any direction. All the top level alphas are perpendicular to the charge field, and channeling is weak both axially and equatorially.

Here are the diagrams for Krypton and Barium, for example:

Krypton





Barium

[Notice how the noble gas krypton has no fourth level “prongs”. It is complete at the third level, and all the disks are perpendicular to the external charge field. This is why it channels charge poorly.]

Now, as we go from group 1 across the periodic table, we plug protons into the outer positions. Normally, the axial position takes precedence, and Rubidium with its one proton in the outer shell will place the proton on the axis. It does this to minimize the lopside. All larger nuclei tend to spin first on the carousel level, since that is the natural place for spin. If you plug in one proton on the carousel level, you create a spin imbalance. So Rubidium prefers its outer proton on the axis. This is the way I have diagrammed it previously. But if you wish to create a condensate from larger nuclei, group 1 is a logical choice, because with group 1 you only have to force one proton to move from axis to carousel. If you chose group 2, you would have to force two protons to move from axis to carousel, and so on. This is why Rubidium was chosen, or why it worked. If you apply a very strong external charge field to a sample of Rubidium, and that charge field is heavily directionalized, it will reposition the outermost proton of Rubidium, forcing it from axis to equator. This charge field has to be applied in the equatorial plane of Rubidium. In that case, it will act like a very strong wind, overpowering the charge channel that is keeping the proton in its "hole." The proton will move to a carousel position, where it can align to the applied field. Once there, it will augment the equatorial channel of the nucleus. The loss of the proton at the axis will short-circuit the axial charge channel, giving us a planar charge field, as with Helium.

I will be asked, "Why doesn't the entire Rubidium nucleus simply turn 90 degrees, to align its axis proton with the applied field?" It can't do that, because the applied field has already increased the equatorial or carousel spin. The nucleus is therefore like a pinwheel or top, and acts like they do. In other words, it resists being turned. The faster it spins, the more it resists being turned. It *can't* align the axis to charge, because the equator is already strongly aligned to charge.

I think anyone can see that this mechanical explanation of condensation is much simpler and more logical than the old statistical explanation of Fritz London from 1938 (which still stands). Calling Helium4 a boson to explain superfluidity could scarcely be more illogical. As I have shown, it isn't statistics that explain superfluidity, it is local spin mechanics, where all spins are real. As a general rule, any time you see a statistical solution to a mechanical problem, you should know you are looking at a fudge. If a physicist could tell you a simple mechanical solution, he or she wouldn't need to fall back on a statistical solution. We have gotten nothing but statistics for a century because that is all particle physicists have been capable of.

Before Bohr and Heisenberg, physics was always the search for a straightforward mechanical explanation. Because Bohr and those after him weren't good at mechanical explanations, they provided what they could: statistical explanations. More than that, they tried to convince the world that statistical explanations were more regal. You were supposed to believe that more complexity and more opacity was preferable for its own sake. Feynman was the ultimate master of this sort of patter, making huge finessed equations seem superior to simple ones. He was constantly bragging about the number of years in graduate school it took to learn these finesses, and the phonies ate it up. They didn't boo him off the stage as a charlatan, they applauded him wildly and put his picture up over their beds. They too wished to be able to fill blackboards with Hamiltonians (and chase short-skirted assistants in sunny California). Real physics was just so—well, boring!

If you also wish to spend years learning finessed math and fake physics, go to it. I am not stopping you. In the plastic future, you will probably always have a job. But if you want real solutions to these problems, I suggest you come with me.