UNA PROFESIÓN
singularmente no femenina

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*Esta conferencia, que se enmarca en el Ciclo "Si ellas pudieron, tú también", organizado por la Universitat de València, se inspira en el relato autobiográfico "**Una Profesión Singularmente No Femenina**" que la Profesora Mary Katharine Gaillard publicó en 2015 y en el que describe episodios de su muy exitosa carrera investigadora en una disciplina científica muy competitiva –la Física de Partículas Elementales– que, en los años 60 y 70, estaba predominantemente dominada por hombres.*

Su recorrido por este mundo de la física fundamental refleja un talento, una dedicación y una determinación verdaderamente formidables. En palabras de Richard P. Feynman, aquellos años, una época probablemente irrepetible, fueron decisivos para establecer las ideas fundamentales que desembocaron en la formulación del Modelo Estándar de Partículas e Interacciones, una de las creaciones científicas más extraordinarias de la segunda mitad del siglo XX. Los logros científicos y la visibilidad conseguidos por Mary K. Gaillard han marcado un punto de inflexión en la lucha de las mujeres para integrarse en un campo de la investigación que hace 50 años contaba con muy poca presencia femenina.

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I have often been asked why I wrote a book. I was having dinner with someone I invited to speak at LBL and his wife. And he spoke about one of his books on the history of physics, it was his third book on the subject. And when dinner was over and we were about to leave, his wife said, "So when are you going to write your book?" And I kind of said, "Huh." And she sort of gave me the impression that it was my duty to write a book, as an inspiration to younger aspiring women physicists. So I started thinking about it a little bit. In a few days later, I had lunch with some Berkeley graduate students and we chatted about this and that. And then all of a sudden one of them said, "Tell us about your own experiences." And then I realized that that's what they really wanted to hear about. And so I did tell them and when I told them some worst of my experiences, one of them said, "Why didn't you give up?" And I answered in part that a longtime friend of mine had told me once that I seem to have something of a survival mechanism. Which I guess means that I was able to survive when many couldn't.

The title of my book comes from a remark that was made to me when I must have been a senior in high school because I had just begun studying physics. Someone asked me what I wanted to do in life and I said, "I want to be a physicist." His reaction to that remark was not positive, but I didn't pay much attention to him. It was one of the very few negative feedback that I got when I was young. The first 20 years of my life, were pretty positive. My parents didn't make any distinction between what they expected from my brother and what they expected from me. I had high school math and physics teachers who were men and football coaches. But they also didn't make any distinction between men and women. They'd call on people equally. I was in a small public school in Ohio, but it just happened to be a good place to be, I guess.

We didn't have much money and I went to the college that gave me the highest scholarship. Which happened to be a small women's college in Virginia with one physics major every two years. But there was a woman there who was the head of the physics department, which consisted of two people, and she had been at Yale. She had been very active in physics. She had worked with Oppenheimer, for example, but she had been at Yale on soft money. And when her husband died, her husband was a professor at Yale, they let her go. She was offered a job at Columbia, but she had young kids and she didn't want to bring them up in New York. So she happened to move to this school where I ended up in and that was probably the best thing that ever happened to me.

I spent a year in Paris as an undergraduate, and she got me to go to work at the *École Polytechnique*. Leaks is a nickname for the *École Polytechnique*, which was one of the two most prestigious *Grande École* in Paris. And there was a cosmic ray lab that got me introduced to sort of something close to particle physics. Then, after my junior and senior year, she sent me to Brookhaven National Lab as a summer student. And there, that's where I got hooked on high energy physics. My supervisor nominally was Bob Adair from Yale, and he gave lectures to myself and another summer student. He taught us the elements of particle physics as they were known at the time, the four forces of nature, which is the first thing everybody learns. There's a strong force which is responsible for nuclear interactions. There's the electromagnetic force, which is responsible for electricity, magnetism, light, most of what we observe in everyday life. And weak interaction, which is responsible for the decay of unstable nuclei. And then, finally, gravity, which along with electromagnetism, accounts for everything we experience in the everyday life. Gravity, electromagnetism have an infinite range, which is why we experience them. The other forces are only effective over a distance scale so small that we can't observe them directly. They have to be observed in laboratory experiments. And then, there were the particles that participated in these various forces, protons, neutrons, and some other unstable particles and so forth. All charged particles participate in electromagnetic interactions, neutrinos in the weak, along with everything else, and everything is subject to gravity.

Now, although Bob was my supervisor, I ended up working with the Columbia group. And then I ended up going to Columbia for graduate school. In class we were probably two women out of 60, but I was fine. I had a lot of friends from Brookhaven. They were all male, but I worked with them. I never felt isolated. And so there was nothing up to that point in my life, which seemed to be presenting a hurdle. But then I married a Frenchman and moved to Paris after one year of graduate school at Columbia. And that's when the bombshell went off. I was told by a friend that I wouldn't get accepted into the theory group. This wasn't meant as a sexist comment. It was just you didn't do theory. Nobody could do theory. I mean, and we got the same line at Columbia, actually, the entire class was all told that, "Don't expect to do theory. You won't be able to." So they told me I should get into a lab because most of the students who were enrolled in the classes I was taking were already affiliated with a lab. So I did the rounds

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of all the labs in the Parisian region and was turned down by every one of them. And this was really a very unpleasant year. At *École Polytechnique* I was told that I came to France to get married and not to do physics, that I did things backwards. Again, you're supposed to do your undergraduate work in Europe and your graduate work in the States. And then, eventually, I got a job at a scanner pass. A scanner is somebody, they were essentially all women at the time, who inspected photographs from various detectors, bubble savers, cloud savers, whatever, and looked for tracks. So they offered me a job and told me I'd never get my name on any paper. That was a pretty devastating encounter. And I was about 10 feet away after I closed the door to his office when I burst into tears. Then a theorist at the *École Polytechnique* said to my husband right in front of me, "I don't doubt her ability because you already bought the merchandise." Now, besides being insulting, that was about the stupidest remark I could imagine. But he said he had already a student and didn't want any more.

And then a theorist at Saclay said to me -he was the only one that even brought up the idea of a reference-, "You couldn't get a recommendation from somebody like Leon Lederman, could you?" And I said, "Yes." And so he changed the subject and told me to go into another field. And then finally my husband's boss at Saclay-- my husband was in an experimental group at Saclay, said he would hire me. I mean, he was not French. He was a very nice Brazilian. But then it turned out-- Saclay was like an atomic energy lab in the States at the time. And when they found I was pregnant, they wouldn't hire me because of radiation hazard. I always wondered what happened to women who got pregnant once they were hired, but anyway.

So that was that

So that was that. And aside from this nice Brazilian, everyone told me that they only took students from the two prestigious schools in Paris, the *École Polytechnique* and the *École Normale*. And those were all male at the time so it was a pretty hopeless situation. I kept on taking courses. In the middle of the year, my husband was going back to Columbia to finish the experiment that he was doing his thesis on which actually turned out to be a Nobel Prize-winning experiment. And so I went back to Columbia and I audited some courses to try to keep up with what I was supposed to be learning in Paris. Anyway, I was happy to be back in Columbia. Then I came back to Paris. I passed the exams in Orsay, and I came out second, and I was taken into the theory group. So I could have maybe skipped all that trauma if I hadn't listened to the friendly advice that I'd gotten in the beginning.

Right after that, we moved to Geneva because my husband was offered a junior staff position at CERN. And so for the next 10 years, I was an unpaid visitor at CERN. I mean, I was being paid by the French, but I was a visitor at CERN. I had three babies and wrote two doctoral theses and commuted

back and forth to tutor students from the *École Polytechnique* to earn my keep. And I started doing research. Now, my research at that time focused on two recent discoveries. One was a discovery that nature is not invariant under the combined operations of turning particles into their antiparticles. That's called charge conjugation and parity which just changed directions, in two dimensions it is like mirror reflection. So if Alice goes through the looking glass and turns herself into the anti-self she should come out looking the same but she doesn't obviously, and CP is violated. And this was the subject of my first paper. And the other important development was that protons and neutrons and other particles were replaced as what we consider the fundamental constituents of nature by quarks. For example, a proton is made out of two up quarks and a down quark, and a neutron is made out of two down quarks and an up quark. And the quark model was actually developed by observing the symmetry patterns in nature. For example, the strong interactions are unchanged if you turn an up quark into a down quark which means you turn a proton into a neutron just by rotating the picture on the left into the picture on the right.

The quarks are like little spinning tops. They can have their axis's spin up or down in any given reference frame, but no two quarks of the same flavor which are just sitting at rest can have the same spin orientation. But for example there was a particle called the omega-minus which was made out of three strange quarks, those are the ones that are constituents of unstable particles, and for example, you can't have an omega-minus which is made out of three identical quarks all spinning in the same direction. We know it has spin stasis where all the quarks are spinning the same direction, but if each quark is a different color then it's allowed. And so now we had a new picture of particle physics and instead of protons and neutrons and so forth we had three different kinds of quarks, up, down, and strange, each coming in three colors, and then we now had two neutrinos -the discovery of the second neutrino was, in fact, the experiment that my husband had

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been doing his thesis on- and then the weak and gravitational interactions.

My work for several years had to do with quarks but in the meantime, I moved up from the basement office where they first put me with five people and a dog, and we moved up a floor at a time to office with just one other person. And finally, I got to move to the fourth floor where all the other theorists were, and I had an office all to myself. But all that time, I was still an unpaid visitor at CERN. And one day the CERN theory group leader came to me and said, "We can't offer you a junior staff position because if we did, you'd have to leave at the end of the six years." And I thought that was kind of weird because my husband had come with a junior staff position, and when it was over, he stayed at CERN for the rest of his career, as did many other people. But they somehow felt compelled to come up with some excuse why they couldn't hire me.

In 1973, my family and I went to Fermilab for a year and that was an important year for me. First, it was a very liberating experience. For one thing, I was paid by Fermilab, and I just felt like a normal physicist, instead of some kind of strange aberration. And it was also the period of the birth of the standard model, so it was a very exciting time in physics. Up until 1970, the theory of electromagnetic interactions was very well understood and it was highly predictive. This was far from the case from the weak and the strong interactions. But in the early 1970s, similar theories for the strong and the weak interactions emerged, but with very important differences. In particular, the standard model interactions have symmetries that affect particles differently at different points in space and time.

So for example, the strong interaction symmetry where you turn an up quark into a down quark, that is a good symmetry of the strong interactions, if you rotate all the quarks all over the universe simultaneously. Whereas in the electroweak theory, you can change an up quark into a down quark here and leave one alone on the moon. Or in the strong interactions, you can turn a blue quark into a red quark here at the table and leave it alone on the moon, and so forth. But the new weak interactions theory was not viable without the combination of the electromagnetic theory into what's called the electro weak theory. So there was a sort of a partial unification of two forces. But the theory only made sense if there were a new particle, a new quark, and that quark had been called charm earlier. And Ben Lee who was the group leader at Fermilab and I would use the experimental data to determine the mass of the new quark. And then with John Rosner we delineated the properties of the particles containing charm quarks and discussed how one should look for them. And our preprint came out in 1974. In November 1974, the first particle containing charm was discovered. And finally, after some agonizing waiting, actual charm particles were definitely established. I must say that was the most exciting period in my career. And it also represented a turning point.

The search for charm was difficult and it was confused in part by the fact that there was a new lepton. A lepton is an electron or a muon. It's a word for electron, muon, and a third one now called tau. These are particles that have no strong interactions but are charged and have electromagnetic interactions. So, while people were looking for charm, they found this tau particle that had roughly the same mass as charm particles, and once that tau particle was established, for the reasons of consistency of the theory, this immediately led to the prediction of another neutrino that goes along with that and another quark doublet which we call top and bottom.

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In 1976, we had a new picture. There are altogether six types of quarks, each in three colors, and no one knows what mediates the interactions. They are eight colored gluons -they're called gluons because it's a glue that holds the nucleons together. We had the electromagnetic interactions which have now become the electroweak interactions. Now there are four mediators, the photon and the three heavy particles called WNC weak neutral bosons, and there's this thing over in the corner called H. And that's the Higgs particle, that's what gives mass to the heavy WNC and the quarks and the charged leptons.

This picture was complete in 1976 and it's never changed. It took some 40 years for all of the entries to be filled in, namely the Higgs particle was discovered on July 4, 2012. And actually the graviton, the mediator of the gravitational interactions, was proposed because there has to be a quantum-- we don't know the quantum theory of gravity, but there has to be one.

So after that year in Fermilab, I went back to CERN. I gave lots of talks about charm. I started doing research with John Ellis and others. And one of the things we did was to delineate the properties of the Higgs boson and discuss how to search for it. There's an important difference between the new electroweak theory and the new strong theory. In the strong theory, the symmetry that turns a green quark into a red quark is unbroken. It's an exact symmetry of nature. So as a consequence, you can never tell in any experiment whether you've produced a red quark or a green quark or a blue quark, because you can't tell the difference. The symmetry is unbroken. The electroweak theory has this symmetry, is a symmetry of the laws of physics. However if the Earth were a perfect sphere and had no magnetic field, an ant crawling around the earth would have no idea where it was. If you turn on the earth's magnetic field and give the ant a compass, then the ant can tell whether it's going north or south, but not east or west. So the spherical symmetry of the sphere without a magnetic field is broken spontaneously by the magnetic field to a cylindrical symmetry. We can imagine an abstract sphere in color space where the North Pole is blue and the South Pole is red. And there's no experiment that you can do that will tell you where you are on

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that sphere. That's just a statement that you don't know what color quark you produced in the experiment. On the other hand, I can make an imaginary abstract sphere that represents the electroweak symmetry with say, the proton on the North Pole and the neutron on the South Pole. And what the Higgs field does is spontaneously break that symmetry, just like the magnetic field in the case of the Earth. It breaks the spherical symmetry to a cylindrical symmetry. The residual symmetry is reflected in the fact that the photon has no mass but the W and the Z particles acquire masses.

For example, if I drop an olive in a glass of wine, it lands at the bottom of the glass and the cylindrical symmetry is unbroken. Whereas if I have a martini glass with a bump on the bottom and I drop an olive in, if the olive lands precisely on the top of the bump you don't break the symmetry. But that is an unstable and unlikely situation. Well, the olive is going to fall down somewhere. It doesn't matter where, but it picks a spot and that breaks the cylindrical symmetry. And then, if you stir the cocktail, then you restore the symmetry because the olive is just bouncing around. And that is actually an analogy for the very tiny, tiny fraction of a second of the very early evolution of the universe. We wrote our paper in 1976. The Higgs was found in 2012. That was a very long wait. We also predicted the existence of gluon jets. People had observed quark jets. In other words, you have collimated beams of particles that reflect the motion -we don't observe quarks and gluons. We observe strongly interacting particles that contain quarks and gluons. If you produce a pair of quarks, then you get these collimated beams of particles they reflect motion of the quarks, the direction of motion of the quarks. And what we predicted was that sometimes you would produce a gluon along with the quarks and then you would have three jets of particles instead of two. Finally with Mike Chanowitz we predicted the mass of the bottom quark. And that was discovered very shortly after our prediction came out.

As time went on, I became more and more unhappy with my situation at CERN and started spending a lot of summers at Fermilab, just to get a breath of fresh air and a different atmosphere. CERN staff did not have students, but I was paid by the French. And I was expected to have students, but I should've realized that I was going to have trouble placing my students. Because at the time in France, people took the best students from their own groups and they didn't take anybody else's students. And I didn't belong to any French group, normally I was at Orsay, but they didn't consider me one of theirs, so they told me they wouldn't take my students. Then I started my own theory group at Annecy and I started commuting between Annecy and Geneva few times a week. Then in 1979, I was an invited speaker to international conferences. One was in Geneva. I spoke about strong interactions. One was at Fermilab. I spoke about weak interactions. My collaborator John Ellis told me that after the series session, which consisted of three speakers, myself, John Ellis, and Frank Wilczek who went on to win a Nobel prize for his contribution to the theory of strong interactions, that he overheard a Japanese elder statesmen say in disgust, “A woman, a hippie, and a schoolboy." But I was never a speaker at the International Conference on High Energy Physics. I mean, never a plenary speaker. And I was told by the theory group leader that one of the organizers of the conference in Tokyo in 1978 wanted me to be a plenary speaker, but Japan couldn't pay for Western speakers and he wanted CERN to pay. But CERN wouldn't pay me because I wasn't a staff member at CERN. There's no way Orsay would pay me because I was never there and so that was that.

Then my friend Ben Lee tragically was killed in a car accident and the following year they organized a conference in his honor in Korea, which was just before the Tokyo conference, and there was no way I couldn't go to that, and the group leader recognized that. So he agreed to pay me provided I found the cheapest possible travel, and I made really complicated travel arrangements, and when that was all done a certain staff member came up to me and said, "Oh, one of our delegates decided not to go. Do you want to go in his place?" So, in other words, they couldn't afford to pay me if I was supposed to be a principal speaker, but they could send me in as a substitute for a delegate, and that just kind of was the last straw. So for the last three years I was in the Geneva area I was commuting to Annecy usually with my CERN colleagues and I had already an offer from Fermilab since 1978. Then a few months after a talk at Berkeley I was offered a position at Berkeley. And so I was sitting on this office wondering what I should do. Then for a reason that I don't quite remember CERN the theory group had a staff meeting on women, but I think it maybe had to do with the fact that someone didn't get a CERN fellowship, but since there were no female CERN staff members, they invited me and they invited the secretaries so they could have some female presence at this meeting, and towards the end of the meeting somebody said that women don't have any more obstacles in doing physics than men do, and I kind of blew my top and said I could write an essay on the subject, and as we were leaving, somebody, no less than John Bell, said, "Why don't you?" And so I did, with the help of a whole lot, lots and lots of other women, and we came up with what is the CERN report on women in physics. Which, actually, I learned a few years ago, when CERN finally got a diversity program, they were using that report as a starting point because they didn't know where to start from. I think one outcome of that meeting was that a CERN fellowship was offered to a woman. But anyway, so our report was distributed as a CERN publication with the only restriction that we take the cartoons off the front cover and put them on the inside of the cover. The cartoon by Wolinski says, “What? There's a woman at the Academy. Can't she wait until the end of the session to clean the room?” That came out when a woman had, for the first time, been elected to the very prestigious French Academy. The real French Academy, not the Academy of Science.

Just about that time, a position opened up for a position of director in the CNRS, the French research organization that I worked for, and also a CERN theory staff position. And contrary to custom, in both cases, outside letters were requested. The person who asked for it in the French case was Raymond Stora. He was the head of the relevant committee. And the Paris people were pushing someone, one of their own, but because of the letters, I got awarded the directorship.

The impostor syndrome

Then I basically decided I was going to leave, but in the meantime I had to organize a school together with the same fellow, Raymond Stora. I was the scientific director, he was the administrative director. It was a six-week school, and I don't know if the fact that there was a woman director had anything to do with it, but we had an enormous number of first-class applicants who were women. Without even thinking about anything, gender, anything, we ended up with 14% of women, which is close to the average number of women faculty members in the States, today. It was really remarkable for the time. And it was a very good school. The majority of the students are still active in high energy physics, including the majority of the women. It was just a big success. It was really a nice thing to do before I left Europe. And finally, after agonizing all summer about whether to go to Fermilab or Berkeley, I left for Berkeley in September with my two younger kids - the oldest was already at the University of Washington. My husband and my son's dog followed a month later. The standard model was complete on July 4th, 2012.

Going back to the question I was asked by that graduate student, "Why didn't you give up?" Well, many people did and some still do. I think people are not quite so blatant as they were in my day. Now prejudices are more implicit rather than so explicit. For example, when I was, in 1992, just beginning research, still a student, somebody, a well-known physicist, in the cafeteria asked me what I was doing. I said I was studying theory and my husband was an experimentalist. And he said, "No, that's the wrong way around. Women are supposed to do experiment and men theory." And I thought it was an insult to both women and experimentalists.

And then, of course, there was the secretary effect. Men would walk down a whole hallway full of open doors until they found a woman and then they would ask for their directions or whatever it was they were looking for. And so it was actually some of the women in technical positions at CERN started wearing pants because most of the secretaries were well dressed. So that was a way to distinguish oneself. And even as late as 1977, I was going to DESY as an invited speaker about a machine DESY was very interested in. And on the train, the director of DESY invited me to his boxcar, offered me a drink and then he spent the whole time telling me that his wife had done the correct thing by giving up her aspirations as a physicist to raise her children. And I thought that was just outrageous. I mean, I was traveling as a guest to his laboratory.

There's also the impostor syndrome. I didn't even know this expression until a few years ago, but I certainly experienced it. The idea that everybody else is better. And I mean, it took me a very long time to figure out that I wasn't worse than all these people who were being offered CERN positions, starting from a CERN fellowship, a postdoc, to a six-year position and so on. I mean, it really took me a long time to figure that out. And at some point, I read Betty Friedan's book and was kind of eye-opening.

Has there been progress? Yes, there has been. When I was a student in 1960, I was one of two women in a class of 60 at Columbia. As of last year, we had 23% women at Berkeley. And Berkeley's probably not the highest, actually. In 1981, I became the first woman on the Physics Faculty at Berkeley, out of 38. As of last year there were nine. That's a measure of progress. When I was elected to the National Academy, I was one of three women out of more than 100. As of last year, there were 14 women out of 196. For many years, I was the only woman on

National Committees. And actually, as late as 2009, I was astonished to find myself on a very big oversight committee as the only woman. But nevertheless, since about the year 2000, there have been lots of women who have taken over governance positions in our community. So there's progress. There's measurable progress. It could be better, the numbers could be better. But what's even more worrying is that we still have reports of put-downs, misogynist comments from our graduate students. In recent years, it's even been worse with the issues of sexual harassment. So there's certainly a long way to go still.

And as for the Physics story, well, that's more complicated. There are many things we still don't understand. We know that the neutrinos have small masses, tiny masses, but we don't know where they come from, we don't know what dark energy is, and we know even less about dark matter, we don't know how to incorporate gravity into the picture. We don't understand the pattern of masses, we don't understand the ratio between the vacuum value of the Higgs, which is the distance that the olive sits in a glass away from the center, relative to the energy where gravitational interactions become strong. This last issue was the rationale for the SSC and the LHC. The SSC which died and the LHC which found the answer in the form of the Higgs boson. But we expected the LHC to find a lot more than the Higgs. And so what's next? Well, there's a lot of data in cosmology, which is very exciting, cosmology and astrophysics. There is the intensity frontier, which is looking for rare processes at low energy, including the studying of neutrino properties. So those are good things going on. But what about the energy frontier? Which means looking for new, heavier particles at ever higher energies. In fact, the outlook is not very bright.

Bio

MARY KATHARINE GAILLARD

Prestigiosa física teórica estadounidense especializada en Física de Partículas Elementales. Es profesora de la Universidad de California en Berkeley –UCB– y científica invitada en el Laboratorio Nacional Lawrence Berkeley –LNLB–. Fue la primera mujer en la Facultad de Física de la Universidad de California en Berkeley, donde dirigió el Grupo de Teoría de Partículas Elementales en 1985-1987.

Los logros en investigación incluyen trabajos pioneros con Benjamin W. Lee sobre la evaluación de correcciones de interacción fuerte en transiciones débiles, incluyendo la predicción exitosa de la masa del quark c (*charm*). También ha trabajado con John Ellis y otros investigadores en el análisis de estados finales en las colisiones electrón-positrón, incluyendo la predicción de eventos de tres jets, y estudios de teorías gauge unificadas, incluyendo la predicción de la masa del quark b (*bottom*). Ha realizado estudios con Michael Chanowitz de señales medidas en colisionadores protón-protón que mostraron, en líneas muy generales, que la nueva física debe presentarse a energías suficientemente altas.

Su trabajo en los últimos años se ha centrado en las teorías de súper-gravedad efectiva basadas en súper-cuerdas y sus implicaciones en fenómenos que pueden detectarse tanto en experimentos de aceleradores como en observaciones cosmológicas.